Alan Lightman

# A sense of the mysterious

Ever since I was a young boy, my passions have been divided between science and art. I was fortunate to make a life in both, as a physicist and a novelist, and even to find creative sympathies between the two, but I have had to live with a constant tension in myself and a continual rumbling in my gut.

In childhood, I wrote dozens of poems. I expressed in verse my questions about death, my loneliness, my admiration for a plum-colored sky, my unrequited love for fourteen-year-old girls. Overdue books of poetry and stories littered my second-floor bedroom. Reading, listening, even thinking, I was mesmerized by the sounds and the movement of words. Words could be sudden, like 'jolt,' or slow, like 'meandering.' Words could be sharp or smooth, cool, silvery, prickly to touch, blaring like a trumpet call, fluid, pitter-pattered in rhythm. And, as if by magic, words could create scenes and emotions. When my grandfather died, I buried my grief in writing a poem, which I showed to my grandmother a month later. She cradled my face with her veined hands and said, "It's beautiful," and then began weeping all over again. How could marks on a white sheet of paper contain such power and force?

Between poems, I did scientific experiments. These I conducted in the cramped little laboratory I built out of a storage closet in my house. In my homemade alchemist's den, I horded resistors and capacitors, coils of wire of various thicknesses and grades, batteries, switches, photoelectric cells, magnets, dangerous chemicals that I had secretly ordered from unsuspecting supply stores, test tubes and Petri dishes, lovely glass flasks, Bunsen burners, scales. I delighted in my equipment. I loved to build things. Around the age of thirteen, I built a remote control device that could activate the lights in various rooms of the house, amazing my three younger brothers. With a thermostat, a lightbulb, and a padded cardboard box, I constructed an incubator for the cell cultures in my biology experiments. After seeing the Frankenstein movie, I built a

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spark-generating induction coil, requiring tedious weeks upon weeks of winding a mile's length of wire around an iron core.

In some of my scientific investigations, I had a partner, John, my best high-school friend. John was a year older than I, and as skinny as a strand of number-30-gauge wire. When he thought something ironic, he would let out a high-pitched shrill laugh that sounded like a hyena. John did not share my interest in poetry and the higher arts. For him, all that was a sissyish waste of calories. John was all practicality. John wanted to seize life by the throat and cut straight to the answer.

As it turned out, he was a genius with his hands. Patching together odds and ends from his house, he could build anything from scratch. John never saved the directions that came with new parts, he never drew up detailed schematic diagrams, and his wiring wandered drunkenly around the circuit board, but he had the magic touch, and when he would sit down cross-legged on the floor of his room and begin fiddling, the transistors hummed. His inventions were not pretty, but they worked, often better than mine.

Weekends, John and I would lie around in his room or mine, bored, listening to Bob Dylan records, occasionally thinking of things to excite our imaginations. Most of our friends filled their weekends with the company of girls, who produced plenty of excitement, but John and I were socially inept. So we listened to Dylan and read back issues of *Popular Science*. Lazily, we perused diagrams for building wrought-iron furniture with rivets instead of welded joints, circuits for fluorescent lamps and voice-activated tape recorders, and one-man flying machines made from plastic beach bottles. And we undertook our ritual expedition to Clark and Fay's on Poplar Avenue, the best-stocked supply store in Memphis. There, we squandered whole Saturdays happily adrift in the aisles of copper wire, socket wrenches, diodes, and oddly shaped metallic brackets that we had no immediate use for but purchased anyway. Clark and Fay's was our home away from home. No, more like our temple. At Clark and Fay's, we spoke to each other in whispers.

Our most successful collaboration was a light-borne communication device. The heart of the thing was a mouthpiece made out of a lid of a shoe polish can, with a flat section of a balloon stretched tightly across it. Onto this rubber membrane we attached a tiny piece of silvered glass, which acted as a mirror. A light beam was focused onto the tiny mirror and reflected from it. When a person talked into the mouthpiece, the rubber vibrated. In turn, the tiny mirror quivered, and those quiverings produced shimmerings in the reflected beam, like the shimmerings of sunlight reflected from a trembling sea. Thus, the information in the speaker's voice was precisely encoded onto light, each rise and dip of uttered sound translating itself into a brightening or dimming of light. After its reflection, the fluttering beam of light traveled across John's messy bedroom to our receiver, which was built from largely off-the-shelf stuff: a photocell to convert varying intensities of light into varying intensities of electrical current, an amplifier, and a microphone to convert electrical current into sound. Finally, the original voice was reproduced at the other end. Like any project in which John was involved, our communication device looked like a snarl of spare parts from a junkyard, but the thing worked.

It was with my rocket project that my scientific and artistic proclivities first collided. Ever since the launch of *Sputnik*

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in October of 1957, around my ninth birthday, I had been entranced with the idea of sending a spacecraft aloft. I imagined the blastoff, the uncoiling plume of smoke, the silvery body of the rocket lit by the sun, the huge acceleration, the beautiful arc of the trajectory in the sky. By the age of fourteen, I was experimenting with my own rocket fuels. A fuel that burned too fast would explode like a bomb; a fuel that burned too slow would smolder like a barbecue grill. What seemed to work best was a mixture of powdered charcoal and zinc, sulfur, and potassium nitrate. For the ignition, I used a flashbulb from a Brownie camera, embedded within the fuel chamber. The sudden heat from the bulb would easily start the combustion, and the bulb could be triggered by thin wires trailing from the tail of the rocket to the battery in my control center, a hundred feet away. The body of the rocket I built from an aluminum tube. The craft had red tail fins. It was beautiful. For a launch pad, I used a V-shaped steel girder, pointed skyward at the appropriate angle and anchored in a wooden Coca-Cola crate filled with concrete.

