Karl Albert van Bibber of the University of California-Berkeley was invested as a member of the American Academy of Sciences and Letters in 2025. In this video, Academy Executive Director Greg Forster interviews Prof. van Bibber about why we want to discover things even if we don't know how we'd use them, the courage of people who investigate theories that aren't backed by funding and jobs, and how the growth and success of a field creates unique challenges.

Greg Forster, American Academy of Sciences and Letters

Hello, I'm Greg Forster with the American Academy of Sciences and Letters, and today I'm interviewing Karl van Bibber of the University of California, Berkeley. Karl, thank you so much for being with us.

Karl Albert van Bibber, University of California-Berkeley

Very nice to be here, Greg.

Forster

My first question is, what are some underappreciated ways in which the work done in your field contributes to human knowledge and well-being?

Van Bibber

Thank you, a very interesting question. As a preface, I should explain what my work is. I have done many things in my career. I describe myself primarily as a particle astrophysicist. My research for going on 40 years now is looking for the dark matter of the universe, somewhat of an arcane and challenging work, but I always go back to something I never forgot about Robert Wilson. He was a very large figure in 20th-century physics, and the name may ring a bell. Yes, he was the Robert Wilson you saw in *Oppenheimer* if you saw the movie. He was part of the Manhattan Project. He was an accelerator physicist, and he later became the founding director of Fermilab, which was built in the late 1960s and for many, many years was the highest energy particle accelerator in the world. As part of the whole authorization and appropriation process to fund the lab, he was actually called in to give testimony to the Senate, and one of the senators on the panel asked him, Dr. Wilson, what does the building of Fermilab do for the national defense? How does high-energy physics contribute to the national defense?

To abbreviate his answer somewhat, and it really hits the nail on the head, he said, well, senator, the study of the most fundamental forces and particles in the cosmos doesn't contribute directly to the national defense, but it makes the country worth defending. A society that can nurture these things in education, research, culture and so forth makes the country worth defending. I have always loved that answer, and I never forgot that. I knew him. He was a very impressive person. He died about 25 years ago.

Particle physics and cosmology are unique in how they appeal to society at large. In that sense, we're very privileged. When we publish papers or go to conferences in the work we do, there is usually a pretty large contingent of the press there, and their popularization of it is done at a pretty good level. I'm impressed with how what's going on in our field does get disseminated to the society at large, and there's no question that very, very young people read about these things and hear what we're doing. There's no question that this is contributing to so many young people going into the study of STEM fields. That's just really palpable. About two or three times a year, I give tours of our laboratory to groups of students that come up as part of this program from Peru. High school students and university students will come up typically 20 to 40 at a time, and I'll give them a tour through our lab and show them on the big screen what we're doing, PowerPoint presentations and videos and so forth about looking for the dark matter of the universe. This really changes things, and some of these young people come to study in the United States.

People also say, okay, you're looking for this very light dark matter particle, the axion. Well, fine, the taxpayer is paying for this. But I don't think the taxpayer is paying too much for what we do, but how would it change the conversation if you ever found the dark matter of the universe? Suppose you found this ultralight particle with this very large quantum wavelength? I never forgot this famous incident that happened in the early 19th century when the queen and the chancellor of the Exchequer visited Michael Faraday. He was one of the pioneers in the study of electricity in the early 19th century, and he was showing all these marvelous experiments he was doing. At some point the queen asked him, well, Faraday, this is all very good, but what is this electricity going to be good for? And he said, well, madam, I don't know what it'd be good for, but the one thing I'm certain of is that someday you will tax it. And truer words have never been spoken.

Forster

The age-old tug of war between science as a pursuit of knowledge for its own sake and science as a contributor to the practical life of humanity, both in terms of power and in terms of flows of money in both directions, as you pointed out. Nothing exemplifies that tension more clearly than dark matter, because knowing almost nothing about it, we don't know what it would do if we found it. We still want to find it. For some reason, we just want to know.

Van Bibber

One thing is very specific to the particle we're looking for, which might be only a trillionth of the mass of an electron but extraordinarily dense: the modality, the way we look for it, is by looking for its resonant conversion from axion into a photon in a microwave cavity and in a very strong magnetic field. We read out this microwave cavity. We listen for this signal. It's almost like the world's most sophisticated radio with these quantum amplifiers and even using the latest tricks in quantum sensing, quantum metrology, to evade fundamental limits set by quantum mechanics. It's interesting to see, it's more of an arcane dive into your question, how this is ping-ponged back between quantum devices, quantum information science that was developed for quantum computing and how we've leveraged that in our search for the dark matter. Then how we've actually added to that corpus of knowledge. We've actually developed new devices, new modalities, new ways of quantum evasion, then in ways that is spun back into quantum computing.

Forster

It's not just the investigation of knowledge, it's the development of new methods.

