My Grandfather Thought He Solved a Cosmic Mystery

His career as an eminent physicist was derailed by an obsession. Was he a genius or a crackpot?

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When my grandfather died last fall, it fell to my sisters and me to sort through the books and papers in his home in East Tennessee. My grandfather was a nuclear physicist, my grandmother a mathematician, and among their novels and magazines were reams of scientific publications. In the wood-paneled study, we passed around great sheaves of papers for sorting, filling the air with dust.

My youngest sister put a pile of yellowing papers in front of me, and I started to leaf through the typewritten letters and scholarly articles. Then my eyes fell on the words *fundamental breakthrough*, *spectacular*, and *revolutionary*. Letters from some of the biggest names in physics fell out of the folders, in correspondence going back to 1979.

In this stack, I found, was evidence of a mystery. My grandfather had a theory, one that he believed to be among the most important work of his career. And it had never been published.

My grandparents had arrived in the low green hills of East Tennessee with their young daughter, my mother, in 1960. The town of Oak Ridge had been rebuilt from the ground up for military research, like Los Alamos, New Mexico, and Hanford, Washington. Together these secret cities became key sites in the Manhattan Project, the push to develop the first atomic bomb. But by the time Francis and Claire Perey came to town, peace had turned the facility into the

Oak Ridge National Laboratory. There they rammed neutrons into the centers of atoms for a living.

In the 1960s and ’70s, they published paper after paper describing the probabilities of various outcomes of their collision experiments—how often a neutron would veer left or right or another direction, how often the atom would emit another particle, and other possibilities. These seem to have been happy years: Their son was born, and their work was well received, cited many times by other scientists.

When I knew them, Francis and Claire were no longer at the lab; during my childhood, my mother would invite them for a month-long visit at Christmastime. Francis was a man of consuming obsessions. One year in the mid-’90s, it was a card game whose simple rules hid deep mathematical truths; another, it was a demonstration, using a chair-back and a length of cord, of the strangeness of quantum mechanics. He had eyebrows like wiry caterpillars, and a constant look of delayed revelation. He wore a single, boldly checked blazer for nearly every occasion, and he often went around with two pairs of glasses balanced on his forehead—one for close reading, one for distance—and his mouth slightly ajar. My dad used to say, half-wonderingly, half-jokingly: “He’s about to utter a theory of everything.”

Indeed, as a child, I’d heard murmurings among the adults that Francis had an idea of some sort. I had never understood what it was, and looking through these papers, I still could not make sense of it. The theory seemed to deal with the fundamental question of where probabilities come from, as well as a specialized subfield of mathematics and even quantum mechanics. To understand my grandfather’s grand obsession, I had to go into the foundations of physics, and to the heart of the stories we tell about science. What was Francis on to?

I always knew there was something weird about Francis. He seemed poorly acquainted with the physical world, despite having built a full-size sailboat by hand (he named it after Évariste Galois, a French mathematician who invented an important branch of modern math before dying in a duel). When our school bus dropped us off in the afternoons, he would come right up to the bus door and stand almost against it, much too close, unnerving the driver. It was often hard to get him to talk at dinner. But ask him about the history of Paris, for instance, or solo sailboat races, and he could go on for an hour, making nutty little jokes along the way. *It must be because he is a physicist*, I thought.

Francis's oddness put him in good company. The image of the scientist as a maverick, the wild thinker with his eyes on the horizon, seeing something that no one has seen before, has a romance that's hard to deny. So many colorful details, from Albert Einstein's distinctive coiffure to Marie Curie's mournful gaze to Richard Feynman's bongo playing, suggest that eccentricity is part of staring at the fundamental nature of the universe.

And it isn't all caricature: When psychologists compare scientists and nonscientists on broad personality traits, they find notable differences. Scientists tend to score higher than average on openness to new experiences, writes the psychologist Gregory Feist in his book *The Psychology of Science and the Origins of the Scientific Mind*. Science appeals to certain kinds of people: They want to work on their own; they have a sense of direction. They are interested in new ideas.