I invited my awed younger brothers and several friends from the neighborhood to attend the launch, which took place one Sunday at dawn at Ridgeway Golf Course. John, who was not the slightest romantic and didn't see anything useful about rockets, elected to stay in his bed and sleep. But even so, I had a good audience.

Because I had estimated from thrust and weight calculations that my rocket might ascend a half mile into space, some of the boys brought binoculars. From my control center, I called out the countdown. I closed the switch. Ignition. With a flash and a whoosh, the rocket shot from its pad. But after rising only a few hundred feet, it did a sickening swerve, spun out of control, and crashed. The fins had come off. With sudden clarity, I remembered that instead of riveting the fins to the rocket body as I should have, I had glued them on. To my eye, the rivets had been far too ugly. How I thought that mere glue would hold under the heat and aerodynamic force, I don't know. Evidently I had sacrificed reality for aesthetics. John would have been horrified.

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Later I learned that I was not the first scientist for whom beauty had ultimately succumbed to reality. Aristotle famously proposed that as the heavens revolve about the Earth, the planets move in circles. Circles because the circle is the simplest and most perfect shape. Even when astronomers discovered that the planets changed in brightness during their orbits, showing that they couldn't remain a constant distance from Earth, scientists remained so enthralled with the circle that they decided the planets must move in little circles attached to big circles. The circle idea was lovely and appealing. But it was proved wrong by the careful observations of Brahe and Kepler in the late sixteenth and early seventeenth centuries. Planets orbit in ellipses, not circles. Equally beautiful was the idea, dating from the 1930s, that all phenomena of nature should be completely identical if right-hand and left-hand are reversed, as if reflected in a mirror. This elegant idea, called parity conservation, was proved wrong in the late 1950s by the experiments proposed by Lee and Yang, showing that some subatomic particles and reactions do not have identical mirror-image twins. Contrary to all expectations, right- and left-handedness are not equal.

When my scientific projects went awry, I could always find certain fulfillment in mathematics. I loved mathe-

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matics just as I loved science and poetry. When my math teachers assigned homework, most other students groaned and complained, but I relished the job. I would save my math problems for last, right before bedtime, like bites of chocolate cake awaiting me after a long and dutiful meal of history and Latin. Then I would devour my cake. In geometry, I loved drawing the diagrams, I loved finding the inexorable and irrefutable relations between lines, angles, and curves. In algebra, I loved the idea of abstraction, letting Xs and Ys stand for the number of nickels in a jar or the height of a building in the distance. And then solving a set of connected equations, one logical step after another. I loved the shining purity of mathematics, the logic, the precision. I loved the certainty. With mathematics, you were guaranteed an answer as clean and crisp as a new twenty-dollar bill. And when you had found that answer, you were right, unquestionably right. The area of a circle is $\pi r^2$. Period.

Mathematics contrasted strongly with the ambiguities and contradictions of people. The world of people had no certainty or logic. People confused me. My mother sometimes said cruel things to me and my brothers, even though I felt that she loved us. My aunt Jean continued to drive recklessly and at great speed, even though everyone told her that she would kill herself in an automobile. My uncle Edwin asked me to do a mathematical calculation that would help him run the family business with more efficiency, but when I showed him the result he brushed it aside with disdain. Blanche, the dear woman who worked forever for our family, deserted her husband after he abused her and then talked about him with affection for years. How does one make sense out of such actions and words?

A long time later, after I became a novelist, I realized that the ambiguities and complexities of the human mind are what give fiction and perhaps all art their power. A good novel gets under our skin, provokes us and haunts us long after the first reading, because we never fully understand the characters. We sweep through the narrative over and over again, searching for meaning. Good characters must retain a certain mystery and unfathomable depth, even for the author. Once we see to the bottom of their hearts, the novel is dead for us.

Eventually, I learned to appreciate both certainty and uncertainty. Both are necessary in the world. Both are part of being human.

In college, I made two important decisions about my career. First, I would put my writing on the back burner until I became well established in science. I knew of a few scientists who later became writers, like C. P. Snow and Rachel Carson, but no writers who later in life became scientists. For some reason, science – at least the creative, research side of science – is a young person's game. In my own field, physics, I found that the average age at which Nobel Prize winners did their prize-winning work was only thirty-six. Perhaps it has something to do with the focus and isolation of the subject. A handiness for visualizing in six dimensions or for abstracting the motion of a pendulum favors an agility of mind but apparently has little to do with anything else. By contrast, the arts and humanities require experience with life and the awkward contradictions of people – experience that accumulates and deepens with age.

Second, I realized that I was better suited to be a theorist than an experimentalist. Although I loved to build things, I simply did not have the hands-

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on dexterity and practical talents of the best students. My junior-year electronics project caught fire when I plugged it in. My senior thesis project, a gorgeous apparatus of brass fittings and mylar windows designed to measure the half-life of certain radioactive atoms, was sidelined on the lab bench instead of being installed in the cyclotron for a real experiment. I never did believe the thing would actually work. And apparently neither did my professor, who kindly gave me high marks for my endless drawings of top views and side views and calculations of solid angles and efficiencies. By graduation, I knew that I was destined to be a theorist, a scientist who worked with abstractions about the physical world, ideas, mathematics. My equipment would be paper and pencil.

