Breakthrough Prize Winner Gerard ’t Hooft Says Quantum Mechanics Is ‘Nonsense’

Lee Billings June 2025

After netting the world’s highest-paying science award, preeminent theoretical physicist Gerard ’t Hooft reflects on his legacy and the future of physics

In the pantheon of modern physics, few figures can match the quiet authority of Gerard ’t Hooft. The theoretical physicist, now a professor emeritus at Utrecht University in the Netherlands, has spent much of the past five decades reshaping our understanding of the fundamental forces that knit together reality. But ’t Hooft’s unassuming, soft-spoken manner belies his towering scientific stature, which is better revealed by the mathematical rigor and deep physical insights that define his work—and by the prodigious numbers of prestigious prizes he has accrued, which include a Nobel Prize, a Wolf Prize, a Franklin Medal, and many more.

His latest accolade, announced last April, is the most lucrative in all of science: a Special Breakthrough Prize in

Fundamental Physics, worth $3 million, in recognition of 't Hooft's myriad contributions to physics across his long career.

His most celebrated discovery—the one that earned him, along with his former Ph.D. thesis adviser, the late Martinus Veltman, the 1999 Nobel Prize in Physics—showed how to make sense of non-Abelian gauge theories, which are complex mathematical frameworks that describe how elementary particles interact. Together, 't Hooft and Veltman demonstrated that these theories could be renormalized, meaning intractable infinite quantities that cropped up in calculations could be tamed in a consistent and precise way. This feat would change the course of science history, laying the groundwork for the Standard Model, the reigning paradigm of particle physics.

But beyond this achievement, 't Hooft has made many other breakthroughs, which are too numerous—and, in most cases, too technical—to thoroughly describe here. Among the most notable is his proposal of the holographic principle in the 1990s. According to this notion, all the information within a three-dimensional volume of space can be encoded on a surrounding two-dimensional surface, akin to a hologram. The idea has since become central to many efforts to unify quantum mechanics and Einstein's general theory of relativity in an all-encompassing theory of quantum gravity.

In a conversation with *Scientific American*, 't Hooft spoke about his Breakthrough Prize, his optimism for the future of particle physics, his dissatisfaction with quantum mechanics, and the scientific and cultural effects that have arisen from some of his most provocative ideas.

An edited transcript of the interview follows.

Courtesy of Puja Sonneveld

It seems you've won practically all the big physics prizes at this point.

Some are still missing! But, yeah, I've won quite a few prizes.

What worries me a little bit is that most of them were for the same thing. You get prize after prize for something that has already been recognized, whereas other things I've done in science are not as well known—not by the general public, at least. But anyway, the Breakthrough Foundation has made a summary of my work for which they gave this prize, and that contains practically all I have done!

Yes, the foundation included it all! But given how many prizes you have won, does this one feel like just another notch in your belt? Has this all become routine for you, or is it still exciting?

I can assure you: nothing is routine. All these things are different. The climax really was the Nobel Prize itself, which is granted to only a very few people every year. And that's something very special. But this one is also very special. It's a big prize, literally speaking.

Your work in the 1970s with Martinus Veltman is celebrated in part because of its importance for the Standard Model of particle physics, the most well-tested and successful scientific theory ever devised. But in some respects the Standard Model has become notorious, too, as its myriad validations have seemingly left physicists with no obvious path forward to further breakthroughs. Does this aspect of the Standard Model's decades-long dominance worry you?

No, not at all. I think it is natural for science that we cannot always have an infinitely continuous stream of discoveries and new insights. There will be periods, like the one we are in now in particle physics, where things seem to be quieter. I just saw the news from CERN, for instance, that at the Large Hadron Collider, they've detected in new channels the absence of CP [charge parity] symmetry. This is a very important finding but not an earth-shattering one. It seems we're in a period where scientists in my field make many smaller discoveries that, in themselves, are very pleasing because they make our understanding more complete. But I think history shows it won't always be like this. There will be more fundamental findings that will again change our views on what is going on.