Highly regarded researchers have an even more pronounced form of these tendencies. "More creative scientists are more confident, open, dominant, independent, and introverted than their less creative peers," Feist writes, although it isn't clear whether this is a cause or an effect of their success; it's probably a mixture of both. Feist, a psychology professor at San José State University, once interviewed more than 100 eminent researchers, many of them members of the National Academy of Sciences. The most prominent among them were also the most hostile, as rated by assistants who listened to the recordings of Feist's interviews without knowing the subjects' identities. Succeeding in science seems to require a degree of pigheadedness. "It takes a real belief that one has something special to offer and that one has a way of doing things that is better than most others," Feist writes.

In addition to being notably more assertive and confident, the most creative scientists are much less interested in following rules. Coming up with something really novel and profound requires both a rush of insight and a leap of faith. It is risky trying to do something new.

Imagine a tossed coin spinning in the air. The likelihood that it will land faceup seems obvious: 50 percent. But why? You might have been taught that if you throw a coin 100 times and keep track of which sides you get, the data will show that it falls on heads half the time. This is called a frequentist interpretation of probability—and it's just one of six interpretations listed in the *Stanford Encyclopedia of Philosophy*.

The frequentist interpretation works well for situations that can be repeated indefinitely. But there are many situations that cannot, and yet we make decisions about probability in those all the same. Do we believe in something because we've seen it happen 1,000 times before? Or is our belief an extrapolation about the nature of the system, whether it is human behavior or a coin toss? The more you think about it, the stranger it seems that we don't exactly know the answer.

Quantum mechanics is mainly a way to calculate the likelihood of one thing, rather than another thing, happening next, says Christopher Fuchs, a quantum theorist at the University of Massachusetts Boston. Niels Bohr and Einstein had a fiery public debate in the late 1920s that hinged on what might seem like a simple question: Is something uncertain at the quantum level because we don't have enough information, or because it is fundamentally unknowable?

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Bohr was of the latter opinion, but Einstein was skeptical. He spent the rest of his life working to see whether a theory that didn't involve this baffling form of uncertainty could replicate the predictions of quantum mechanics. But the kind of theory Einstein preferred turns out to be unlikely to work, as the physicist John Stewart Bell showed in a 1964 theorem. In the following decades, other scientists conducted experiments that corroborated Bell's ideas, which remain key to standard theories in quantum mechanics today.

Meanwhile, a revolution in probability was brewing. In the '70s, Edwin Thompson Jaynes, a professor at Washington University in St. Louis, was spreading the idea that probability is based on a form of inference. In the most basic sense, this just means you take what you know and from that draw certain logical conclusions about what's likely to happen next. But Jaynes was especially interested in situations where it seemed, at first glance, that there was not enough information to make a prediction. Take the example of a die with an unknown number of sides, each of which has an equal chance of falling up. What, in this situation, is the chance of rolling a one?

This may seem like an impossible question—but it's the kind of thing that scientists must deal with constantly when they work with measurements laced with unknown errors, or when they must take tiny amounts of data about, say, rain-forest species diversity and extrapolate to the rest of the planet. In such cases, the question is how you decide to handle something whose exact details are a mystery to you. Using beautifully sophisticated math, Jaynes demonstrated that thinking of probabilities in a particular way—as a description of our own knowledge and ignorance—can lead to surprising and powerful tools for physics.

In Francis's belongings, there was a copy of the landmark 1973 article in which Jaynes solved a classic problem in probability known as Bertrand's paradox. Using Jaynes's ideas, you could solve that paradox and likely others in a way that made sense. But there was one problem, known as the water-and-wine paradox, that Jaynes couldn't solve. This, I believe, was where the seed of Francis's ideas was planted.

There is no way to know exactly when Francis's eureka moment came. But it must have been before 1980. In May of that year, a paper of Francis's was being honored as a classic. In his acknowledgment, he writes modestly that this old paper doesn't contain truly new ideas, just inferences drawn from what was already known. Indeed, he says, inference is at the heart of his current, still-unpublished work, too. The work involves an idea long overlooked in the study of probability. "Although most scientists today do not know this formal theory, they have heard of it, I am sure, without fully realizing what it was," he writes, enigmatically. "I hope the above comments will be stimulating."