A year or two later, I had my first true experience with original research. It was an experience that I can compare only to my first love affair. At the time, I was twenty-two years old, a graduate student in physics at the California Institute of Technology. My thesis advisor at Caltech was Kip Thorne, only thirty himself but already a full professor. Kip had grown up in Mormon Utah but had completely acclimatized to the hip zone of California in the early 1970s. He sported long red hair, starting to thin, a red beard, sandals, loose kaftan-like shirts splotched with colors, sometimes a gold chain around his neck. Freckled, lean-limbed, wiry. And brilliant. His specialty was the study of general relativity, Einstein's theory of gravity. In fact, there was at this time a renaissance of interest in Einstein's arcane theory because astronomers had recently discovered new objects in space, such as neutron stars, that had enormous gravity and would require general relativity for a proper understanding.

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One of Kip's programs was to compare general relativity to other modern theories of gravity. And it was in that program that he assigned me my first research problem. I was supposed to show, by mathematical calculation, whether a particular experimental result required that gravity be geometrical. The known experimental result was that all objects fall under gravity with the same acceleration. Drop a book and a cannonball from the same height and they will hit the floor at the same time, if air resistance is small. By 'geometrical,' Kip meant that gravity could be described completely as a warping of space. In such a picture, a mass like the sun acts as if it were a heavy weight sitting on a stretched rubber sheet, and orbiting planets follow along the sagging surface of the sheet. In the early 1970s, some modern theories of gravity, such as Einstein's general relativity, were geometrical. Some were not. To be 'geometrical,' to be equivalent to a bending of space, a theory had to have a particular mathematical form. So my project amounted to writing down on a piece of paper the equations representing a giant umbrella theory of gravity, a theory of theories that encompassed many different possible theories, next imposing the restriction that all objects fall with the same acceleration, and then finding out whether that restriction were sufficiently powerful to rule out all nongeometrical theories.

I was both thrilled and terrified by my assignment. Until this point of my academic life, my theoretical adventures had consisted mainly of solving homework problems. With homework problems, the answer was known. If you couldn't solve the problem yourself, you could look up the answer in the back of the book or ask a smarter student for help. But this research problem with

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gravity was different. The answer wasn't known. And even though I understood that my problem was inconsequential in the grand sweep of science, it was still original research. No one would know the answer until I found it. Or failed to find it.

After an initial period of study and work, I succeeded in writing down all the equations I thought relevant. Then I hit a wall. I knew something was amiss, because a simple result at an early stage of the calculation was not coming out right. But I could not find my error. And I didn't even know what kind of error. Perhaps one of the equations was wrong. Or maybe the equations were right but I was making a silly arithmetic mistake. Or perhaps the conjecture was false but would require an especially devious counterexample to disprove it. Day after day, I checked each equation, I paced back and forth in my little windowless office, but I didn't know what I was doing wrong. This confusion and failure went on for months. For months, I ate, drank, and slept my research problem. I began keeping cans of tuna fish in the lower drawer of my desk and eating meals in my office.

Then one morning, I remember that it was a Sunday morning, I woke up about 5 A.M. and couldn't sleep. I felt terribly excited. Something strange was happening in my mind. I was thinking about my research problem, and I was seeing deeply into it. I was seeing it in ways I never had before. The physical sensation was that my head was lifting off my shoulders. I felt weightless. And I had absolutely no sense of my self. It was an experience completely without ego, without any thought about consequences or approval or fame. I didn't know who I was or where I was. I was simply spirit, in a state of pure exhilaration.

The best analogy I've been able to find for that intense feeling of the creative moment is sailing a round-bottomed boat in strong wind. Normally, the hull stays down in the water, with the frictional drag greatly limiting the speed of the boat. But in high wind, every once in a while the hull lifts out of the water and the drag goes instantly to near zero. It feels like a great hand has suddenly grabbed hold and flung you across the surface like a skimming stone. It's called planing.

So I woke up at five to find myself planing. Although I had no sense of my ego, I did have a feeling of rightness. I had a strong sensation of seeing deeply into this problem and understanding it and knowing that I was right – a certain kind of inevitability. With these sensations surging through me, I tiptoed out of my bedroom, almost reverently, afraid to disturb whatever strange magic was going on in my head, and I went to the kitchen. There, I sat down at my ramshackle table. I got out the pages of my calculations, by now curling and stained. A tiny bit of daylight was starting to seep through the window. Although I was oblivious to myself, my body, and everything around me, the fact is that I was completely alone. I don't think any other person in the world would have been able to help me at that moment. And I didn't want any help. I had all of these sensations and revelations going on in my head, and being alone with all that was an essential part of it.

Somehow, I had reconceptualized the project, spotting my error of thinking, and began anew. I'm not sure how this rethinking happened, but it wasn't by going from one equation to the next. After a while at the kitchen table, I solved my research problem. I had proved that the conjecture was true. The equal acceleration of the book and the cannonball does indeed require that gravity be geometrical. I strode out of the kitchen, feeling stunned and power-

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ful. Suddenly I heard a noise and looked up at the clock on the wall and saw that it was two o'clock in the afternoon.

I was to experience this creative moment again, with other scientific projects. But this was my first time. As a novelist, I've experienced the same sensation. I've read the accounts of other writers, musicians, and actors, and I think the sensation and process are almost identical in all creative activities. The pattern seems universal: The study and hard work. The prepared mind. The being stuck. The sudden shift. The letting go of control. The letting go of self.

