

A SCIENTIST BY CHOICE

What must a person be like if he or she is to become an accomplished scientist? If you pose this question to a group of scientists, you will probably receive a number of conflicting answers. You can expect an even greater diversity of opinion if you ask people outside the scientific community. In this Commemorative Lecture I wish to offer my own ideas, and then use my own scientific and nonscientific life as a case study, in an attempt to support my assertions.

First of all, a potential scientist must have an intense interest in the subject matter of science—phenomena of all sorts that occur in our world and our universe. He must wonder how a stone falling into a pool can produce expanding rings of waves, or how the coming of autumn can turn maple leaves to brilliant reds, oranges, and yellows, or how a volcano can send glowing ash high into the sky, but, if he is to rise to the top, he must also want to know why these phenomena *must* occur, and not simply why they may. Some persons from other walks of life may be charmed by the symmetry of the waves, thrilled by the hues of an autumn forest, or awed by the vista of a smoking peak, but yet might find themselves bored by any explanation as to how or why these sights arise. This will not do for a scientist.

Next, the successful scientist must possess the ability to pursue and ultimately discover answers to the questions that intrigue him. Talents differ, and those who can write the most exquisite poetry or compose the most sublime music may nevertheless be lacking in scientific ability. Naturally, there are also many truly competent scientists who could never write any poetry or compose any music that anyone else

would care to read or listen to.

Finally, the scientist must always be on the lookout for other explanations than those that have been commonly disseminated and perhaps commonly accepted. It is not enough for him to be able to understand the explanations that are correct and clearly presented; he must recognize that when he fails to follow an argument, even when it has been put forth by a leader in the field, the fault sometimes lies not with his ability to understand but with the argument itself. He must recognize at just what point he can no longer follow the argument, and he must then be prepared to restate it in an understandable manner, or else reject it altogether and seek an explanation of his own.

Are these traits something that must be apparent early in life? I believe that they often show up in childhood, but I have not seen the evidence that they cannot emerge at a more mature age.

Let me turn to the case study with which I am most familiar—my own life. I hope that you can view my talk not primarily as a rather incomplete autobiography, but instead as an account of how a person may decide to follow a scientific career, and how he or she may become exposed to the ideas of other scientists, may react to these ideas, may encounter and develop ideas of his or her own, and may sometimes make more universally valuable contributions by extending these ideas beyond his or her special field.

I was born in West Hartford, Connecticut, a town of about 8,000 that tripled its size while I was growing up, and that lay adjacent to Hartford, the capital of

Connecticut, with about 150,000 inhabitants. Like many well-to-do suburbs, West Hartford had its own town hall, fire and police departments, and school system, but it had few businesses or industries, and most of the residents worked in Hartford. I attended the West Hartford public schools until I was seventeen years old, but my real teachers were my parents.

My father, Edward Henry Lorenz, was a mechanical engineer who had grown up in Hartford. His work involved designing machinery to make bottles and other glass articles, but he was fascinated by all aspects of science, and particularly mathematics. My mother, née Grace Norton, had been teaching in Chicago, where she was also deeply involved in community affairs. When my father and mother married in 1916, they bought a home in West Hartford, in which I was born the following year.

At an early age I became fascinated with numbers. My mother told me that before I was two years old, when she would take me for a walk in a go-cart, I would read all the numbers on the houses. A few years later, when I learned what multiplication was, I became fond of the numbers that were perfect squares, and I could recite them from 1 to 10000. Still later I would spend many hours with my father, playing with mathematical puzzles. I also enjoyed taking square roots by the long-hand method, and even learned a method for extracting cube roots—a procedure which is now rejected as unnecessarily cumbersome, and which I have long since forgotten.

I clearly remember a Sunday afternoon when I was nearly seven, when our

family went to visit some friends on a farm a few miles east of Hartford. I had by then become fond of maps, and I especially enjoyed looking at maps that were enlargements of sections of other maps. I even used to draw maps of places that I had invented, where I could enlarge a section, and then a section of a section. At our friends' house I found an atlas which I began to read, and eventually came to a page showing a number of circular objects of different sizes. I was especially struck by something that looked like a ball with a big ring around it and reminded me of a peculiar hat that I had seen in a cartoon. I asked my father what it was, and he told me about all the planets, and about Saturn's rings. That afternoon was the start of a love of astronomy that I have never lost. I was rewarded less than a year later when a total eclipse of the sun came to Hartford on a bitterly cold day, and I could see the shadow bands shimmering across the fields of snow.

