

**Episode 2,412: Is Bogus Scholarship the Rule or the Exception?**

**Guest: John Staddon**

**WOODS:**  You wrote to me because I guess you you'd heard my conversation with Terence Kealey on the subject of science, and you said you had a bit to add to the conversation. And did you ever! I absolutely devoured this book of yours, and I'm delighted to have a chance to discuss it with you.

**STADDON:** Well, thank you so much, Tom. I've admired your work for years. I first saw you at an early *Mises* meeting. I can't even remember which one it was, probably the one at Jekyll Island.

**WOODS:** How about that? How devilishly appropriate that setting was, indeed. So, I've written down so many things I want to talk to you about, the publication process, the multiplication of journals. Definitely, I want to get to all that.

I also want to talk about the social sciences a little bit. I'm in history and sometimes that's classified as a social science, sometimes it's in the humanities. But it still has this some of the same problems. But let's start with a point that rather surprised me.

And yet, once I thought about it, I realized that maybe it shouldn't have surprised me. You were discussing in your book at one point the multiplication of scientists themselves. And you make the obvious point that, of course, 100% of mankind being scientists would indeed be too many.

So, certainly, there is some upper bound, optimal bound, to how many scientists there are. It's somewhere below 100% and somewhere above zero. But it seems likely that perhaps we have exceeded – and perhaps this is because of state subsidy – we've exceeded the optimal number.

And that this can have – you might think: *Oh, what's the harm? You have more people investigating more questions. How can that be bad? How can there be too many scientists?*

Well, how indeed?

**STADDON:** Well, that's a very good question. I mean, not everyone is suited to be a scientist. This is another point I've made in this book and in other books. America has a great deal of difficulty coping with the fact that people are not actually equal.

They may be legally equal. They should be morally equal. They are not equal in their ability to do anything. Not everybody can be a football player – or a good one anyway. Not everybody can be a good scientist. So, the optimum number of scientists is set by two things really.

One is the number of truly competent people, that is, they have the virtues of character, intellect, and so on – they're curious, and all the rest of it. And the number of soluble problems. I mean, there are problems that can be solved now that could not be solved.

They did not have the technology available, and so on. They couldn't have been solved a hundred years ago. So, there's a kind of balance between the set of soluble problems and the set of, the size, the class of scientists.

And if the number of scientists exceeds the number of soluble problems, what will happen? Well, will they go away? Will they quit? Well, no, they won't, but the field will be modified in such a way that something can be found for everybody.

But the something will not necessarily be reality-based. That's the problem. And this, of course, is what's happened in social sciences. Social sciences are very, very, very difficult. You can't control the material.

You can't put people in cages and raise them for their whole lives under controlled conditions and so on. I mean, they're really, really difficult. So, what are being created are a lot of bizarre non-disciplines that talk about a little bit in the book.

**WOODS:** I think there seems to be a link between this problem of an excess of scientists and obviously the almost absurd multiplication of academic journals. Because not only do you have all these scientists, you also have placed on each one of these scientists, enormous pressure to publish.

And also, as you emphasize in your book, the emphasis is not so much on the quality of the research they're doing, the papers they're publishing, how much they advance the field and advance our understanding. But simply a crude measure, like, how many papers have you written?

And so, now you get – you have a rather amusing anecdote in there, in which a journal that could not possibly be more remote from your field, wrote to you and invited you to be the editor-in-chief.

**STADDON:** [laughing] I was, of course, deeply flattered by this, but I had to decline. Unfortunately, no money was mentioned.

**WOODS:** [laughing] There never is.

**STADDON:** No. So, obviously intellectual honesty demanded that I declined this prestigious thing. But yeah, the number of journals that have cropped up, it's got to be 20,000, 50,000? I don't know, it's incredible. But this process is forced by the need to publish.

Now, you mentioned citations. Citation index is the number of times you've been cited, one of your papers or all of your papers have been cited over a period of years. Okay, fine. People cite because your work is relevant to their work. That's how it's done.

As a little sidebar, I might mention a new concept has appeared, which you may or may not be aware of. But there's now something called "citation justice". Had you heard of that?

**WOODS:** Is it like diversity, equity, and inclusion?

**STADDON:** [laughing]You've got it. The normally anonymized (I mean, or gender silent) authors of papers should be identified so we know what race they are, and what gender they are, and so on and so on, so that these can all be balanced in your citations.

I mean, this is so grotesquely absurd, I could hardly believe it when I first saw it. I mean, a citation is a paper that is relevant to your work, right? What's the gender/sex whatever of the author got to do with it? I'd say nothing, nothing whatever.

