

PATHOLOGICAL SCIENCE

Certain symptoms seen in studies of 'N rays' and other elusive phenomena characterize 'the science of things that aren't so.'

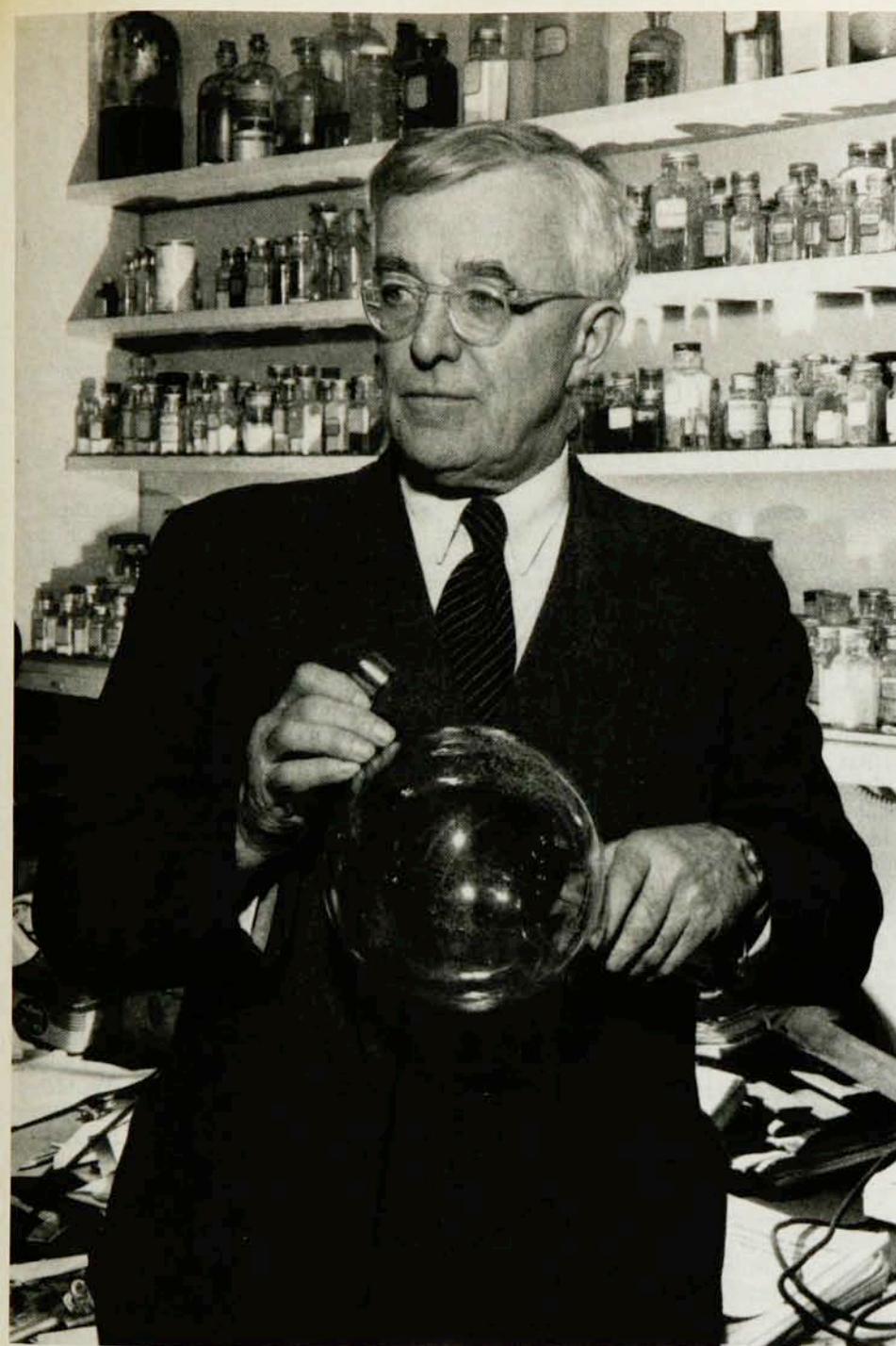
Irving Langmuir
Transcribed and edited
by Robert N. Hall

Irving Langmuir spent many productive years pursuing Nobel-caliber research (see the photo on the opposite page). Over the years, he also explored the subject of what he called "pathological science." Although he never published his investigations in this area, on 18 December 1953 at General Electric's Knolls Atomic Power Laboratory, he gave a colloquium on the subject that will long be remembered by those in his audience. This talk was a colorful account of a particular kind of pitfall into which scientists may stumble.

