IN THE UNITED STATES DISTRICT COURT
FOR THE MIDDLE DISTRICT OF PENNSYLVANIA

TAMMY KITZMILLER, et al	:	
	:	CASE NO.
v.	:	4:04-CR-002688
	:	
DOVER AREA SCHOOL DISTRICT,	:	
et al	:	

TRANSCRIPT OF PROCEEDINGS
BENCH TRIAL

MORNING SESSION

BEFORE: HON. JOHN E. JONES, III

DATE : October 24, 2005
9:09 a.m.

PLACE : Courtroom No. 2, 9th Floor
Federal Building
Harrisburg, Pennsylvania

BY : Wendy C. Yinger, RPR
U.S. Official Court Reporter

APPEARANCES:

ERIC J. ROTHSCHILD, ESQUIRE
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STEPHEN G. HARVEY, ESQUIRE
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For the Plaintiffs

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ROBERT J. MUISE, ESQUIRE
For the Defendants

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THE COURT: All right. Good morning to all. And we are going to take testimony out of order, is that correct?

MR. GILLEN: That's correct, Your Honor.

THE COURT: Okay. Are you prepared? Then you may proceed.

MR. GILLEN: Thank you, Your Honor. The defense calls Dr. Steve Fuller.

Whereupon,

STEVE WILLIAM FULLER

having been duly sworn, testified as follows:

COURTROOM DEPUTY: If you'll state your name and spell your name for the record.

THE WITNESS: My name is Steve William Fuller. S-T-E-V-E. W-I-double L-I-A-M. F-U-double L-E-R.

DIRECT EXAMINATION

ON QUALIFICATIONS

BY MR. GILLEN:

Q. Good morning, Dr. Fuller.

A. Good morning.

Q. We've brought you here to offer an opinion on behalf of the Defendants in this action, and I'd like to briefly introduce you and your academic credentials to the Court. Would you please give us your current position of employment?

A. I'm a professor of sociology at the University of Warwick in the United Kingdom.

Q. What is the standing of the University of Warwick as in the British education system?

A. It's normally regarded as one of the top five research universities in Britain, and we do have a national ranking system, so this is pretty consistent.

Q. Do you have a chair at that university?

A. Yes, I do. I've had that since 1999.

Q. And what does it mean to have a chair?

A. Well, in the United Kingdom, only about 10 to 15 percent of academics are full professor, which is what a chair amounts to. And I've held a chair in that sense since 1994, since moving to the United Kingdom. So I was also a chair at the University of Durham before then.

Q. Let's take a brief look at your educational background. Where did you do your undergraduate work?

A. I did my undergraduate work at Columbia University in New York, and I graduated summa cum laude in 1979.

Q. After that, did you go on for further study?

A. Yes, I won a Kellett fellowship to Cambridge University, which was my first trip to the United

Kingdom. That was in 1979. And I was there for two years. I earned a Master of Philosophy and then went on and did a Ph.D. at the University of Pittsburgh, which I completed in 1985.

Q. And what is the standing of the University of Pittsburgh as it relates to your academic pursuits?

A. My Ph.D. is in history and philosophy of science, and the University of Pittsburgh is probably the best department, certainly in the United States, and probably in the world.

Q. Okay.

MR. GILLEN: Your Honor, may I approach the witness?

THE COURT: You may.

MR. GILLEN: Thank you.

BY MR. GILLEN:

Q. Steve, I've just given you a copy of your CV, which is Defendants' Exhibit 243. I'd like you to take a look at that, and I'm going to ask you a little bit about your credentials. As we go on, let me ask you, have you been a visiting professor at other institutions?

A. Yes, at several different countries, in fact, including Sweden, Israel, Japan, and, of course, I've been back in the United States as well.

Q. In terms of your -- let's take a look, a brief look at your publications. Can you give us an idea in general for the number and kind of your academic publications?

A. Well, roughly speaking, I have 200 published articles or book chapters, vast majority of which have been peer reviewed. And also, I have a lot of book reviews and incidental pieces, including pieces in the media. And this has been over the last 20 years.

And in terms of books, I have -- well, nine books actually published at the moment. There will be two more coming out by the beginning of next year. And altogether, my works, one sort or another, have been translated into about 15 languages.

Q. Have you given academic presentations and talks?

A. Yes. I have given them throughout the world, 500 maybe altogether. They are listed in the curriculum vitae. They've been on every continent. Many keynote addresses in a wide variety of fields. Yeah.

Q. How many countries approximately?

A. About 25 to 30.

Q. I'd like to draw attention to two elements of your CV. I notice that you received a post-doc from, was it the National Science Foundation?

A. Yes, I was the first National Science Foundation post-doctoral fellow in history and philosophy of science in 1989, and that was at the University of Iowa.

Q. You mentioned history and philosophy of science. What was your nature of your work in that post-doctoral fellowship?

A. Well, I was working on the rhetoric of science, and that is to say, the means by which science is made persuasive for larger public social audience, and they have a program there. And the idea was basically to bring scholars into places where they would have some kind of synergy.

Q. Then in terms of firsts, I note you were also the first research fellow in the public understanding of science at the United Kingdom's Economic and Science Research Council?

A. Social Research Council.

Q. Thank you. What did that position entail?

A. Well, the United Kingdom has been very much in the vanguard of public understanding of science; that's to say, the need to study the role of science in society and how people perceive it. And I was the first fellow in this while I was at the University of Durham.

And during that time, I ran a global cyber conference where people around the world were able to discuss matters having to do with the, you know, their perceptions of science and forth. And a lot of different issues got raised in that context.

Q. You mentioned peer review. Do you participate in that process?

A. Yes, very heavily. In fact, I've just about peer reviewed anything you could peer review. I mean, people, books, articles. In my CV, I list -- I've peer reviewed for about 50 journals. I mean, at the moment, while I'm here I'm supposed to be peer reviewing eight articles, which I'm not being able to get to.

And these are in a wide range of disciplines, mostly in the humanities and social sciences, but there have been a couple of occasions in the natural sciences where I would be a peer reviewer, having to do with issues in the history, philosophy, or sociology of science that would arise in those adjourns.

I also peer review for academic publishers both in Britain and the United States. And I also peer review grant applications, including still in the United States, as well as in Britain for the European Union and for Australian and Canadian Research Councils. I recently chaired the International Advisory Board that basically signs off on peer review grants for the Academy of Finland, and -- yeah, that about sums it up, I suppose.

Oh, also not to mention tenure and promotion cases which are, in a sense, kind of, of that kind as well academically.

Q. You've mentioned that your work is in philosophy and the history of science. I take it that work started with your Ph.D. dissertation?

A. That's correct. Yes.

Q. Tell us about that briefly.

A. My Ph.D. at the University of Pittsburgh was done under the supervision of J.E. a/k/a Ted McGuire, James Edward McGuire, who's probably America's leading expert on Sir Isaac Newton's, the relation between Sir Isaac Newton's science and his religious beliefs.

I mean, my Ph.D. wasn't on that topic specifically, but I took a lot of courses with regard to that and have followed that up in many respects. But the Ph.D. itself was on bounded rationality in a legal and scientific decision making. And there I was --

Q. I'm sorry. Tell us, just give us an idea for what that bounded rationality means?

A. Bounded rationality is an expression from Herbert Simon, and it has to do with basically making decisions under conditions of material constraints; so whether we're talking about resource constraints, time restraints, so forth.

For Simon, who was a Nobel Prize winner in economics and originally trained as a political scientist, this was kind of the, main kind of reasoning that was involved in a field that he called the sciences of the artificial, which was meant to be a kind of universal science of design, and in which case, one could, as it were, interpret all sorts of issues that wouldn't be normally thought of as designed based issues as designed based ones.

Q. Do you see that work you did on bounded rationality as having relevance to this case?

A. Yes, indeed, because it seems to me that one of the things that's at stake here is the idea that intelligent design, as it were, is something more than just a kind of a fig leaf for the idea of God or some other kind of religious entity.

And the point here about Herbert Simon, who has no very clear, no theistic views whatsoever, is that he actually thought it was possible to have a universal science of design, and that was what the sciences of the artificial were about. And bounded rationality was a key kind of inference and form of reasoning within that.

Q. Let me take a brief look at some of your books. And just, we'll briefly describe the subject matter and how it bears on your expertise. The first book I see listed is Social Epistemology. Would you briefly describe the subject matter of that text?

A. Yes. Social Epistemology, it's not a phrase that I coined, but in the sense I'm most closely associated with it. It was the title of my fist book. It basically kind of lays out the foundations for the kind of work I currently do, which has to do with looking at the social foundations of knowledge, as the title indicates, both from an empirical and historical standpoint, but also what you might say, enormative in policy standpoint.

Given what we know about the nature of knowledge and how it's developed, what sorts of policy should we be setting for it, and how, and for whom. And that's the general scope of the book. And --

Q. I'm sorry. Does that book relate to some of the issues in this case?

A. Yes. The one chapter of my Ph.D. that I ever published is, in fact, a chapter of this book. And it's on consensus formation in science. And one of the things that I address there, which I do think is relevant to the case, is how exactly does consensus form in the scientific community.

Given that there are many scientists working in many different locations, how does one get a sense that there is a dominant theory or paradigm operating at any given point. And my view on this, which I developed, is, in fact, there is never -- it's very rare to actually find a decision point where you say, well, some crucial test has been done, and this theory has been shown to be true, and this one has been shown to be false.

But rather, what you have is kind of a statistical drift in allegiances among people working in the scientific community over time, and especially if you add to it generational change. What you end up getting is kind of a, what Thomas Kuhn would call, a paradigm shift; that is to say that, where over a relatively short period of time, simply by virtue of the fact that the new people come in with new assumptions and new ideas, that you actually do get a massive shift, but not necessarily because there's ever been any decisive moment where someone has proven one theory to be true and another theory to be false.

THE COURT: Wendy, is he going too fast?

COURT REPORTER. Yes.

THE WITNESS: I'm sorry. My apologies.

THE COURT: I sensed that. A little slower. And it's important that we get a good record here, so just take the pace down.

MR. GILLEN: I warned him, Your Honor.

THE WITNESS: I'm sorry. My apologies, Your Honor.

THE COURT: That's all right.

MR. GILLEN: It's just part of the process.

THE COURT: I'm trying to help Wendy out.

BY MR. GILLEN:

Q. Let's take a look at your second book, Philosophy of Science and Its Discontents. Briefly describe, if you would, the subject matter of that text?

A. Yes. This is a book, as the title may suggest to you, it's relatively critical of the current state of the philosophy of science. But one of the -- I guess the key thing, as far as this case is concerned, that is of interest, is that I very strongly identify myself as being a philosophical naturalist.

Q. And if you would just briefly explain what that means?

A. Well, a naturalist basically is someone who believes that everything that happens in reality, as it were, can be understood as part of the natural world. And more specifically, that can be understood in terms, at least in principle, in terms of the methods of the natural sciences.

And that includes human, social, life as well.

That's the general perspective that naturalism offers. And I identify specifically with that view in the book, and I haven't retracted it either.

Q. Well, let me ask you, does that philosophical disposition you've described relate back to your work with Newton?

A. Well, I mean, the issue here -- not in a very direct way actually. But it does relate to the idea of what happens over time regardless of where scientific beliefs come from, that there is a tendency, in fact, to be assimilated into this naturalistic view.

Q. Does it speak to science and the nature of science?

A. What does?

Q. Your text, Philosophy of Sciences --

A. Yes, it does. Yes. See, one of the problems that I argue about in the book is that there's a sense in which, if we're going to understand the nature of science, we have so sort of study it naturalistically. One of the consequences of that may be that we find out things about the nature of science that we didn't quite realize were true.

And one conclusion that I think is very relevant to this case is that, ironically perhaps, from a naturalistic standpoint, if you study how you actually come about to a culture or a society that thinks seriously about scientific questions and the way that we're used to, you may have had to start off with something like a monotheistic standpoint that, that may, in fact, be a natural fact about the way science develops. And that is a point that I first raise in that book and then subsequently develop.

Q. Let's look at your next book, Philosophy, Rhetoric and the End of Knowledge. Would you briefly describe what that text addresses?

A. Well, that one has to do again, as the title suggests, with the rhetorical character of science. And here, I think one has to understand rhetoric as kind of the arts and sciences of persuasion. And I'm talking about this here not only in terms of, as it were, how science or organized bodies of knowledge make themselves persuasive to the larger society, but I'm also talking about how scientists amongst themselves persuade each other to be part of a common group or a common paradigm that move together despite perhaps some internal disagreements.

And one thing I would say that is relevant to this case from this book is that, some concepts from this book have, in fact, been inspirational for people who have been writing about the rhetoric about how the neo-Darwinian synthesis was forged in the middle third of the 20th century, because that is an example of where there's been a lot of strategic ambiguity and suppressed disagreements among people operating in many various disciplines in order to move forward with this general picture that the neo-Darwinian synthesis puts forward.

Q. Does that text speak to the science, non-science boundary?

A. Yes, in the sense that this always has to be negotiated. It is, in fact, very easy, as it were, for things to fall out that, in a sense, the boundary between science and non-science isn't something one can ever take for granted. It is actively being negotiated at all times because there are all kinds of people who are trying to make claims that what they're doing is scientific.

Insofar as science is the most authoritative body of knowledge in society. So in that respect, there's a kind of policing, you might say, and an occasional negotiation of the boundary that takes place.

Q. How about your next book, Science. Give us an idea for the subject matter of that text.

A. Well, that book, in a way, really gets, I think, very close to the heart of the issues. This is a book that, in fact, I developed a part -- from my undergraduate teaching in Britain. It's been published both in Britain and the United States.

And the idea here is, I basically look at what is the concept of science from a social standpoint. So this is a book in a series called Concepts in the Social Sciences. And one of the points that I make very much up front is that, if you want to identify something as a science, it's going to be very difficult to identify it purely in terms of what the practitioners do, okay, because, in fact, if you look at the various fields that we normally call science, ranging from physics to chemistry to biology and including many of the social sciences and so forth, people are doing vastly different things even within the disciplines themselves.

So there's a sense in which one can grant that there's a lot of technical expertise required of people who do science and get trained in science, but that in itself does not explain the thing being science. There's something in addition. Okay. And that has to do with the way in which this body of knowledge called science relates to the larger society.

And in a sense, the question then becomes, how does science establish this kind of authority? And it's in this context that issues like testability, some of the issues that have been arising in this trial, are, in fact, quite important and, in fact, then serve as a kind of umbrella notion for understanding the way in which vastly different practices are relating to the larger society.

Q. Your next text is the Governance of Science. Give us an idea of the subject matter of that text.

A. Well, The Governance of Science again, as the title suggests, addresses sort of the political structure of science, you might say, and the occasion for it. And this is something I think that would be very familiar to people who are in the kinds of fields I operate in.

There has been a kind of, you might say, a shift in the burden of proof with regard to the way in which one defends the value of science in the post Cold War era. There's a sense in which the, if you look at the Cold War era, that was the period where science, especially in this country, in the United States, was very much centrally funded, where there were national agendas, where it was seen as very obviously a bowl work of national security.