But these folks – I mean, it's beyond comedy. Anyway, the point is, scientists are now professionals. I mean, I talk about this in this book and in other ones. Scientists are now professionals. That is, they have a salaries, they are promoted, and they have to be, therefore, ranked in some way, or rated in some way.

And who's going to rate them? Well, in years past, the most creative scientists were independently wealthy or had a source of money that was personal, was directed at them, not at the details of what they did. Citations were more or less irrelevant.

The people who were most creative were not in any way the most published. They were not the most published. I remember looking a few years ago at the publication rate of WD Hamilton, who's one of the handful of founders of modern biology.

He died, unfortunately, in Africa when he was doing some research there. But I looked at Hamilton's publication rate. First, his publication rate, then the citation rate. His publication rate was about one paper a year.

That would absolutely not make it these days. Absolutely not make it. An even more striking example, perhaps slightly more well known, is Charles Darwin. Now he was independently wealthy. His father was the incredibly successful (and massively overweight, I may say) physician.

He made a lot of money. So, Darwin had his own source of income. And he waited 19 years to publish his idea of natural selection, which is the basis of modern evolutionary theory. Waited 19 years, and he only did it when Alfred Russel Wallace, a young guy working in Indonesia, sent him a paper with the same idea.

Now, even smart people who don't understand science, blame Darwin for not giving the priority (priority being a big deal in science) to Wallace. Most noticeably Tom Wolfe (one of my heroes, wonderful writer, and so on) that wrote a book about this in which he painted Darwin as this sort of evil, high status snob, and poor Wallace is the underdog and so on.

And of course, it's absolute nonsense. Darwin spent 20 years not out of laziness, but to get evidence to really support his idea. If he'd published it immediately, it wouldn't have been credible. And indeed, to repeat the story, Wallace recognized this.

One of his later books he called "Darwiniana". I mean, he didn't feel he'd been stomped on by Darwin in any way. But this is illustrative of the fact that Tom Wolfe is not a scientist, but a very bright fellow, is so infected by the contemporary ethos: *Publish, publish, publish, precedent, precedent*.

That he blamed Darwin in this case. So, anyway, to loop back to my story, academics are professionals. They're not like artists and individuals. And they're treated now – they are, in fact, professionals. They have salaries. who's going to promote them?

Well, not other academics, generally. That used to be the case. It used to be the case that other academics had a lot of influence. Now it's increasingly administrators. And administrators want numbers. And what can they count?

They can't count the quality of the person's work. Creative science, for one thing, involves a lot of failure. It's the guys that may, like Darwin, do very little for 20 years on an idea. No, they want something numerical.

The publications and citations go into it, even though they are almost completely irrelevant to the whole thing. And going on too long. I'll mention one more thing.

Because citation rate has become so important, there is in fact a trend – and I think I cite a paper (or maybe in a later article) pointing out that the number of multi-author papers has massively increased.

During my whole publication career, for example, I published, I think, two papers with more than two authors. Mostly it was one author or two author, maybe three. Now you get papers with dozens of authors. How can this be?

**WOODS:** It must be because it counts as a publication for them.

**STADDON:** You've got it. You've got it precisely. I think that's what's going on. Anyway, I'll shut up and let you ask a question.

**WOODS:** No, no. I love to hear these stories from the field, even if they're depressing. I wish I knew the specifics of this particular statistic. I don't know if it's the social sciences, or history or economics in particular, or all academic journals.

I heard some statistic along the lines of: 60% of published papers are never cited by anyone. I'll grant you, I don't remember what field this is, but that has to ring true on some level. If anything, I'm surprised it's not a higher number.

**STADDON:** I think that's probably true. I can't bring up the statistic on the top of my head. Yeah, that is probably true. I mean, it's notoriously a complaint that the only people who read your papers are the peer reviewers. You know, that's it.

So, that is probably true, but that's not necessarily a crippling statistic. I mean, papers that debunk something, they're not going to get very many citations. The issue's abandoned. People work on something else. So, I'm not too worried about that.

**WOODS:** Oh, that that is a good point. On the other hand, I guess I'm thinking about all the articles I've seen in historical journals where I think: *These articles are the result entirely of government subsidy, because no private person would be interested in any of this.*

And not that the only measure of the worth of something is, is there a patron who's interested in it? But for heaven's sake, the super-hyper-specialized papers about extremely, extremely, absurdly, narrow subjects that I can't imagine anyone would be interested in, means that I can't flip through the *Journal of American History* and – maybe the book reviews.

Sometimes I find an interesting book in the book reviews, but other than that. Now, you mentioned the peer reviewers. Why don't we talk about that? Peer review is supposed to be some kind of a bulwark against the publication of bad scholarship, and these days it's come in for a little bit of criticism.