He does get a letter from at least one intrigued reader, and in 1981, when he starts passing around a draft of his paper, an excited flurry of correspondence arrives. The physicist F. H. Fröhner replies that he's been expecting Francis's blockbuster ever since the two discussed Jaynes the previous summer. Richard T. Cox, an important figure in the study of probability, writes that he has a feeling that "what you have written is extremely thoughtful and basic and may become a landmark," though he is not able to understand all of it. A description of the paper "sounded very interesting indeed, and I very much want to include it," writes Roger D. Rosenkrantz, a mathematician and philosopher at Dartmouth putting together a special issue of a journal called *Synthese*.

Francis, convinced of the work's importance, submits it instead to one of the most prestigious journals in all of science, *Nature*. It is rejected, he later reports, the same day it is received. Undaunted, in the spring he mails out another volley of drafts and submits the paper to another journal. This time, an anonymous reviewer, or referee, weighs in. And this time, the feedback is clear:

> Incomprehensible … if there is any core of actual results anywhere in this incredible work, it is hidden completely from me by the page after page of maddeningly repetitious philosophical froth … No purpose could be served by publishing the work in its present form, because I do not think there is one reader in the world who could make sense of it.

Shocked, Francis wonders, *Have I not made the discovery clear*? "I will try, within the limits of decency, to communicate the great excitement of having achieved what is to me a synthesis of the sort that I believe is the goal of science," he writes to Cox in some anxiety after receiving the notice. It is in fact not the first time he has gotten a negative reaction; he has already told another correspondent that colleagues at the lab are not able to follow him. "I would never have written this paper had I thought anyone would find it absurd," he confesses. In a moment of bravado, he writes to another colleague, "I have more exciting things to do than argue with a referee."

But his increasingly agitated letters are met with more confusion. "Your ideas are certainly novel—and correspondingly difficult to grasp," writes Bell himself, he of the 1964 theorem, in longhand scrawl on a single sheet of paper from Geneva. Another rejection of another version of the paper arrives in the mail. "A vague essay … I can find no substantial relation to Bell's Theorem," one reviewer writes. And yet it is clear that Francis thinks this paper has something profound to say.

In this period of turbulence he persists in keeping records. On the list of about 70 people he's sent the paper to, including at least one Nobel laureate, 17 are colleagues at the Oak Ridge National Lab. Next to their names in blue ink, he has written their responses. Most say, "I do not understand it." Others say, "Not my field," or "I am too busy," or "A useless theory." Next to my grandmother's name is written simply, "I trust you." The response of one of his bosses displays no such tolerance: Next to his name is written, "A waste of time—salary frozen."

Things come to a head by the summer of 1982, when Francis writes to a distinguished colleague at Boston University, begging to be invited to give a seminar. He has worked on nothing else for the past four years, and if he does not get some sign from outside the lab that his idea has merit, he will soon be fired. No one else would have to come to the seminar; they could just discuss it together—please let him know.

There is, in the end, no seminar. The respectable physicist has gone astray.

The mavericks of science make the best stories. Many profound insights began as heresies, their proponents mocked, degraded, or ignored. Birds are descended from dinosaurs, argued the American paleontologist John Ostrom, a kooky and unpopular claim when he made it in the 1970s. Earth orbits the sun, Copernicus asserted. In our cells live the descendants of bacteria, Lynn Margulis said. One interpretation of quantum mechanics is that there are many worlds, Hugh Everett proposed, and it was decades before anyone agreed. It's almost like first you must be outcast for an idea before you can be applauded for it.

In his famous 1962 book, *The Structure of Scientific Revolutions*, Thomas Kuhn writes that science tends to be fundamentally resistant to change. "Normal science often suppresses fundamental novelties," he writes, because they conflict with the received wisdom on which fields are built. Such tales of misunderstood genius are satisfying, righteous—almost expected, even among scientists. When I first began to ask other physicists to take a look at what my grandfather was working on, I was surprised by how many entertained the idea, before even looking at it, that he was on to something.