In the past few centuries there were long periods in which very little seemed to be happening. James Clerk Maxwell joined electricity and magnetism in the late 1800s, and around 1900 Max Planck made the first observations about energy being quantized. In reality, of course, many things did happen in other fields such as statistical physics and other fundamental branches of science. And both then and now, there's been steady progress in those domains. Look at astronomy right now; the astronomers have their great moments all the time, and you can't say there's a dull moment at all! They're discovering many new things in the universe as their telescopes become bigger and more accurate and as they use more and more fundamental scientific techniques to enhance their resolution. You can say much the same thing about biophysics or medicine, where discoveries are made nearly every day.

But in my field, you're right, it seems to be that nothing is happening. I don't agree with that, though. Things are happening, just at a more modest scale.

Are you optimistic, then, that this situation will change, and we'll see a resurgence in big particle physics discoveries?

That's a very good question because it looks as if there's nothing we can do. If the situation proceeds in such a way that every new breakthrough requires a 10-fold, or even larger, increase in the size, power and cost of machines, then clearly we won't get much beyond where we are now. I cannot exclude such obstacles standing in the way of progress, but the history of science suggests that in such a case progress will simply go in different directions. One may think of not only precision improvements but also totally different avenues of discovery such as cosmology and black hole physics.

I would like to advise the new generation of scientists: don't worry about that, because the real reason there's nothing new coming is that everybody's thinking the same way!

I'm a bit puzzled and disappointed about this problem. Many people continue to think the same way—and the way people now try to introduce new theories doesn't seem to work as well. We have lots of new theories about quantum gravity, about statistical physics, about the universe and cosmology, but they're not really "new" in their basic structure. People don't seem to want to make the daring new steps that I think are really necessary. For instance, we see everybody sending their new ideas first to the preprint server arXiv.org and then to the journals to have them published. And in arXiv.org, you see thousands of papers coming in every year, and none of them really has this great, bright, new, fine kind of insight that changes things. There are insights, of course, but not the ones that are needed to make a basic new breakthrough in our field.

> *"We know superposition in the macroscopic world is nonsense. That's clear. And I believe that in the microscopic world it's clearly nonsense, too." —Gerard 't Hooft, theoretical physicist*

I think we have to start thinking in a different way. And I have always had the attitude that I was thinking in a different way.

Particularly in the 1970s, there was a very efficient way of making further progress: think differently than your friends, and then you find something new!

I think that is still true. Now, however, I'm getting old and am no longer getting brilliant new ideas every week. But in principle, there are ways—in, one could argue, quantum mechanics, cosmology, biology—that are not the conventional ways of looking at things. And to my mind, people think in ways that are not novel enough.

Could you give an example of the novelty or difference you're referring to?

Sure. My way of thinking about the world, about physics, about the other disciplines related to physics is that everything should be much more logical, much more direct, much more "down to Earth."

Many people who write papers on quantum mechanics like to keep some sense of mysticism about it, as if there's something strange, almost religious, about the subject. I think that's totally false. Quantum mechanics is based on a mathematical method used to describe very ordinary physical effects. I think the physical world itself is a very ordinary one that is completely classical. But in this completely classical world, there are still too many things that we don't know today; there are steps we're basically missing on our path to deeper understanding.

What kinds of steps?

I'm talking about steps that would exploit the fact that the whole world is very simple and straightforward. The trouble is, the world still appears complicated to us now, which is why we're in this situation.

You already mentioned the Standard Model, this marvelous discovery from the previous century. It's an instructive example because, basically, it's very simple, but if you look deeper, you see there's something very important missing from it. The Standard Model is based on quantum mechanics, and quantum mechanics tells you what happens when particles approach one another and scatter. But they can scatter in many different ways; they have a large number of choices about it, and the Standard Model doesn't give any sound prediction there. It gives you only statistics. The Standard Model is a fantastic theory that handles the statistics of what things are doing. But the theory never tells you with infinite precision which choice nature makes; it tells you only that these different possibilities are there at a certain probability amplitude. That is the world as we know it. That's how we know how to phrase the laws of nature. But it's not the laws of nature themselves.