What do you think about it?

**STADDON:** Well, it has all sorts of flaws. I think I can mention three. One is it cannot be a comprehensive analysis of the data. That would take too much time. The reviewers are never paid. They have plenty of other things to do.

So, all the reviewers can do is detect obvious errors and unclarity. If the guy who's writing the paper doesn't explain himself clearly or something. So, those are the two positive things. It can find obvious errors and it can detect obscurity. But it can also censor, and this is a problem.

If you look at the prestigious journals like *Nature* and *Science*, just a neutral review, it will kill a paper. The 2 or 3 reviewers have to be very enthusiastic about it if it's going to get published. The result is – this is a little incidental result – the papers that get published usually have quite startling findings, quite startling findings.

And studies have recently shown those startling findings are often unreplicable. The magically big result will often be unreplicable. So, there are all sorts of flaws in this. There are other flaws – I talk about these in the book a little bit.

It's so bloody slow. I mean, it can take a year or more to get a paper – maybe even two years – between submission of a paper and seeing it actually published. Whereas the real progress in whatever field you're in (if there is progress) is going to be a lot faster than that.

So, a lot of other alternative methods are being explored, in physics and so on, where people publish open-source papers. They just publish their paper and leave it to other people to comment. It seems to me nobody quite knows how it's all going to work out.

So long as publishing in prestigious, peer reviewed publications is the coin of promotion, that's going to be maintained. But in terms of the progress of science, I suspect more and more of it is going to be these open-source journals.

I mean, actual publication now is almost costless. The journal system evolved when publishing, it was quite costly. You had to actually print things and circulate journals and so on and so on. So, there are forces that are pushing the system in different direction.

But as I say, peer review, which is often presented as a sort of gold standard (a phrase I hate, by the way) is not in any way the gold standard. It can suppress good work, it can encourage bad work, and it cannot catch frauds. And there are plenty of examples of frauds.

The business [garbled words] is notorious the last few years. There's a guy called Diederik Stapel, I think, from Holland, who fraudulently published dozens of papers. I mean, it can't catch that sort of thing.

**WOODS:** You include in your book a passage from *Times Higher Education* that's relevant to this, *"Peer review is self-evidently useful in protecting established paradigms and disadvantaging challenges to entrenched scientific authority.*

*And by controlling access to publication in the most prestigious peer reviewed journals, helps to maintain the clearly recognized hierarchies of journals of researchers and of universities and research institutes."*

So, very much along the lines of what you've just said. It reinforces whatever the existing consensus is, and sometimes the consensus is wrong. A lot of times the consensus is wrong, but it gets the money and the attention.

And the dissenters get, what? Obscurity? And nobody wants obscurity, so there's an implicit incentive just to go along with whatever it is.

**STADDON:** Terence Kealey pointed to one of the sources of this problem, and that is monopoly funding. If all the funding (or the majority of funding) comes from the government (which these days, inevitably, it does) then the tendency to conform is enhanced and accelerated.

So, I think that really has happened.

**WOODS:**  Let me bring up something that you made brief, fleeting mention of, but you also discuss in the book. And this is something that listeners of my show will know I've become a bit interested in over the past few months.

And that is the so-called replication crisis, which at first, I thought was confined to psychology. I did not realize, until a little bit, until I wrote something about it. I had just learned about this, and I wrote this to my newsletter audience.

And then I was getting people in all different fields saying: *Oh, we've had the same problem in our field. We can't replicate these results, sometimes from papers that have been very influential. No doubt they had passed peer review. No question about that*.

And so, I'm curious, can you give me – especially because I know you were retired by then, but I'm sure you keep one ear to this.

**STADDON:** Oh yeah, I've paid a lot of attention to it. I mean, it's this thing. My own research doesn't use any statistics at all, that sort of fundamental Baconian method.

You have one condition at one time. You give another condition, you observe a result. You go back to the first condition, you go back to the second.

That is an individual subject replication, that's 100% reliable. So, no need for statistics. The problem with this non-replicable, it's all statistical. It's all to do with statistical experiments. I can't think of a single case where a single-subject kind of experiment was not replicable.

And I think of Priestley's experiment, showing that oxygen enhances burning, it enhances it, makes burning proceed faster. He can do that over and over and over again. There's no replication. But if you're doing an experiment, let's say, on a drug, I'm trying to back up here to give you the basis.

The basis of all this is the work of a brilliant man, a genius, Ronald Aylmer Fisher. RA Fisher was a really brilliant guy, no question about it, a prodigy. When he was in school – which, his school being, I think, something like Eden in England.