I learned many things about science from Kip. One of the most important was the concept of the 'well-posed problem.' A well-posed problem is a problem that can be stated with enough clarity and definiteness that it is guaranteed a solution. Such a solution might require ten years, or a hundred, but there should be a definite solution. Such a solution may be arrived at by a variety of different approaches – such as Schrödinger's wave equation versus Heisenberg's matrix formulation of quantum mechanics – and these different expressions may involve very different mental pictures and interpretations and even psychological force. But they are mathematically and logically equivalent, and they all lead to the same numerical answers. They are all tools in the service of the well-posed problem. While it is true that science is constantly revising itself to respond to new information and ideas, at any moment in time scientists are working on well-posed problems.

I often think of Kip's idea of the well-posed problem as closely related to Karl Popper's notion of what makes a scientific proposition. According to Popper, who was an important early-twentieth-century British philosopher of science, a scientific proposition is a statement that can in principle be proved false. Unlike with mathematics, which exists completely within its own world of logical abstraction, you can never prove a scientific proposition or theory true because you can never be sure that tomorrow you might not find a counterexample in nature. Scientific theories are just simplified models of nature. Such a model might be mathematically correct but its beginning premises may not be in sufficient accord with physical reality. But you can certainly prove any scientific theory false. You can find a counterexample, an experiment that disagrees with the theory. And, according to Popper, unless you can at least *imagine* an experiment that might falsify the theory, that theory or statement is not scientific.

In direct and indirect ways, Kip emphasized to his students that we should not waste time on problems that weren't well posed. I have since come to understand that there are many interesting problems that are not well posed in the Popper or Thorne sense. For example: Does God exist? Or, What is love? Or, Would we be happier if we lived a thousand years? These questions are terribly interesting, but they lie outside the domain of science. Never will a physics student receive his or her degree working on such a question. One cannot falsify the statement that God exists (or doesn't exist). One cannot falsify the statement that we would be happier (or not happier) if we lived longer. Yet these are still fascinating questions, questions that provoke us and bring forth all kinds of creative thought and invention. For many artists and humanists, the question is more important than the answer. One of my favorite passages from Rilke's *Letters to a Young Poet*: "We should try to love the questions themselves, like locked rooms and like books that are written in a very foreign tongue." Science is powerful, but it has limitations. Just as the world needs both certainty

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and uncertainty, the world needs questions with answers and questions without answers.

Another thing I learned from Kip, more a matter of personal style, was generosity. Kip bent over backwards to give credit first to his students. He would put his name last on joint papers, he would heap praise on his students at public lectures. Kip was well aware of his strengths, but he was modest at the same time, and he was deeply generous in his heart. I believe that he inherited these virtues from his own thesis advisor at Princeton, John Wheeler. Wheeler, in turn, absorbed much of his personal style from his mentor, the great atomic physicist Niels Bohr in Copenhagen. In a sense, I was a great-grandstudent of Bohr.

Three Caltech professors served on my thesis committee, charged with examining me at my final thesis defense. Richard Feynman was one of the three. For some years, Feynman had taken an interest in Kip's students and, every couple of months, would go to lunch with us and pepper us with questions about the latest findings in gravitational waves or black holes or some other topic in general relativity. At my thesis defense, I stood at a blackboard in a small room while these guys sat comfortably and asked me questions. Feynman asked the first two questions. His first question was rather easy, and I answered it without too much trouble. His second question was just a little beyond my reach. I struggled with it, I went sideways and backwards, I circled around. Finally, after about twenty minutes of fumbling at the blackboard, I managed an answer. Feynman asked no more questions. Later, I realized that with his two questions he had precisely bracketed my ability. He had launched two artillery shells at me, one falling short, one long, and he knew exactly where I was in the intellectual landscape of physics.

I vividly remember a scene from sometime in 1975. It takes place during my two years as a postdoctoral fellow at Cornell. I am sitting on a couch in Edwin Salpeter's house. Ed, suffering from one of his recurring back problems, lies on the floor. From that low vantage, he is helping me think through a problem involving stars being ripped apart and consumed by a giant black hole. It is a theoretical problem of course.

At this time, Ed would have been about fifty years old. He was widely regarded as one of the two or three greatest theoretical astrophysicists in the world. His most famous work, done in the 1950s, involved the theoretical recipe for how helium atoms in stars can combine to make carbon and then heavier elements beyond that. It is believed that all of the chemical elements in the universe heavier than the two lightest, hydrogen and helium, were forged at the centers of stars. Ed and his colleagues showed how that process was possible. Among some of his other accomplishments, he calculated how many stars should be created in each range of mass – a sort of birth weight chart for newborn stars.

When I first arrived at Cornell, in the fall of 1974, Ed immediately dragged me out to the tennis court to find out what I was made of. I was a fair tennis player myself. After a number of exhausting matches over the season, we were approximately tied, but Ed could not refrain from quietly gloating whenever he beat me. And I could see that same gentlemanly but competitive edge in his science. He didn't like to lose.

On and off the tennis court, Ed dressed in tattered short-sleeve sports shirts. These, combined with his loafers

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and stylishly long hair and faint Austrian accent, gave him an air of casual elegance. But Ed was enormously serious about his physics. When he was talking about a physics problem, he would sometimes stop, turn his head, and just stare off into space for a few moments, and you knew that he was delving into deeper layers of thought.

What I found most brilliant about Ed was his physical intuition. He could visualize a physical problem and almost feel his way to the core of it, all in his head. This ability arose from his vast knowledge of physics and astronomy and his talent for making analogies from one subject to another. Many of the greatest scientists have had this talent for analogies. Planck compared the inside surface of a container to a collection of springs with different oscillation frequencies. Bohr compared the nucleus of an atom to a drop of liquid.