Could one have predicted at that time that as an adult I would turn to the sciences? Perhaps so, if numbers and maps and planets had been my only interests, but there were other things. I loved card games and board games of almost all sorts. Most of these my mother taught me, when she had to think of some way to amuse me when I was not at school. She was an excellent chess player, and naturally I learned the game; years later I became captain of my high-school and then my college chess teams. Unfortunately the games I learned did not include Go.

Besides regular games there were crossword puzzles and jigsaw puzzles, which I still love. I still have a collection of about twenty high-quality hand-cut wooden jigsaw puzzles that I had as a boy. My father and I both virtually learned all the

pieces by heart. We used to compete to see who could put each puzzle together more rapidly, and our times are still recorded on the inside covers of the boxes.

I had a good ear for music, and knew before I was three that my mother was singing off key, but I loved to hear her sing anyway. At nine years of age I began lessons on the violin, but, although I enjoyed them at first, I didn't have the necessary dexterity in my fingers, and I never could produce a really pleasing sound or play with vibrato. I realize now that what I really wanted was to learn about music instead of how to play music.

I was smaller than most boys my age, and was also a year younger than most of my classmates, and partly for this reason I never became adept at team sports, nor was I particularly welcome when I tried to enter a game. By the time I reached high school I had managed to equal my companions in swimming. Even though most of them could swim a bit faster, I could swim farther under water than any of them. During the summers I also became fond of hiking, and soon I could reach the top of a mountain faster than most of my friends; I didn't have much weight to carry. To this day the mountains, along with music, are my greatest spare-time interests.

I don't suppose that my activities as a child were any more varied than those of most other children whom I knew. Certainly many of them had interests that I lacked. Perhaps, however, my interests were diversified enough to have made something other than a scientific career a possibility.

Nevertheless, when I entered Dartmouth College I had already made up my

mind to major in mathematics. Neither the suggestion of one advisor that it might be better to major in something like history, and take mathematics courses on the side, nor the appeal of the elementary courses in physics and geology that I attended, changed my decision. There were instances where I simply could not follow the logic of the arguments presented in the latter courses. In retrospect, it seems likely that the textbooks, in trying to present simple arguments, had oversimplified them so much that the logical chain of ideas was broken, but I failed to recognize this at the time. When as a senior I took a course in mathematical physics, offered by the Mathematics Department, some of the points that I had not understood became clear when written as mathematical equations.

Before graduating I realized that I wanted to continue studying mathematics, and in the autumn of 1938 I entered the Graduate School at Harvard University. The idea of a curriculum where I would enjoy every subject was almost unimaginably attractive. I found that my basic preparation at Dartmouth was excellent, but I was somewhat overwhelmed by the number of concepts and fields of study of which I had been completely unaware, such as group theory, set theory, and combinatorial topology. Other graduate students in the Mathematics Department became my close friends, and our many technical discussions were invaluable in helping me adapt to the new ways of thinking.

I eventually chose to work under the guidance of George Birkhoff on a problem in mathematical physics. I am not sure whether he appreciated my reference to some of these problems as things that looked like physics but were not physics. Birkhoff

was one of the very top American mathematicians, whose works covered virtually every branch of the field. He was noted for having formulated a mathematical theory of aesthetics. In elementary courses his classroom lectures were sometimes hard to follow, but in advanced courses, which often dealt with problems that he was currently investigating, he was fascinating. We would sometimes watch him derive on the blackboard, during class, some new results that he himself had not obtained before.

Oddly enough, in a field to which I had originally been attracted because of my love for numbers, I seldom saw any numbers except 0, 1, and 2. There were plenty of symbols representing numbers, but the numbers themselves almost never appeared, even in the lectures in number theory.

With the outbreak of the war it became apparent that I would not be able to complete my final year. The Army had meanwhile been circulating notices of courses in meteorology to be offered at a few universities, where suitably qualified persons would be allowed to enroll after enlisting, and upon graduation would become weather officers. The weather and its sudden changes had always fascinated me. Thus it was that a few months before I had expected to receive a doctorate from Harvard, I found myself moving just two miles down the Charles River to the Massachusetts Institute of Technology and joining a hundred other students, ostensibly to study to become weather forecasters.