And he worked for a long time at Rothamsted Horticultural Station. So, his job was not basic science, it was applied science. That was what he did. So, he was asked, for example, to design an experiment to compare two fertilizers, or a fertilizer versus no fertilizer.

So, how would he do that? Well, land is variable, rainfall is variable, and so on and so on. So, his method was to come up with a whole bunch of randomly selected plots, randomly assign the two treatments to those plots, okay.

And come up with a decision as to which one is better, given that there's going to be variation in the yield of the plants and so on, and these various plots. So, his subject is not basic science. His subject is making a decision, making a practical decision.

And for that, his method is totally appropriate. In other words, he finds a difference between these two plots, an average difference. He looks at the variability within each plot and says: *Well, if this variability is random,* [emphasizing] *IF this variability is random, then the chance of this bigger difference is* – well, he decided, arbitrarily 5%.

If the difference is 5%, okay, then we'll pick that. Now, notice, if you do it – totally plausible. The subject of the experiment is the population of plots, not an individual plot or anything.

And the decision between these two entails a positive result if you really have picked the better one, and a rather modest cost if you pick the wrong one. Okay? A rather modest cost. So, he's deciding between fertilizer A and fertilizer B.

If I pick the wrong one? Okay, I got the one that's a little bad. No big deal. But this method has now been used – and I must say, I had been skeptical of this method ever since I was an undergraduate. I thought, what the hell is going on here?

I didn't really know what was wrong with it, but I was always skeptical of it. Okay, what's happening here? Now you're deciding in your experiment, not between treatment A and treatment B, but between truth and falsehood.

You're deciding truth and falsehood, and that's absolute nonsense. And I try to go through examples in the book to show that obviously that's what it is. If you accept the null hypothesis – the null hypothesis being, there's no difference.

If you reject that hypothesis, if you reject the null hypothesis, that's treated as truth. But of course, there's a possibility of error. It's called "type two error" and so on and so on. And in science the cost of an error is unknowable, but probably very large. It's unknowable, but probably very large.

In Fisher's case, the cost of an error was knowable and not too much. You know? Wrong fertilizer. But this method has been universally adopted, and it seems to me utterly, utterly crazy. So, that's maybe as much as I need to say about it.

But I'll say one more thing. In Fisher's case, his assumption was that the variation among the plots was random. It's a pretty simple case. That the null hypothesis was the results of a toss of a coin, that's the randomizer.

But in experimental science, or survey science or whatever, the model is much more complicated and much more uncertain. And I try and give some examples in the book where this model would give you a "significant result" and other one would not.

Which one is right? We don't know. Because in these papers the model is really explicitly laid out. And there's always of course this cost thing, but the cost of an error is very substantial. Anyway, I try to give an example, I think, in the book, of a drug research.

You do an experiment on a drug, a controlled situation with a placebo versus an actual drug. And you assume that the probability the drug is going to be successful is 50/50. Is it doing better than that? And so on.

But the assumption is that the – well, this gets a bit technical – that the sample space is entirely defined by your experiment. But suppose other people have done experiments on this drug, a thousand of them. And in fact, it's not effective.

But if a thousand different labs have done this experiment and the drug is ineffective, some of those drug labs will find it to be effective, particularly if the criterion is only 5%. And the 5%, as I say, was a pragmatic criteria adopted by Fisher, totally inappropriate for basic research.

People have pointed out .05 or something would be much better, which would shut down most of social science research. So, the incentives won't allow it to happen. I don't know. This is a tough subject.

I do urge people to read the book. It's a lot clearer than my rambling.

**WOODS:** Well, let me remind people the title, *Science in an Age of Unreason*. So, you should check it. I'll have it linked in the description of the video and at TomWoods.com/2412.

I would have to assume, knowing nothing about the formal discipline of psychology in particular, that given that I think at least maybe that's where the replication crisis began anyway, or at least where it was most notorious.

Surely there must have been some calls for reform of the way we – even if they were feckless, there would have to at least have been a show of a call for reform, wouldn't there?

**STADDON:** Yeah, I don't think there really was. I mean, the precipitating event was a paper by a wonderful California statistician called John Ioannidis, which was published in 2005 with the wonderful title – surely you've read it – "Why Most Published Findings are False".

**WOODS:** [laughing] Yes.

**STADDON:** An admirably direct title, not just the sort of thing you see in a scientific paper. So, that was the precipitating event. I what one of the things that happened in psychology – now, I work in a very particular kind of psychology, with animal subjects because they're much more controllable, and so on.

And what people in that field – there was the same division. There was a division between people who used the "between groups" method and people who studied single animals. Unfortunately, these two divisions were associated with rather radical philosophies.