So we're in Ed's living room, me on the couch, Ed on his back on the floor, some kind of classical music floating in from the next room, and Ed draws an analogy between stars being swallowed by the big black hole and a drunk wandering on a street with an uncovered sewer hole. If a star comes too close to the black hole it will be destroyed, just as if the drunk stumbles to the sewer hole he will fall in. Each star, in each orbit around the central black hole, is given a random jostle by the gravity of the other stars, just as the drunk takes a random step every minute. Such random steps can lead a star, or a drunk, to fall into the hole. The star bumps about in two-dimensional 'angular-momentum space,' just as the drunk wanders around on a two-dimensional street. The critical question, Ed announces from the floor, is whether each random step of the drunk is bigger or smaller than the diameter of the hole. With this insight, I and the other postdoctoral fellow collaborating with me on the problem can now work out the details. The result will be a prediction for the Hubble Space Telescope, more than a decade away. Ed asks if I would please bring him a cup of tea. He has other things to think about this morning.

Some months later, I had a severe emotional upheaval with a different scientific project. I was working on the arrangement of stars in a globular cluster. A globular cluster is a congregation of about a hundred thousand stars, all orbiting each other under their mutual gravitational attraction. There are about a hundred globular clusters in our galaxy. Through the telescope, a globular cluster appears as a beautiful, shining ball of light. Imagine: a hundred thousand stars all concentrated together in a tight ball, whizzing about like angry bees in a bees' nest.

Since about 1970, astrophysicists had begun to simulate the structure and evolution of globular clusters on a computer. You feed the computer the initial position of a lot of points, each representing a star or group of stars, you put in the effects of gravity, each point gravitationally attracting all the others, and you let the computer tell you what happens in time. In a sense, the computer is doing an experiment for you. Each minute of computer time might represent a million years for the globular cluster. One of the findings of these 'experiments' was that the simulated globular clusters begin collapsing. The inner stars lose energy and move closer to the center, while the outer stars gain energy and move farther from the center. For extra gratification, there were even observations of actual globular clusters in space, observations suggesting that some globular clusters may indeed have undergone such collapse.

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Many of the computer simulations had been done with the simplification that all stars have the same mass. I wanted to investigate what happens under the more realistic assumption that there is a range of masses of stars. But instead of doing a computer simulation, which is extremely time-consuming to set up and costly to run, I found an approximate way to attack the problem using only pencil and paper. As I suspected, having a range of masses of stars made the cluster collapse even sooner and faster.

While in the final stages of writing up my results for publication, I strolled into the astronomy library to complete my list of references to previous work. And there, to my horror, I discovered a brand new issue of *Astrophysics and Space Science* in which two Japanese scientists had solved the same problem. With my pulse racing, I checked their results against mine. Our figures and graphs agreed to within three decimal places. I had been scooped! Of course, most people get scooped at various times of their lives if they're working on anything at all interesting. But this was the first time for me.

I experienced a complex set of reactions. I was embarrassed. I was humiliated. I grieved the loss of several months of my time. I worried whether the wasted effort would compromise my chances for an assistant professorship. But then, another emotion began working its way through my body. Amazement. I was utterly amazed that people on the other side of the planet, with no correspondence between us, no comparing of notes, had decided to solve the same problem and had gotten the same answer to three decimal places. There was something wonderful and thrilling about that. Here was powerful evidence of a thing – part science, part mathematics – that exists outside of our own heads. Presumably, Martians would have also gotten the same answer to three decimal places. There was a terrible precision in the world.

After this feeling of awe at the terrible precision and exactness of the world, I began to experience another emotion: irrelevancy. If the physical universe is reducible to precise equations with precise answers to three decimal places (and more), then why was I, as a particular person, needed to find those answers? For the globular cluster problem with multiple masses, Saito and Yoshizawa had found the answer before me. If neither they nor I had found the answer, then in another month or another year somebody else would have found the answer. Another scientist might have used a different formulation of the problem, or described his or her results with different language, but the answer would have been the same. It seems to me that science is not the best occupation for a person who wants to make a mark as an individual, accomplishing something only that individual can do. In science, it is the final measured number or the final equation that matters most. If Heisenberg and Schrödinger hadn't formulated quantum mechanics, then someone else would have. If Einstein hadn't formulated relativity, then someone else would. If Watson and Crick hadn't discovered the double-helical structure of DNA, then someone else would. Science brims with colorful personalities, but the most important thing about a scientific result is not the scientist who found it but the result itself. Because that result is universal. In a sense, that result already exists. It is found by the scientist. For me, this impersonal, disembodied character of science is both its great strength and its great weakness.

I couldn't help comparing the situation to my other passion, the arts. In the

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arts, individual expression is everything. You can separate Einstein from the equations of relativity, but you cannot separate Beethoven from the *Moonlight Sonata*. No one will ever write the *The Tempest* except Shakespeare or *The Trial* except Kafka.

I loved the grandeur, the power, the beauty, the logic, and the precision of science, but I also ached to express something of myself, my individuality, the particular way that I saw the world, my unique way of being. On that day in the Cornell library as I feverishly turned the pages of *Astrophysics and Space Science*, I learned something about science, and I also learned something about myself. I would continue following my passion for science, but I could no longer suppress my passion for writing.