It soon became evident that we were studying to be meteorologists. The distinction is one that I was slow to appreciate. Meteorology deals with all aspects

of the atmosphere. It is concerned with answering simply worded questions that any child might ask, such as "Why does it rain?", or more specialized questions that would mean little to a layman, such as "Does vorticity or divergence exert a greater influence on tropical weather fluctuations?" Weather forecasting does occur among the prominent topics, and it has commanded a large share of the efforts of meteorologists because of its relevance to many human activities, but it is possible for one to have a distinguished career in meteorology without having any idea of how to draw a weather map or forecast the weather.

We were, in fact, enrolled in the regular graduate program in meteorology at M.I.T., except that what would ordinarily have occupied two years was crowded into about eight months. In the afternoons we learned to forecast, using selected sequences of past weather maps as case studies. In the mornings we attended classes devoted largely to theory, most of which seemed relevant to forecasting, but some of which had not been shown to lead to improved forecasts. Our faculty in meteorology was as outstanding as any in the world, and it was natural that they should want to teach real science to their students. This was probably compatible with the Army's philosophy that an officer is a gentleman.

The subject that seemed to fit in most naturally with my mathematical background was dynamic meteorology. The dynamic meteorologist looks at the atmosphere as a large inhomogeneous mass of gas infused with some liquid drops and solid particles, enveloping an approximately spherical earth with an irregular surface. In practice he often overlooks the inhomogeneities and irregularities. He

regards the weather at any location as consisting of the local values of density, pressure, temperature, three-dimensional wind velocity, and gaseous, liquid, and solid water content, and strictly speaking the concentrations of such impurities as sea salt, dust, and smoke. The simultaneous values of these quantities at all locations constitute the state of the atmosphere. This state changes from one time to the next according to a set of physical laws. The dynamic meteorologist expresses these laws as a system of equations, and dynamic meteorology consists of the application of these equations to a wide variety of problems.

The practicing forecaster would regard any such view of the atmosphere as at best incomplete. He would note how the weather is organized into structures, which include globe-encircling jet streams, migratory storms of subcontinental size, smaller more intense storms known as typhoons, cyclones, or hurricanes, towering clouds that give rise to showers and sometimes spawn tornados, and smaller innocuous clouds. The list is far from complete. He would observe how these structures change their locations or intensities from one day or one hour to the next, and he would learn the telltale signs for the appearance of new structures or the decay of old ones. He would forecast the weather by applying this acquired knowledge, and would make little explicit use of the underlying physical principles. Of course, the dynamic meteorologist is also aware of these structures, but his interest in them may be confined to explaining their existence—often not an easy task.

After completing the course I received orders along with four classmates to remain at M.I.T. as instructors for the next class. This gave us the opportunity

to attend some advanced classes, and I began to feel more like an experienced meteorologist, but, despite our excellent faculty, a few basic ideas seemed to be missing. Not only were we never shown how to use the dynamic equations to make weather forecasts, which I had naively assumed was the reason for our studying dynamic meteorology, but we were not even told whether they could be used in this manner. Only later did I learn that our instructors not only did not know how to use the equations for forecasting, but they did not know whether this was possible. I also learned that some outstanding meteorologists at other universities believed that it was impossible.

In due time I was sent to the tropics as a forecaster. There I discovered that many of the rules for forecasting in temperate latitudes that we had so carefully learned did not work in the tropics. To some extent I had to learn forecasting again, and this time not in a university classroom. I also became more acutely aware of what anybody can see by looking at a globe, that the tropics cover a huge portion of the earth, and, what is not so obvious, that their influence on the weather extends far beyond their boundaries.

Following the war I had to decide whether to return to mathematics at Harvard or continue in meteorology at M.I.T. After much deliberation I chose meteorology. Mathematics deals with concepts and their interrelations, and establishing a theory of any concept has required or will require much intensive research. The study of prime numbers, for example, began centuries ago, and there are still some unanswered questions. Mathematicians seem to have no difficulty in creating new con-

cepts faster than the old ones become well understood, and there will undoubtedly always be many challenging problems to solve. Nevertheless, I believed that some of the unsolved meteorological problems were more fundamental, and I felt confident that I could contribute to some of their solutions. For example, I had learned, first in the classroom and then in the field, a simple rule that every weather forecaster knows, namely, that storms are likely to continue to move for a while in the directions in which they have been moving, and I had used this rule in making forecasts, but I had never learned *why* storms would continue to move in one direction, nor, for that matter, why they moved at all. It was basic questions like these that offered a real challenge.