And, in fact, in a sense, the Cold War was being conducted as a race between the U.S. and the Soviet Union, kind of at a surrogate level, as a science race. But now with the end of the Cold War, there's kind of an open question about what the value of science is.

So there's been a tendency to devolve funding away from the central authorities, from the Government. And then the question becomes, okay, if we're not worried about science as a bowl work to national security, why should we be supporting science, and should the state be supporting science, or should it just be completely devolved to private authorities? And that's kind of the central problem of the book.

Q. Does the text Governance of Science speak to the role of peer review in science?

A. Well, yes. And one of the things that it says is that, while the scientific community is nominally governed by a peer review process, as a matter of fact, relatively few scientists ever participate in it.

So if one were to look at the structure of science from a sort of, you might say, political science standpoint, and ask, well, what kind of regime governs science, it wouldn't be a democracy in the sense that everyone has an equal say, or even that there are clear representative bodies in terms of which the bulk of the scientific community, as it were, could turn to and who would then, in turn, be held accountable.

There is a tendency, in fact, for science to be governed by a kind of, to put it bluntly, self-perpetuating elite.

Q. Well, let's skip for a moment to your text Knowledgement Management Foundations. Is that a related work?

A. Yes, I mean the Knowledgement Management Foundations book, the phrase knowledgement management, which is probably one of the -- now one of the hottest topics in business school research in a way reflects kind of what's happened to organized knowledge in our time.

Namely, it's a kind of -- it's something that's seen as very powerful, very important as a resource, but as it were, doesn't have a kind of natural home anymore. So that when one talks about knowledge management, it could be knowledge produced not only in universities, but in R and D divisions of industrial labs, or think tanks, or all kinds of places.

And then the question becomes, is there some kind of, you know, organized uniform way of regulating what's going on, you know, given that the universities no longer seem to have a monopoly over this? So I deal with that. In that context, I actually spend more time talking about the role of peer review and the strengths and weaknesses of it.

Q. You've got a text entitled Thomas Kuhn. Would you give us the general idea for that text's subject matter?

A. Thomas Kuhn was probably the most influential theorist of science, certainly in the second half the 20th century, and maybe the entire 20th century. Certainly one still to this day, he is one of the five most cited people in the humanities and social sciences.

And he published this book called The Structure of Scientific Revolutions in 1962, which is probably the most important book that people in my field ever read and very influential outside of it.

What I argue in my book called Thomas Kuhn, which is probably the book that's been most highly reviewed, 50, 60 reviews, from the New York Times to esoteric academic journals around the world, is that basically his theory is not only false, but also in a way, bad policy, you might say, in terms of the way one thinks about the governance of science, and in a sense, has had a very bad influence in the way we think about science, because the key thing about Kuhn's book, and again, this is quite relevant to the case, is that, Kuhn is very big on the idea that, at any given point in the history of science, there is a dominant paradigm, and that's, in fact, how you know there's a science.

So there's always one dominant paradigm, and that the only way in which you can have alternative points of views that have anything any kind of legitimacy is if that paradigm is, in a sense, in a self-destruct mode.

So when it has accumulated so many anomalies, that then people start looking for alternatives. But otherwise, there is no incentive within science to be looking for an alternative while the dominant paradigm is still strong. It seems to me, while this may cover about 300 years of the history of physics, that's historically all that it covers.

And in any case, it is bad as a kind of policy recommendation in terms of how to organize your science generally.

Q. Well, looking at Kuhn versus Popper, does that take up the idea of normal science or paradigm that Kuhn developed?

A. Yes. I mean, Karl Popper had a -- Karl Popper is originally a Viennese philosopher of science who, under the Nazi occupation, moved to Britain and spent most of his career at the London School of Economics, had a very famous debate with Kuhn in 1965.

Popper was a believer that, of the idea that science was kind of the vanguard of what he called the open society. That is to say, a society where all claims in principle are open to criticism and that, in fact, the way we make progress both socially and scientifically is through mutual criticism and learning collectively through that mutual criticism.

But the question then becomes, under what kinds of social arrangements is that possible? And the big debate with Kuhn was basically over this point, because Kuhn basically said you really couldn't have science if, in fact, you allowed free flowing criticism at all times.

There's a sense in which science has to close ranks, has to be dogmatic, and, in a sense, has to start excluding people. And that's, in fact, one of the secrets of science's success, is that kind of monolithic structure that goes on as long as possible. And what I do in this book is basically take Popper's side of the issue.

Q. And is that -- describe just the thrust of your text as it relates to distinguishing Kuhn's position?

A. Okay. Well, it seems to me that one problem that we have nowadays where, you might say, the start-up costs for coming up with alternative theories in science are so high, not only in terms of the academic background that people need to have, but also the amount of material resources one needs to have to mount labs and research teams and stuff of that kind, that it's, in fact, very difficult in the current climate to mount very serious fundamental criticisms, because you really have to do a lot of front loading before you actually get to the point where criticism will be taken seriously at a fundamental level. And this is a relatively recent development, certainly a 20th century development.

Q. Is your discussion of Popper in this book linked to ideas of testability, and if so, how?

A. Well, Popper is primarily known in philosophy of science for having put forward the criteria of falsification, which is his preferred way of talking about testability, which is -- basically what you do is, you set up a very stiff test where, in a sense, if the theory actually passes it, it's kind of unique in passing it, you wouldn't expect it to pass it, and, therefore, it supposedly says something very significant about the theory's knowledge claims.

Popper primarily imagined this kind of in the context of what is known in the trade as a crucial experiment where, in a sense, you have a kind of two theories facing off over some kind of common phenomena where they say radically different things about.

And that's -- and the point being, right, how do you get two theories to be sufficiently equalized in status that they will be tested by one case? See, Popper is kind of imagining science is a bit like a game, right, where you go in and match and both sides are imagined to be fundamentally equal, and then they test their wits against themselves.

But, of course, in the kind of world we live in, theories don't come in equal. Some theories come in with a lot more resources, a lot more back story that provides a kind of authority and makes it very difficult for these theories to be tested adequately.

Q. You mentioned the Open Society. How about the Open University. I note that your CV reveals you've done work there and some work in an area that touches directly on this case. What is the Open University?

A. Yes, the Open University is the original -- I believe it's the original, and probably still the largest, or one of the world's largest, distance learning institutions.

It was created in the 1960's as part of a labor government initiative in the United Kingdom to enable people in Britain to get higher education more easily; so the idea being that you would purchase these books and study guides and things, there would be television programs that would be shown very early in the morning that would cover the courses, and every week there would be classes taught basically in classrooms that aren't being used, you know.

So it would be like evening classes, things of that kind. So 3 to 400,000 people currently are enrolled in this. And it has a very high academic reputation.

Q. And you've done a course in the Open University that touches on the subject matter of this litigation, correct?

A. That's correct.

Q. Describe it, please.

A. A few years ago, maybe 10 years ago, the Open University established a Master's of Science in science communication. And within that, there is a module, which I'm the author of it, called Are Science and Religion Compatible? And the way in which this module is set up is basically a text by me where I'm taking the students through a set of readings.

And the basic thrust of this is that, science and religion are compatible at an intellectual level, but there have been institutional reasons why there has been conflict -- and actually, it is focused on the United States -- and saying that there is some idiosyncratic features of the way in which the separation of church and state and how these things have developed in this country that have exacerbated differences between science and religion more than is intellectually warranted.

Q. There's a course, I believe, or a section entitled Will Science Recreate Creationism? Is that correct?

A. Yes. That is toward the end of the module. One thing I should point out, as a sort of back drop to this, the module was originally published in 1998, and so one of the things that comes up toward the end of it, there is a piece from Michael Behe in there, so this is at the beginning of what we now call the intelligent design stuff coming out. And there is a discussion of the significance of that movement.

And what I'm talking about in that part of the module is basically that, the kind of design based impulses, the idea of doing science from a design standpoint -- and let me be clear by what I take that to mean. That is to say, imagining yourself in the mind of God.

I think that is kind of what we're talking about here. Is something that may, in fact, be recreated within what we call mainstream ordinary science, especially as computer programming and the whole idea having to design programs becomes a more integral part of how science is done.

So this sort of idea of design which, you know, a lot of people think of as a purely religious idea is, in fact, an idea that is probably going to be of great significance as a kind of heuristic for doing science in the future as more and more science goes on computers.

And I also argue in the module that this will not be, in a sense, a radically new thing that, in fact, there is a lot of precedent for this way of thinking about how science is done throughout the history of science.

Q. Let me ask you to just give a little detail about, you mentioned, history of science, philosophy of science, and sociology of science. I just want to get a brief description of how those disciplines are defined and how they relate. Let's look first at history of science. What is the field of inquiry known as history of science?

A. Okay. I think the best way to answer that, I mean, other than stating the obvious, it's about the history of science, is that there is a sense in which this field, the question to ask about is, why is this field different from science? The reason is because, in fact, when most scientists learn science, they don't learn very much of their history or the kind of history that they learn is self-serving.

That is to say, it is a history that is written from the standpoint of leading up to whatever the current state of research is. Now Thomas Kuhn called this Orwellian, right, thinking about the, you know, the ministry of truth in 1984, right, which is constantly rewriting the history to justify whatever happens to be current government policy.

Well, this is, in a sense, the kind of history that scientists normally learn about their own fields, which means that there needs to be this other field, history of science, done by historians, that actually tells you what did happen in the history of science in a not scientifically self-serving way.

Typically, that subject, the history of science, turns out to be quite critical of the taken for granted notions that scientists operate with today.

Q. You mentioned philosophy of science. What is that field of inquiry?

A. Now philosophy of science is a field that, first of all, historically used to be quite co-extensive with science. So if you look at somebody like Sir Isaac Newton, not only does he give you the laws of motion, he gives you the laws of the scientific method as to how he got the laws of motion.

That used to be quite common. So that was a sense in which, back in those days, you know, 17th, 18th century, it was all natural philosophy. So it was like science and philosophy of science at the same time. But the field now is an independent field just like history of science is.

And it has been that way certainly since the middle third of the 20th century, and it basically tries to come up with criteria of what it is to be scientific, that is specifiable independently of what is the dominant theory in any given scientific discipline.

And this is where issues of testability get their legs, because there's a sense in which one can talk about testability in a way that is abstracted from what the dominant sciences are at the moment and provides, you might say, a kind of neutral court of appeal.

I mean, that's kind of a -- in fact, it is a kind of quasi-judicial traditional discipline traditionally, which makes judgments about what is science and not science from a punitively neutral standpoint.

Q. You mentioned sociology of science. Give us an idea of the subject matter of that inquiry.

A. The sociology of science is the most recent of these disciplines, and it is a field that is concerned with the institutional conditions under which science, however one defines it, is made possible, and also kind of the internal arrangements that have to take place.

So, for example, you know, a philosopher of science might say, well, you know, what makes a science scientific is that it's testable. A sociologist might come back and say, yeah, but what if it's impossible for anybody to pay attention to your tests?

There has to be some kind of social conditions, as it were, before, in fact, a lot of this science can get off the ground and be maintained. And sociologists are very sensitive to that. And very much like the historians, they tend to look at the ways in which things have been excluded or marginalized over the course of the history of science.

Q. You're identified with a journal Social Epistemology. What is social epistemology?

A. Social epistemology, in a way, is designed to be a kind of synthesis of these three fields that we were talking about -- history, philosophy, and sociology of science -- and basically take the incites from these fields, and with a kind of normative orientation -- now normative, the word normative in philosophy basically has to do with what ought to be the case, right, policy, right, to put it in a kind of practical way.

And so, in other words, given what we know about the way in which science has been organized in the past and many different cultures and so forth, how should it be organized now, and are there problems, and how might they be remedied, and all of that kind of stuff. And that's what social epistemology is concerned with.

Q. Well, the Plaintiffs have had an expert here in history and philosophy of science also, and he has addressed some of the issues that you've sketched out in connection with your work.

But in connection with that, I'd like to ask you, how is it then that your training, your area of academic expertise qualifies you to address the issues in this case that relate to science? You're not a scientist.

A. Well, I think the key thing is that, if you have noticed from what I said about the history, philosophy, and sociology of science, the kinds of things that are, as it were, relevant to know about science aren't necessarily the things that would be in a science curriculum, especially if we're talking about people who are being professionally trained to be scientists.

Nowadays, to be professionally trained to be a scientist, is, in effect, to be a technical specialist in a very small area, a small branch even of your own science. And very often, these technical specialists have to take largely on faith what people from other branches of their own field are doing because they have only the most cursory understanding of it.

Now if what we're doing here in this case is making judgments about what is science and not science, we're making very general global kinds of judgments, right, the kinds of information and knowledge and forms of reasoning that one needs to have would not normally be part of an ordinary scientific education, but would, in fact, require this additional kind of knowledge, the kind of knowledge that one gets from studying the history, philosophy, and sociology of science.

Q. So is it true then that the training you have actually makes you better equipped to answer that issue than a scientist that's practicing?

A. Yes.

MR. GILLEN: Your Honor, at this time I would proffer Dr. Fuller as an expert in the history of science, the philosophy of science, and the sociology of science.

THE COURT: All right. Is there a stipulation with respect to his testimony?

MR. WALCZAK: There is, Your Honor.

THE COURT: All right. Then he's admitted for that purpose, and you may proceed with your direct examination.

MR. GILLEN: Thank you, Your Honor.

DIRECT EXAMINATION

BY MR. GILLEN:

Q. Dr. Fuller, as we begin, I'd like to-

THE COURT: Keep the --

THE WITNESS: I'm sorry, Your Honor.

THE COURT: That's all right. It's the afternoon in the UK.

THE WITNESS: I'm just kind of wound up.

THE COURT: We're not quite as awake as you are perhaps, but if you just keep it at a modest pace, then we'll have no problem. You may proceed.

MR. GILLEN: Thank you, Your Honor.

BY MR. GILLEN:

Q. Dr. Fuller, as we begin your direct examination, which is my opportunity to elicit your opinions, I want to ask you a few questions, which we'll go back and explain. Do you have an opinion concerning whether intelligent design is science?

A. Yes.

Q. What is that opinion?

A. It is.

Q. Do you have an opinion concerning whether intelligent design is religion?

A. It is not.

Q. Do you have an opinion concerning whether intelligent design is inherently religious?

A. It is not.

Q. Do you have an opinion concerning whether intelligent design is creation-science?

A. Nope, it is not.

Q. Do you have an opinion --

A. I do have an opinion. The opinion is, it is not.

Q. Thank you. Do you have an opinion concerning whether intelligent design is creationism?

A. I do, and it is not.

Q. Do you have an opinion concerning whether methodological naturalism is an essential element of science?

A. It is not an essential element of science.

Q. Do you have an opinion concerning whether any testability criteria, if applied evenhandedly, makes intelligent design as much a testable scientific theory as evolutionary theory?

A. Yes, it does.

Q. What is it your opinion?

A. It is. Yes, it does.

Q. The remainder of your testimony will be our opportunity to explain the basis for your opinions. And I'd like to start at the outset by explaining the basis for your opinion that intelligent design is science.

Explain why you believe intelligent design qualifies as science.

