

Being True to Our Own Imaginations

Gregory Benford

Physics has been the forerunner of much of modern science, but perhaps we lack the true courage of our convictions. Take just one example from our grandest province—cosmology—where physics and astronomy merge. Half a century ago, we should have seen the Big Bang coming—indeed, we did see it. And ignored it.

In the late 1940s, George Gamow, Ralph Alpher, and Robert Herman worked out element formation and the entire scenario that led to the now-famous 3-K background radiation. Yet the steady-state model held sway, and their work had faded from view by the mid-1950s.

“We never quite thought through to the realization that the peak emission was observable in the microwave sky,” Alpher said when I asked him about it after a colloquium at the University of California at Irvine in the 1980s.

“Why didn’t you go to the radio astronomers and ask if they could see the emission?” I prodded.

It turned out that Alpher and Herman did ask. Radio astronomers and radar experts at the Naval Research Laboratory, the National Bureau of Standards, and Johns Hopkins University didn’t think viewing the emission was possible. Alpher took their word for it, he acknowledged regretfully.

But in a way, Joe Weber did want to do observational cosmology. I turned to him, standing at the wine-and-cheese reception after Alpher’s talk. Joe couldn’t recall if, in 1950, he had even heard of the Gamow-Alpher-Herman work, but by then he knew of Gamow’s reputation. He asked Gamow for a thesis problem. Joe had learned about radar in the US Navy, ranging into the microwave region. Was there some cosmological use for observations in such a range? “Gamow couldn’t think of anything relevant,” Joe said, shaking his head. “So I looked at stimulated emission instead.” In that effort he provided some of the theory that led to the maser.

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In the early 1960s, steady state was in retreat and a group at Princeton University, including Robert Dicke and James Peebles, began working on implications of an earlier hot stage. Motivated by a desire to find a prior “big crunch” in a cyclic universe, they began building a radiometer. Apparently they did not recall the Gamow, Alpher, and Herman work and replicated it. By startling coincidence, while they were still thinking through the details of how to detect the 3-K emission, it turned up nearby. Arno Penzias and Robert Wilson at Bell Laboratories were trying to see sources of noise in the sky, and they came upon the classic blackbody signature, at just the right equivalent temperature, netting Penzias and Wilson a Nobel Prize.

But could the 3-K background have been seen earlier?

In 1976, I took a sabbatical from Irvine to work at Cambridge University’s Institute for Astronomy. Martin Ryle and Anthony Hewish had won the Nobel Prize for pulsars, and I wanted to work on the plasma physics of rotating magnetized neutron stars. (A true, closed-system solution to that plasma problem remains elusive, decades later.) In discussing the problem with Ryle, I asked, “If someone had come to you suggesting that relic radiation around 3 K was detectable, say, around 1950, when could you have detected it?”

He thought and said, “It would have been a challenge, getting the signal-to-noise ratio down, but . . . perhaps a few years.”

“Would you have put in that level of research investment?”

He shrugged ruefully. “Probably not without a big authority behind the idea.”

“An authority like Gamow?”

He laughed. “I’m really not sure. I think [Fred] Hoyle would’ve frowned at the idea.”

“When could it have been detected?” I asked Hoyle. He pointed out that there had been tantalizing detections much earlier, which nobody thought to relate to the Gamow-Alpher-Herman work. In 1941 Walter S. Adams found a puzzling excitation

temperature of 2.3 K in interstellar cyanogen absorption and remarked on the lack of any obvious exciting source. The 2.64-mm measurement was near the blackbody peak, yet it escaped general notice for decades.

By 1956 Hoyle had seen Andrew McKellar’s report on interstellar molecules, in which he proposed that the temperature of space is about 3 K. Gamow visited Hoyle in La Jolla, California, in 1956 and told Hoyle he thought space was filled with microwaves at a temperature of about 10 or 20 K. Hoyle said the temperature could not be so high because of McKellar’s work. He thought it should be 0 K, the steady-state view.

Counting radio sources

Hoyle said that he would have encouraged such a test, but it had not seemed plausible to him at the time. Ryle might have encouraged the test, but he was far from the particle physics–cosmology community in the US. Hoyle was in a scrimmage with Ryle by then, and not likely to tell him of odd ideas from across the Atlantic. They were disputing the issue of radio source counts.

Ryle’s collaborators found that the slope of the number of radio sources versus distance did not fit the steady-state prediction. There were too many sources at great distances, which implied some evolution of the sources over time.

