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'The current situation is a golden opportunity'

Our model of the evolution of the universe is an amazing achievement. But astronomical anomalies point the way to a deeper theory, if not a complete one, cosmologist **Jim Peebles** tells Michael Brooks

JIM PEEBLES is widely known as the architect of modern cosmology – and its nice-guy-in-chief. Awarding his half-share of the 2019 Nobel prize for physics, the committee said he "took on the cosmos", helping to create a framework now considered "the foundation of our modern understanding of the universe's history", known as the standard model of cosmology. Others have described him as "an extraordinary physicist", and "uncommonly thoughtful, gracious and kind".

Now the Albert Einstein Professor of Science, emeritus, at Princeton University, Peebles's career began there in the 1960s, focusing on Einstein's general relativity, which casts gravity as the result of mass warping space-time. He later worked out the characteristics of cosmic microwave background (CMB) radiation, the "echo" of the big bang, whose discovery made cosmology an experimental science. He also showed that dark matter haloes around galaxies would create a mass distribution that matched astronomers' observations, and persuaded the field that our description of the cosmos needed to reinstate Einstein's much-derided cosmological constant. This was originally stuck into the equations of general relativity as an awkward fudge, but we now think of it as dark energy, the repulsive force driving the universe's accelerating expansion.

Despite the success of the standard cosmological model, Peebles has always sought to undermine it. In the past few years, he has been musing on astronomical anomalies – observations of weird galaxies and other curious phenomena – that might expose flaws in our thinking.

He tells *New Scientist* about his vision for cosmology, why it is important to stray from the mainstream and whether it is really worth pursuing a theory of everything.

Michael Brooks: When you said that a proper explanation of the evolution of the cosmos needed both dark matter and dark energy, were you aware of how important they might become?
Jim Peebles: Not at all. They were just sensible guesses about how the data might be reconciled with what we had in the way of theory. In the 1990s, most people, including me, felt that the most sensible, elegant universe would not have Einstein's cosmological constant. Einstein didn't like it. Particle physicists didn't like it.

However, the evidence said that the mass density of the universe is too low compared to what is required from the expansion rate we have measured unless we add in Einstein's cosmological constant. So I felt it was worth considering. The community had to be dragged kicking and screaming into acceptance that we must learn to live with lambda [the Greek letter that denotes the cosmological constant in the standard model of cosmology, which also includes dark matter]. I remember well one then-very-young theorist saying: "You're only doing this to annoy us!"

Are you concerned that we haven't yet identified the true nature of dark matter, whether that is a particle or a whole range of them?
No, I'm completely comfortable with it. We don't have a guarantee that dark matter ever will be detected directly, and the wonderful successes of cosmology are saying that we're on the right track and that this track requires dark matter to exist.

That said, I do think we need a better understanding. In the standard

“It’s a challenge to be iconoclastic and also not nutty”

computations, dark matter is treated as a nearly collision-less gas of particles. That was the original idea that I introduced back in the 1980s, and it certainly works pretty well, but I introduced it with the simplest properties that I could get away with. To me, it is just crazy to think that the dark matter, which accounts for some 27 per cent of the universe’s mass, is so simple. The physics of the matter we can observe is really very complicated. Surely this dark matter is more interesting than that?

So I expect there will be a great discovery, maybe triggered by the discovery of a better explanation for the way galaxies form, maybe some other way. But it will show us that the dark sector – dark matter and dark energy – is more interesting.

Some people talk of a crisis in cosmology because of the mystery surrounding dark energy and dark matter. There is also the Hubble tension, where different ways of measuring the expansion rate of the universe, known as the Hubble constant, give different answers. What do you make of anomalies like these?

I am deeply amazed at how well cosmology has done since I started working on this in the mid-1960s. We never had a guarantee we would get as far as we did, and there’s no guarantee that we can continue making great discovery after great discovery. But I see the current situation as a golden opportunity. There are lots of anomalies, but the promise is amazing. We are going after well-defined problems: detect the dark matter; detect more evidence of the dark energy; detect the way galaxies form and evolve.