The tape recording that was made of Langmuir's colloquium has been lost or erased. However, in 1966, a microgroove disk transcription that was made of this tape was found among the Langmuir papers in the Library of Congress. The disk recording is of poor quality, but most of what Langmuir said can be understood with a little practice. Robert N. Hall, a former colleague of Langmuir's at General Electric, transcribed the disk and edited it to make an internal report for the company. At that time, a small amount of editing was felt to be desirable. Some abortive or repetitious sentences were eliminated. Hall wrote the epilogue. Figures from corresponding publications were used to represent Langmuir's blackboard sketches. These agree in essence, if not in every detail, with Langmuir's descriptions. Some references were added for the benefit of anyone wishing to undertake a further investigation of this subject.

*The disk recording has been transcribed back onto tape, and a copy is on file in the Whitney Library at General Electric. A slightly abbreviated version of the talk was published in *Speculations in Science and Technology* 8:77 (1985).*

PHYSICS TODAY has added a few more illustrations and has done some additional light editing to improve the readability while still maintaining the spontaneous flavor of this talk. Bracketed phrases in roman type are substitutions for the original words made for clarity or precision. Italicized bracketed phrases are editorial insertions. Deletions from the original text are marked with ellipses.



Irving Langmuir earned the 1932 Nobel Prize in Chemistry for work dealing with the adsorption of monolayers of molecules on surfaces. He spent his career at the General Electric Company in Schenectady, New York, working there from 1909 until his retirement in 1950. His research included such phenomena as thermionic emission and the properties of liquid surfaces. Over the years, Langmuir also explored the subject of "pathological science," although he never published his investigations on this topic. He died in 1957.

The thing started in this way. On 23 April 1929, Professor Bergen Davis from Columbia University came up and gave a colloquium in this Laboratory, in the old building, and it was very interesting. He told Dr. Whitney and myself and a few others something about his talk beforehand. He was very enthusiastic about it and he got us interested in it. *[Langmuir may have remembered this date incorrectly. The date on a letter Langmuir wrote just a few days after Davis's talk is 23 April 1930. Willis R. Whitney was director of the General Electric Research Laboratory.]*

Davis and Barnes experiment

I'll show you right on this diagram what kind of thing happened. *[See the figure on page 39].* Davis *[and his*

colleague Arthur Barnes] produced a beam of alpha rays from polonium in a vacuum tube. *[They]* had a parabolic hot cathode electron emitter with a hole in the middle, and the alpha rays came through it and could be counted by scintillations on a zinc sulfide screen with a microscope. The electrons were focused on *[a]* plate so that for a distance there was a stream of electrons moving along with the alpha particles.

Now you could accelerate the electrons and get them up to the velocity of the alpha particles. To get an electron to move with that velocity takes about 590 volts, so if you put 590 volts *[on the grid]*, accelerating the electrons, the electrons would travel along with the alpha particles. The idea of the experiment was that if they moved along

together at the same velocity they might [combine]. Thus the alpha particle would lose one of its charges. It would pick up one electron so that, instead of being a helium [ion] with two positive charges, it would only have one charge. Well, if an alpha particle with a double charge has one electron, [its energy levels are] just like [those of] the Bohr theory of the hydrogen atom . . . , with a Balmer series, and you can calculate the energy necessary to knock off the electron and so on.

Well, Davis and Barnes found that if [the electron's] velocity was made to be the same as that of the alpha particle, there was a loss in the number of deflected particles. If there were no electrons, for example, and no magnetic field, all the alpha particles would be collected [at the upper screen]. They counted something of the order of 50 counts per minute there. If you put on a magnetic field you could deflect the alpha particles so they would go down [to the lower screen]. But if they picked up an electron, then they would only have half the charge, and therefore they would only be deflected half as much and they would not strike the screen.