Van Bibber

Oh, absolutely. In fact, a long-time collaborator of ours – we actually pulled him into the dark matter game 30 years ago – was John Clarke, professor emeritus here at Berkeley, whom I've collaborated with on and off over the years. He was just awarded the Nobel Prize this year for work he did in macroscopic quantum tunneling, which is really at the heart of all these qubits that are used in quantum computing these days. He has really been at the center, really the nexus, of this two-dimensional flow between applied and very basic use of quantum information and quantum sensing.

Forster

Let me ask you my next question. What are some recent examples of people in your field who have demonstrated intellectual courage?

Van Bibber

Interesting question. I have to give a little history here, which is maybe well known, but we really only became convinced in the 1970s that the dark matter of the universe is not a chimera of any sort, it is something really there and very likely a particle relic from the Big Bang. By the early 80s, the hunt was on for the previously undiscovered particle beyond the standard model particle that could account for this overwhelmingly predominant fraction of the matter density of the universe. Two candidates emerged, one of them called the WIMP, the Weakly Interacting Massive Particle, a very heavy particle, maybe 10,000, 100,000 times the mass of a proton, that one would look for conventionally in particle detectors, but it would be very feebly interacting with ordinary matter. These have given rise over the decades to these multi-ton vessels of ultra-pure xenon buried a mile underground to screen out cosmic rays, looking for a little bump in the night, a tiny signal that would occur when one of these particles would interact maybe once a week in a vessel of many tons of xenon.

The other was this very light particle called the axion. What was very frustrating to us, the early workers in this business, was that we've always been a backwater. People said, yes, the axion, it does solve this other problem in the standard model, but no one paid any attention. It was always the very dark horse in the whole race. So, for quite a few decades, you would go to conferences, and if they had a session on axions at all you and your students would go there, it was like giving a talk in an echo chamber, there was just nobody there. Of course, there was no money either.

The people I admire were my colleagues from the early workers in this field who kept the faith over many decades, including John Clarke, whom I just mentioned, who came in and developed some amazing technology for us that really allowed the field to go ahead when it looked like we just might run into a dead end and not be able to make our experiments sensitive enough to see the dark matter. No money came from DOE or NSF, or very little. More important, more germane, there were no faculty positions being offered in this particular field. So young people were going into this field out of the

sheer excitement and love and this feeling in their bones that they were onto something, but without really a clear career path afterward.

The early people, a dozen or a few dozen people in this field around the world, really need to be recognized. At the headwaters of this was Pierre Sikive, a Belgian-born theorist at the University of Florida, who wrote some of the seminal theory papers on why the axion might be the dark matter and how one would go about looking for it, which at the time seemed a bit crazy. The sea change, of course, came about 15 years ago, when all of a sudden these more and more sensitive experiments looking for the WIMP were just not going anywhere. We were going down decade after decade in sensitivity and not seeing what these theories beyond standard model theories told us ought to be there. At that point, the pivot began to happen, and all of a sudden, axions became more front and center.

Forster

Every discipline in the academy has its stories of the little theory that could. I especially appreciate your highlighting the funding and job lines issue, that it's so difficult to maintain intellectual honesty in an environment where you will get funded and you will get jobs if you follow the path that everybody else has worn. The people who are willing to stick with the little theory that could through those desert years until finally the discovery gets made that turns everybody your way, they're unsung heroes or they're heroes we could always sing a little more about.

Van Bibber

I love that turn of phrase, "the little theory that could." I'm going to remember that. Thank you, Greg.

Forster

Oh, no, my pleasure. My last question, what would you say are one or two most important challenges to maintaining high intellectual standards in your field?

Van Bibber

That's an interesting question, and you warned me I might be asked that. I have to choose my words carefully here because, interestingly, the popularity of the field has now brought us to a situation that's fraught in that regard. The early days of this field remind me of the early days of the automobile, when there were very few Model Ts in North America. Somebody's car would break down in Los Angeles, and somebody would drive from Minnesota in their Model T to help repair it. There was a great sense of comity and collegiality among Model T owners on the North American continent.

When a field becomes too popular, there's a behavioral change, and it's not always a welcome one. Particularly when the field is crowded, there's greater competition for resources. We're now in a situation, I hope it's a temporary situation, where there's more than the usual stress on resources and that competition can produce a lot of sharp elbows. In the early days of our field, there wasn't really an imperative to publish so immediately. People dealt with their data and their theoretical calculations more meticulously. Today, there's really an imperative to get out there quickly.

Part of the problem has also been exacerbated by the journals. In fact, the editor of one journal told me some years ago, we'll publish anything with the word "axion" on it right now. That's fine for a while, but the journals themselves have to be careful to make sure that after a field becomes mature there is a little more discrimination. Not that the thing would not be of good scientific quality, but really, is there the novelty there to publish and so forth? There are behaviors that are, let's say, a little less than collegial these days. That's a sign of maturity of a field. Sometimes I think to myself, well, maybe it's time to move on to something else.

Forster

Well, they're good problems to have. They're associated with growth. Funding and professional opportunities are things we need to have, but also things that can have a distorting effect. And those are difficult tightropes to walk.

Karl, thank you so much for being with us. I really appreciate it.

Van Bibber

Thank you, Greg. It's been a lot of fun talking with you.