The single subject method was promoted by a man called BF Skinner – you've probably heard of him – who was a brilliant experimenter, a wonderful writer, and a very poor philosopher.

So, his idea was: *It's all stimulus and response and reinforcement. If you reward the animal, they'll repeat it. If he doesn't, you punish him. Repeat, and so on.*

So, a very simplistic view of the organism. The other side within the animal area were centered around Yale University. Skinner, of course, was at Harvard. And they looked at groups. They were much more sophisticated, theoretically.

They were interested in processes going on with learning, memory changes, and all this kind of stuff. But their method was this fallible method. And I'll give you one example. I have to give you one example. This is a hypothesis proposed by a senior colleague of mine many, many years ago in Canada.

And it's called "the frustration hypothesis". I'll bore you with a description of an actual experiment, because it's a simple experiment that gives you the idea.

So, his idea (the frustration theory idea) was that if an organism, an animal, human, whatever, is used to getting food in a certain place, and then he comes and there's no food, he's frustrated. And that energizes his behavior.

Just like when you pull a door and it's sticky, you pull harder, right? I mean, so, intuitively, a totally plausible thing. So, how to test this? Well, he tested it with what's called a "double runway". These are rats. Rat experiments, right?

So, here's the start box. And he runs to a box in the middle, three feet away, gets food, runs another ten feet and gets more food. And you repeat this. So, he's used to getting food in the middle box, okay? It's simple. He keeps doing this.

And now, to test your frustration hypothesis, on half the trials you omit the food in the middle box. So, he runs, expects to give food, doesn't get it, and, lo and behold, he runs faster in the second runway. Aha! This is frustration.

Now there are conceptual problems with this. I'll skip those for now. But I'll just talk about two things. One thing, how would you test that? How many rats would you need to test that hypothesis, do you think?

**WOODS:** I don't know, being in the so-called soft sciences. I would think the more the better.

**STADDON:** But this is the hypothesis about individual rats, right? And you can repeat it. It's one of these repeatable things, yeah? You should need one rat. Think about it.

**WOODS:** Fair enough. Okay, I understand your point.

**STADDON:** You run it. It gets food, it doesn't get it. You're comparing two measures of one rat. Well, of course the averaging method was completely standard in those days.

So, Abram Amsel, the guy who promotes this theory, he had 20 or 30 – maybe 18 rats in his first experiment, and he got a significant result. Only just, it wasn't really big, only just. So, that's my first point.

Why on earth would you need more than 1 or 2 rats? By your hypothesis, it should be the case. Well, I won't go into all the history of this, but the bottom line is, what was really happening was nothing to do with frustration.

The rat comes to the middle box, gets food. The second runway is much longer than the first, so there's a bit of a delay, and that automatically produces some hesitation in the rat. When there's no food there. No hesitation. He runs faster.

But my point is that the group method was so inbuilt that it was used even in situations for which it's completely unnecessary. Well, it was necessary in this case because the effect was weak.

But he could have done a better job by having the second runway even longer, and he would probably have gotten a bigger effect. But nobody did that.

Okay, so Amsel in 1962? '50s? I can't remember. It was a long time ago when he did this, had a good excuse. Because everybody did it. But the most recent example of that was a man called – and this became quite famous because of a book by Stuart Ritchie called, *Science Fictions*, I think the book was called.

This is a guy called Daryl Bem who thought he had shown precognition in humans – precognition! And he ran dozens of humans and got this unreplicable (needless to say) result. So, the method goes on. But Daryl Bem didn't really have Amsel's excuse to do this bad method.

Anyway, I'll shut up and let you ask another question.

**WOODS:** Well, I could keep you for three days and don't want to. So, I have a few things that I'll never forgive myself if I don't cover.

**STADDON:** Well, this is fun, go ahead.

**WOODS:** Okay, well, I appreciate that. Thank you. You have a statement that I would just love to hear you elaborate on. You say – I don't know what the page number is.

But you say, *"Much of social science is now either obscure, or false, or nonsensical."* And that was just music to my ears. And now, you have a lengthy discussion of sociology in particular, but are there some areas of study that are worse offenders than others? And what would be examples of this?

**STADDON:** Well, I think any area which touches on a politically sensitive topic. In the second edition of my book on scientific method (which is hopefully about to come out within a few months) I talk about some studies.

For instance, there's one set of studies – and this is classic hokum, I think, in many ways. This is a study of something called "benevolent sexism". Have you ever heard of benevolent sexism?

**WOODS:** I have not.

**STADDON:** Well, it's a whole literature on benevolent sexism, which purports to show that men being nice to women, and favoring them, opening the door for them, and so on, it's just a way of controlling them.