Not every maverick has a shortcut to the truth, however. The Nobel laureate Linus Pauling promulgated the idea later in his career that large doses of vitamin

C could treat cancer. Lynn Margulis, who was right about the bacterial origins of mitochondria, supported the idea that butterflies and caterpillars were different species. Even Einstein could be thought of as a failed rebel, says Dean Keith Simonton, a professor emeritus of psychology at the University of California, Davis. "Thinking that he had emerged victorious, he tried to devise a non-quantum theory of everything, and just miserably failed," Simonton says. And of course there are mavericks on the fringes of science, people who, though they have no training, believe they have solved fundamental problems.

Margaret Wertheim, a science writer who wrote the book *Physics on the Fringe*, told me about the Talk to a Scientist service, which people can pay to have someone try to answer their questions. For $50 per 20 minutes, you can, if you so desire, chat about your new theory.

Intrigued, I wrote to Talk to a Scientist's founder, a physicist named Sabine Hossenfelder, who connected me with one of the service's experts on quantum foundations in Oxford. He was cordial and polite, and he had in fact been one of the graduate students of the colleague at Boston University, the one who declined to host the seminar more than 30 years ago. Over the course of several days, I sent him abstracts, letters, a cornucopia of records relating to Francis's idea. He wrote that he recognized what Francis was talking about—although as an experimentalist rather than a theorist, Francis was not experienced in the area of physics he had become so interested in, and didn't know the right terms. I confided that in reading these letters, I could see the pain and uncertainty of a researcher drifting out of the mainstream. He wrote back, yes—it was like that for him, too.

What? I stepped back and Googled him, and found that he had been, at one point, affiliated with the University of Oxford. He had been associated with a number of institutions. But some years ago, he fell into heresy after arguing that Bell's theorem was incorrect. The backlash had been significant, the whole spectacle quite ugly, unfurling across physics blogs and in online forums as well as in university administrators' offices. My quest to learn about my grandfather's crackpot theory, if that's what it was, had led me to another outsider.

Perhaps the same things that make someone interested in being a scientist make that person vulnerable, in some cases, to going over the edge. Gregory Feist tells the story of one National Academy of Sciences member he interviewed in 1990. They met at his beautiful lab at the University of California, Berkeley. "He was kind of a big deal in immunology," Feist recalled when we spoke on the phone. "I was naive … I didn't know his story."

He was talking about Peter Duesberg, who believed that HIV was not the cause of AIDS. "There was a part of me that thought, *Wouldn't that be fascinating if that were true, if he were right?*" Feist says. Three years earlier, when Duesberg first proposed the idea, researchers were just zeroing in on the virus as a definitive cause. But by 1990, the scientific consensus had solidified, and Duesberg, who would not give up, was starting to be ostracized. "Once he stopped listening to the evidence … he stopped being a scientist, to be honest," Feist says. "There's a fine line between being a maverick scientist and being a little bit lost."

One rainy day this past March, deep in the thicket of papers and lost myself, I called my father. Trained as a scientist, he had always been fascinated by Francis's obsession, which Francis had continued to talk about his whole life. Francis was not fired in the end. But my grandparents did eventually retire early. They moved onto their boat and spent many years sailing across the world. But on their ultimate return to the United States, Francis submitted his papers several more times and engaged in ever-more-tangled correspondence, which I now had spread across the floor of my home.

My father described a picture he'd seen long ago. "It was a painting of this immensely complicated structure made up of linear pieces. It climbs, and it climbs, and it climbs, and above it is this perfect sphere floating in space. And when I saw that … I looked at that painting and I thought, *I know exactly what that is: You can't get there from here!*" he said. There is a gap between what you can prove with the tools available to you and what you believe to be true. Mathematicians spend decades constructing proofs for intuitions they had years earlier. Artists struggle to capture an inspiration on canvas. Scientists follow hunches, writers follow stories, all of us stumble forward on a tightrope of our own making without any guarantee that it will bear us. The painting is by Paul Klee; it is called *Limits of Understanding*.

There are many reasons to suppose that Francis may not, in fact, have discovered some fundamental truth. But perhaps he saw something glinting up there, above the tangled mess of human science, that was to him profoundly beautiful. "I just keep thinking," my father said, "that that must have been what he was experiencing."