What's missing is our understanding of what it is that makes a particle go sometimes this way, sometimes that way. Well, you can easily argue particles can hit each other at a tiny distance. They don't hit each other directly head-on but hit at some angle, and then they scatter away at some angle. That may be true. But what the theory today is not saying is what I should actually be looking at if two particles approach each other so I can predict how they'll scatter ahead of time.

Imagine if you knew the way such interactions would go as precisely as you could know what will happen when two grand pianos hit each other. In principle, for the pianos, you could say exactly which wire will hit each other wire; you could predict exactly what happens when two grand pianos collide. Could it be the same with particles? In practice, such predictions for particles are considered to be too hard, and you turn to statistics, and you conclude that your piano particles can scatter in all directions, and that's all there is to be said. Well, for looking at pianos, maybe you can say something more. If you know exactly where and at which angle they will hit each other, you can predict ahead of time how they will scatter. And that should be in our theories of the elementary particles as well—and it isn't.

I'm saying we should start to think in these ways. People refuse because they think quantum mechanics is too beautiful to be wrong, whereas I believe quantum mechanics is not the right way of ultimately saying what basic laws objects obey when they hit each other.

While I was preparing for this interview, I found [a conversation] you had in 2013 with one of my predecessors here at *Scientific American*, George Musser. And one of the things you discussed was [the work of physicist John Bell] and [its implications for the nature of reality]. You said that you considered locality to be "an essential ingredient for any simple, ultimate law governing the universe." It sounds like that's still your view.

Very much, absolutely. I think, in fact, that you can understand and explain quantum mechanics very well if you assume the laws are only local laws. Let us say that what these particles do when they collide is determined by the exact spot they are in when they hit each other. That is, what happens at other spots in the universe, in principle, should not matter. And if it does matter, then you have what we call nonlocality. But nonlocality would be a disaster for most solid scientific theories!

I don't believe nonlocality is necessary. We don't know exactly what to do when two particles collide because we don't know whether particles look like grand pianos or like pure points. But then again, they can't be pure points because pure points can't do anything. There's something in there, and we should be able to write down all the laws on what's in there for these particles: How can they collide against each other? Why is it that they sometimes go this way and sometimes go that way? How can they exhibit spin?

We should be able to phrase such things as solid laws, and we are not even close to that. And this is why I think other breakthroughs should still be possible—many of them!—to help us get closer to this level of understanding that we simply don't have for particles today, not even as something approximate.

In my talks with theoretical physicists, I've noticed that the greater and more accomplished the individual is, the more likely they are to say, "The real challenge is not in answering old questions but rather in finding new, better questions for whatever problem you're addressing." I think that's because there's this temptation for optimism about what can be known—this feeling that if we ask the "right" questions, meaningful answers must emerge. Do you really think the problem is that we're not asking the right questions, or might it instead be that we're asking the right ones, and their answers are, against our hopes, simply beyond our reach?

What you just said, that the questions are beyond our reach, is exactly what people said a decade and a century and a millennium ago. And of course, that was the wrong answer each time. We can answer these questions, but doing so requires lots and lots of science. Before Maxwell, nobody understood how exactly electric and magnetic fields hang together, and they thought, “Oh, this is impossible to find out because it’s weird!” But then Maxwell said, no, you just need this one term, and then it all straightens out! And now we understand exactly what electric and magnetic interactions do. It’s simply not correct that you cannot answer such questions. You can, but you have to start from the beginning, like I said about quantum mechanics.

If you believe right from the beginning that quantum mechanics is a theory that gives you only statistical answers and never anything better than that, then I think you’re on the wrong track. And people refuse to drop the idea that quantum mechanics is some strange kind of supernatural feature of the particles that we will never understand. No! We will understand, but we need to step backward first, and that’s always my message in science in general: before you understand something, just take a few steps back. Maybe you have to make a big march back, all the way back to the beginning.

