Naomi Oreskes

Welcome to the New Books Network.

Marshall Poe
Hello, everybody. This is Marshall Poe. I'm the editor of the New Books Network and you're listening to an episode in the Princeton University Press Ideas podcast. And today I'm very pleased to say we have Naomi Oreskes on the show and we will be talking about her terrific book: *Why Trust Science?* It's out from Princeton—was out in 2019 and it could hardly be more timely and I'm really looking forward to talking to Naomi about the book. Naomi, welcome to the show.

Naomi Oreskes
Thank you. It's a pleasure to be here with you.

Marshall Poe
Yeah, could you begin the interview by telling us a little bit about yourself?

Naomi Oreskes
Sure, my name is Naomi Oreskes. I'm a Professor of the History of Science and also affiliated Professor of Earth and Planetary Sciences at Harvard University. I work on the development of scientific knowledge. I try to understand how scientists come to the conclusions they do about the natural world, how they judge and evaluate evidence, and how they decide when there's enough evidence to say that something is established scientifically. My work focuses primarily on the earth and environmental sciences, but I am interested in these questions as a general problem in all scientific knowledge.

Marshall Poe
It's funny you mention that, because I did an interview a couple of weeks ago with a philosopher of science in which we went back and forth on how much evidence is enough. That's a really tough problem.

Naomi Oreskes
It is especially interesting in science because unlike the law, where there are at least stated standards such as beyond the reasonable doubt or preponderance of evidence, in science we don't even really have that.

Marshall Poe
Yeah, right. I think a lot of people get confused by statistical validity, which is also a little bit arbitrary.

Naomi Oreskes
Well, that's right. I mean, statistical validity is this very strange thing that has this aura of absoluteness. But it's completely a value judgment. People decided somewhere along the line that a certain level would be considered statistically significant, I've written about that actually.

I think I mentioned this actually in *Why Trust Science?,* but if not, I know for sure we talked about it in *Merchants of Doubt*, you know, we could argue that when it comes to public policy, 51% ought to be enough to tilt the balance, to decide what to do.

Marshall Poe
Right, right. This is a very tough problem with no easy answer. It's a certified hard problem, some mathematicians say. Can you tell me a little bit about the origins of *Why Trust Science?* I know it's based on some lectures and how you decided to put the book together with Princeton?

Naomi Oreskes
The book *Why Trust Science?* is really based entirely on my engagement with ordinary people talking about climate science. So, after the book *Merchants of Doubt* came out in 2010, my co-author Eric Conway and I got a lot of invitations to talk about climate change, climate science information, and disinformation. And in many of my talks, I would talk in great detail about the history of climate science, explain what the evidence was that led scientists to conclude that man-made climate change was underway and then use that as a foundation to talk about why some people were trying to muddy the waters and poison the pool of public debate.

So, when I would give these carefully crafted lectures, and you work hard to explain the science in a gentle and kind and compassionate way, almost invariably there would be someone in the audience who would stand up at the end, typically with a bit of a belligerent body language, "Well, that's all very well and good. But why should we trust the science?" And I remember thinking, "Well, you know, this guy might be belligerent but he has a point. It's a legitimate question."

It's not enough just to say to people, "Oh, trust us, we're experts." There is an obligation, I think both the kind of intellectual obligation, and a moral obligation to explain the basis of trust in science. And I came to things that actually most scientists were taking for granted, that they were sort of entitled to public trust.

And I think we live in a world where even though maybe in theory that would be nice, in practice nobody gets to be entitled to anything anymore. We all have to explain. If we want people to trust us, we have to give an account of why we are trustworthy.

Marshall Poe
And this is a hard thing to do actually because a lot of science is very complicated. And also, there are these value judgments. We'll get to that in the interview. And you have to be explicit about those value judgments like statistical validity. It's not like it's written in the stars that X percent is statistically valid. Well, we'll get to that in a moment, but I—

Naomi Oreskes
I just want to say one more thing on that. I don't think it's so hard because there's no absolute rule about it, because let's face it, very few things in life have absolute rules.

I think it's hard because it's so taken for granted that most scientists if you said to them, "Well, why should we trust science?" They'd be like, "Well, because it's science," and that's just not an adequate answer.

Marshall Poe
The logicians would say that's a tautology. You can't do that. Sorry, that's not an explanation in the Popperian sense.

Naomi Oreskes
That's not an explanation in any sense.