A. Okay. Having looked at some of the materials in intelligent design, and I guess I'm most familiar with the work of Dembski and Behe, that, first of all, there are some salient phenomena. One of the things that you want, a science needs to be grounded in something, needs to have a kind of subject matter.

And Dembski and Behe have identified something. They identify it in quite different ways. And here I'm referring to the sort of irreducible complexity complex specified information kind of notion. Dembski comes at it from a kind of, you might take, top down standpoint, where in a sense he's trying to define a sort of domain of design that is separable from necessity and chance.

And his most motivation, intellectual motivation for it has to do with the difficulty, if not impossibility, of coming up with a random number generator.

The elusiveness of the idea of chance which, in other words, whenever you try to come up with a random number generator, it seems as though you can always figure out what the program is, which means it's really designed. Okay.

And that's kind of what motivates him to think, well, you know, why is it so hard to come up with a kind of formula for randomness? Okay. And that kind, you know, led him in that direction.

There is a problem and a problem that is generally recognized by mathematicians and statisticians, regardless of what they think of Dembski, there is an issue there that deserves attention.

In the case of Behe, he's a bottom up guy. He's a more inductive guy. And he sees phenomena, biochemical systems in particular, the structure of the cell, that natural selection historically at least has had difficulty trying to explain. And he thinks, well, you know, that might indicate that there is something quite special in terms of its status as a biological entity.

And design would enter there. So there is this issue of salient phenomena that aren't readily being explained by the already existing theories that then create a kind of pretext for thinking that one then can perhaps, you know, have an extended field of research. Moreover -- oh, sorry.

Q. I'm sorry. I didn't mean to cut you off. Go ahead.

A. The other point I just want to raise is that, design isn't just the name of particular phenomena that other theories can't explain. But also it is, as I mentioned with regard to Dembski, meant to be a kind of general explanatory framework for a research program that covers basically anything that could be regarded as design.

I mean, so, for example, in evolution, there is a tendency to kind of use design sometimes literally and sometimes metaphorically, and there's a kind of ambiguity that's there in the discussion in the evolution literature.

But I think, with these guys who do intelligent design, design is meant to be literal. That is to say, you're going to have one science at the end of the day that is going to explain how artifacts are, and is going to explain how the biological systems are, and social systems perhaps, all under a common science of design. So there is, in a sense, a kind of general explanatory framework here that is also at play.

Q. You contrasted the approaches taken by Dembski and Behe. What did you mean by that?

A. Well, in science, you might say that some scientists work deductively, other scientists work inductively. With intelligent design, you've got a bit of both. Okay. So that Dembski, who is a mathematician by training, and in many respects, has a kind of intellectual background that one, let's say Sir Isaac Newton, had, right, tends to think about these things very much from the top down, Right.

So he's thinking in terms of, where do the fundamental -- what is designed in the most fundamental abstract mathematically specifiable way? Now Behe, right, is a lab scientist, and so he's used to looking at phenomena, and he sees phenomena that don't lend themselves to very easy explanations. And so then he tries to then induce the kind of explanation for it.

Q. If part of what has been said in the courtroom is that intelligent design is not science because it would be necessary to revolutionize science for intelligent design to be considered science, does the aim of revolution disqualify intelligent design from the realm of scientific theory?

A. No, not at all. And I think -- I mean, this word scientific revolution, as I mentioned earlier, is largely associated with Thomas Kuhn, who I wrote these books about. And I think there are two things I would draw your attention to with regard to the concept of scientific revolution.

One is, first of all, we should -- you know, it's a dramatic term. That's the first point. It's not a political revolution, a scientific revolution, and I do think that sometimes some of the rhetoric of that expression, of the term revolution leaks out, and one thinks, oh, my God, if we have a scientific revolution, there goes civilization or something.

Okay. So a scientific revolution isn't meant to be quite like a political revolution. But one thing it does draw attention to, it seems to me, is, you don't have revolutions unless you have a clear sense of what is currently dominant, because what are you revolting against after all?

In other words, if we lived in a world, a scientific world where there were multiple theories around, all roughly equal, all pursuing their own lines of research, and doing things, you know, wherever the truth may lead these respective research programs, there would never be a clear enough sense of a dominant theory to then have to say, we've got to revolt against it.

The idea of revolution presupposes a dominant paradigm, that there is, in fact, a dominant power base in the science at the moment. And that's, in a sense, the most powerful kind of background conception to a scientific revolution. And I do think, in the kind of environment in which we live for science, where resources are so highly concentrated, that, in effect, if you want to make a fundamental intellectual or conceptual change, it's going to -- you're going to have to do something like a revolution.

Q. There's been some discussion in the courtroom thus far about the historical dimensions of this, the issue that's being litigated. I want to ask you, in light of that, are scientific revolutionists unprecedented?

A. No. I mean, in fact, Thomas Kuhn thought that they were a normal part of how science operates. His theory, which is based on the idea that a science can be identified by the fact that it has a dominant theory or paradigm at any given time, his view was that, these theories do their research, eventually accumulate anomalies, that is to say unsolved problems, both at an empirical and conceptual level, and then over time eventually, they get so many of these problems, that people begin to start looking for alternatives.

But Kuhn's point is that, it only happens at that point. It doesn't happen while the theory is still doing well. And this is where he and Popper disagreed substantially. But point is that, yes, one can talk about scientific revolutions. Some of them have even been planned.

I guess that's kind of the point that's relevant to this case, because a lot of revolutions in science are revolutions that are sort of seen in retrospect, okay, that in retrospect, we see that there was a scientific revolution in the 17th century.

That phrase, scientific revolution, was not coined until the 1940's, okay. But there are revolutions that have been planned.

Q. Give us a sense, just sketch out a few, to give us an idea of how the phenomena manifests itself?

A. The most self-conscious scientific revolution in the sense that the guy says, I'm doing a revolution, watch out, okay, and succeeds, is Antoine Lavoisier, who is associated with the chemical revolution in the late 18th century.

And in the history of science, Lavoisier is primarily known as the discoverer of oxygen. And the way he did this, and this is quite symptomatic of the way he did science generally, was, he was in correspondence with Joseph Priestly in the United Kingdom, who was actually a very good experimentalist and who discovered this thing that he called dephlogisticated air.

The thing to keep in mind is that, before Lavoisier, chemistry was a very practical kind of subject, not very mathematical, kind of a thing that, you know, a bit like pharmacy, you know. It had this kind of element, practical applied kind of element to it.

And people were trying to come together with some fundamental notions. And Priestly came up with this idea of dephlogisticated air, that is air without phlogiston, which was regarded as the fundamental element of chemistry at the time. But this element was very strange because, basically, when it was around, things lost weight. When you added phlogiston, it would lose weight. Very strange element.

Lavoisier reinterpreted all of Priestly's experiments and a load of other experiments that chemists had been doing in the 18th century and basically said, look, these guys are misrepresenting what they're actually discovering. In a sense, we need a new kind of classifications system for chemistry so we can make sense of all of these very weird results.

See, because the issue here is, you can have a lot of weird results in science and do a lot of very good practical work, and what you need is a kind of incentive to unify stuff in a way that hadn't been unified before in order to get a real science off the ground.

And that's what Lavoisier did. He wasn't that great an experimentalist. He did some experiments, but for the most part, what would launch the chemical revolution was a systematic reinterpretation of a lot of stuff that other chemists had been doing for centuries.

Q. Well, there's been, you know, the subject here is the neo-Darwinian synthesis. And there's been talk of genetics. And I know you and I have discussed Mendel and his role, which seems to bear directly on the neo-Darwinian synthesis. So please describe -- let me ask you are first. Do you regard Mendel's work as a scientific revolution?

A. Well, it's one of those cases of revolution in retrospect in the sense that Mendel's work -- maybe I should say something about who Mendel is?

Q. Certainly.

A. You know. Well, Mendel, who's regarded normally as the Father of Genetics, was a monk, a Catholic monk in Moravia, which is now part of the Czech Republic, whose writing in the mid 19th century, and did these very famous experiments with peas where he basically came out with a kind of a prototype for the fundamental laws of heredity.

And one problem that he had was trying to get the stuff published. It was a very difficult sort of idea to get across to people, because he was writing in a period where, even though Darwin's work wasn't completely accepted, nevertheless there was a view that evolution was more or less right.

And what that suggested to botanists at the time was that, through heredity, there would be over time a kind of blending of characteristics, right, that that would be kind of the incremental change, the evolution over time, as plants with different traits, right, sort of bred together.

But what Mendel showed, or claimed to have shown, was that, in fact, there are some fixed ratios between what we now called dominant and recessive traits, right, that are reproduced each generation, right, because they are intrinsic to the peas regardless of what the individual peas, what they looked like, okay.

Now the head of the leading botany journal just couldn't buy this, and, in fact, Mendel was a special creationist. I mean, he believed that these were like inherent in the peas and they were kind of created that way. And so it was only much later on when -- that Mendel's work got accepted, basically when you got to a point where people could come up with some kind of naturalistic interpretation, you know, understood in that methodological naturalistic way, of what he was doing.

Q. Well, carrying that forward in terms of the neo-Darwinian synthesis, let me ask you, was that synthesis regarded or described as a revolution in time?

A. Well, this is the -- you're raising a very interesting point here, because obviously, in this talk of scientific revolutions, you know, one thinks of Newton, one thinks of Einstein, and I mentioned Lavoisier with the chemical revolution, and, of course, one things there's a Darwinian revolution.

And Michael Ruse wrote a book in 1979 called The Darwinian Revolution. So when did it happen? And this is an interesting question. If you read Michael Ruse's book, and this is the first time -- I mean, this is the first time where in print people talk about Darwinian revolution, he thinks it actually happened shortly after Darwin published Origin of the Species, 1859.

But in fact, for reasons, you know, that I'm not going to go into here, it's not until you get to the neo-Darwinian synthesis, which is being forged in the 1930's and 40's, that you actually have something that does look like a scientific revolution in the sense that you get biology in a state that looks something like the way Newton brought physics into in the late 18th century.

And what the neo-Darwinian synthesis is, what it synthesizes is genetics with the kind of natural historical framework that Darwinians already have. So basically, to go back to the example of Mendel, you know, you basically bring the two sides together.

You bring together Mendel and the genetic viewpoint, which, in a sense, is very much looking at life from a design standpoint or the fundamental bits of life, how do they combine to produce the things of things we see in the world, and you combine that with the natural history standpoint of Darwin, which is one that kind of looks at nature as it's already out there in nature, and then tries to make inferences about what's the source of that variety that we see.

It's only in the 1930's and 40's that you actually get those two parts of the puzzle put together that enables the kind of people, you know, who have been testifying for the Plaintiffs to all say, they're part of the same science.

Q. You mentioned Einstein. Just give us a brief discussion of the way in which his theory might be regarded as revolutionary?

A. Now Einstein is a kind of case that Thomas Kuhn talks about and people normally talk about as a scientific revolution. And there are lots of aspects of it that are quite interesting, I think, from, you know, in terms of bench marks for thinking about what's going on in this case.

One is that, when Einstein published his famous papers in 1905, you know, in relativity theory, in Brownian motion. He was, in fact, a patent clerk in Baron, Switzerland, having failed his entrance examinations in science -- by the way, Mendel also failed his entrance examinations in science.

There's a long history of revolutionaries being academic failures. I don't know if that's so easy anymore, but it certainly historically has been the case. And so he writes -- but he was someone who, you know, was following developments in physics. And this was during a period in physics where still you could make major breakthroughs just by doing, you know, chalk on blackboard stuff, you know, mathematics and relatively simple experiments.

And, in fact, there were several experiments, the most famous of which being a Michaelson-Morley experiment, which seemed to suggest that light could bend, that light would slow down if it's moving against the motion of the Earth, that needed to be explained. It was an anomaly within Newtonian mechanics. These were generally well-known.

Anyone who was following physics would know that Newtonian mechanics had some serious problems that physicists themselves couldn't quite get.

So Einstein writes up these equations, which basically end up saying, well, you got to drop absolute space in time, which is what all the Newtonians were presupposing, and say instead that, light is constant, and then that would make sense out of everything. He submits this paper.

It's a very -- it's a very clever kind of move, but it's very radical as well. And he submits it to the leading physics journal. And Max Planck, Father of Quantum Mechanics, is the editor. And he sees that the mathematics in Einstein's paper is a little goofy, but he fixes it up and makes it publishable. And then, of course, people really start to take it seriously.

Some interesting things about this is, Einstein was inspired to actually think along these lines that, in fact, there may be some fundamental problem with Newtonian mechanics, and that was the reason why it couldn't explain these experiments I just mentioned.

By reading a book by Aernst Mach, M-a-c-h, called The Science of Mechanics, which is largely a historical work kind of putting together in a nice summary package all of the objections that people had been maintaining about Newtonian mechanics for the previous 200 years.

You see, Newtonian mechanics had some unresolved conceptual problems from its very outset, including how do you justify absolute space in time. That's just taken on faith by Newton. And the Newtonians did as well, because it was able to solve a lot of empirical problems for many years.

However, by the late 19th century, problems are starting to accumulate empirically, so people are beginning to question the conceptual basis. And Mach, as kind of this historian of all of this, said, you know, Einstein reads this to say, wow, so there were objections there for a long time, it was just, you know, that there was no incentive, as it were, to actually try to put these objections together and think if we can come up with some kind of positive alternative.

But now at this stage in the history of physics, there seem to be. And that's kind of what Einstein did. And he mentions this, that he was inspired this way.

Q. Well, you've mentioned this accumulated set of problems for Newtonian physics. Let me ask you, looking at this state of affairs today with respect to evolutionary theory, do you, in your opinion, think there's reason to believe that there are an accumulating set of problems that may be a pre-cursor to a similar development in biology?

A. Well, there are certainly some longstanding conceptual issues that just don't seem to go away. And some of them are quite -- and some of them reflect kind of the fault lines of the neo-Darwinian synthesis. As I mentioned earlier, right, it has to do with the relationship between genetics and natural history being brought together.

But these two disciplines are really quite fundamentally different in how they think about life. So, for example, one way, one area where this is coming to a head has to do with exactly how one defines the idea of common descent; that is to say, the idea that there are common ancestors for all organisms, which is very much a key, a corner stone of the evolutionary synthesis.

Traditionally, common descent was identified morphologically, which is to say, you sort of, as it were, give the precedence the natural historians looking at the way the animals, how they appear to you in the field, what their physiologies are like, and so forth, what they're shaped like, all that kind of thing.

But with the advent of genetics, one then comes up with a kind of alternative way of doing this, right, which actually looks at genetic similarity between organisms, and then one comes up with a somewhat different tree of life, as it were.

This is kind of an ongoing debate. And you end up getting somewhat different trees of life often with some surprising consequences and surprising divergences. In a sense, that's a residue of the fact that the two main bodies of disciplines that were brought together in the neo-Darwinian synthesis are really, you know, sort of approach the nature of life in fundamentally different ways.

And so that issue kind of revives itself in the debates over what common descent means. Now there are other issues as well. So, for example, how much does natural selection explain survival of the species? Different biologists have different angles on this. Some, like Richard Dawkins, takes what's called a very strong adaptationist approach where everything is the product of natural selection.