So instead of a direct test, we sat through the slow battles over source counts. Rather than testing the Gamow-Alpher-Herman model, cosmologists spent more than a decade falsifying steady state’s predictions. No one followed on the nucleosynthesis path, let alone thought of observations. On the theoretical front, Hoyle and others tried to get the same element abundance that Gamow, Alpher, and Herman had found by 1950. Not until 1964, just before the accidental discovery of the background radiation, did Hoyle and Roger Tayler realize that steady state could not explain helium formation.

No one set out to directly prove the Big Bang. Instead, they showed where steady state was wrong. That approach

may have emerged partly from the philosophical bias of the time, which stressed falsification of theories.

To outsiders, such events might seem a scandal. Not in the sense of personal failure, but in a more serious way, a failing in our approach. We didn't treat both models fairly, and lost more than a decade before discovering the truth.

If so, what were the scandal's roots? Can they tell us something about ourselves?

Alpher and Herman, in their reflections on early work on Big-Bang cosmology (PHYSICS TODAY, August 1988, page 24), said they suspected a cultural bias was at work: "It is possible, but regrettable, that Gamow's fun-loving and irrepressible approach to physics led some scientists not to take seriously his work, and perhaps our work too because of our close identification with him." Further, they noted, they worked far from the academic astrophysics community, mostly in industry—General Electric and General Motors. Physics historian Stephen Brush¹ says that one reason the Gamow-Alpher-Herman work was relatively neglected even after the Penzias and Wilson discovery "may be that it was not presented as a cosmology but as a hypothesis about the origin of the elements. As such it was not generally successful; nucleosynthesis in the big bang is needed to explain the cosmic abundance of helium, but nucleosynthesis in stars is needed to account for the formation of heavier elements."

Gamow, Alpher, and Herman committed a minor social sin: They weren't in the club. Neither were Penzias and Wilson, but they had a firm result in hand, not to be denied.

Surrounded by dark energy

Are there similar scandals in our own era? Say, in cosmology—the most philosophically striking arena we have? We now know that the universal expansion is accelerating, and that acceleration may be even rising—an effect known among cosmologists as, appropriately, the "jerk."

But shouldn't we be the ones feeling like jerks? Despite 30 years of their drum-beating, the string theorists never suspected that the dark energy could comprise the majority of the energy density of our universe. Of course, string theory is mind-numbing in its mathematical complexity. Predictions are very difficult.

Still, many of us presume string theory to be the most promising approach to a Theory of Everything, with striking implications. To list one

of the spotlight prospects on offer, we may have many more dimensions lurking about our universe, unsuspected until now. But as a flashlight for showing us where to step, string theory seems useless. It has made no prediction of a past event not anticipated by conventional, inflated Big-Bang theory.

When a bug of this size hits your conceptual windshield, it makes a big splash. The dark-energy scandal is that the bug was the size of an eagle. String theory is an idea that functions as the opposite of the Gamow-Alpher-Herman scandal: no predictions, yet widespread acceptance. Theorists can fall in love with mathematical beauty. Philosophical elegance, which steady state had, is even more glamorous. Gamow, Alpher, and Herman had to fight steady state's shiny splendor, which blinded our field.

What causes such scandals? One can point to possible culprits in our methods. An unspoken timidity lurks in the close-focus grant-approval process—small steps are rewarded as more reliable than conceptual leaps. Possibilities beyond our current conceptual horizons get little attention. In academia, we maximize publication numbers rather than originality. This approach also gets us through the incremental mindset of review committees, which are seldom noted for their leaps of insight. Our review process puts progress on cruise control, so no one gets much beyond the perceived path. This arises partly from the widespread difficulty of getting the very best people to serve on the committees.

More ominously, perhaps we show a lack of faith in our own calculations. Maybe we are uneasy with that mysterious precursor of the entire scientific process: Formulate a hypothesis, test it against experiment or observation, and look for other implications. The shadowy beginnings of the long march lie in a mystery: How do we have ideas?

Do we suffer from anxiety over imagination? Rigor is reassuring, but it should come at the end of that powerful chain that starts with intuition and proceeds to experimental checks—not at the beginning. To set our work in motion, we reason mostly by analogy, not by rigorous deduction. Imagination is not incremental.

Yet among reviewers, "speculation" is a word mostly deployed as a pejorative. We should not allow it to be.

Reference

1. S. G. Brush, *Perspectives on Science* 1 (4), 565 (1993). ■