**WOODS:** Of course.

**STADDON:** And the way in which this is established is a lot of statistics. It's a tedious method, but the way it's done is you come up with a concept like benevolent sexism – or intelligence. That's another one. You come up with intelligence, right?

Let's start with intelligence. How would you go about validating that concept? This is the domain within which benevolent sexism belongs, right? You give them a test, you get a number out of it, you validate it. How is intelligence validated?

Well, it's an interestingly and rather impromptu sort of approach. People come up with questions. There's no rule for defining those questions, except it looks like they're intelligence-related question. So, you come up with this test. You give it to people.

And then you've created this construct called "intelligence". Then you validate it somehow. How do you validate it? Well, there are two ways. One is repeatability, replicability. If you give the test repeatedly, do you get more or less the same result?

For IQ, yeah, you do. That's the reliability test. But there's also a validity test. How do you test the validity of it? Well, do the numbers you get from your test predict what most people think is intelligence? Performance in mathematics, difficult technical tasks, executives, and so on.

Does it promote grades in college and all that? And the answer is, it does. So, that validates it. Well, it's this model that this benevolent sexism purported to follow. So, the inventors of this test came up with a bunch of questions.

I wish I could remember them. It would take me a little checking on the computer to find them. But they came up with, I think, 22 questions. And they applied these questions to a group of people, a whole bunch of people.

Now, I won't go into the detail, we probably don't have time, but did they test the reliability of it? Well, yeah, they did, and it wasn't very reliable, actually. How about validity? How would you test the validity of something like this?

Well, there isn't any way. I mean, is it correlated with happiness of marriages? You know, if you're low in benevolent sexism does that mean you're more honest? So, on marriage, I don't know. I don't think it really was validated. I think they were content to come up with this measure.

Now, this was all in service of a war against sexism. This whole thing was a war against sexism. Which is not, of course, a scientific notion. They never defined sexism, it's just simply assumed.

Anyway, there's a long analysis of this which will be helpful sleep aid to some of your readers in the scientific method book.

But the whole point of it is it's completely arbitrary. It doesn't satisfy the kinds of tests which have validated IQ as a measure. I imagined an alternative measure.

By the way, women like benevolent sexism, of course, they do, anyhow. But this, I suppose, is what Marxists will call "false consciousness". You like it, but it's really bad for you.

Anyway, I invented another one called "mutual respect". You could come up with 20 questions on mutual respect. And you could show that maybe people are happily married if they have this, and you've got your case.

So, this is all completely arbitrary unless you really, really validate these measures. By the way, the same thing goes for this implicit association test, which has become a kind of political weapon for the DEI community. I'm sure you know a lot about that.

But yeah, to really validate these tests, IQ (which is vilified) is in fact the model. Because that's one of the very few, that has actually been validated and proves to be a reliable predictor and so on. It does all the other things the test this supposed to do.

**WOODS:** You mentioned DEI just now, and it reminds me that in your book you have – I think it's the National Science Foundation, but I'm sure it's other institutions as well – issuing statements about the problem of systemic racism and underrepresentation of minority groups in STEM and what they're going to do about.

So, they just assume that because people in certain groups aren't in STEM in proportion to their percentage in the population, that something sinister is going on. That is assumed to be not in need of demonstration.

**STADDON:** That is, I'm afraid, a sort of criminal thing. I mean, not only the NSF, but many of the major scientific societies, the American Psychological Association, blah, blah, all adopt this absurdity.

Two colleagues of mine just got something like $9 million to increase the number of women in computer science, from NSF.

And I remember I wrote to one of these ladies before the grant was awarded (before I even knew about the grant) and said: *Well, why do we need more women in computer science? Are they being discriminated against? (In which case, it would be very bad.) Why is this even an issue?*

And she got very mad. She wrote quite a nasty letter back. And it's grotesque. I mean, who cares how many people are in what, so long as there's no discrimination, as long as they're judged fairly? It seems absolutely obvious, but it's totally thrown out the window by our modern science organizations.

I mean, NSF gives – I don't know, I think the last figure I saw was $50 million – to this nonsense, increasing the numbers of people who are disproportionately represented. Yeah, of course, you know, in football, blacks and males are disproportionately represented, right?

In nursing, women are disproportionately represented. So what? All of this has as its kind of conceptual basis, or philosophical basis, the idea that people are really all identical and if they're not, it's some social evil that is causing this difference, which is absolute rubbish.

Sorry, I'm a little vocal about this.