Others say, well, there's sexual selection, there's random genetic drift, there's maybe punctuated equilibrium. You know, there may even be some version of the inheritance of acquired traits in some aspects of things. And different biologists, you might say, would apportion the explanatory merit of these mechanisms differently.

And there is no consensus on this, though most agree that natural selection, in some sense, is dominant. But then that raises the question of, at what level of organic reality does natural selection operate? So there's a very -- especially in the philosophy of biology, but it definitely affects biology itself, an issue over units of selection. What exactly is selected?

Are we talking -- Richard Dawkins thinks selection occurs at the gene level, right. When he says, selfish genes, what he means is, that, as it were, evolution is written from the standpoint of the gene. The genes are what is being selected, and everything else, like the organisms that contain the genes, they are mere vehicles for genes, that genes are really where the selection is.

Darwin himself believed selection occurred at the level of the organism, that you guys see natural selection in principle happening if you were actually there whatever billions of years ago, because it's happening on organisms. They live or die. That was kind of how he saw it.

Then you can think about, well, maybe there's group selection or kin selection. So that's to say, larger and larger units where selection is occurring. And throughout the history of evolution, you've got people pitching the claim at all these different levels, and then again, lots of disagreements.

And again, these things are not being resolved. They're just kind of continuing. They're rumbling along, you might say.

Q. Well, do you see reason to believe that, how should I say this, that there are, there's a way in which the theory at the level you've described it, is not actually shaping science as practice?

A. Well, this is the issue, right, because if, you know, what I've just been sort of laying out for you in terms of these theoretical disputes that exist within evolution, in a sense, what I'm talking about there is what is most directly identified with evolution. If one wants to -- and when people have been testifying in this case, whenever they've talked about evolution, they've used the kinds of concepts I've just been talking about, all of which are essentially contested by people in the biological community.

I'm not saying they don't believe these concepts. But exactly their definition and how they apply and their explanatory scope, all of this is being contested. So you wonder, how is it possible for biology to be conducted on a day-to-day basis, given all of this kind of conflict at this supposedly fundamental level of biology?

Well, the answer is, it isn't fundamental for doing biology. In other words, these debates over evolutionary theory, that, in fact, define what evolutionary theory is, kind of continue in the kind of parallel universe to the rest of biology.

And in a sense, one way you can see this is that, if you look at the Nobel prizes that have been awarded for physiology in medicine, which is the field, the biological field, essentially, you don't find anyone ever getting the prize specifically for evolution. Okay.

What they get prizes for are genetics, for ethology, for various branches of medicine, for physiology, animal behavior, right. In other words, they get the prizes for areas of research that are much closer to the phenomena than the sort of generalizing, universalizing level in which evolution operates.

This is not to say that these different disciplines cannot be explained or cannot be illuminated by evolution. But the point is, one doesn't need to be an evolutionist in order to do the work in these respective fields, at least sufficiently to be able to be recognized as important practitioners of those fields.

Q. Well, in light of what you're saying, do you see a meaningful connection between the work of the scientists winning the Nobel Prize or working the lab day-to-day and the theory? Is there evidence that the theory exerts a powerful influence over their work?

A. I mean, this is the thing that's very difficult, it's a very difficult thing to document. I mean, of course, we certainly had enormous numbers of pronouncements telling us that evolutionary theory is the foundation or the corner stone of biology.

The National Academy of Sciences, I believe, says this. But you see, is this literally true? Because at least from the standpoint of someone like myself, who's looking at this as a historian philosopher or sociologist of science, when we think about foundation or corner stone of a science, we're always thinking about Newtonian mechanics.

There's a sense in which physics is kind of always the bench mark for us, because there you have a very clear sense of a science where you have fundamental laws, right, and where you can deduce conclusions, and where different aspects of reality, in a sense, can be sort of figured into it in various ways.

There's a kind of tight theoretical deductive connection that leads to predictions that can be validated or not, as the case may be. And now, of course, after Newton, we've got Einstein, and we see physicists struggling very hard to come up with a sort of grand unified theory.

And what they mean by that is, something that's very deductively tight in that kind of way. And they recognize that there is a sense in which there is a crisis in physics. Now evolutionary theory isn't structured this way. Biology isn't structured this way as a discipline where there's any sense in which one is talking about unification in that very tight kind of sense.

Rather, what you have is lots of different disciplines within the biological sciences -- and, you know, I've rattled off a few already -- kind of doing their own work, you know, with their own theories and methods that pertain to the branches of life that they're concerned with, right, and then every now and then, paying lip service to some concept in evolutionary theory.

And one way in which I try to show this in the expert witness statement that I provided for this trial is this testimony of the guy, Nicholas Rasmussen, who is a historian of biology at the University of New South Wales, who basically makes the point that it's a mistake to treat evolutionary theory as if it were the same thing as contemporary biology, that, in fact, biology is all of these different fields.

They have radically different histories. They come from many different directions, some of which are more or less related to developments in evolutionary theory. The problem, however, is that evolutionary theory is, in a sense, a kind of universal rhetoric of biology; that is to say, a repository for terms and concepts that people from all these different biological fields can regularly use to explain and illuminate what they're talking about.

Q. How did Rasmussen go about substantiating his point concerning the relative --

A. Well, Rasmussen was someone who was himself initially trained as a biologist. I mean, a lot of people in my field, though not myself, but a lot of people in my field originally have a kind of science training, and for various reasons of disinterest, disenchantment, or disillusionment move into history, philosophy, and sociology, instead of staying with the original science.

So Rasmussen had some sense that, if you look at day-to-day work of biologists in the lab or in the field, all of this evolutionary stuff doesn't really happen. It happens somewhere else. So what he did was, he did a data base search of all of the -- of all the journals that are listed, biology journals that are listed for the year. The year he looked at was 1989.

And he found that, in a generous estimation, that is to say, if you look at the key words and abstracts of articles -- and abstracts of articles are the things that typically have what are the main points and the main things that the author wants to get across to the scientific community -- if you look at those things for the year 1989, and you look for the occurrence of the word evolution and the word -- and the phrase natural selection, you will find no more than 10 percent of articles include this in 1989. No more than 10 percent.

Q. Is it in 1989 or was there a period of inquiry?

A. Well, it was 1989. But then I checked this. I was very, you know, concerned, is this right? I mean -- and is it the same today, because we're now 15 years later? And what does this look like as a kind of historical phenomenon?

I mean, I think one thing to keep in mind here is, this is against the back drop of everybody saying, you know, evolutionary theory is taken for granted. And so you wonder, okay, maybe that's why it's not being talked about very much.

So what I did was, I looked at the data bases --

and now it's a lot easier to do it because we've got computer search programs -- for the biological sciences and biology, all of the articles, books, websites, whatever, from 1960 to the present. And here we're talking about 1.3 million items. And --

MR. WALCZAK: Your Honor, I'm sorry. I'm just going to object. This is nowhere in his expert report.

MR. GILLEN: I mean, he's referenced the Rasmussen article in his --

MR. WALCZAK: But we're now talking about a study that is not part of his expert report. I certainly don't find it. And I could be mistaken, but I don't think so.

THE COURT: Well, let's use this as an appropriate time to take a break. I have something else I must attend to at this point. I was going to break at 10:20 anyway. Why don't you look and see if you can find it either directly or in the context of the expert report, and I'll hear your objection or renewed objection after the break. Why don't we take about a 20 minute break. Water or decaff only.

THE WITNESS: My apologies, again, Your Honor.

MR. GILLEN: I understand.

THE COURT: And we'll return in 20 minutes.

MR. GILLEN: I got a paddle back there.

THE COURT: We'll be in recess.

(Whereupon, a recess was taken at 10:20 a.m. and proceedings reconvened at 10:44 a.m.)

THE COURT: All right. We resume with direct examination of Dr. Fuller.

MR. GILLEN: Thank you, Your Honor.

THE COURT: And do we have an objection? Do you want to restate the objection?

MR. WALCZAK: I would just object to Dr. Fuller testifying about some study that he apparently did on periodicals and publications, because that's nowhere in his expert report.

MR. GILLEN: And I acknowledge the objection, Judge, and withdraw the question. The article is in his report, but his curiosity and what he was getting into is not.

THE COURT: Then there's no reason to rule on the objection. The question is withdrawn, and you may move on.

MR. GILLEN: Thank you, Your Honor.

DIRECT EXAMINATION (CONTINUED)

BY MR. GILLEN:

Q. Dr. Fuller, there's been some discussion of a notion of the relationship between a given theory and its service as a big tent. And so I'd like to briefly get your opinion on that sort of the sub issue in this case.

ID has been described as a big tent. Do you see this as distinguishing intelligent design, ID, from evolutionary theory?

A. Well, I was actually quite surprised of the use of the term big tent, which I had not run across previously to describe intelligent design, especially by people supporting evolutionary theory, because, for me, evolutionary theory is the biggest of big tents.

Q. What do you mean by that?

A. Well, in a sense, it's not an unusual thing. And I don't want my remarks to be taken in some way I'm demeaning evolutionary theory or scientific theory in general, because there is a sense in which all scientific theories that attempt to be very universal in general do end up becoming big tent theories, at least in the beginning.

But the specific thing I have in mind here with regard to evolutionary theory, and I've mentioned this a little bit already, is that, really the people who are brought under this tent of the neo-Darwinian synthesis come from really quite different, radically different research cultures historically.

And one reason why this particularly interests me, and I think is of significance is, it's -- the range of fields that you find under the neo-Darwinian synthesis ranging from laboratory based genetics, and now more recently, computer based simulations, all the way over to the paleontologists and the natural historians who study animals and plants in the field.

That kind of range methodologically is very similar to what you find in the social sciences, which are my own fields, where we range from anthropology, which studies natives and their habitats, and then moves along, and we have political scientists doing surveys, and we have economists doing modeling themselves, and psychologists doing laboratory based experiments.

So the range of methods are just as broad as in biology, and arguably, the subject matter of the social sciences is narrower than biology given the species as contained in just one species, as in the case of social science.

Yet neo-Darwinism was able to bring together all of these vastly different fields under one umbrella theoretical framework in a way which never happened in the social sciences, even though there was attempts at roughly the same time in the 1930's and 40's to do so.

So there's a kind of interesting question there from the standpoint of the history, philosophy, and sociology of science about, how did this thing work, because you would think it didn't really have a chance to work.

Q. Has that phenomena you described been the subject of study?

A. Yes. And I was eluding earlier when I was talking about the uptake of one of my books, Philosophy, Rhetoric, and the End of Knowledge, the people who study the rhetoric of science have paid particular attention to this business of the forging of neo-Darwinian synthesis.

And the key thing that they focus on is the -- is certain key texts. And the one text I think is the most important text for launching the synthesis is the book Genetics and the Origin of Species by Theodosius Dobzhansky. Should I spell now?

Q. Please spell that.

A. Okay. Theodosius, T-H-E-O-D-O-S-I-U-S. Dobzhansky, D-O-B-Z-H-A-N-S-K-Y.

Q. Thank you.

A. And Dobzhansky was a very unique figure in the history -- and for me, I would say, personally, this is the man who I would identify as the Newton of the Darwinian revolution. If we were imagining, you know, Newton as having set a paradigm for physics that physicists, for 200 years, worked under, okay, the comparable thing in the history of biology was provided by this guy, Dobzhansky, in 1937, with genetics and the origin of species.

Because Darwin himself was more like a Copernicus figure in the sense he kind of makes the big intellectual change, but he doesn't really provide a basis for research so people from a lot of different fields can work under. But Dobzhansky did this.

But he didn't do it the way Newton did it, because Newton, in fact, had some very specific methods and very specific kind of mathematics that was very much a part of how he would -- how his program would develop.

Whereas Dobzhansky was a big tent guy. He was a guy who, when he was still in Russia, was a natural historian. He migrated to the United States in the early 20th century and worked in the major genetics laboratory in Columbia university under Thomas Hunt Morgan.

So he had like a bit of both worlds in him, and so he was able to communicate across this great divide that had existed in biology in the beginning of the 20th century.

And I think the key thing to point out in this respect is that, at that time, so we're talking like the first third of the 20th century, genetics is the ascendend biological science, and it's doing perfectly well without Darwinism.

And Darwinism is, generally speaking, in decline and seen as a kind of, you know, old fashioned natural history, guys who like to look at animals and plants and give just-so stories about how they managed to survive but with no clear sense of causally how it happens.

And this is where Dobzhansky comes in, because he's the man who introduces the language of mechanism. And you've heard a lot in this trial, and we've always hearing about mechanisms of natural selection.

Well, this concept of mechanism was not one that comes from, as it were, the natural history, the Darwin side, because the Darwinists tended to think of natural history as a kind of emerging process, you might say, that, in a sense, you couldn't actually break down into analytically discernable parts saying, this part is caused by genes, and this part is caused by environment.

Whereas nowadays, in scientific biology, that's exactly how we think about it. We think about there being mechanisms of natural selection, which work by some kind of combination of genes and organisms operating in environments. And it's easy to get this impression that, in a sense, if you took apart animals and environments, you could figure out how it all worked.

Well, Dobzhansky is responsible for getting that mind set into Darwinism, because Darwinism itself did not have it naturally. It was more a science of just sitting around watches animals and birds and collecting artifacts like fossils and things like that. So this was very important.

But what they have figured out, looking at this book very closely and looking at the reviews of it and the way it was taken up by various branches of biology, was that, you know, mechanism is a word that has a lot of resonance in lots of different ways.

So as it were, one can talk about mechanism as a force. One can talk about mechanism as an actual part of a machine. In other words, there was a lot of strategic ambiguity that was located in this book that enabled to bring everybody on board without having to challenge their fundamental assumptions about, that they brought in. Whereas, you know, so geneticists would normally think, all of science is done in labs under artificial conditions.

Whereas the natural historians thought, no, the way you do life science is by looking at animals in their native habitats. Well, Dobzhansky squared the circle rhetorically by making both sides feel comfortable with this kind of arrangement.

But he didn't do it because -- by, in some way, logically and mathematically synthesizing things the way Newton did.

Q. Well, if he didn't do it that way, what is the purpose of the synthesis? What makes it hang together?

A. Well, it is a common rhetoric. I just mentioned the issue of mechanism here. If you look at the Plaintiffs' experts in this trial, and I'll give three, because, in a sense, three of them represent a kind of range that exists today in biology.

And you think to yourself, what do these people have in common? And so let's think for a moment of Padian, Kevin Padian, who is a paleontologist who spends his time looking at fossils and classifying them. And then we've got Kenneth Miller, who's a cell biologist who spends his time in laboratories looking at very small things in peatry dishes and so forth.

And then you've got Pennock, who is basically doing a kind of computer modeling, artificial life research, as it's normally called. And all these three guys think they're part of neo-Darwinian synthesis. And the way you see is, of course, when they come to having to make ultimate explanations of what they're doing, that goes beyond the actual research environment and actual organism or actual work setting, they will appeal to these various notions of natural selection and mechanism and so forth.

So there is, where this kind of multi-purpose rhetoric that is equally available to all of these people who otherwise are doing research that really has very little to do with each other. And, in fact, I would even go further. I would -- it's interesting that none of these three guys, and it could be actually any such people who represent this diversity of the field of biology, were asked really to comment on the work of the others.