Marshall Poe
Yeah, I guess that's true. You begin the book with a history of answers to the question, “Why trust science?” I thought that was really quite interesting. I studied philosophy of science a bit myself, and this gets us into the territory that philosophers called epistemology and then the philosophy of science itself. Can you briefly take us through this history? I want to call it like a potted history, but I don't know whether that's derogatory or not. English people say that, right?

Naomi Oreskes
Yes, that bad. That’s what historians say when scientists or philosophers try to do history. It’s not a potted history, it’s kind of what in the ancient world they would have called an epitome or a kind of digest.

So, what I did in writing this book was to go back to works that I had read over the years as a graduate student, as a professor, and to try to make sense for my readers. Well, what kinds of answers have people given before? Because as you said, it's not obvious what the answer to this question is. And so, one way to deal with a question for which the answer is not obvious is to survey the field of other answers that people have considered and rejected. So, I look at what is essentially a history of the philosophy of science since the mid-19th century, and what I show in the first half of the book is that people have tried a lot of things, a lot of the early work in this area, say from the mid-19th to the early 20th century, is focused around what we would generally call the scientific method, the idea that there's some kind of method in science that guarantees the correctness of its results.

And so, if we could identify what that method is, then we would have the answer to our question. And so, I talk about what some of the methods that people thought were mostly what boils down to inductive versus deductive. That is to say, observational methods, based on going out into the world, seeing what's happening, and trying to make some kind of reliable generalization or deductive methods, where you have a hypothesis or theory, you deduce the consequences and then you say, “Okay, well, do those consequences hold up?”

What I show is that even though there has been a very robust discussion about these ideas by very smart people, some of them definitely smarter than me, neither one of those ideas really holds up in practice and they don't hold up logically. You can find logical objections to both approaches. And they don't really hold up empirically, because in neither case can we say that's what all scientists have done. And it was that observation by historians and sociologists of science, particularly around the 1960s to 70s, which really led to the explosion of the area known as the sociology of scientific knowledge, sometimes referred to as science and technology studies, sometimes reduced to the notion of social construction of science. But the gist of that is people saying well, hold on a minute. If we actually look at what scientists really do, we find a couple of important things. And one of the most important things we find is that scientists don't work alone. So many of these other theories focused on method, as if so long as the individual used the appropriate method, then everything would be fine.

But really that's not an accurate representation of what scientists do. What scientists do is very much to work together in teams and collaborations and to judge and evaluate each other's work. And if you really look at the practice of science, publications in peer review journals, workshops where papers get pre-circulated, preprint services where things can be posted nowadays on the internet, all of this is organized around giving the opportunity for other people to critique your claims. And so, what comes out of this and what was particularly emphasized in the 1980s and 90s by feminist philosophers of science, is that it doesn't really matter how you get to the claim in the first place.

Some scientists use inductive methods, some scientists use deductive methods. Some scientists build physical or numerical models. The really crucial thing is what happens once you've got the claim. And so, what I come to in the book, drawing on the feminist philosophers of science, is that the critical vetting of claims is really where the rubber hits the road epistemologically, because it's in that kind of trial by epistemological fire that we see what claims can stand up to critical scrutiny, and what claims can't.

Marshall Poe
That's an excellent answer. And I can say that as a historian, this seems like common sense to me because when you publish a history paper, people come and attack you or they take it apart and you know, are you right or are you wrong? It's not that simple. It's much more complicated than right or wrong. So, for example, you know, verifiability or its opposite, which its name is escaping me right now. Falsifiability. It's not like that. Yeah, it's not like it just gets—it's not just false. Nothing is just false.

Naomi Oreskes
Right, and this is something else that I talk about in the book. So, one of the things that Ludwik Fleck pointed out back in the 1930s, was that in a way you can think of science as a game of telephone. The person introduces an idea, then there's an exchange of ideas. There are arguments, maybe that person goes back and collects more evidence, or maybe some other people collect more evidence. And then the original idea gets modified in light of the new evidence or in light of the critiques.

And over time, something new emerges, which is actually typically quite different from what you started with, and Fleck says, so whose idea is it? The person at the beginning, the person at the end? He says, well actually it's everybody. We all get credit. We all *should* get credit because the knowledge emerges out of that process of proposal adjustment, modification, refinement. So eventually what comes out at the end could be quite different from what started and everyone who has been part of that process has some, you know, deserves some credit for the final outcome.

Marshall Poe
Well, this is why in history books, I don't know about scientific papers, but in history books there is usually an introductory chapter that has lots of historiography and you have to go back through it and list everything back to Aristotle.