On the Hubble tension, I’ve been conditioned by the fact that, through most of my career, the Hubble constant has been a really difficult problem – and we’ve done well with it! We have two very different measurements relating to what has happened since the universe was one-thousandth of its present size, and they give consistent results to around 10 per cent [of one another]. To me, wow, that’s wonderful! But of course, that 10 per cent is important.

My gut says the tension between the two results is to do with some systematic error in the way people reckon the distances of galaxies. But maybe the issue is a hint towards something we need to improve in our theory. There are other predictions in that same nature. The big hope, to my mind, is that other anomalies will appear.

You mentioned this in a paper you published in 2022, saying that not enough attention is being paid to anomalies in cosmology. What are the ones that interest you most?

Well, there’s the bulk flow anomaly: our entire galaxy is moving through this sea of radiation with a well-measured speed and direction. In the standard theory, we’re moving because of the gravitational pull of some fluctuation from uniformity [in the distribution of matter in the universe]. But it’s not pulling us in the direction that you would expect.

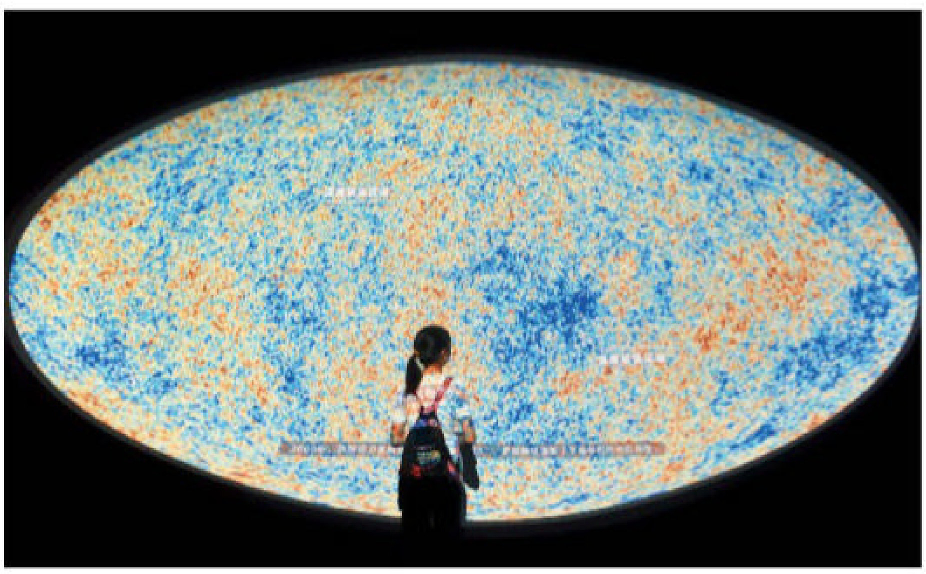
The community is pretty sure that this is some kind of systematic measurement error.

NASA, ESA, CFHT, CXO, M.J. JEE (UC DAVIS); A. MAHDAVI (SFSU)

It’s very hard to measure the large-scale fluctuations, the departures from homogeneity that would pull on us most strongly. That said, there is a small subset of the community who have worked very hard to take account of the uncertainties, including people whose opinion I particularly respect, and if the anomaly really is there, then it’s exciting because it could be a hint about the initial conditions of the universe.

The void anomaly is another really curious phenomenon. We are on the edge of a void, that is to say a region that has very few objects in it. You would expect dwarf galaxies in voids in greater abundance than we observe. And you would not expect large spiral galaxies like ours in a void, but there are a few. It just doesn’t seem to hang together. That suggests we don’t have quite the right theory about the material from which galaxies are made, which of course includes dark matter.

The formation of galaxies certainly has a bearing on dark matter. If we take the standard cosmology we have now as initial conditions and follow the evolution of the distribution of matter as it coalesces into galaxies of stars, it seems to me that certain aspects of nearby galaxies do not fit with results. The exact



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The cosmic microwave background tells us about the early universe

Galaxy cluster Abell 520, with hot gas falsely coloured in green and dark matter in blue

means by which galactic structure first forms, the way stellar velocities appear to be dispersed within the galaxies, the origin of supermassive black holes – these and other observations are all not yet fully explained by the standard cosmology. But things like the James Webb Space Telescope are extremely promising: it's teaching us new things about the way galaxies form, and that's eventually going to inform us about the nature of dark matter.