The water-and-wine paradox—the one Jaynes did not solve—involves a theoretical glass containing a mixture of wine and water. The exact quantity of each liquid is unknown; your task is to determine how likely it is that the ratio of wine to water is within a certain range. You swiftly arrive at a reasonable-sounding answer. But when you repeat the problem from the angle of water to wine instead, you get another, equally valid-seeming answer, one that's completely different from the first. There is no clear way to decide which is right; that's where Francis's idea came in.

"Your grandfather was suggesting a criterion for saying that one of these choices was actually more reasonable than the other one," Sean Carroll, a physicist at the California Institute of Technology, told me. Then he suggested I get in touch with Harry Crane.

In the autumn of the year after my grandfather died, I spoke to Crane, a statistician at Rutgers University who studies probability theory and the logic of personal belief. He told me that from looking at a paper Francis wrote about the water-and-wine paradox, he could see the shape of his unborn idea. At this point I had long since put aside any hopes that the idea was something big, something misunderstood but brilliant. But I wanted, so many years after it first turned things upside down in Francis's life, to know what he had seen.

Francis Perey (Courtesy of Veronique Greenwood)

The criterion Francis was working on, Crane said, is deceptively simple. In a later paper, Francis uses the example of a coin on a table to describe how the concept works. Let's say the coin is covered by a sheet of paper. You don't know which side is up, because it's covered. But you do know that if it is heads, it's only a 180-degree turn from being tails, and vice versa. You can think of all the separate states that a system can be in—heads, tails, half wine, three-quarters water—and all the different ways it can be manipulated. The coin can't bend; the water can't suddenly change to wine partway through the process. If you require that your calculation of the probability respects these relationships—there is a mathematical way to do this—there will be only one right answer.

In essence, what he thought he'd found was a way in which probabilities arose naturally, a way in which they could be derived from the basic laws of the physical world rather than deduced from experiments. And he thought this should apply in the quantum world as well. It was both as simple, and as grandiose, as that. All the other papers, down through the years, involve elaborations of this main idea.

Now, such ideas hinge on technical details. And it isn't clear whether those details are correct (as well, quantum and classical probabilities operate so differently that such a claim raises many eyebrows). In fact, from some of the letters and notes in Francis's papers, I am inclined to think that he may have made a mistake, somewhere down in the numbers, and had been so enamored of the idea that he couldn't let it go.

But Crane is curious about what would have happened, so many decades ago, if the idea hadn't been blocked at every turn. "Let's think of an alternative universe in which the first time he submitted this same exact idea, it got accepted and published," Crane says. It might have been ignored by everyone, or maybe even criticized publicly, and discarded. Maybe it would have been enough for Francis, though, to have it out there. His long, slow, ultimately fruitless attempt to make the idea clear and publish it seems to be evidence of a kind of turning inward, a fixation on this unimpartible vision. Even ideas that are wrong may somewhere down the road lead to something, Crane says, if they can be put out into the open. Francis seems to have been caught in a purgatory of being unable to put the idea into the world and being unable to leave it alone.

Crane, who is the cofounder of Researchers.One, a nonprofit publishing platform, sees this as a failure of the scientific-publishing system. (Recently, he made Francis's work available for public review on the Researchers.One website.) But I wonder if it isn't more a failure—on the part of Francis himself, and maybe on the part of those of us who tell stories about science—of the recognition of the limits of inspiration. "Just because you have an epiphany doesn't mean it's going to be true," says Gregory Feist. "That's the starting point of science, that great insight. It's not the end."

At the memorial service we held for Francis, on the edge of the Tennessee River, an old friend of my grandparents' sat in a folding chair and recounted what my grandmother Claire had told her once when asked about Francis's theory. "She said, 'Well, in physics there's something called the Uncertainty Principle. And he is convinced that there *is* certainty.'

"I don't think it quite concluded in the way he hoped it would," the friend told us. "But he was excited about it all the same." Having been married to a scientist herself, she said that one of the great pleasures was witnessing this passion for ideas.

"I have never enjoyed doing physics so much," Francis wrote to Cox in 1982. In the end, that joy would have to be enough.

ABOUT THE AUTHOR

VERONIQUE GREENWOOD's work has appeared in *The New York Times Magazine*, *National Geographic*, *Aeon*, *Pacific Standard*, and many others.

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