Naomi Oreskes
Scientists used to do that, but they don't do that so much anymore. And so, it's very easy to lose sight of what that longer process looks like. And the only way you really get a sense of it is to stand back as a historian can do and say, okay, well, here's what this debate looked like. Here's how it evolved over time. Here's what the trajectory of knowledge looked like, you know, here's where they began, here's where they ended up. If you take that seriously, you get a very different picture of science, which is much less individualistic. You know, heroic tales of lonely geniuses pretty much fall by the wayside. And instead, what you get is something that is much more social.

So, it doesn't mean that the theory is socially constructed in the sense of bearing no relationship to nature. No, of course it bears a lot of relationship to nature. In fact, I mean the closer it is to nature, the better the theory would be, but we get there through a process that's very, very social.

Marshall Poe
And in a sense, it's more of a, I mean, we think about the scientific method as more of a scientific process. It involves lots of different—I want to use the word institutions, and I want to talk a bit about institutions because they have come under fire, wrongly, I think, and people have kind of lost trust in them. I founded and edited an academic journal. I don't think it has come under fire, but it

was an institution and is an institution. Can you talk about the role that institutions play like that? And I mean, universities and colleges and departments and research centers and so on and so forth. And journals in that process.

Naomi Oreskes
Sure, institutions are a crucial part of modern science. In some sense, we can say science itself is an institution or enterprise. I like to use the word enterprise, but I think it kind of conveys the sense that in any enterprise there will be parts and those parts include the individuals who are in the enterprise, the methods and techniques and tools and machines and instruments they use. But also, the institutional structures, like research laboratories, research universities, government laboratories, the scientific societies that sponsor the conferences where people come together to debate their findings, the journals where they published peer-reviewed articles, the academic presses that might publish scientific textbooks. So, all of this is part of the enterprise.

And again, it's when you step back and you think of it that way, then you see this as this very social process. Now, when people first started focusing on the social dimensions of science, back in the 1980s, a lot of scientists took great offense. “How dare you say that what I do is social, how dare you compare what I do to a bowling league, what I do as an exalted intellectual.”

I think those scientists so missed the point, and I say this in the book, if they hadn't been so busy taking offense, as if anyone should have the temerity to actually ask questions about what they do or why the public should pay for it, they would have realized that, actually, this is an argument for the strength of science and not the weakness.

And this is sort of the take-home message of the book: the strength of science is in its social fabric. The fact that so many of these people can work together. I mean, in a way, it's a very fancy way of saying two heads are better than one, right. We have a sense of this. If you could bring a lot of smart people together, in some kind of process in which they freely give their own time and their own intellectual capital and you submit a paper for peer review and without paying me any money, I read that review. I say, “I think these things are good, but I think there's some problems here, there.” I send it back to you, you go back, and you fix those problems. I've done you a great service, right? And then you do the same for me. I mean, not necessarily me personally, but we all participate in this process of sharing our knowledge and our insights.

And so, we produce something that is stronger, and more robust and more reliable than if any one of us had done it alone. So, rather than seeing the social basis of science as a weakness, I argue it's actually not just a strength. It is the foundation for why we should trust science.

Marshall Poe
And you get to go to conferences.

Naomi Oreskes
And you get to go to conferences. Sometimes in fun places, but often not. Or you go to fun places, but you never get to spend any time there because you’re busy.

Marshall Poe
Yeah, that's right. So, is it correct to say that this is kind of a—I want to use the word mechanism, but that's not the right word— a self-correcting set of institutions? Is self-correcting too strong or—

Naomi Oreskes
I always like to say science isn't self-correcting but scientists correct each other. Yeah, I mean the whole point of this process is that people are fallible. That we know that we all make mistakes and

we all have blind spots. We all succumb to motivated reasoning, and particularly once we think a theory is right, we may have a tendency to push for it. So, rather than expect scientists to be some kind of superhuman uber objective creature, which is not possible, we say no, I mean we know that nobody's perfect. But if we work together, we can identify flaws and fix them. And so, I don't like to say that science is self-correcting. I think that's kind of a weird thing to say, but the process is highly focused on identifying errors and correcting them.

Marshall Poe
Yeah, I think that's well said. It is, or should be, if it's operating correctly, focused on those things. And I think that again, as a historian, this is not a problem for me, but you have to be ready to be wrong.