What about modified gravity theories such as modified Newtonian dynamics (MOND) – which alters Isaac Newton's law of universal gravitation in a way that changes the strength of gravitational attraction between two masses over cosmological scales. Can they provide an answer to the dark matter problem?
Well, I have to be polite, because some people who I respect have signed on to it, but I can see no hope for MOND. If you had only galaxies to think about, you would certainly take MOND seriously. But you don't have only that: you have tests on larger scales that make wonderfully demanding predictions with great precision. Those predictions depend on the presence of dark matter. If you don't have the dark matter and you instead have MOND, how in the world could those predictions have worked so well?

Can researchers investigate these anomalies without risking damage to their reputation?
You do have to be very careful about this, because it's a challenge to be both iconoclastic and not nutty. But I think that, in cosmology, there is a slight underemphasis on small projects that look outside of mainstream research. My recommendation for people doing observational cosmology is to pay more attention to slightly iconoclastic ideas, but don't go over the rails. Look into some of these odd properties and explore why they don't fall in line so neatly with the standard theory – but maybe only if you have tenure!

I guess one could say this was exactly what you did. Was that a deliberate strategy or did you fall into it by accident?
I was in the right place at the right time. I came to Princeton thinking I would do theoretical particle physics, but, by good fortune, I wandered into the research group run by Bob Dicke. He had decided that gravity was not receiving the attention it deserved, because the classical pre-war experiments had been done and it was hard to see how they could be done better. He realised that the technology that had been developed during the second world war, and improved after the war, made it possible to redo the old experiments better and to do new experiments. So he started a wonderful programme that tested all aspects of gravity physics. It was a fascinating time.

Recently, you have been looking back at your career and have composed a forthcoming paper entitled a "physicist's philosophy of physics". Do you wish you'd had something like that at the start of your career?
No, I think it would just have made me self-conscious! I only became fascinated with this in later life – you tend to ask yourself, what have I been doing? And I've decided I've been doing something philosophically interesting. I'm an odd sort of physicist: I'm not really a theorist of any merit, and I'm not really an experimentalist of any ability. One of my earliest memories is throwing a tantrum because I could not put the coffee percolator back together, having taken it apart. I loved that sort of thing, small things that I could get my hands on and try to understand.

I think that's still my characteristic. I'm pretty good with my hands, but I've never been at a telescope when it was being used for something productive. I'm more of an intuitive thinker. In the end, my philosophy is pretty simple-minded: do what interests you, but make sure you keep in close contact with physical phenomena.

One of the questions you ask in your philosophy is whether it is worth pursuing a theory of everything. What is your conclusion?
I have become fascinated with this. I have often wondered why we assume that the universe operates by rules that we can understand. But the truth is, we have a few clues that nature has given us this wonderful gift.

For me, physics can be said to have begun when people first traced the motions of the stars and planets. They could see that the motions of the planets were not obviously simple, but they were regular. Thousands of years ago, people could predict the timings of solar eclipses well ahead of time. That predictability is now, I think, at the very heart of what we do: we try to create a theory that predicts many things that were not anticipated. If successful, that tells us that the theory is a pretty good approximation to reality. That's the whole thesis really.

While the best of our physical theories are really excellent – wonderfully predictive – not one of them is complete. When applied in the wrong situation, they fail. That's just the way it is. So it's pretty clear, I think, that physics has no guarantee of arriving at a final theory.

Instead, my bet is it's going to be successive approximations to reality all the way down. You'll do better and better, but you'll never get there. Because to get there, in my world view, you have to have experimental checks of predictions, and experiments are finite: they cannot explore all eventualities to all accuracy. So my conclusion is that we'll never get there. ■

Michael Brooks is a *New Scientist* consultant. His latest book is *The Maths that Made Us*