**WOODS:** Oh, me too. It makes me crazy. And there's so much evidence against it. And the automatic assumption that: Everybody is sinister, except me, of course, the author of this holier-than-thou statement. I, of course, am morally superior to the rest of you.

But the rest of you are all perpetrators and perpetuators of injustice. And after a while you have to wonder, if we keep throwing $50 million, and $1 billion, and this and that, and nothing changes, maybe this doesn't work. I mean, at some point we have to say: *The hypothesis is invalidated.*

**STADDON:** Well, unfortunately, the population of people making that judgment will be deteriorating as a consequence of this because merit has been downgraded. There's a wonderful paper.

One of the authors is – I'm trying to remember her name – a Russian immigrant lady, on merit, defending merit. It was published, I think, in the *Journal of Controversial Ideas*. This is an article defending merit in science, signed by a whole bunch of people.

And the only place they could get it published was the *Journal of Controversial Ideas*. This is somehow regarded as controversial. So, if you keep admitting on non-merit-based criteria, the population will deteriorate. The smart people will be excluded or ignored.

And so no, no, it won't necessarily rectify itself. That's the problem, I think.

**WOODS:** I think if you and I co-edited the *Journal of Controversial Ideas*, it would become very interesting, everybody's favorite journal all of a sudden.

**STADDON:** Yeah. I'd never heard of it until this. So, if I remember, if you're interested, I'll send you a copy of this paper. I mean, I think it was written up in the *Wall Street Journal*, actually. I'm not sure.

**WOODS:** How about that? Last thing I want to cover – obviously, we cannot do justice to it. You spend a lot of time on it in the book. You spend a good deal of time on the subject of climate change.

And in particular, I want to ask you about what's wrong with the idea that: *Look, all the respectable scientists say so and so, and you don't have the credentials even to speak about it.*

I think that is part of what annoys me. Because during Covid, I was told that because I wasn't a medical doctor – I was only a PhD, I wasn't a medical doctor – I had no business saying anything. But I'm not speaking about how Covid-19 works in the body.

I'm speaking about charts that anyone can read. I look at all these places that are using all these mitigation measures, and the results seem to be just noise. They're just random. There's no clear indicator that they're doing any good.

Any layman can do that. But we've been intimidated because we're supposed to believe that science and scientists constitute a priesthood who can't be questioned. And so, I think some of this is bleeding over into climate change study.

**STADDON:** Yeah, absolutely. I mean, scientists may be a priesthood, but there are a hell of a lot of sects in that priesthood.

**WOODS:** And there are a lot of people who've been sacrificed by this priesthood.

**STADDON:** That's right. That's right. I think there is a lot of conformism. I remember a year or two ago, the Duke Nicholas School of the environment got a new head. I didn't know this lady. She had very good credentials.

But at the same time, I read a paper by two guys, one Will Happer, and I think the other guy was called Wijngaarden. Both of them physicists, not biologists. And they had a bunch of arguments that seemed to me pretty compelling arguments against the anthropogenic carbon dioxide hypothesis.

And again, they are physicists, right? So, I suggested, wouldn't it be nice to invite these guys? And if you're skeptical, have a physicist come in and comment on it and so on. Of course. Nada. Zero result, which is sad.

So, that's part of the problem, that there's an established view and people who don't agree are just not addressed. I mean, it's like a talk I went to just this Friday by a chap talking about – this is off the topic, but same issue – who was writing about black economists.

Oh, I thought: *This ought to be interesting.* Well, there was one name that wasn't mentioned: Thomas Sowell.

**WOODS:** [both laughing] Of course.

**STADDON:** What on earth is going on? It's just absolutely bizarre. Anyway, so, the point is that people who are not playing the company song are excluded. And I have a friend who's an electrical engineer, and he got me interested in all this stuff several years ago.

And that's why we wrote a couple of chapters that went into the book. We collaborated on those two chapters, and I've thought about it quite a bit since. And there's reason to be skeptical of the carbon dioxide hypothesis.

And I don't have a graph here, but I'll tell you about three graphs which absolutely do it – make one skeptical. The first one goes back millions of years, and it uses proxies to measure the temperature and the carbon dioxide.

The first thing you see is that the current level of carbon dioxide is almost the lowest it's ever been, and in fact, if it goes very much lower, plants are in trouble. They won't have their food. Anyway, it's low.

But the other thing is, if you look back on it, you look for a correlation between carbon dioxide and temperature. And there basically is none, over this period of hundreds of millions of years, okay? The carbon dioxide is high, temperature is low, blah, blah, blah.

So, this shows a flag to be raised. The next graph looks at a shorter period of years where there is a correlation. If you look at 700,000 years, there appears to be – and this is a very good correlation. Graphs are all over the place, you can get them on the Internet.