So, for example, would Padian or -- and Miller think that Pennock was doing biology? You see. And if so, to what extent is the biology he's doing really contributing to some kind of validation of the evolutionary synthesis? It seems to me, there would be a variety of views that would be on this issue here.

But nevertheless, they're all talking the same language at the most general level of explanation, and that is largely due to Dobzhansky's work.

Q. Would status as a big tent theory disqualify a theory from science?

A. No. I mean, I think that's an important point to bring to bear here, because what basically I am trying to challenge is not that one shouldn't have big tent theories. Big tent theories are, in fact, part of what it takes to unify fields that do start off very different. That's not surprising.

One is always looking for higher levels of abstraction and stuff like that. But the value of it at the end of the day comes as a kind of, you might say, what we say in philosophy of science as a metaphysical research program, and that is, in fact, how I would describe the neo-Darwinian synthesis, a metaphysical research program in biology that suggests some very interesting ways of understanding and interpreting phenomena in many different disciplines that otherwise would have very little to do with each other.

Q. If you look at evolutionary theory in that light, are there key terms that are hallmarks of the synthesis?

A. Well, I mean, natural selection, obviously, common descent. The issue about origins, exactly what we mean by that, because if you think about it for a moment, there are some interesting kinds of, you might say, strategic conflations when one things about origins, because what do we mean by origins?

Do we mean what was actually there at the beginning of natural history, whatever, 4 billion years ago or whatever the paleontologists tells us it is? Or do we mean, what is from a biochemical standpoint the most primitive form that can sort of self-reproduce or self-change itself in a way that we would recognize as life?

Now, obviously, one would be the sort of thing a paleontologist would study, and the other would be the sort of thing a biochemist or someone like that would study. And there's a presumption that somehow there would be the same answer, that, in some sense, that the historically earliest form of life, origin in that sense, would also be the most biochemically primitive form of life.

And it seems to me, this is kind of part of what the neo-Darwinian synthesis does. Namely, it makes you suppose these things are going to be the same. But unless you actually thought these two disciplines had to speak to each other, it's not at all obvious that there would be a convergence.

Q. In terms of the -- of this evolutionary synthesis, the neo-Darwinian synthesis, does any one person speak for -- can anyone one person speak for that?

A. No. I mean, you know, there's a sense in which

-- that's the whole idea of the big tent, after all, right. Dobzhansky gives you a kind of protective cover, you might say, linguistic protective cover under which all kinds of research can be conducted as long as, you know, as they are being discussed ultimately in this common rubric.

So, for example, Richard Dawkins, right, emphasizes very much almost exclusively natural selection. He's an adaptationist. He thinks it's at the level of genes. There's massive disagreement with him across all of evolutionary biology. Yet he's probably the best selling author at the popular level and the person through whom most people find out about evolutionary biology today.

But his view is, by no means, the dominant one in any kind of statistical sense within the field. So in that sense, no one person does it. And if you look at textbooks, because textbooks might be the place where you think you get some kind of consensual view, I think we see this in this trial, and this is again not unique to this trial, but textbooks are things that are, in a way, cobbled up by committee, right.

There's a sense in which you got a lot of interest that needs to be satisfied to give a kind of common story. And so as a result, you're not actually going to tell the story of various aspects of life exactly as those people who are the experts would think would be the best way to tell it, but rather in a way that will enable all those different bodies of knowledge to be brought together in some coherent fashion so the students think, ah, this is biology and not just some collection of specialized disciplines.

So there isn't going to be one person or even one book that is going to adequately capture what this, what this synthesis is.

Q. Well, given what you said about the situation with respect to the neo-Darwinian synthesis, would you expect the situation to be any different for intelligent design theory?

A. No, not at all. And, in fact, I think, you know, the main problem intelligent design theory suffers from at the moment is a paucity of developers, right. There are basically a handful of people doing it. And so what you don't have is really a lot of room for theory development, for developing the terms of the argument, and for developing research programs in the area.

And that is the -- that would be the main problem. But the fact that there are people coming at it from different angles, you know, from different perspectives, and thinking of different phenomena as salient to design, that itself is not a problem.

Q. Well, you described that the thin ranks is a problem. Is that -- how would you explain that in light of your discipline?

A. Well, I mean, this is the issue here. We go back to this issue of there being a dominant paradigm. As I mentioned, you know, if we want to talk about biology as having achieved the status of a paradigm where there is a dominant theory that basically becomes the covering term of research, this is the neo-Darwin synthesis since the 1930's and 40's in biology.

And one of the consequences of that is, that becomes sort of the lingua franka in which all kind of biological knowledge claims need to be transacted. So that if you actually start to come in with predices that are fundamentally different, or maybe even challenged, fundamental assumptions of the dominant paradigm, it's not exactly clear how you get in given this situation, because you have this massive amount of resources that have accumulated that, in a sense, control the show.

Q. You've mentioned the terms or concepts of Darwinian synthesis as providing a lingua franka. Do you see signs that that may be changing?

A. Well, I mean, I think that -- I mean the issue -- the thing I raised earlier about there being all of these kind of conceptual problems that don't get resolved and just kind of rumble along is indicative that it's not clear what's going to happen in the long-term.

I think here, intelligent design, in a way, could be making some inroads. If one -- if -- I think there's certain constituencies within the neo-Darwinian synthesis that, in a sense, could pull apart from the synthesis more easily than others. And in particular, I'm thinking of the people who work on computer modeling, who work, as one might say, the design side of evolution, the genetic side, the biochemical side, where people are very much thinking in terms of mechanisms normally.

It seems to me that, there, it is possible for that to pull away from the more natural history paleontological side. So there's no natural necessity that all these fields have to be together. And there's a sense in which some of the stuff in intelligent design is naturally better suited for some of this other stuff going on in biology.

Q. Well, that points to another way in which people have linked intelligent design with religion or natural theology, which you've just mentioned. There's a sense that its historical roots are religious in nature. How do you approach that claim?

A. Well, I mean, I think the first point always to put on the table about this is, just about, you know, all of modern science has religious roots. And this is where this idea of methodological naturalism as being the nature of science is just compete rubbish from a historical standpoint.

If you look at all of the people who are most responsible for the scientific revolution, which is, after all, the benchmark of what we call natural science today, they were all people with very strong religious beliefs, typically non-conformist beliefs, and typically people who, in a sense, had to hide their beliefs from public inspection for fear of persecution.

And I'm talking here, Renee Decaur, Sir Isaac Newton, you name them, Robert Boyle. And so in that respect, the religious origins of science doesn't really speak badly to it at all per se, because, in fact, that's the normal thing in the history of science.

Q. Well, let me ask you. Do you see that intelligent design is necessarily linked to natural theology and its origins, such as the worth of Paley?

A. Here's, I think, a real problem that intelligent design has. It doesn't know its own history. It's not really properly acquainted with its own history. And so as a result, it really can't recover -- it hasn't yet recovered all of the intellectual roots that, in a sense, could provide sustenance for it.

And the first person who I, you know, if I were offering advice to intelligent design people, I would say, Sir Isaac Newton. He is the 400 pound gorilla of intelligent design theory, because this is a man who quite clearly thought he got into God's mind and figured out the basic principles by which all of physical reality was governed both in the heavens and on Earth.

And in fact, and the work, some of the work of my dissertation advisor was relevant to this, you know, he has all this you unpublished stuff where he's going through, you know, Biblical exegesis and alchemy and all this stuff, and it's quite clear that all of the published work, the prekibia (phonetic) mathematica and all the physics that he did was in service of trying to figure out, right, in the coin of science, right, how the creator's mind worked.

So he took -- this is what I mean when I say, taking a design standpoint. You put yourself in the position of the creator, and you think, how would I create the world given what we know about it? And this is what Newton did. And in that respect, he is the greatest of all the intelligent designers, okay.

Now when we get to Paley, who was kind of the poster boy for intelligent design theory these days, we're basically talking about a guy who's writing at a point where he's responding to skeptics of design. So all of this stuff about the Watchmaker from 1802, all this kind of stuff, is already written in the context that there are people challenging design and he has to defend it. Okay.

And so there's a sense in which the whole Paley, the framing of the Paley situation is kind of wrong footed from the standpoint of intelligent design, because he introduces the issue of design from the standpoint of someone who discovers design, discovers the watch on the beach, rather than from the standpoint of someone who could do the designing, which is what Newton did.

So from that standpoint, the intelligent design people do themselves a disservice by falling back on Paley.

Q. Well, you mentioned computer modeling and the way in which some people self-consciously try to put themselves in the mind of someone creating to grasp natural laws. How about someone who looks at it from a more, what shall I say, a more computer oriented standpoint historically?

A. Well, okay. And here, another hidden presence in the history of intelligent design, who is very relevant to -- because, you know, nowadays, if we think about getting into the mind of the creator, and we don't want to be explicitly theistic about it, the most natural way is to think in terms of computer programming where you are designing virtual realities and worlds and things like this, like Pennock is doing.

The person who is the benchmark for that, and the man who we normally credit with having invented the idea of the programmable computer, is the guy by the name of Charles Babbage, B-A-double B-A-G-E, who was one of the successors to Newton's chair at Cambridge. So he held Newton's chair. And he was writing in the 1830's and 40's, and he called the computer the analytic engine.

And what he wrote, he wrote one -- a series of treatises that came out in the 1830's and 40's, basically trying to square science and religion, called The Bridgewater Treatises. And the one that he wrote was one where he sort of imagines God, we would say by our terms, as a big computer programmer; and indeed, a computer programmer who, as it were, programs free will by including not just natural laws a la Newton, which are deterministic, but actually sticks in some, what would be called, stochastic variables, that is to say, randomizing elements. Get stuck into the program.

I mean, I think the interesting thing here is, probability theory was only in its infancy when Newton was writing, but by the time Babbage is writing, it's pretty well developed. And Babbage is thinking that God might have been the kind of guy who designed the world such as there are these deterministic laws, but every now and then, you throw in a random variable.

So God knows the program, but he doesn't actually know what the creatures are going to do, because what the creatures are going to do is going to be determined by how this random variable plays itself out. And so for Babbage, that would be a kind of operationalization of free will. That's what he thought. That was how you square the determinism free will problem.

You can imagine -- in fact, this is not a million miles from what Pennock is doing, it seems to me, and in that artificial life thing that he was talking about. And for Babbage, this would be an example of intelligent design, because, after all -- Babbage's point would be, God just needs to know the program, but the program can include variables, the outcomes of which he doesn't know.

Q. Well, at several points in this discussion, you mentioned the notion that the scientists, in approaching a given problem, has adopted a mind set, which assumes creating rules. And you've used the term creator. You've opined intelligent design is not creationism. How do you see the relationship between this mind set you're describing, which assumes a creator, and the nature of the work of these individuals you've mentioned as scientific nonetheless?

A. Well, I mean, the issue here -- and here, I think it's important one introduces a distinction that's very important in the philosophy of science that I think, in a way, gets blurred in the discussions we've been having in the courtroom, and that is between the context of discovery and the context of justification.

And this is a very classic kind of, you know, even somewhat old fashioned philosophy of science distinction that nevertheless is worth bringing up here; the idea being, right, that there is a context of discovery for science.

And that is to say, the kind of mind sets, the kind of ways of looking at the world that are, in fact, useful for coming up with scientific ideas and hypotheses. And here I would include the design standpoint, the creator's standpoint, putting yourself in the mind of God, thinking how would God do this. That's, in fact, a very useful way of coming up with theories and hypotheses and so forth.

However, that's the context of discovery. That doesn't show its truth. What that shows is, it's a fruitful way of coming up with ideas, but at the end of the day, what makes the thing science is whether it's testable, and that is the context of justification. Okay.

And the key thing there that's very important is that, that has got to be testable in a way that you don't have to actually share the mind set of the people. Babbage, Newton, Paley, all these people are theists. No doubt about it. But you don't have to be a theist in order to test the theories they're putting forward.

That is the key thing about science, that there is the context of discovery and the context of justification. And they're both vital, but they're both -- but they're separate.

Q. Well, we'll talk some about that later. But as we go forward, I want to ask you, in terms of these theories that you're describing as they develop historically, and then again in terms of intelligent design, is new research, new experimentation a necessary ingredient of scientific progress?

A. Well, eventually it is. But, I mean, the thing is that, you actually do need a kind of critical mass of theory and interpretation of data before it happens. I mean, one of the things that's always worth pointing out in this context is that, all new theories are born refuted. Okay.

I mean, especially if you have this view that there is always a dominant paradigm in science, right, because, in a sense, the deck is stacked against you, because the dominant paradigm sets the terms under which, you know, the domain is conceptualized, the terms under which tests are to take place, and so forth.

So there's an uphill struggle from the outset. So it then becomes very important for people who want to put forward a new theory to actually engage in what we call would theory construction, namely elaborating the consequences of the theory in many different settings, kind of develop the theoretical imagination, you might say, and also to reinterpret a lot of the data that other people have already been studying.

And those two things are very crucial to lay the groundwork. Now I say, in saying all this, what I have in the back of my mind as a precedent is actually Newtonian mechanics, because, of course, Newton -- I mean, I'm not going to deny this. Newton -- the big thing is, Newton had a very important achievement to begin with. But where to take that, where to go forward, how to go forward with that into domain's Newton himself did not study was not at all clear.

And so it took quite a while, several decades, for people, in a sense, to play around with the theory, to work with it, to reinterpret things in light of his theory that previously weren't thought about as thinkable in those terms before you actually can come up with some serious experiments that could then test the merits of the theory. So this does take a certain amount of time to do.

Q. Well, just, if you would, give us an example of this either the reinterpretation and then the testability based on some sort of agreed upon test in this area, how a scientific theory that initially means doesn't have a strong experimental showing comes to enter into that feature of scientific progress?

A. Well, I think within Newtonian mechanics, you got a clear case in terms of optics. Newton did some experiments with optics in the 1670's. Results were very inconclusive. At least, the Royal Society didn't believe him. And he always believed that light was a particle, right.

And, of course, the natural way of thinking about light is kind of an as a wave. It's only in the 19th century, once people start to really kind of play around with how do you test the difference between these two things, because at a certain level, given the invisibility of light, right, that it seems that this is just a different difference in metaphors here, right.

I mean, how are you ever going to test this? But indeed, after people start to develop these ideas, you know, in more details, then clever experiments are come up with, and you are, you know, and you get kind of, throughout the 19th century, you might say, tit for tat. Some people supporting waves. Some people supporting particle.

And they go back and forth, back and forth with clever experiments, and then eventually you get to -- sorry. Am I interrupting you?

MR. WALCZAK: I'm sorry. I was trying to be polite here, but, Your Honor, I think this is outside the scope of his expert report. There's no reference to optics. There's no reference to the wave particle theory.

MR. GILLEN: The report sketches the general subject matter of the expert's approach. These are specific examples of the point that he made throughout the report. No expert here has been held chapter and versus, if I day dare say, to the words uttered in the report. These are just examples.