Finally, in the early 1980s, I began writing essays. For some years I had been publishing poems in small literary magazines. The essay gave me the greater flexibility I wanted. With an essay, I could be informative, poetic, philosophical, personal. And, at a time when most of my self-identity and confidence were still based on my achievements as a scientist, with the essay I could connect my scientific and artistic interests. I would come home in the evening, elated from a day of research at the Harvard-Smithsonian Center for Astrophysics, and ponder an essay.

One of my first essays concerned Joseph Weber, a distinguished professor of physics at the University of Maryland. Weber had pioneered the first gravitational wave detectors. And he had become somewhat of an outcast in the scientific community because he claimed to see gravitational waves when no one else could.

When you shake an electrical charge, it emits waves of electricity and magnetism that travel through space at the speed of light. Likewise, Einstein's general relativity predicted that when you shake a mass of any kind, whether electrically charged or not, it emits gravitational waves, waves of oscillating gravity that travel through space at the speed of light. Hypothetically, the strongest sources of such waves would be cataclysmic cosmic events, like the collision of black holes in space.

How does one observe a gravitational wave? When a gravitational wave strikes a mass, it causes that mass to expand and contract like a working billows pump. Gravitational waves, however, are fantastically weaker than electromagnetic waves. A typical expansion or contraction expected for a cosmic gravitational wave might be one part in $10^{21}$ or smaller, corresponding to a thousand-mile-long ruler changing its length by the width of a single atomic nucleus. Consequently, while a high-school student can build a crystal radio set to detect electromagnetic waves, gravitational waves require extraordinarily sensitive equipment to measure them.

In 1960, when no one else was dreaming of detecting gravitational waves, Weber conceived of the idea of a resonant cylinder, a metallic cylinder that would ring like a bell (but an extremely soft bell) when struck by a gravitational wave. One of the problems of building such a resonant cylinder, or any detector, is that it is always expanding and contracting a little bit from tiny random disturbances, such as a truck turning a corner a half a mile away. It is extremely difficult to discriminate such noise from the minuscule motions expected from a gravitational wave. So you build two cylinders, thousands of miles apart, and monitor them closely. If both of them begin softly ringing in precisely the same way at the same time, then perhaps

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they've just been struck by a gravitational wave.

In the early 1960s, Weber began building such cylinders, the first one located at the University of Maryland near Washington, D.C., the second at Argonne National Laboratory near Chicago. Each cylinder had a length of five feet, a diameter of about two feet, and a weight of about three thousand pounds. In 1968, not long after the completion of his second cylinder, Weber began reporting the observation of simultaneous oscillations of his two cylinders. He claimed to have discovered the first gravitational waves.

In the following decade, other groups of scientists attempted to duplicate Weber's results. They built their own cylinders, hooked them up to their own piezoelectric crystals to measure minute oscillations, compared their own charts of the oscillations in time. No one saw oscillations of the magnitude claimed by Weber, and no one saw simultaneous oscillations of their cylinders except what would be expected by chance. In fact, other detectors were built with a hundred times more sensitivity than Weber's, and they failed to find gravitational waves.

Weber published his results. Other scientists published theirs. Weber dismissed the negative findings of other scientists. Experimental physicists studied Weber's results and said he was making mistakes. Perhaps the tape recorders he used to combine the data from the two cylinders were themselves accidentally injecting simultaneous signals. Or perhaps small magnetic fluctuations in electric power lines or lightning bolts could mimic gravitational waves. Weber held his ground. Theorists got into the act. They calculated the amount of expansion and contraction that would be expected from realistic sources of gravitational waves in space. According to these calculations, Weber's resonant cylinders were not remotely sensitive enough to detect gravitational waves, even if such waves did indeed exist. A few theorists proposed the possibility of exotic mechanisms to generate gravitational waves with enormous power, and these proposals confused the discussion. Weber passionately held his ground. In telephone conversations, in personal visits, at scientific conferences, he got into scathing arguments. He lost friends and colleagues. Yet, in the face of a mountain of contradictory evidence, he continued to maintain that he was measuring gravitational waves. Clearly, Weber was not behaving in the traditions of science. Joseph Weber was allowing his personal investment to interfere with good judgment.

Then I, a greenhorn essayist, leaped into the fray. I wrote an essay about emotional prejudice in scientists for the magazine *Science 83*. The title: "Nothing but the Truth." In this essay, I ridiculed several scientists, including Weber. I cringe when I reread it. With self-righteous flourish, I wrote that "The white-haired Weber has become something of a tragic figure in the scientific community, continuing to declare his rightness in the face of incontrovertible evidence."

A few months after the essay was published, I found myself ten feet from Weber at a scientific conference. Some unsuspecting colleague introduced us. Weber's face immediately turned purple, he snarled something at me, and he stomped away.

Later, I decided that I deserved his contempt, and I hated myself for what I had written. Because Joseph Weber was really a hero. Yes, he was almost certainly sloppy in his experiment. And he should have graciously accepted the opposing results of other scientists. But he

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had imagined the first gravitational wave detector, he had built the first gravitational wave detector, and his insights about gravitational wave detectors had created the field. Today, the most advanced gravitational wave detector in the world, the Laser Interferometer Gravitational Wave Observatory (LIGO), has just recently begun operations. If LIGO does not detect the first gravitational wave, then its upgraded version probably will. LIGO would not exist without Weber's seminal work.

And it is quite possible that Weber would not have accomplished that work without his emotional prejudice and passion. In the book *Personal Knowledge*, the chemist Michael Polanyi argues that such personal passion is vital to the advance of science. I agree. Without a powerful emotional commitment, scientists could not summon up the enormous energy needed for pursuing an idea for years, working day and night in the lab or at their desks doing calculations, often sacrificing the rest of their lives. It is little wonder that such a personal commitment sometimes causes the scientist to defend his or her beliefs regardless of facts.