With my interest in dynamic meteorology and my continuing belief that weather forecasting was an important part of meteorology, I proceeded to write a doctoral thesis that proposed a method of applying the dynamic equations to the prediction of the motions of storms. I believe that with some modifications the method might have been practical, but it was more cumbersome than some others that were currently being developed, and I do not expect that it will ever be put to use.

Our faculty did, however, accept the thesis. This was indeed my greatest month, for a few weeks later Jane Loban and I were married. Jane had been working in our department as a research assistant. After our marriage I began to work as a postdoctoral scientist, still at M.I.T., on a project directed by Victor Starr, who had joined the faculty a few months earlier. Jane continued with her job for a while.

Although not yet forty, Starr was already recognized as one of the world's top dynamic meteorologists. He became my mentor, and also a close friend, and I worked with him, first as a protégé and then as a colleague after I received a faculty appointment, for over 25 years, until his death shortly after his retirement.

He presented his ideas, in his lectures and in his writings, with remarkable clarity. He had the ability to draw meaningful conclusions about the atmosphere by properly applying the dynamic equations, where others before him had failed. During the early years of our acquaintance he presented what was, if not the first explanation of why storms move as they do, the first one that I could really understand. Ironically, this was his only paper, as far as I know, that was rejected for publication, not as being technically incorrect but as being somewhat ambiguous. Personally I still find Starr's explanation clearer than any other.

This was the midpoint of the twentieth century, and meteorology was beginning to move rapidly ahead. The most exciting new development was numerical weather prediction—forecasting the weather by solving the dynamic equations—prophesied years earlier as being possible, even though others were maintaining that it was impossible, but now becoming practical because computers were becoming available to some meteorological groups. I followed the developments eagerly, and became acquainted with some of those involved in the work, but never became directly involved myself.

Like most meteorological research going on at American universities, then as well as now, our work was funded by an outside agency. Our contract gave us a great

deal of leeway, but it did stipulate that we should investigate the general circulation of the atmosphere. This term refers to the globe-encircling westerly and easterly wind currents and the accompanying poleward and equatorward and upward and downward drifts. It is also generally taken to include the temperature and moisture patterns that must accompany these motions. I became principally involved with the dynamics of the general circulation.

In the course of my work I was invited to spend one summer at the Lowell Observatory in Flagstaff, Arizona, where a similarly organized project was investigating the general circulations of the atmospheres of the other planets. I accepted eagerly. I obtained some results that, although interesting, were not startling, and I did not seriously consider changing my field from meteorology to astronomy, but one of my childhood dreams was fulfilled when I learned to use the large refracting telescope, and spent many nighttime hours observing Jupiter.

My first significant finding came a few years later. One problem that had not lacked attention but was unsolved was the chain of events through which a small percentage of the solar energy reaching the earth is converted into the kinetic energy of the atmospheric motions, thereby replacing the energy that is dissipated by friction. Starr and I had often talked about the problem, and he felt that there ought to be some measure of how much energy already present in the atmosphere in the form of heat is *available* for conversion into the energy of the motions.

Because of its mathematical nature, most research in dynamic meteorology was carried out on the blackboard or with pencil and paper; computers were not yet

generally available. Not all of this work was performed in the office. Starr soon acquired many postdoctoral associates and graduate students, and we would often have lunch together at one of the local eating places. As often as not, before we left, the paper napkins and place mats were filled with equations or diagrammatic sketches.

One night at home I woke up shortly after midnight and began to think about available energy again. Jane and the children were out of town, and everything was very quiet. Within minutes I had thought of a way in which available energy might be formulated. In the course of an hour or so, before falling asleep again, I had worked out all of the equations needed to specify this "new" form of energy and to express the rates at which it was produced by solar heating and then transformed into kinetic energy by systems of updrafts and downdrafts. I became rather excited, but finally fell asleep again, only to wake up after another hour or so and derive another set of equations showing how the available energy contained in the global currents was converted into the available energy of the superposed storms—the final missing link in the atmosphere's energy cycle. When I awoke again and daylight had arrived I grabbed a pencil and a pad of paper and began writing down the equations that had passed through my head during the night, to see whether they were more than a fantasy. I could find no mathematical error, and, after arriving at work, I showed the equations to Starr, who was at first a bit surprised that everything worked through so nicely, but soon advised me to publish the results as quickly as I could. I still regard the paper that resulted as one of my most important, even

though the real work was done in an hour or two, without a pencil or a light.