THE COURT: We could go into the report, and I'm reluctant to do that. I think what I'll do is, I'll overrule the objection and ask you to sort of lead it back into the report. I'll give some latitude. So the objection is overruled.

THE WITNESS: I think I finished. I made the point I wanted to make with that example. So I don't want to --

BY MR. GILLEN:

Q. Okay. Well, let's see. Where were we then? Do you regard the, which some asserts to be, the failure of intelligent design at this point in time to produce experiments along those lines to disqualify it from science?

A. No.

Q. Why is that?

A. Well, I mean, it's too young basically at this point. And it hasn't really done all of the theoretical elaboration or the recovery of the appropriate history to set itself in a proper tradition that then would kind of field the imagination to come up with the right kinds of experiments.

Q. Well, in terms of the claim for design and the way it relates to some of the mechanisms that have been testified here, adaptation or natural selection, do you see a way in which intelligent design claims can involve a reinterpretation of currently existing data?

A. Yes. In fact, one of the things that's very striking, if you look at the philosophical literature that ponders this debate, is the degree to which there's a kind of interchange between the word adaptation and the word design. In a sense, what the evolutionists call adaptation could be easily reinterpreted as design.

And, in fact, this is one thing that, in fact, leads a lot of evolutionists to be very skeptical about the kind of omnipresence of the word adaptation in evolutionary theory because it looks like a kind of surrogate word for design.

In fact, I believe Padian talked about, well, you know, irreducible complexity is what we call adaptational packages. You know, there was this kind of equation made here in the testimony, that the kinds of things, you know -- so there is a sense in which, there is the -- there is at least the possibility of doing some very direct translations across these two paradigms.

Q. If the neo-Darwinian synthesis hasn't served at a functional level as uniting scientific and creating the biological area, what do you see as historically doing that?

A. Excuse me. Can you repeat that?

Q. Yeah. If you're saying that the neo-Darwinian synthesis hasn't really served in a functional operative way to guide much of the work that's being done, what are the premises, the implicit premises that seem to be driving?

A. Well, I do think it does provide a kind of metaphysical basis for research, but I also do think there's a lot of, kind of, policing of boundaries going on. In other words, the neo-Darwinian synthesis -- and this is true, I think, of many general scientific theories -- they're doing two things at once.

They're sort of trying to guide research inside, but in the case of the neo-Darwinian synthesis and in a kind of rather loose way among the different biological disciplines, there is also a kind of a gate keeping function that it plays in terms of trying to keep out certain things from being discussed.

And in the origin of the neo-Darwinian synthesis, going back to Dobzhansky's work, there was this concern about eugenics, and that if genetics was made the foundational discipline of biology, full stock, without any consideration of natural history or anything like that, that this would lead down the road of eugenics.

I think in more recent times, there has been this concern about trying to keep religion out. That's been, in a way, kind of perennial, and that's kind of come back again. So there's a sense of which it's a policing function going on with the synthesis.

Q. Well, in terms of that function, many people, scientists have come in here and testified, it's this methodological mechanism which is the hallmark of modern science. And I want to ask you to explain your opinion that methodological naturalism is not an essential ingredient of scientific inquiry

A. Well, to my ears, as a philosopher, I find methodological naturalism kind of strange. As I said earlier, I am a naturalis. But naturalism is primarily a metaphysical position. It is not a methodological position.

And, in fact, it seems to me, and I have not found precedent elsewhere, that this is, this phrase, especially when regarded as the hallmark of the scientific method, is kind of a creature of the cottage industry that's developed around this particular debate.

In other words, you might say, there's a kind of parallel universe of philosophy of science in which this debate is conducted that bears some, but not complete relationship to the real philosophy of science, or real philosophy, for that matter.

And so methodological naturalism seems to be a way of building in a kind of metaphysical commitment without having to say so. So in other words, in order to be able to do science, you have to have a certain --

COURT REPORTER: Could you please slow down?

THE WITNESS: Sorry. So in order to be able to do science, one has to come in with a certain way of seeing the world. It's not enough just to be able to test theories and test them fairly, but one has to think about the world in a certain way first to be able to do science. That is to say that, you know, there is this kind of nature that it's all happening in this one natural world, whatever that may be.

And the implicit contrast is with the supernatural. And if one looks at the history of testability, which is indeed a proper criteria for scientific method, one sees that its relationship with naturalism is incredibly checkered and vexed. It is not any straight -- you cannot read off not naturalism from testability as the criterion for science.

Q. Well, explain that. What do you mean by that?

A. Okay. The key thing about testability that -- because it is the hallmark of the scientific method, no disputing that -- is that it has to be able to -- the theories have to be able to be tested fairly; that is to say, without stacking the deck in favor of one or the other theory and especially not in terms of one of the other theory's assumptions.

So this is turned out to actually be a very difficult thing to kind of make clear and practice, what exactly constitutes a fair test in science. And I think the tendency nowadays in methodological naturalism, as it's being used in this trial and elsewhere, is trying to give you the impression that the way you test a scientific theory is by the terms of the dominant theory, right.

So if you're intelligent design, the test gets conducted by the evolutionists on the evolutionists' terms, and you got to pass those first. But that's not the spirit in which the criteria of testability was meant. Here the benchmark for it, to go back to it, is to Francis Bacon, okay. He talks about the Baconian method in philosophy, 17th century, the lord chancellor of England, a lawyer.

Testability, as the criteria of the scientific method, was essentially an invention of a lawyer. And a lawyer who was very interested in the development of science saw it as, in fact, producing a lot of potential good in the world, but also realizing that scientists come with a lot of religious and political baggage that's very controversial, very hard to see through because they're talking all these different languages and making all these different claims, most of which you cannot verify or validate and so forth.

So we're going to have to figure out some way of figuring out what exactly is true and false and what these guys are saying, because we know they're saying something that's valuable. But how are we going to do it? And so Bacon introduced the idea of setting up a crucial experiment, which is like a trial, right. That's his idea. It was like a trial.

And the idea would be that the judge, who was this independent party, would decide between the two theories that are contesting some point. That's the original image that you're supposed to get. Now as this idea develops through the history of philosophy, the real kind of, you know, modern day benchmark is through logical positivism.

And there the word testability gets used a lot and falsifiability and verifiability and all of these terms that we associate with the logic of theory testing comes from that tradition. Those guys wanted to find a neutral language of science. And they were very preoccupied with figuring out, how can you strip any scientific theory down to its bear logical structure --

so in a sense, we don't need to know the jargon, right.

We don't need to know all the tricky things about it. We just need to know what follows from what and how can you prove it in some empirical way. That's what they wanted. And that's testability. Testability does not commit you to the big assumptions of a particular theoretical framework.

Rather, it strips them down and gets them to a point where you can see what really matters here on the ground level. That was their idea.

Q. Were the positivists working out testability criteria in contrast or with reference to an alternative approach to science and nature?

A. Well the positivists initially had a flirtation with naturalism, but in the end, they believed that it, too, was kind of metaphysical. So they took a very agnostic stance on this. In fact, they thought, well, look, given the developments that were taking place in physics, which were creating rather weird conceptions of reality which really hadn't been worked out, they weren't like the kinds of conceptions of reality associated with traditional naturalism.

If we think about naturalism as Aristotle or Newton, the way objects move causally in some sort of an observable space, these things of things. These very fundamental assumptions, which are associated with naturalism historically, were being challenged by science.

So one couldn't really assume even that bare metaphysics in the sense that one would even have to strip that off if one wanted to be able to test scientific theories appropriately. So this is the whole idea of getting rid of the metaphysics.

Q. Well, in light of that, do you see a meaningful distinction between the claims made here for methodological naturalism and philosophical naturalism?

A. I think -- I mean, I really think methodological naturalism is just a fig leaf for metaphysical naturalism when it gets right down to it, especially when you see how it's elaborated by its defenders and the kinds of things they want to include and exclude and also the kind of rather sort of tenuous history of science that provides the back story for it.

Q. What is that? Just give us a brief sketch.

A. Well, okay. A couple of the people who have testified here, and I've seen this before in the writings of these guys, these methodological naturalists, have talked about Hippocrates as the founder of medicine, the great founder of scientific medicine.

And the way methodological naturalists spins the story is, okay, before Hippocrates hit the scene, the Greeks believed that, in fact, the Gods were causing all kinds of illnesses, right. And here's Hippocrates actually looking at natural causes and looking at the sources inside the body and so forth.

And he collected evidence, you know, and he did things that one might consider rudiments of experiments, and he was a methodological naturalist. Well, it's not so straight forward, because basically, if you were back there in Ancient Greece -- I mean, this is what the historians would say -- that there were basically two approaches to medicine there.

And there are two approaches that, in fact, are very much part of the tradition of scientific medicine; one being a kind of patient centered medicine, which is what Hippocrates was about. What Hippocrates did wasn't just collect evidence from patients, he talked to them. He actually thought that the patients had some knowledge that might be useful in trying to cure them. And that was a very important part of what he was doing.

Whereas all these guys who thought that the Gods were descending upon people were, in fact, disease based, the disease based approach to medicine. You know, what were they talking about?

Well, they had something like the rudiments of what we would now call the germ theory of disease where external agents are, in fact, the causes, right, rather than some sort of disequilibrium in the body. Some external agents are, in fact, the causes of what make people ill and so forth.

Now that's naturalistic, too, of course, right, under a certain description. And similarly, you know, you could turn the tables around and say, well, Hippocrates is asking people for information about their illness, why does he think people would have good information? Well, Hippocrates thinks they've got a soul, that they've got something inside of them that provides privileged access.

Well, that sounds a little supernatural to me, you know. In other words, you can play this game either way. You can run the supernatural as the natural or the natural as a supernatural. So there's a sense in which this distinction is useless for understanding the history of science.

Q. Well, if we take it forward to the present date, do you see areas in which -- areas of science in which there's a sense that methodological naturalism is a deficient analytical framework for inquiry?

A. Well, first of all, I don't think methodological naturalism is used. I mean, I think testability is used. But I think that, in a sense, these metaphysicals, this metaphysical issue of naturalism, I don't think matters one way or another, I mean, as far as scientists are concerned.

They're concerned about testing hypotheses, and they're quite willing to entertain hypotheses from almost anywhere if they end up actually bearing some kind of fruit in research. So the issue of naturalism is, in a way, a kind of way of setting up a kind of metaphysical barrier as it were to only let in certain people who think the right way to do science.

Q. Well, how about in areas like mind, you mentioned to me. Is that an area where some people have reservations about whether this approach is even going to be adequate?

A. Well, it's true that, if you look within the discipline of philosophy, you might get the impression from hearing some of the things here that, in fact, naturalism is the dominant view as a metaphysical view. And it isn't.

I mean, it is quite -- I mean, it is quite dominant among people who do philosophy of biology and certain other areas of the philosophy of science, but in the philosophy of mind, there is a strong resistance to some of the more radical forms of naturalism, you know, largely because it's very difficult in practice, and even conceptually, to reduce, you know, all the properties of the mind to matter.

I mean, so there is a sort of lingering kind of problem there. It hasn't quite gone away.

Q. Is just the fact that intelligent design, at least in light of some proponents, takes issue with that claim to methodological naturalism, does that, in your opinion, rule it out of science?

A. No, not at all. In fact, I think anyone in their right mind who knows something about the history of science or the history of philosophy ought to be contesting methodological naturalism.

Q. Do you see evidence that scientists, practicing scientists today see a commitment to methodological naturalism as integral to their actual scientific work?

A. No. Only the philosophical defenders of a certain kind see this.

Q. You've discussed dichotomy between natural and supernatural in your testimony as we've discussed methodological naturalism. Let me ask you about that. Do you think that the openness of intelligent design to the possibility of causation deemed supernatural, at least by current knowledge, disqualifies intelligent design from science?

A. No. And I think -- what forms my answer is here is, if you look at the history of science, the kinds of things that in the past had been considered supernatural before they were subject to proper tests and empirical evidence and so forth.

One shouldn't think about supernatural as necessarily referring to God, because supernatural also applies to the level that is below observation, because you might say God is above observation. He's sort of up there infinitely.

But, of course, a lot of the things that were called supernatural include things like, well, Mendel's genes or atoms, right. Before it was possible to actually detect empirically the motion of atoms and so forth, Atoms were regarded as cult entities.

Robert Boyle believed in them. Newton believed in them. But those guys had non-confirmist religious views that justified them. But there was a lot of skepticism about atoms, okay, because they weren't observable. They weren't part of the observable level of reality, which was, you know, typically the kind of coin of the realm for naturalism.

Q. Well, let's look at that and what you've just said in light of the testability which has been discussed. Do you think that intelligent design is not science because it's not testable in the sense that evolutionary theory is testable?

A. Well, no. It does not make it science because it's not that, that's true.

Q. Okay. Well, what is your response to the notion that intelligent design is not testable?

A. Well, I think, here we have to think about the ways in which disciplines are testable, okay. And as I was saying earlier about logical positivism, they were very concerned about metaphysical assumptions being built into the conditions of testing, which would, in effect, bias the outcome of the test.

And so there is a sense in which, when we see say that evolutionary theory is testable, and I'm quite willing to accept that locution, we don't actually mean that the most general propositions of evolutionary theory are directly testable. What a we mean is that, the constituentive disciplines that they, that evolutionary theory explains, the claims coming from them are testable.

So we have testable claims in genetics, right, that can be explained in terms of evolutionary theory. We have testable claims in natural history that perhaps could be explained in terms of evolutionary theory. But the testing is of the claims in the particular biological disciplines.

So when Miller, for example, was here with the bacterium, okay, what's -- this is a test of the bacterium and about whether the bacterium flagellum can survive and function under certain kinds of conditions. What is this a test of? Whether that thing can happen. Does this vindicate natural selection in some general kind of way?

Well, only if you add in a whole lot of other assumptions; otherwise, it's making a very specific point about the survivability of the flagellum in a particular kind of environment.

Q. Are those other assumptions you're talking about testable in the sense of the claim with respect to the flagellum?

A. Not at the moment certainly, no.

Q. Well, let me ask you. If you contrast the higher order claims made by evolutionary theorists with the claims made by intelligent design, do you see a comparative or a different situation with respect to testability?

A. Well, frankly, I don't think you can do any -- both -- the theoretical frameworks in which both evolutionary theory and intelligent design operate are largely both metaphysical.

And in that sense, they cannot either be directly tested. The difference is, evolutionary theory is much more developed, much more elaborate, and in that way, much more suggestive of forms of research to do, which then, in turn, can be tested. So it's got that advantage.

So I'm not taking that away from it at all. But I think it is very loose to say, oh, evolutionary theory is being tested directly every time we do an experiment in a cell biology lab, because that is not the case at all. One has to build in a lot of other assumptions in order to reach that sort of conclusion, each of which could be contested.

Q. And that's what I'm trying to get at. Do you see the situation with respect to evolutionary theory as different, marketedly different in principle from --

A. Not in principle, not in principle.

Q. Okay. But you see a difference between --

A. In fact --

Q. Based on what?

A. Based on the stage of the history that they're in. There are two different stages in their respective histories.

Q. Which are significant with respect to the criteria of testability how?

A. Well, because you actually need a certain amount of time for the theory to develop, to construct its implications, to sort of widen its scope, to do the reinterpretation of already existing phenomena. You need to scope all that out before you can actually set up an adequate research program on the basis of which then you can do some tests.