Naomi Oreskes
Absolutely. I often said this, I mean, this is the thing I think that a lot of people don't understand about science and maybe it's because the media tends to highlight, super uber self-confident, typically male scientists, you know, someone like Neil deGrasse Tyson or you know, even Carl Sagan in his heyday, you know, these incredibly smooth confident guys who come across as if, you know, they could never possibly be wrong. But that's a really, really terrible image to put forward because it should be the opposite, right? In my opinion, the ideal scientist is the one who can say, "Hey, you know, you might be right about that."

And the story I love to tell comes out of one of my earlier books. So, in my earlier book on the history of plate tectonics, I assembled a group of scientists who had been crucial to the development of plate tectonic theory, and one of them, Xavier Le Pichon, told this incredibly wonderful story that I thought encapsulated everything that's right about science when it works well. So, in 1967 or 68, sorry, 68, he had just finished doing his PhD thesis at Columbia University, proving, based on heat flow data, why plate tectonics could not be right. Then, he went to the annual meeting of the American Geophysical Union meeting that year in San Francisco. And it was one of those conferences that everybody who ever went to it will, you know, they could tell you about it today and get goosebumps just thinking about it, was kind of the moment when a lot of really key aspects of this theory came together, and many people thought, oh my goodness. Wow, like we are witnessing a scientific revolution and so Xavier tells the story of going home and saying to his wife, "Darling, pour me a stiff drink because everything in my PhD dissertation is wrong." Imagine that moment. And what courage and bravery it took to admit that he has just spent five years of his life writing a thesis that's completely wrong. And then though he writes the next day, he went back to the office. He gathered up the data and he started again and then he wrote what would turn out to be one of the most highly cited papers ever published in the history of our science up to that time.

Realizing that he had made a mistake, you've been wrong, but regrouping and then quickly adjusting, pivoting and contributing to this new theory. Like I get goosebumps telling that story.

Marshall Poe
That's a great story.

Naomi Oreskes
It's really a story of great personal intellectual courage and that's what we ask scientists to do. They don't always [unclear] to do it, but collectively science as an enterprise does do it.

Marshall Poe
Yeah, it's funny you mention this, because we're starting a new podcast on the New Books Network called, "How To Be Wrong." Yeah, in which we're going to have scholars come on and explain mistakes that they've made.

Naomi Oreskes
That's a great idea because, you know, we all make mistakes and if we would just not feel like it was such a big deal—I mean, well, I won't say bad things about my university, but I think, you know, many people, the more famous they get and the more successful they are, the harder it becomes for them to admit mistakes and so science in a way gives you coverage, gives you an institutional structure where, you know, if you get a tough review, you can say, "Oh, well, that reviewer was really a bit of a glass bowl, but, you know, he might have been a little bit right about that and I'll see what I can do to fix it," right?

Marshall Poe
Yeah. Yeah. Yeah, that's exactly right. So, just to introduce the concept which I think a lot of, I will call them lay people, or just people, think about in terms of science and that is objectivity. It's somehow central to science and trust in science, but objectivity has kind of taken it on the chin in the last 50 years. Where does objectivity, this concept of objectivity, if it is philosophically sustainable anymore or not, I don't know, how does it fit into your picture about the trustworthiness of science? How does objectivity arise? How is it practiced?

Naomi Oreskes
Well, here I drew heavily on the work of feminist philosophers of science, particularly Sandra Harding and Helen Longino, who made an argument some years ago for what Sandra Harding called strong objectivity.

So, previous scholars had a model of objectivity as a characteristic of a person. So, objectivity is something that inheres in me as a trait and a virtue that if I am objective, then I'm a good person or I'm a good scientist. Whereas if I'm subjective, I'm a bad scientist. So, what Harding and Longino did was to really challenge that and to say, no the crux of scientific objectivity is not about the characteristics of the individual. It's about what the community can achieve through this process of critical scrutiny. And so, what Harding calls strong objectivity and what I embrace and use in my work is to say, we achieve objectivity, to think of objectivity not as a trait, but as an achievement, and the way we achieve it is through this give-and-take, through this interchange of different people looking at the problem from different perspectives.

So, we achieve an objective perspective by looking at it from many different angles. And then, this becomes the argument for why diversity in science is not just a moral virtue, but an epistemic virtue as well. Because the argument that Harding and Longino made, which I think is correct and I build on, is to say, if the community is not diverse and if everyone is looking at the problem for more or less the same angle, then there's a good chance that you miss things and we certainly have plenty of examples in the history of science where that's true.