It seems appropriate at this point to make some remarks about establishing results and publishing them. Obviously you do not want to submit a piece of work, only to discover that someone has already published the same thing. I have even seen it stated that before investigating a problem a competent research scientist will familiarize himself with all of the previously performed work relevant to the problem. I do not entirely agree.

If you are continually coming up with new ideas that are not confined to a single narrow topic, you may well need more time to search through the literature and discover whether your hypotheses have previously been proven or disproven than to plunge in and establish the result yourself. You will also gain a much deeper appreciation for your topic and its possible extensions by succeeding in duplicating someone else's result than by simply reading about the result.

If you do come up with something worth publishing, it is time to search the literature for possible duplication, but even then you should not make the search so extensive that it seriously delays the publication date. If you cannot find anything fairly soon, your result may be new, and, if it is not, perhaps a reviewer will be aware of this. In the paper that has been cited more often than any other that I have written, the original reviewer found that some of the results had appeared before, while most of them were new. It was easy to revise the manuscript and resubmit it, presenting the earlier part as background material, with references that the reviewer had given me, and then including the new part in its original form.

A year or so later Thomas Malone resigned from our faculty in order to establish a new weather research center, and I was appointed to fill his place. Malone had been directing a project in statistical weather forecasting. The principal tool was linear regression—a procedure based upon the observed past behavior of the weather rather than the dynamical equations that govern it. An example of a forecast by linear regression might be one that predicts that tomorrow's temperature at Tokyo will equal 0.7 times today's temperature at Kyoto plus 0.3 times yesterday's temperature at Kagoshima, although this particular rule would probably not give very good results. With the development of computers it became feasible to combine the weather elements at many locations, after multiplying their values by numbers that would be determined by established methods, to produce single formulas. With Malone's position I also acquired his project, which included some excellent graduate students, and I proceeded to learn something about statistics.

It had been claimed that there was a mathematical proof that linear regression was inherently capable of performing as well as any other procedure, including numerical weather prediction. I was skeptical, and I proposed to test the idea by using a simple system of equations—today we would call such a system a “model”—to generate an artificial set of weather data, after which I would determine whether a linear formula could reproduce the data. I soon realized that if the artificial sequences turned out to be periodic, repeating their previous values at regular intervals, linear regression would produce perfect forecasts, so, for the test, I had to find a model whose solutions would vary irregularly from one time to the next,

just as the real atmosphere appears to do.

Computers were becoming faster and more compact, and one day Robert White, a postdoctoral scientist in our department who later went on to become Chief of the U.S. Weather Bureau, suggested that I acquire a computer for use in my own office. In this age of personal computers one might wonder why I had not already done so, but in 1958 such a thing was almost unprecedented, and the possibility had never occurred to me. I soon obtained a Royal-McBee LGP-30 computer about the size of a large desk. Suddenly I realized that my desire to do things with numbers would also be fulfilled.

After learning how to write computer programs and optimize them, I tested one model after another, and finally arrived at one that consisted of twelve equations. The twelve variables represented gross features of the weather, such as the speed of the globe-encircling westerly winds. After being given twelve numbers to represent the weather pattern at the starting time, the computer would advance the weather in six-hour time steps, each step requiring about ten seconds of computation. After every fourth step, or every simulated day, the computer would print out the new values of the twelve variables; this required another ten seconds. After a few hours a large array of numbers would be produced, and it was easy to look at one of the twelve columns and see how the numbers were varying. There was no sign of periodicity. At times during the next few weeks I would let the computer grind out more solutions, sometimes with new starting conditions, and it became evident that the general behavior was nonperiodic. When I applied the linear-regression method

to the simulated weather, I found that it produced only mediocre results.

At one point I wanted to examine a solution in greater detail, so I stopped the computer and typed in the twelve numbers from a row that the computer had printed earlier. I started the computer again, and went out for a cup of coffee. When I returned about an hour later, after the computer had generated about two months of data, I found that the new solution did not agree with the original one. At first I suspected trouble with the computer, which occurred fairly often, but, when I compared the new solution step by step with the older one, I found that at first the solutions were the same, and then they would differ by one unit in the last decimal place, and then the differences would become larger and larger, doubling in magnitude in about four simulated days, until, after sixty days, the solutions were unrecognizably different.