Q. Well, in terms of testability again, let me ask you. Is this openness to the supernatural, does that render ID, therefore, not testable and, therefore, not science?

A. No, it does not. In fact, it may turn out to be a product of the imagination that may lead to hypotheses that then can go on and be testable.

Q. And do you see analogies for that in the history of science?

A. This is the point about bringing up Newton and bringing up Mendel and bringing up Babbage and bringing up all of these people who, in their variously sacrilegious ways, thought they could get inside the mind of God. And they tried to figure out how God's mind worked and what he was doing when he was trying to set up various things.

Q. Do you believe that intelligent design necessarily relies on the supernatural for causation of phenomena in the natural world?

A. No. It relies on intelligent design.

Q. Do you believe that the openness of intelligent design to the possibility of supernatural causation disqualifies it from science?

A. No.

Q. Let's look at the definition of theory and how a theory is viewed by someone with your training. A lot of attention has been drawn to the fact that there are certain definitions of theory which require a theory to be well-tested, well-substantiated. Do you, in your discipline, accept that definition of theory as accurate?

A. No. If what you mean is, does a theory have to be well-substantiated in order to be scientific, the answer is, no, because then no minority theory would ever get off the ground. It would only mean that the dominant theories count as science ever. So how would there ever be any scientific change unless the dominant theory imploded?

That seems to be the implication if one says that only well-substantiated theories count as science. You would never have change except from the inside.

Q. Well, I mean, in terms of that, a related assertion has been that intelligent design is not a theory, because it's just really a negative argument. It doesn't offer anything in terms of the positive explanation. Do you agree with that?

A. No. No. I think one of the things that it does do is, it does provide a kind of a different way of grouping together phenomena. I mean, because I think one thing that one needs to take seriously when assessing the prospects for intelligent design is that, intelligent design is not an alternative theory of biology strictly speaking.

I mean, I think it's -- in fact, it's really covering a somewhat different range, and a broader range, basically anything that can be designed. I mean, I mentioned earlier that one difference between intelligent design people and evolutionists is that intelligent design people take the word design literally across domains.

That is to say, when a human is designing something and when, you know, organisms are being designed by some intelligence, that's literally a design thing happening in both cases. It's the same kind of process going on in principle. And in terms of the way in which biologists want to explain the nature of life, there is, I think, a distinction made between how artifacts are designed and how organisms come about.

And then in that sense, the word design is used more metaphorically in biology. So there is a difference in the way in which the domain is being scoped out. So in that sense, what intelligent design promises is kind of a different sort of way of scoping out phenomena and explaining it.

Q. Well, in terms of that testability and the difficulty of formulating a test for a new theory, do you see precedence? I mean, I think you mentioned Einstein's relativity to mean in terms of how someone comes to grips with the implications of a new theory and has to do that in order to determine a test. Can you give an example that explains what you're getting at?

A. Well, I mean, one thing about the Einstein example is, Einstein, obviously, was really changing the foundations of physics in a very fundamental way, and here I'm thinking particularly of general relativity, which talks about space time being curved, which is a very kind of unusual idea, sort of, to get your head around in a way.

So people thought, well, this is just going to be kind of a metaphysical or something. But the Royal Society in 1919, having studied Einstein's work and having elaborated, suggested a test of the theory, which

Einstein agreed to, which had to do with looking at a solar eclipse in West Africa. And basically, it ended up validating what Einstein would have predicted.

Q. Do you believe that intelligent design is religious?

A. No, not inherently religious, no.

Q. And explain that.

A. Well, the point is, you don't have to be religious to be able to develop it. I mean, I think that's the key point here, that even though historically it's been associated with a lot of religious people, one doesn't need to be religious.

In fact, I would say, and, in fact, this is one of the scopes for development of intelligent design theory across its current constituency, is to look at things like the sciences of the artificial, artificial intelligence, and artificial life, because those ideas, those research programs, in fact, have a design orientation that's quite similar to intelligent design.

Q. Well, you know, in your testimony here today, you have, what shall I say, described a certain sympathy of viewpoint between creator and the scientific mind set that has led to scientific discoveries. How do you separate? How do you police that boundary? How does the discipline, which you work in, create distinction between the religious origins or inspiration and the actual work that's being conducted?

A. Well, this is where the context of discovery and justification distinction comes up. It's precisely for that reason. I think it's worth pointing out kind of the origin of this in terms of what was really motivating him.

So the idea being, you don't want to judge the validity of a scientific theory just in terms of who happens to be promoting it and what their background beliefs and assumptions are.

This distinction was originally coined in the 1930's, and it was basically to get around genetic-based arguments that were being made in Germany at the time trying to invalidate modern physics because of Jewish origins, because the people who were involved were from a -- to a large extent, Jewish, and that this physics was very counterintuitive, relativity, quantum mechanics, and there was a sense of, ah, yes, you know, Jews, very tricky, they say all these kinds of things that, in fact, are trying to befuddle us and all this.

And people were disqualified just on those grounds, sort of racialist theories of knowledge.

Q. Well, how does the distinction that you've voiced addressed that concern?

A. Well, the point being is, you know, any -- that any physicist can work with, develop, and test these physical theories, that one doesn't have to have -- in fact, one doesn't judge the merits of those theories by the origins of the people who happen to have promoted them.

If we actually did do that, if we actually did judge theories by the motives of people who promoted them, we would never have gotten Newton, because Newton was theologically suspect. We would never have gotten Mendel. In fact, we almost didn't get Mendel, because people figured he was theologically suspect.

And you could go down the line of a lot of very important figures in the history of science who do have, you know, very, you know -- you know, if we're going to be banning religion, you know, religiously suspect motives behind their work.

Q. Well, let me ask you, and we've talked about this, but I'd like you to explain to the judge. In this courtroom, there's been this discussion of theistic evolution and a notion ventured that theistic evolution is an acceptable position with respect to science.

And what I've been trying to figure out is, is that -- go ahead --

MR. WALCZAK: Finish your question.

MR. GILLEN: What I'm trying to figure out is, if we look at this relationship between context of discovery and context of justification, is the situation different in any material way than the position posited for theistic evolution in principle?

MR. WALCZAK: Objection. Your Honor, I don't believe anybody in this trial has posited theistic evolution as a scientific concept.

MR. GILLEN: That's not what I asked him.

THE WITNESS: I'm not sure I actually got your question.

MR. GILLEN: Okay.

THE COURT: Hold on.

MR. GILLEN: I'm not taking that point at all, Judge. And I --

THE COURT: Why don't you restate --

MR. GILLEN: Certainly.

THE COURT: -- and we'll see if Mr. Walczak has a continuing objection to the restated question. Go ahead. Restate it.

MR. GILLEN: It may, in fact, be that my question wasn't precise. Vic had that sense, and Steve didn't get it. Plainly, I need to clarify.

BY MR. GILLEN:

Q. You talked about context of discovery, context of justification. In this courtroom, the Plaintiffs' experts, for example, Ken Miller, have taken the position that theistic evolution, his position, is acceptable because it separates religion from science.

I'm asking you, is the context of discovery and context of justification any different when applied to the situation concerning intelligent design?

MR. WALCZAK: Objection. Professor Miller did not testify in any way that theistic evolution is acceptable in science. He's talking about, there are different explanations and they are not inconsistent when viewed as different explanations.

But nobody is talking about the scientific legitimacy or acceptability of any particular religious belief. Our view is that these things need to remain separate.

MR. GILLEN: And that's precisely the point of my question.

THE COURT: Well, you attributed to Professor Miller a particular position as it relates to theistic evolution. That's the basis of your objection, is it not? I think that might be a mischaracterization, so I'll sustain the objection on that basis, but you can rephrase.

MR. GILLEN: Thank you, Your Honor. And I did not mean to mischaracterize Ken Miller's position. Let me rephrase and make it abstract.

BY MR. GILLEN:

Q. There's been discussion by experts of the position, including Dr. Pennock, of a position called theistic evolution, which is regarded by as acceptable by adherence of methodological naturalism, so-called, because it represents an opinion that distinguishes religion and science.

MR. WALCZAK: Objection.

THE COURT: Let him finish his question.

MR. GILLEN: What I am asking you is, is the situation any different in principle insofar as religion relates to intelligent design?

MR. WALCZAK: Your Honor, I still think it's a mischaracterization. I don't believe there's been any testimony that methodological naturalism has taken a position that theistic evolution is acceptable. I mean, science, I think we've had testimony to the contrary, that science is religiously neutral and doesn't take a position on religion.

THE COURT: All right. Well, I understand the question. He can answer it. The objection is overruled.

THE WITNESS: I still don't know if I

understand the question. Sorry.

MR. GILLEN: Okay.

THE COURT: Well, it's first important that you understand it. I understand the question not to be objectionable.

MR. GILLEN: But that doesn't mean it's a good question.

THE COURT: Well, that's right. I don't pass on the question itself as it's answerable. Restate it.

MR. GILLEN: Thank you, Your Honor.

BY MR. GILLEN:

Q. And forgive me, Steve, if this is hard. But what I'm getting at is this notion that there's a position which we know as theistic evolution. Do you understand that position?

A. Yeah.

Q. Is the relationship between religion and science, which characterizes the position theistic evolution, any different in principle between the relationship between religion and science as it exists with respect to intelligent design?

A. I'm having a hard time understanding what you're getting at actually.

Q. Okay. Well, then it must be a bad question.

Give me a minute here, and I'll see if I can --

A. I only want to answer the question if I really understand it, because I hear several things going on.

Q. Well, and I'm not trying to say several things, so maybe we can look at it this way. Do you see the situation with respect to evolutionary theory and its relationship to religion as different in principle from the relationship between religion and intelligent design theory?

A. Oh, I see. No, no difference.

Q. And why is that? Explain.

A. Well, I mean, if -- in terms of the kinds of motivations that people would have for doing both, they could be quite similar. They could be religious or non-religious.

Q. And in your judgment, in either case, would the operative critical inquiry for determining whether the theory of science being that they have a context of justification apart from --

A. Yes.

Q. Okay. And how do you go about demonstrating that a given idea has made that leap into a context of justification?

A. Well, okay. You're able to actually test and criticize and evaluate and develop the theory without sharing the fundamental motivating assumptions of its originators, okay. So, for example, one thing that, in terms of this trial that counts in favor of intelligent design is that it's possible to discuss the theory and criticize it without actually making reference to its religious motives.

So, I mean, I'm thinking in particular about the way in which Dembski's work has been treated, and also Behe's work for that matter, where it is possible to kind of discuss the matter without ever, you know, and if you didn't know in advance, you know, you would not necessarily guess that these people had a religious background.

So the mode of discussion in the academic literature is such that it can be done without reference to that. So that is a sense in which the theory has made the cross-over into the context of justification.

Q. Well, let me ask you. In your testimony, you've demonstrated a sort of linkage between this creationism and/or creator's mind set and intelligent design. Do you see that intelligent design is creationism?

A. No.

Q. Do you think there is some element of continuity there?

A. Well, they're motivationally at the context of discovery level. I mean, I think that's kind of undeniable historically because, in a sense, the context of discovery is something you determined by looking at the histories of the theories and who the people are and all that.

But that is not, at the end of the day, what determines whether it's science. It's what happens once it passes over to the context of justification. I mean, in a sense, it's almost like, you know, you really need other people other than the people with the vested interest in it, to sort of look at it before it can be said to be science.

Q. Would the linkage you pointed to, as historical point of origin or inspiration, would that disqualify intelligent design from science?

A. No.

Q. And again, why exactly? What's your point?

A. Well, it's the distinction between context of discovery and justification. I mean, again, if you look at successful scientific theories, the people who put them forward had all kinds of strange views. And in a sense, you know, were those views taken into account in evaluating their theories? They would immediately be overruled because they often were politically or religiously subversive.

Q. There's a notion in which the intelligent design is said to be a science stopper because of that context of discovery. Do you agree with the notion that a religious context of discovery makes a theory a science stopper?

A. No, not at all. And, in fact, I would say, and this is, I think, this is something I would say about. I made an elusion to this earlier. If you actually look at the history of the way knowledge has developed across cultures, modern science, starting with the scientific revolution, is a very distinctive thing.

And I think there's been no disagreement on that point. But there is always a disagreement about what makes is distinctive. And the point that I would make in relation to this, in relation to the religion point, is that, actually believing, and I know prima facie this sounds strange, but it's a very unique feature, namely that the people who started modern western science and started thinking in these terms was people who believed in a mono-theistic God, and human beings were in the image and likeness of this God.

I'm not just talking about the people in the 17th century. But if you look at the kind of impulse that led the Muslims to unify Greek and Roman knowledge as some kind of common legacy of humanity to work on, which then kind of got carried over, over the centuries, why do that?

Well, there is this idea that human beings in principle have kind of access to the nature of reality, to maybe what the creator was up to. And these guys in Greece and Rome may be able to help us out with this, so we're putting it altogether in one package.

And, in fact, this goes even further, because one of the things that very striking about western culture, and has been very instrumental in the scientific revolution, is the idea that nature has a unity, that indeed one can have, as it were, unified theories of nature, whether we're talking about Newton's theory or Darwin's theory.

And that's actually a very rare thing. First, the idea of thinking of reality as a unified thing, one thing, and thinking of it as something that has, as it were, a kind of structure that is sufficiently both intricate and knowable, okay.

And this is where the idea of human beings being in the image and likeness of God helps, because it suggests, first of all, that there is this creator who makes this one thing, right. And the powers this creator has is, in a way, not that different, at least in principle, to what human beings, as the privileged part of creation, has.

Q. Well, I want to ask you. Has this benefit of a certain western mind set been discussed by a proponent of evolutionary theory?

A. Well, yes. In fact, Dobzhansky, who I mentioned earlier, he was a Russian Orthodox Christian, and one of his later books called The Biology of Ultimate Concern, and there he actually very explicitly says, you know, evolutionary theory is necessary for having a sort of satisfying cosmology, one that is able to actually give us meaning in the universe.

Q. Well, now that's a fairly recent 20th century example. How about, you mentioned Thomas Huxley to me. Did he recognize this same --

A. Well, Thomas Huxley, in a sense, was the person who I first -- the person who first clued me in, you might say, into this aspect of the history of western culture. Toward the end of his life, he gave a very famous lecture called Evolution and Ethics.

And at that point, you know, Darwinism is already a generation old. It's already very important as a kind of cultural presence in England. And there are a lot of people, like Herbert Spencer, for example, Darwin's nephew, Gaulton, all these guys who are basically saying that evolution can provide a basis for ethics.

And Huxley disputes this. And, in fact, one of the things that really concerns Huxley is the fact that it's very important that evolution, given the sort of deprivileging of humanity that goes on in evolution -- in evolution, right, all species, human and otherwise, are subject to the same laws, the same principles, extinction, all the rest of it.

There's a flattening of the antilogical differences, you might say, between different species in Darwinism. Huxley realizes this, and he accepts this as kind of a fact. But he said, had we discovered this very early on, right, we would never have been motivated to do very systematic kind of science, because you think about has -- you take the Darwinian world view as kind of a basis for conducting your life, you just basically say you survive and you die.