Just imagine: What would your basic laws possibly be if you didn’t have quantum mechanics? Answering that, of course, requires saying what quantum mechanics is.

Okay. So what is [quantum mechanics](#)?

Quantum mechanics is the possibility that you can consider superpositions of states. That's really all there is to it. And I'd argue that superpositions of states are not real. If you look very carefully, things never superimpose. Erwin Schrödinger asked the right questions here—you know, take my cat; it can be dead, it can be alive. Can it be in a superposition? That's nonsense!

And he was quite right. People shouldn't continue to insist that a dead cat and a live cat superimpose. That's complete nonsense—yet at that level, it seems to be the only correct answer to say exactly where the particle is, what its velocity is, what its spin is, and so on. There must, however, be different kinds of variables that evolve in time, such as integer-valued variables or discretely moving variables, to name just two possibilities. These would be variables in terms of which you can't move a cat, you can't say whether it's dead or alive, unless you would make more nonlocal changes. There must be ways to describe all states for live cats and for dead cats, but these states will mix with states that don't describe cats at all.

Using superpositions, then, is just a trick that works at first but doesn't get at the states we want to understand. We have to make that step backward.

Walk me through this for a moment. If superpositions are illusory in that they are purely mathematical concepts

that have no basis in physical reality, how does that square with the ongoing success of quantum information science and quantum computing, where it seems as if superposition is a real physical phenomenon that can be leveraged, for instance, to do things that can't be done classically?

I think quantum technology is just what you get if you assume the reality of superimposed systems. What do I mean by that? We know superposition in the macroscopic world is nonsense. That's clear. And I believe that in the microscopic world it's clearly nonsense, too, even though it may seem we have nothing besides superposition to use to understand atoms. What people in quantum technology probably don't realize is that they're doing the very converse of what they think they are doing. They think they're understanding quantum mechanics. I think what they should be doing instead is trying to remove the quantum mechanics from the description, trying to use more fundamental degrees of freedom, like those discrete states I mentioned.

They're not asking the right questions, and that failure makes things look more and more complicated—more and more quantum mechanical—whereas in reality they shouldn't be interpreted that way.

Weren't we just discussing the tendency of eminent theorists to talk about not asking the right questions?

Let me say that, yes, they do the right experiments. Yes, they try to make the right things. And, yes, their quantum computers may be more powerful than anything else for certain applications because they understand "quantum mechanics." By that, I mean they understand how these microscopic systems actually act, in great detail, because this knowledge is something that actually came out of studying the quantum world. Yes, we know how small objects react and interact. But our problem is that at present we can only make statistical predictions. As soon as a quantum computer gives you statistical distributions instead of correct answers, well, that's the end of your "computer"; you can't use it for most applications anymore.

For most things, you want to use a computer in such a way that you avoid making superpositions—because you want to get a sharp answer. For instance, you want to decipher a secret code or something like that. You want to have the exact answer: "*This* is what it means, not *that*!" And let's not equate this answer to a superposition of those two possibilities—again, that's nonsense.

What I'm saying is we must unwind quantum mechanics, so to speak, to see what happens underneath. And until the quantum technologists start doing that, I believe they won't make really big progress. As an example, quantum computers always make errors, and their designers and operators try to correct them. To me, if you're trying to correct these errors, that means you want to go to more basic degrees of freedom that do not ever carry any error in them because they're exact—they're just classical. But to have this realization is apparently very difficult.

This is my feeling as to why we don't make breakthroughs. We should think about things in a different manner.

It seems you're saying we must live in a clockwork universe, one in which things must be purely deterministic at a very fundamental level, and thus there's very little room for any kind of quasi-mystical speculation. One consequence of that would seem to be the dissolution of mystery to some degree. And you mentioned the stubborn persistence of an almost religious approach to nondeterminism in quantum mechanics within the scientific community, not to mention in popular culture. Perhaps this attitude endures because, for so many people, it lets us preserve something ineffable about all that we experience in the world rather than assuming everything can be known if we fill in the right equations. So if you do believe in this kind of clockwork universe, I wonder what you'd say its most mysterious aspect is.