Soon I realized what had happened. The computer was carrying its numbers to about six decimal places, but, in order to have twelve numbers together on one line, I had instructed it to round off the printed values to three places. The numbers that I typed in were therefore not the original numbers, but only rounded-off approximations. The model evidently had the property that small differences between solutions would proceed to amplify, until they became as large as differences between randomly selected solutions.

At this point I became rather excited. I realized that if the real atmosphere behaved in the same manner as the model, long-range weather prediction would be impossible, since most real weather elements were certainly not measured accurately

to three decimal places. During the following months I convinced myself that the lack of periodicity and the growth of the small differences were somehow related, and I was eventually able to prove that under fairly general conditions either type of behavior implied the other.

Phenomena that behave in this manner are now often collectively referred to as "chaos." Atmospheric predictability—the extent to which it is possible to predict various features of the weather at various ranges—soon became one of my principal interests, and in recent years it has replaced the general circulation as the meteorological topic that occupies the largest part of my attention. In introducing me as a seminar speaker a host once called me "Mr. Predictability," but there is no way of knowing whether I would have become involved with predictability at all if it had not been for that minute or so when I typed the wrong numbers.

The importance of the chaotic behavior of the atmosphere extends well beyond its effect on our ability to predict. Consider experimentation. When a physicist or a chemist performs an experiment, it is commonly believed that someone else performing the same experiment ought to obtain the same result. If the results differ, one of the experiments may become suspect. Meteorologists and other earth scientists frequently find that they cannot repeat their results very closely, and they may be pleased with a general qualitative resemblance. This does not imply that meteorologists tend to be less careful than physicists and chemists; it simply indicates that they are dealing with phenomena that are more chaotic.

Consider a hypothetical experiment involving the development of convective

clouds—the kind where the air is continually overturning. These clouds cannot easily be reproduced in the laboratory, and the experiment may consist of traveling to a suitable location and waiting for the right sort of weather to arrive. Suppose that throughout the vicinity of the chosen location the weather conditions on two different early mornings appear to be identical. As noontime approaches, small innocuous clouds may appear on one of the these days, while towering clouds and showers appear on the other. The difference may occur because atmospheric convection is inherently chaotic, so that undetectable small differences at sunrise can amplify many fold. Even if the clouds could have been simulated in the laboratory, the successive experiments might also have diverged after beginning with undetectably small differences.

In recent years many meteorological experiments have been performed on the computer. Here it would seem that any experiment should be repeatable, but this is still not the case unless the experimental set-up is repeated exactly. As simple a change as transferring the experiment to a different computer with a different rounding-off procedure can cause the experiments to diverge. When this happens, who can say which experiment is more definitive?

I should like to close by noting that the presence of chaos can increase the likelihood that unjustified or even dishonest results may be published or otherwise disseminated. Poor science and dishonest science are not the same thing, since someone with the best of intentions can make a mistake without becoming aware of it. Falsification of results is obviously dishonest, but, in a science where chaotic

phenomena are abundant, dishonesty can be more subtle.

Suppose that a meteorologist has formulated a hypothesis that he wishes to confirm experimentally, and suppose that he performs a large set of experiments and then reports what percentage of these experiments are favorable to his hypothesis. This can be good science. If instead he selects from the set only the one that is most favorable to his hypothesis, he can do so without falsifying any measurements, but this is nevertheless dishonest science, unless he specifically states that he has chosen a favorable experiment for demonstrations purposes.

If he performs only one experiment and reports on it, and is quite unaware that a second experiment might have given a different result, this may be poor science, but it is not dishonest. If he performs only one experiment, and, seeing that the result is favorable to his hypothesis, decides at that point not to perform any more, even though he suspects that a second experiment might be unfavorable, this again is not completely honest, unless he explains that he stopped when the indications were favorable. Stopping when you are ahead may be good policy in a gambling hall, but a series of scientific experiments is not a game.

The sciences that have sometimes been called less exact, like some of the earth sciences and life sciences, are sciences just as surely as are the so-called exact sciences, like some branches of physics and chemistry. They simply require additional care in their practice, because they are more likely to involve chaotic processes. I have been awed by the precision that physicists sometimes attain, but I have never regretted my decision to follow a less exact science.