But if we can assemble a diverse community, where people look at the problem from many angles and are open to each other's points of view, then we have the possibility of achieving what Harding called strong objectivity. And that's what I argue for in the book. And so, diversity isn't just the right thing to do. It's actually necessary to get the right answers.

Marshall Poe
Yeah. This brings me back to something that John Stuart Mill did, or did not say, I don't know. But it was something like, "you need to get all the ideas out there in order to find out which ones are right."

Naomi Oreskes
Yeah. And I mean, the problem with Mill, of course, is that he has an excessively idealized view of how public debate can yield the correct answer and certainly in the period we live in now, we know that disinformation, lies, falsehoods, and other kinds of things can poison the well of public debate.

So, I'm not a [unclear]. I don't take a naive view that if you just let everything bloom, it will all somehow be fine. But within the constraints of scientific communities where people have agreed on the ground rules and you have reason to believe that people are operating in good faith, and the good faith assumption, I think, is a really important one—

Marshall Poe
Yes, it is.

Naomi Oreskes
But if you have reason to believe people are operating in good faith, then I do think that hearing from a diversity of standpoints and letting a lot of different scientific flowers bloom is really important for what I think we call the progress of science.

Marshall Poe
Yeah, and this brings us back to institutions and even credentials and things like this and experience. I mean, you know, we tend to poopoo these things as Americans like, oh, well credentials. We don't care about those and the fact of the matter is, you need to get your ticket to the game. You also have to go to the game, and the game has rules, and it's not, everybody gets to play.

Naomi Oreskes
People who say, “Oh, I don't believe experts.” Frankly, that's just bogus, right?

Marshall Poe
Well, yeah, and then they go to the doctor.

Naomi Oreskes
I use the example of the dentist. I mean, if you have a sore tooth, you don’t call the plumber, right? And we all know that in ordinary life. So, we all know, we all understand that there is a reason for expertise and so the same in science, right? If I have a problem in climate change, I don't drag out some PR guy. Well, the fossil fuel industry does, but we have to understand that that's propaganda.

So scientific communities are based on people who are essentially the dentists of that question, people who have relevant expertise and are qualified to judge the claims that are being put forward and have a commitment to operating in good faith.

Marshall Poe
Yeah, and just to editorialize a little bit. This is something that's bad about social media because everybody gets to talk, that's not good.

Naomi Oreskes
I just like to say everyone has the right to talk, but I don't have to listen. The problem we are having in our society is that we've lost track of who we should be listening to.

Marshall Poe
Yeah. I think that's exactly right. Let me ask kind of an odd question. We had Michael Gordon on the show. I've talked to him a couple of times, and he says it's kind of hard to differentiate in the moment between pseudo-sciences and sciences. Like he would inspire us to say that there are things that we're calling pseudo-sciences today that in 50 years will be called sciences. I don't know if maybe he was exaggerating.

Naomi Oreskes
I think he was trying to make a point. I mean, I understand why he says that because Michael has done some very fine work on some interesting lines of investigation that sit in the liminal zone between accepted science and not and it's certainly true there are things that in the past were considered completely legitimate, like alchemy or astrology, that we would look back on now and reject. So, it is essential to historicize these things in order to really understand and the whole notion of a clear demarcation criteria where we can easily say, “Yes, this is science” and, “No, this isn't.” Michael Gordon is right to say that, yeah it’s not that simple.

But that said, like anything that has a spectrum and a gray zone, there may be stuff in the middle that’s gray, but there's also stuff at the ends that are black and white. Yeah, we can sometimes say, “That is propaganda.” We can identify that as “not science” because it's being run by an advertising agency or PR company or because we have the historical documents to show that the fossil fuel industry had an organized program that they planned in advance to confuse people about climate science.

So, we can say, “That is not science,” and I don't call those things pseudoscience. They're not even—pseudoscience implies that there's some relationship with science. I mean, this is propaganda. This is disinformation. These people are not scientists. They have no scientific training. They have no scientific credentials.

That's not a hard call. And the fact that there are some things that might be a hard call doesn't obviate—

Marshall Poe
Yeah, it doesn’t follow.

Naomi Oreskes
Right, it’s like if you are a referee in baseball, you know, there may be some hard calls, but then there are obviously things that are just way outside the strike zone.

Marshall Poe
Yeah. That's an excellent metaphor. I'm going to mention that to Michael next time I talk to him. It’s a good one.

Naomi Oreskes
And then there are some things that aren't even baseball at all.