There are still many mysteries that make our problem very, very difficult. And this deterministic universe we discuss is something that could be fully understood only by someone with a much bigger mind, a much bigger brain, than I have because they'll have to consider all possibilities. And as soon as you make some wrong assumption, you again get this quantum-mechanical situation in which things get to superimpose one another.

A simpler question is: Can you formulate quantum mechanics without a superposition principle? And my answer is yes. In one of my last papers on arXiv.org, I wrote a little simple model—too simple to be useful in the real world. But the model is just a clock with a pendulum that moves in a very organized way, and that pendulum drives a wheel that shows the time, the hands that show the minutes and seconds. I call it my "grandfather's clock" model. From the pendulum, you can derive what time the hands should show. And these hands are deterministic. They are just showing a time with infinite precision, say. The pendulum is really a quantum pendulum—it can be quantized; we can write quantum equations for it.

I found the connection to the mathematics of this pendulum and the mathematics of these hands that show the time. Keep in mind, the hands are completely classical, and the pendulum is completely quantum mechanical, but one is related to the other—it's one machine.

I got very few reactions to this model. I would have thought people would say, “Oh, yes, of course. Now we understand how to continue!” But instead they’ve said, “Okay, right, ’t Hooft has another hot idea, another crazy idea. And he has many of those crazy ideas. Let him be happy with it; we’re going to do our own thing.” That’s the most common reaction I’ve gotten.

I’d suspect the reasons for that reaction are, in some sense, not scientific and rather more “cultural,” right? I’m thinking of this in terms of the signal-to-noise ratio that exists for anyone trying to drink from the firehose of new preprint papers on arXiv.org and elsewhere. It can be very tough to know what to pay attention to and how to evaluate whatever does get one’s attention.

That leads me to one more question. I wonder how you feel about the cultural impacts of your scientific contributions, in particular the holographic principle, which you first proposed in the early 1990s.

Arguably because of this idea, there are people—mostly nonscientists, I’d imagine—who truly believe that the cosmos is in fact within a black hole or that it’s all some simulation in a higher-dimensional computer. The idea for this “simulation hypothesis” is that perhaps nothing is “real” besides information itself, and everything else might be just a projection of patterns of 1s and 0s

encoded on the outermost boundary of the observable universe. So, you put forth a provocative theoretical insight more than 30 years ago, and it has somehow led to the world's richest man seriously suggesting on a popular podcast that "we are most likely" all just avatars in some cosmic-scale video game. I'm curious about your thoughts on this phenomenon.

I do have some reservations. Maybe I never should have talked about the holographic principle because, yes, some people are galloping away into nonsense, linking this idea with supernatural features and poorly defined dimensionality, all to sound very mysterious. I have a big problem with that. I think you shouldn't phrase the laws of nature in more complicated terms than strictly necessary. You should simplify as much as possible. Even Albert Einstein once said something like this—that you have to simplify things as much as possible but not beyond reality, not beyond the truth. We should try not to be supernatural; if we scientists leave only a wake of mysteries behind us, we're not doing the right thing.

I am a bit worried that the holographic principle has only invited people to be more mysterious; I want the extreme opposite. I want people to try to be super rational. For me, even quantum mechanics is already too far away from reason. And if you rephrase quantum mechanics to treat

Hilbert space [a type of vector space that allows for infinite dimensions] as something used for practical purposes rather than its being a fundamental property of nature, you don’t even need this type of holography anymore. I wish more people understood that. We have to try to phrase things more precisely to keep public misunderstandings from wreaking havoc on science.

Editor’s Note (4/9/25): This article was edited after posting to better clarify some of Gerard ’t Hooft’s comments.