And everything happened -- and then the genes just get recycled, as Richard Dawkins would say now. And Huxley points out that, in fact, such -- the metaphysics behind Darwinism, which I just described, was, in fact, known to the ancients, both in the east and the west, and it never motivated them to do science, right.

So, in a sense, there was all kind of primitive versions, what we would call natural selection and so forth, and even notions that there might be some kind of circulation of germ plasm through successive forms, which is like what we talk about when we talk about differences and changes in life forms.

And that never motivated people to do science systematically. What it motivated people to do was to cope with the inevitability of death. Okay. And it's only when you get to a point where you have people thinking, well, you know, the universe may have been the created thing, and the creator may be someone like us, and then maybe we can figure all this out.

And that, in fact, leads to the movement towards science, and that gives, of course, an enormous amount of human arrogance and hubris and so forth. And in light of that, Huxley says, maybe it's not such a bad idea human beings get taken down a peg a little bit, right, in terms of Darwinism, kind of making people a little more moderate, a little more humble about what their aspirations could be.

But it's very important that the humans started thinking about themselves as being in the image and likeness of God in order to motivate all of the effort, all of the thinking, all of the work of a very systematic and specific kind that goes into doing science, because it is really unprecedented in the history of culture.

Q. Is he saying that the, this particular context of discovery was necessary for evolutionary theory to develop?

A. In a sense, yes.

Q. Well, let me ask you. Does that context of discovery also have a relationship to the development of theory?

A. Well, I mean, if you think about theory as something that aims to unify (inaudible) phenomena, which is, of course, the very normal way we think about it in science, there's always a question to ask, why unify? Why unify?

In other words, why not -- because one of the things you find when you look at knowledge in other cultures, especially cultures that have very developed forms of knowledge, like it had in Ancient China or India, places like this, where you actually have very developed disciplines of mathematics, let's say, various forms of technology, medicine, things of that kind, but what you don't have in those cultures is this drive toward unifying all these things under some one large picture of reality that, in some sense, is integrated and interconnected.

And that's largely because they didn't really have a sense of a universe in this kind of modern sense. They basically thought reality was multiple. It moves in many different places, different practices for different kinds of aspects of reality. So there was -- they didn't feel there was any kind of impulse. Why unify?

So I think that's always a question that we need to ask when we think about the motivation for doing science, especially when we're doing theoretical science, is why unify. Why do you want to unify things that otherwise can be explained and worked with perfectly well in their own independent settings?

So Dobzhansky, why does he want to unify genetics, natural history, all these branches of biology, is because he has this kind of universal, unifying view of the cosmos, okay. He doesn't talk about God in his major book. But that's adamanting it.

It becomes very clear in the later writings that that's, in fact, motivating it. And what you even do see in his writings is an attempt to sort of figure out what is a science that, in fact, will, if not serve humanity by being put together in this way, will at least give a kind of coherence to our understanding.

And that's, you know, that kind of drive, that motivation is not something you find in every culture historically, even ones that are intellectually very developed.

Q. Well, just to close off this point. You mentioned these differences between cultures and contexts of discovery as they relate to science. But you've also said that science takes root in non-western cultures. How is that communication possible although there's not the shared context of discovery?

A. Well, because it is possible -- this is where the context of justification comes in. And in the little book I wrote on science, I always use the example of Japan, where Japan is an example of, you know, an obviously non-western place that for many centuries closed off its doors to any kind of external influences until the 1860's, and then very selective appropriated aspects of western culture.

They brought in loads of western advisors and they sort of picked and mixed, you might say, what they wanted and what they didn't want. They kept the science bit. And within 25 years, they became one of the five, ten leading scientific powers in the world, and they've sort of maintained that.

So there's a sense in which, as it were, the testing of the science, that it works, and that you can produce results doesn't actually require that you have this particular mind set that the west had.

Q. All right. There's been some discussion of peer review in this case, and I want to get your sense for peer review and how it affects scientific progress. You've done work on the sociology of science. Just give us a sense for, in brief, for the sociology, the sociological factors that affect the reception of scientific theories?

A. Well, I think one thing, when one talks about this in terms of peer review, I think one thing that's very important to understand is that the function of peer review has kind of, in a way, expanded over the years.

When we talk about peer review initially, I suppose the benchmark is the Royal Society where, you know, it's a self-organizing, self-selecting group of self-defined scientists in the 17th century received a charter from the King of England, and they basically decided who were the members, and they decided what got published in their proceedings and so forth.

The thing that's very important about that early type of peer review was that, what was reviewed, other than your membership into the Royal Society, was the work, whether the work passed muster. And typically, what that involved was, back in those days, not only that you did work that had observations and reasoning that was transparent to other people, but that you didn't insult other people's political and religious views as well.

There was a sense which that was forbidden from the outset. Now over the years, peer review has kind of mutated in a way. And so now peer review is used for a lot more things, not just for publications, but it's also used for determining who gets grants to be able to do research.

And so there's a sense in which, back in the old days with the Royal Society, in a sense, if you were kind of a wealthy person, a person with leisure, you had the time and the wit, you could do some work and publish it, and they might accept it at the Royal Society.

And, in fact, somebody like Darwin was a bit like this. But nowadays, because of the costs of research, the start-up costs in various ways, there is a sense in which people need to get grants in order to be able to set up the labs, in order to do the research that's necessary to then produce peer reviewable publications. But that's peer review, too.

So we get peer review at the very beginning of the process in terms effectively who's allowed to do research, because the way you get money for a grant going through the peer review system is typically in terms of your track record, which gives you a kind of rich gets richer, poor gets poorer situation, because they basically look, has this guy done reliable research before.

Well, you know, we'll then give him some more money to do it. So what happens then is that, the peer review system, in effect, turns out to be a kind of self-perpetuating, you know, elite network where, in some sense, you kind of have to get into that in some way, and it's very difficult if you're not there at the beginning.

So if you don't actually go to the best universities, if you don't get the best post-doc or the best first job, if you don't actually get in to all of those gatekeeping practices, it's actually quite hard to make it through the peer review system.

Q. Well, can peer review, which plainly has benefit in mind, can it be used to stultify or retard scientific progress?

A. Well, here's the problem. As scientific research has become more and more specialized, the number of peers for any given piece of research that gets peer reviewed gets smaller and smaller, which means, there's a greater and greater likelihood that you know who you're reviewing, even though it's supposed to be blind peer review.

So there is this issue of the potential for a conflict of interest to arise in peer review increasingly as time goes on. This is one of the reasons why there's been this great concern about intellectual property law and research ethics boards and all this kind of stuff.

It's a kind of a biproduct of peer review becoming very specialized and the ability of people to be able to sort of, kind of, yes, I know his work so well, you know, I might benefit from it more than he would, you know.

Q. Well, how about in terms of the process you described earlier of an idea trying to get started? Can peer review serve to stultify that starting of a new theory in the professional community?

A. Yeah. I mean, it can and will happen that way. One of the problems with the peer review process generally, and I think one needs to appreciate this, too, it's supposedly a mark of a good citizen of science that you do peer review when you're asked for it. So if I get sent an article to review from a colleague, you're supposed to do it.

You're the guy who knows about it. You're doing a favor to your field. But, in fact, fewer and fewer people are willing to give their time to do it. So it turns out that the peer reviewers, in effect, become a relatively small group of people in the field, even smaller than the potential number, okay.

And so what happens then is, you end up getting fields pretty much bottlenecked by a few people who kind of make all the decisions in effect. And this is kind of the problem. It's not a problem, you might say, that's deliberately set up, but it's a kind of default problem.

And journal editors are always struggling with this. When I was a journal editor, trying to find people who are willing to take the time to peer review work. And you always have to fall back on the same people. And, of course, those people may be very reliable, but it's very risky as well.

Q. And why do you say risky?

A. Well, because you basically have a few people's judgments on which large portions of the field depend. They are peers, but they're not, you know, as it were, you know, they are a very small percentage.

Q. How about the professional societies and the role that they play in mediating claims for scientific theories? Do they present this risk that you've described?

A. Well, I mean, one of the things that's very tricky about science is that, there are lots of different professional bodies represented. All of them get called peer bodies, but, you know, one wants to see how these peers are actually selected and maintained.

So some bodies, you know, are, as it were, self-selecting, where people already in the society select others, you know, the more elite societies, like the National Academy of Sciences would be in that category.

Professional societies are different in the sense that people who claim to be members of the field just pay a contribution and so forth. And so those tend to be quite large, but they're not necessarily democratically represented bodies, right, in the sense of the people who govern those professional bodies aren't necessarily, you know, their accountability to the larger constituency is not so straight forward.

They maybe get elected to office at one point, but then they have kind of a free hand very often in what they can do. So there are issues of accountability here with these professional societies. So it's always uncertain exactly to what extent do official pronouncements reflect actually rank and file views of people in a given field.

Q. Well, at the same time, you peer review. So what's your take on the process as a whole? Is this a risk that's inherent in it or one that potentially crops up in certain situations? Give us your sense for that.

A. Well, it's very difficult. I think one thing is -- well, I mean, there are several things that could be done to deal with this. Peer review, it's kind of like democracies. It's the worst political system, except every other one. Right. I mean, it has that kind of quality to it, that it's not clear exactly what the alternative would be.

But it is -- it's -- in terms of putting, you know, saying, something's intellectual value is proven by the fact it's been peer reviewed. I think one should not make that kind of inference. It's not that peer review is awful, right, but it is sufficiently unreliable and sufficiently questionable that you at least want to find some other means of showing intellectual merit.

You want some other way of doing it. I say this as someone who found a journal and does a lot of peer reviewing all the time. And there's all kinds of work that just doesn't get published in journals. Okay. And so it's not that peer review is intrinsically bad, but it's not a gold standard.

Q. Okay. And you're pointing there to reliability in light of sociological factors?

A. Well, yes, in terms of how the peers are selected, in terms of what percentage they represent of the overall group of people in the discipline. Yeah, I think so. I mean, in the past, it was a little better. I mean, if you look at the history of academic journals, it used to be that academic journals were -- the editors of the journals were these kinds of personalities who, in a sense, you know, very strongly associated themselves with the contents of their journals.

So there would be kind of almost competition among journals to be more distinctive and more innovative. So there would be incentives for these guys to take risks in terms of publication, like Max Planck with regard to Albert Einstein. In a sense, you know, hey, we published this guy, and this guy might turn out to be something, and it shows what an innovative guy I am, and maybe you'd like to publish in my journal, too, kind of thing.

But journals nowadays don't quite have that character. The most prestigious journals in academic disciplines tend to be associated with professional societies, and there the journal editors are typically elected or at least maintained by the professional societies, okay, which means that they operate as kind of, you know, kind of like a chairman of the board where they're responsible as shareholders.

There's a sense in which their hands are tied on a lot of things. And peer reviewed, in a way, in that context serves as serve as a way of not introducing too much distinctiveness or bias that might offend the membership.

So there's a kind of conservative tendency as a result in these kinds of publications, and that the editor doesn't really have a free reign in the matter.

THE COURT: We should wrap up shortly, and we'll take our lunch break. So I just want to alert you as you get through this particular area.

MR. GILLEN: We are wrapping up, Your Honor.

THE COURT: All right.

BY MR. GILLEN:

Q. Steve, let me ask you. Do the concerns you've referenced with respect to the peer system and its potential to stultify scientific progress in some cases explain why you're here?

A. Well, yes. It seems to me that, because of the way -- I really do think, in many respects, the cards are stacked against radical innovative views from getting a fair hearing in science today because of the way peer review is run, the way in which resources are concentrated, and so forth, much more so than in the past actually.

It was a kind of much freer field back in the old days. And so there's a sense in which, unless special efforts are made to make space for views that do show some promise, okay, they're never actually going to be able to develop to the level at which then they could become properly testable and then their true scientific merit can be judged.

So special efforts have to be made. And in one of my earlier books, The Governance of Science, I actually talked about this as an affirmative action strategy with regard to disadvantaged theories. It's not obvious in the normal system of science that these theories will get a fair hearing.

Q. Well, does that concern you have for encouraging scientific progress explain in part why you're supporting Dover's small step in this case?

A. Yes. Well, in fact, that is, in a sense, the main reason, because if you think about this sociologically, how do you expect any kind of minority view with any promise to get a toe hold in science?

Okay. And you basically need new recruits.

This has been the secret of any kind of scientific revolution or any kind of science that has been able to maintain itself. You need enough people on the ground, a critical mass to develop it. You just can't count on three or four people and somehow expect them to spontaneously generate followers, especially when they're being constantly criticized by the establishment.

You have to provide openings and opportunities where in principle new recruits to the theory could be brought about. And, of course, the way to do it, the most straight forward way is by making people aware of it early on, and to show promise, not to mandate it, but to show that it's there. Take it or leave it.

And some will take it. And they may go on and develop it further. And then you'll see the full fruits of the theory down the line. But unless you put it into the school system, it's not going to happen spontaneously from the way in which science has been developing at this point.

Q. And as we wrap up here, let me ask you, first of all, I mean, do you see intelligent design as religion?

A. No.

Q. Do you see intelligent design as science?

A. Yes.

Q. Do you see intelligent design as at least holding out the prospect for a scientific advance?

A. Yes.

Q. Just briefly describe some of the ways in which you see that.

A. Well, I mean, I think that the main thing would be a kind of unified science of design where, you know, the kinds -- the design of artifacts, the design of computer programs, and the design of biological systems and social systems would be covered under one unifying science.

It would be a somewhat different conception of the, you know, map science differently from the way we currently do it, but it's one that's very promising and I think will become increasingly relevant, especially as computers form a larger and larger part of not only how we do science, but, in fact, how we think about the scientific enterprise itself.

And I think the fact that, for example, Pennock claims to be doing biology on a computer, he's showing natural selection on a computer and not by looking at actual animals or even doing lab experiments is very striking. It seems to me, that is moving us in the direction of this design mentality.

Q. Well, how about the openness to the supernatural? Does that militate against the possibility of the benefits you described?

A. No, because, historically, the people who have had these interests have gone on to do important science, whether we're talking about Newton or Mendel, which has been the main examples here, because, in fact, when other people take it up, take up the science they've been doing, they don't necessarily have to share those background assumptions. But nevertheless, once the science has reached a certain point, they can take it further and test the science on its own terms.

Q. Standing here and thinking about it from the perspective of your academic training, do you see that openness that leads to the possibility to the supernatural causation as potentially eristic?

A. Yes, indeed. And it has been eristic. This is not a speculation. It has been eristic.

MR. GILLEN: I have no other questions, Your Honor.

THE COURT: Thank you, Mr. Gillen. This is an appropriate place for us to break for lunch. We will reconvene at 1:40 this afternoon, and we'll pick up cross examination at that point. We'll be in recess.

(Whereupon, a lunch recess was taken at

12:15 p.m.)

CERTIFICATION

I hereby certify that the proceedings and evidence are contained fully and accurately in the notes taken by me on the within proceedings, and that this copy is a correct transcript of the same.

/s/ Wendy C. Yinger

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