Even extraordinary physicists such as Einstein and Planck have defended their prejudices in the face of opposing evidence. Soon after Einstein published his theory of special relativity in 1905, a German experimental physicist named Walter Kaufmann repeated a crucial experiment to measure the mass of electrons moving at high speed. According to Einstein's theory, the mass of a moving particle should increase with speed in a particular way. A competing theory by Max Abraham, a colleague of Kaufmann's at Göttingen University, proposed a different formula for the increase in mass. Kaufmann's experimental results were closer to Abraham's predictions than to Einstein's. Over the next year, the great Max Planck, father of the quantum, carefully studied Kaufmann's experiment but could find no flaw. Nevertheless, Planck threw his support behind Einstein's theory.

Einstein himself, in a review article in 1907, said he could see nothing wrong with Kaufmann's experiments and agreed that they fit Abraham's theory better than his. Yet, he continued, "In my opinion other theories [theories other than his own] have a rather small probability because their fundamental assumptions concerning the mass of the moving electrons are not explainable in terms of theoretical systems which embrace a greater complex of phenomena." Here and elsewhere, Einstein clearly preferred his prejudice for comprehensive theoretical systems over actual experimental data. And data do sometimes change. A few years later, the experiments of Kaufmann were proved to be in error, and Einstein was vindicated. In future years, however, his prejudices sometimes led him astray. For decades, Einstein was personally committed to his nonquantum unified theory that combined gravity and electromagnetism. In a letter to his friend Paul Ehrenfest in 1929, Einstein wrote, "[My] latest results are so beautiful that I have every confidence in having found the natural field equations of such a variety." This time, Einstein turned out to be wrong. But that is not the point. When right and when wrong, Einstein's passion, his aesthetic and philosophical prejudices, and his personal commitment were probably essential to his scientific creativity.

All of which led me to question the meaning of 'the scientific method.' Since high school, I had been taught that scientists must wear sterile gloves at all times and remain detached from their work, that the distinguishing feature of

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science is the much vaunted scientific method, whereby hypotheses and theories are objectively tested against experiments. If the theory is contradicted by experiments, then it must be revised or discarded. If one experiment is contradicted by many other experiments, then it must be critically examined. Such an objective procedure would seem to leave little room for personal prejudice.

I have since come to understand that the situation is more complex. The scientific method does not derive from the actions and behavior of *individual* scientists. Individual scientists are not emotionally detached from their research. Rather, the scientific method draws its strength from the *community* of scientists, who are always eager to criticize and test each other's work. Every week there are many journal articles, conferences, and informal gatherings at the blackboard in which scientists analyze the latest ideas and results from all over the world. It is through this collective activity that objectivity emerges.

So how could I reconcile the Popperian view of science, with its unbudging demand for objective experimental tests, against the Polanyian view, with its emphasis on the personal commitments and passions of individual scientists? The answer, perhaps obvious but at first shocking to a young scientist, is that one must distinguish between science and the practice of science. Science is an ideal, a conception of logical laws acting in the world and a set of tools for discovering those laws. By contrast, the practice of science is a human affair, complicated by all the bedraggled but marvelous psychology that makes us human.

About the time of my ill-considered essay on Joseph Weber, I had a most beautiful experience with scientific discovery, perhaps the most beautiful of my life. I was studying the effects of particle creation in high-temperature gases. According to Einstein's famous formula, $E = mc^2$, energy can be created from matter and matter can be created from energy. The phenomenon has been observed in the lab. It should also occur in space. Whenever the temperature of a gas is high enough, as should happen in strong gravity, then some of that thermal energy can be transformed into electrons and their antiparticles, the positrons. In turn, the creation of those particles will act back on the properties and emitted radiation of the gas. Thus, a good theoretical understanding of the nature of such a 'relativistic thermal plasma' would be interesting not only in its own right, but also as a diagnostic for interpreting the gamma rays and X-rays observed from high-energy objects in space.

This research problem had been suggested to me by Martin Rees of the Institute of Astronomy in England. I first met Martin during a visit to his institute in the summer of 1974, just after receiving my Ph.D. Martin was only thirty-two at the time. In the world of astrophysics, he was already a natural phenomenon. Among his many accomplishments, he was one of the first to point out that the distribution of quasars in space was inconsistent with the steady-state theory of cosmology, thus lending support to the big bang theory. He has made major contributions to the astrophysics of black holes, the theory of galaxy formation, the origin of the cosmic background radiation, and many other topics. In fact, there has been practically no area of modern astronomy and cosmology that has not benefited from Martin Rees's imagination. Martin is always erupting with new ideas, and he freely shares these without seeking acknowledgment or credit. Many of the nearly

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*A sense of the mysterious*

illegible letters I received from him during the middle and late 1970s, when we were working on similar problems, would begin, "Thank you Alan for your very interesting preprint on X. I agree almost entirely with you, except for one or two small points." And then he would go on to elaborate on a number of important and often critical effects that I had missed in my investigation.

Many a pleasant summer I spent enjoying the unhurried pace and intimacy of Cambridge, England, walking through the luxurious gardens of the colleges and bicycling up Madingley Road to the Institute of Astronomy. At that time, it was a modest one-story building bordered by a wooden fence and a cow pasture. In the 1970s and 1980s, nearly everyone in the world worth their salt in astrophysics visited that building – to quietly work, to gather for British tea at four in the afternoon, and to catch ideas thrown out by the youthful but silver-haired Martin Rees, Plumian Professor of Astronomy and Experimental Philosophy. (In the 1990s, Martin became Sir Martin and was further elevated to Astronomer Royal of England.)