Marshall Poe
Right, exactly. Well, you've got to play by the rules of the game. That's the thing. And people don't understand this. That there are credentials and seminars and labs. They have rules, right? You need to follow them. Journals and peer review, this is all very rule-bound activity—

Naomi Oreskes
Right, and the rules are not—it doesn't mean that the rules don't sometimes get broken or bent and it's not like the Ten Commandments. It's not like I can hand you a piece of paper that says, you know, here are the Ten Commandments of science. It's not like that, but there are established practices and many of those practices are based on track records of experience and they may change over time. They're not absolute but we can still recognize: what does it mean to be educated? Why do we expect scientists to have a PhD? We can answer those kinds of questions.

Marshall Poe
Yeah. I think we could actually take a page out of the business world, which I'm kind of in now, having been a professor, and they don't talk about rules, they talk about best practices. This works often, but they're not going to say this is an absolute commandment. This is probably the way you should do things.

Naomi Oreskes
Frankly, I don't really like the best practice thing because, well, having been a university administrator, I think sometimes it's an excuse for just like checking the boxes. Oh, I did that, I did that, I did that, it's all fine and not actually paying attention to the outcome.

And again, scientific practices are about outcomes, right? It's not enough just to have a paper be peer-reviewed. You also have to have an interesting result.

Marshall Poe
Yeah, result. Did you get a result like a capital r? A Result.

So, you mentioned the petroleum industry and things like this. And, you know, again related to objectivity, this touches on the idea of values and bringing values to your work. So, objectivity is a process, a social process, how do values fit into that then?

Naomi Oreskes
Well, values are a big part of the argument for strong objectivity. So, the idea that a scientist could expunge their values and be completely value neutral is not a claim that is supported by any scientific [unclear], in fact, the opposite. We have lots and lots of evidence from cognitive science and social psychology that tells us that even if people think they're being objective, chances are they're not. So rather than sort of aspire to some kind of unachievable ideal, or some sort of false idol of pure objectivity, better in my opinion to acknowledge, yes, of course, we have values.

And so again, this becomes an argument for diversity in science. I do bring my values to the table, I admit, but I work in communities of people who don't necessarily share my values. So, their perspectives will be different from mine and that's important. And so, it is really important that scientific communities be diverse because it's one of the ways that we can try to make sure that no one set of values unduly influences the outcome.

The other thing that's important about this is that values play a role in two senses. One is in terms of the actual work and then the other is in terms of how we talk about and communicate our work. So, most scientists I know were raised to think that values were bad, that they were something you have to deny, and that you should never ever talk about your values. Because if you did, that would imply to people that your science was biased, but I think that's really wrong. And I think we have evidence from, you know, we have evidence from marketing and from business case studies and also just from ordinary life and common sense. We know, from research and social psychology, that what is it that makes you trust another person?

Well, one of the things that makes you trust another person is that you think that they share your values and we see this a lot in electoral politics where people will vote for a candidate even though they may disagree with that candidates' positions in many specific areas, but they feel that that person shares their values.

And we also see this in advertising, right? Why do advertisers use celebrities to advertise a product? Well, because the audience, we feel as if we know that celebrity because we watch their television program, we listen to their music and we like them and we connect with them and relate to them on a personal level. And so, therefore, we want to use the same shampoo that Jennifer Aniston uses, even though my hair is totally different.

Marshall Poe
Yeah, she has better hair than I do.

Naomi Oreskes
Yeah, she has better hair than almost anybody, which is why she is selling shampoo.

Here's the thing. When scientists don't talk about their values, they're not giving anybody anything to hang on to. There's no way to evaluate whether we share values with a person if they won't talk about those values, but when scientists do talk about their values, often they find that people sympathize with them and we often find that we actually share a lot in common with our audiences, even sometimes our audiences that we might not think that.

And so, in the book, I give some examples of that. Many scientists would assume they have nothing in common with an Evangelical Christian. And yet, if you think about the fact that many climate scientists are concerned with preserving, what many Christians would call God's creation, then suddenly you realize, oh, you actually do have something in common, you might just be using different words to describe it. But actually, what you want is the same, but you can't get to that point of recognizing it as the same if you deny that it's even present.

Marshall Poe
Exactly. Exactly. Well, that's very well said. Well, Naomi, thanks very much for being on the show. We've been talking to Naomi Oreskes about her book: *Why Trust Science?* And this is an episode of the Princeton University Press Ideas podcast. I'm Marshall Poe, the editor of the New Books Network and thank you for listening. Naomi, thanks for being on the show.

Naomi Oreskes
You're very welcome. It's been a pleasure.