But this one paper (and several other comments by other people) shows that there is a peculiarity about this graph, in that if you look at the details, the carbon dioxide increase follows the temperature increase, not precedes it.

Now, unless the rules of causality have been reversed, this is a little odd. How come the temperature rises, then the CO2 rises? Within 400 to 800 years, it seems to be the period. Well, the explanation is that if the earth heats up, the ocean can hold less dissolved gas.

Carbon dioxide comes out of the ocean (where most of it is, actually) and into the atmosphere. So, that's the reason for this.

And you can make an argument (and a few people have) in terms of feedback loops, and so on. That this is, in fact, self-reinforcing and it really is the carbon dioxide that's the problem. That's the second graph.

The third graph is a physics graph. I'm not a physicist. I can't judge this, but it looks pretty good to me. This is a graph owing to these two guys, Princeton physicist Will Happer, and another physicist called Wijngaarden.

And the graph shows – this may, again, put your audience to sleep. Imagine that the Earth is devoid of atmosphere, just like the moon. The sun's radiation comes in. How much of that radiation is retained, and how much of it is sent back into space? The result is a sort of bell-shaped curve.

Now, put in an atmosphere and carbon dioxide, with its current concentration, 400 parts per million. How much of that – and what you see is that there's a big drop in the amount radiating into space, in a certain wave band. So, the carbon dioxide does indeed retain heat.

Now, Happer and Wijngaarden say, what happens if you double the concentration of carbon dioxide? It's already very small, 400 parts per million. If you double it to 800, what happens to that retained energy?

And the answer is, almost nothing. In other words, beyond a certain point (according to them) the amount of heat retained by the Earth doesn't change at all. So, these are all three pretty powerful points.

I'm not qualified as a physicist to judge them, but my conclusion is there's a great deal of reason to be skeptical in the discussion of it. The problem is most of the people in so-called climate science are not physicists.

They don't understand this end of the problem. And they're probably more concerned about biological issues – about which, concern is legitimate. I mean, you deforest the planet, the effect on the ecology and so on. All of those are totally legit, but may or may not be due to carbon dioxide.

My guess is that – if I had to bet, I'd say that carbon dioxide, at this point, has a negligible effect on climate, and the standard curve that's given for it, the so-called "hockey stick" curve shows a complete, almost perfect similarity between the rise of carbon dioxide owing to industrial activity and the increase in temperature.

That's probably not right, because usually there's a delay of 800 years. They're not synchronous. But my bottom line is there's reason to be skeptical. And there ought to be more discussion of it in environmental institutions.

They ought not to be just trotting along and accepting these things unexamined.

**WOODS:** Well, I think one other point to bear in mind is – again, it comes from our experience with Covid. Is that we've learned that a lot of scientists have real trouble discussing tradeoffs. Whatever their particular obsession is at the moment, no cost is too great for them to try to accomplish it.

Now, I think in the case of Covid and in the case of climate change, it's not even clear that human effort can really accomplish a lot with either of these things. That you can ruin a lot of people's lives. You can make (in this case) energy very expensive.

You can make the lives of people in developing world borderline impossible. You can do all that. And still, it's quite likely you'll accomplish nothing other than spread misery. But there's no real attempt to reckon with the costs of anything that they're doing.

And incidentally, we shouldn't even look to scientists for that. That's a philosophical question...

**STADDON:** Absolutely.

**WOODS:** ...of what do you value? What is important for us to value in life? Maybe obsessing over climate is the highest value. But maybe the quality of life is a higher value.

That's not something that I look to a scientist to solve for me, anyway. I certainly shouldn't look to him for that, unless I'm extremely superstitious.

**STADDON:** Yeah, I mean, I've argued, I think, in this book and in this scientific method book that's coming out, that science is like a map. It tells you how to get to a destination. It doesn't tell you what destination to get to.

**WOODS:** Right.

**STADDON:** I mean, it should be absolutely obvious to people that the ethics of science (and science does have ethics) are to do with science, is to do with discovery, verification, and so on. They're not guides to good living, believe me.

**WOODS:** Well, I want to urge people to read your book, which I had not known about (shame on me!) until you wrote to me. And I felt like I had been taken on quite an intellectual ride by the time I finished it.

So, once again, it is *Science in an Age of Unreason.* It'll be linked in the description of the video, also on the show notes page, TomWoods.com/2412.

Dr. Staddon, I very much appreciate your time today and thanks so much for writing to me.

**STADDON:** Well, thank you so much, Tom. I say, I've admired your work for years and now I'll be able to watch TomWoods.com.

**WOODS:** That's the spirit! Thank you very much again.