Sometime around 1980, Martin suggested the importance of understanding the theoretical properties of high-temperature gases. The problem nagged at me for a couple of years before I found a way to approach it. There were two obvious extreme cases. When the temperatures were low, there would be no creation of particles. The properties of such a gas were well understood. In particular, the emitted radiation increased with increasing temperature in a known way. (All gases emit some radiation, except at zero temperature.) Also well understood was the case of extremely high temperatures. Here, there would be such a huge number of electrons and positrons created that the radiation would be trapped, except for a thin layer at the outer edge of the gas. The properties of this gas were also well understood. In such a situation, the emerging radiation would have a well-known form, called black-body radiation, that would increase with temperature in a known way. However, because of the prodigious energy requirements, such extremely high-temperature gases with black-body radiation would not actually exist in space. Most interesting, therefore, was the intermediate case, when the temperature is high enough to create particles but not so high to produce enough particles to trap the radiation and yield black-body radiation.

I was fascinated by the question of how the intermediate case would join to the others. I expected that as energy was put into the gas at a higher and higher rate, the temperature would first start to increase according to the low-temperature case, then increase at some other intermediate rate, then finally begin increasing according to the ultra high-temperature case.

To my astonishment, I discovered something entirely different. With increasing energy input, the temperature at first did indeed rise as expected. But after increasing to a critical value, the temperature began *decreasing* with further increase of the rate of energy input and emitted radiation. Finally, at a very high rate of energy input, the temperature turned around and began increasing again, in the known way for a very high-temperature gas.

At first, this result seemed absolutely counter to my physical intuition. Put more energy into something and you expect its temperature to go up, not down. Then I understood. The temperature of a gas is the average energy of a particle in that gas. Once you begin creating new particles, the additional parti-

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cles can soak up all the increased energy, so much so that the average energy per particle actually can decrease. By analogy, when you give increasing quantities of food to a nation, the amount of food per person normally increases. But if the people of that nation produce children at a fast enough rate, then the food per person can actually begin decreasing even though there is more and more total food.

The result was not only astonishing. It was delightful, it was beautiful, and it was a little mysterious. Again, I experienced a kaleidoscope of emotions. Initially, I was surprised. Then, I was puzzled. Then, when I understood the result, I was extremely happy. I had found something new – again not terribly important in the grand scheme of science, but something that no one had ever known before me – and I felt elated and powerful with the knowledge. (In fact, a Swedish physicist, Roland Svensson, independently found the same result about the same time, and we published nearly simultaneously.)

Then I felt a sense of mystery. I had shed light on a small corner of nature. Other scientists had illuminated larger regions. But there were almost certainly vast chambers and ballrooms that remained in the dark. So many beautiful and strange things as yet unknown. In an article published in 1931, Einstein wrote, "The most beautiful experience we can have is the mysterious. It is the fundamental emotion which stands at the cradle of true art and true science." What did Einstein mean by "the mysterious?" I don't think he meant that science is full of unpredictable or unknowable or supernatural forces. I believe that he meant a sense of awe, a sense that there are things larger than us, that we do not have all the answers. A sense that we can stand right at the edge between known and unknown and gaze into that cavern and be exhilarated rather than frightened. I have experienced that beautiful mystery both as a physicist and as a novelist. As a physicist, in the infinite mystery of physical nature. As a novelist, in the infinite mystery of human nature and the power of words to portray some of that mystery.

In the decade after my project on high-temperature gases, my science began gently subsiding, like a retreating blue tide. I looked out at the horizon and felt that my best work as a scientist was moving away into my past. At the same time, I gazed into the future and began pushing the boundaries of my essays, which took on more of a fabulist quality, like the writings of Italo Calvino and Primo Levi. I invented. I told stories. I wrote about life and society on a planet made entirely of iron. I wrote about a moody Isaac Newton visiting my office. The science in my essays became only a doorway to what lay beyond. Eventually, when I was about forty years old, I began writing fiction. The time had arrived for my other passion to take over. Around 1990, when I left Harvard for MIT, I had stopped doing scientific research altogether. I miss it terribly, despite the many pleasures and rewards of being a writer.

But I am still a scientist. I am still fascinated by how things work, by the beauty and logic of the natural world. When I see something interesting, like a particular angle made by the wake of a boat, I still take out a pencil and calculate why. When I travel on airplanes, I still amuse myself by rederiving mathematical theorems that I learned years ago. Even when I write a scene for a novel, I sometimes subconsciously begin a paragraph with a topic sentence – a perfect metaphor for science, but nearly fatal for art.

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Every writer has a source for his writing, a deep hidden well that he draws from to create. For me that source is science. In ways that I cannot explain, science suffuses all of my novels, characters, scenes, sentences, even individual words. Some people have told me that my novels have an architectural quality, a prominence of design. Perhaps that is a sign of the source.

Over the years, I have learned to recognize the different sensations of science and of art in my body. Some of the sensations, such as the creative moment, are the same. But I know the feeling in my body of deriving an equation. I know the different feeling in my body of listening to one of my characters speak before I have told her what to say. I know the line. I know the swoop of an idea. I know the wavering note. Most of the time, these feelings swirl all together as a rumbling in my stomach, a wondrous and beautiful and finally mysterious cry of the world, logic and illogic, certainty and uncertainty, questions with answers and questions without.

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