Now the results that they got, or said that they got at the time, were very extraordinary. They found that not only did these electrons combine with the alpha particles when the electron velocity [corresponded to] 590 volts, but also at a series of discrete differences of voltage. When the velocity of the electrons was less or more than that velocity by perfectly discrete amounts, then they could also combine. All the results seemed to show that about 80% of them combined. In other words, there was about an 80% change in the current when the conditions were right. Then they found that the velocity differences had to be exactly the velocities that you can calculate from the Bohr theory. In other words, if the electron happened to be going with a velocity equal to the velocity that it would have if it were in a Bohr orbit, then it would be captured.

Of course that makes a difficulty right away, because in the Bohr theory when there is an electron coming in from infinity it has to give up half its energy to settle into the Bohr orbit. Since it must conserve energy, it has to radiate out, and it radiates out an amount equal to the energy that it has left in the orbit. So if the electron comes in with an amount of energy equal to the amount it is going to end up with, then it has to radiate an amount of energy equal to twice that. Nobody had any evidence for that. So there was a little difficulty, which never was quite resolved, although there were two or three people including some in Germany who worked up theories to account for how that might be. Sommerfeld, for example, in Germany, worked up a theory to account for how the electron could be captured if it had a velocity equal to what

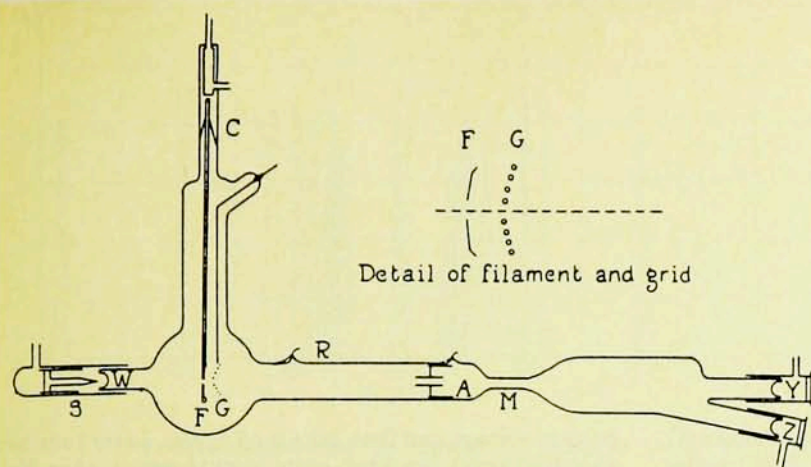
it was going to have after it settled down into the orbit.

Well, there were these discrete peaks, each one corresponding to one of the energy levels in the Bohr theory of the helium atom, and nothing else. Those were the only things they recorded. So you had these discrete peaks. Well, how wide were they? They were *one-hundredth of a volt wide*. In other words, you had to have 590 volts. That would give you equal velocities. But there were other peaks, and I think the next velocity would be at about 325.01 volts. If you had that voltage, then you got beautiful capture. If you didn't, if you changed it by one hundredth of a volt—nothing. [The capture rate] would go right from 80% down to nothing. It was sharp. They were only able to measure to a hundredth of a volt so it was an all-or-nothing effect. Well, besides the peak [at 590 volts], there were 10 or 12 different lines in the Balmer series, all of which could be detected, and all of which had an 80% efficiency. [See the figure on page 41.] [The alpha particles] almost completely captured all the electrons when they got exactly on the peak.

Well, in the discussion [following Davis's talk], we questioned how, experimentally, you could examine the whole spectrum, because each count, you see, took a long time. There was a long series of alpha particle counts that took two minutes at a time, and you had to do [them] 10 or 15 times and you had to adjust the voltage to a hundredth of a volt. If you had to go through steps of a hundredth of a volt each and cover all the range from 330 up to 900 volts, you'd have quite a job. Well, they said they didn't do it quite that way. They had found by some preliminary work that [the peak voltages] did check with the Bohr orbit velocities, so they knew where to look for them. They sometimes found them not exactly where they expected them, but they explored around in that neighborhood and the result was that they got [the peaks] with extraordinary precision—so high, in fact, that they were sure they'd be able to check the Rydberg constant more accurately than it can be done by studying the hydrogen spectrum, which is something like one in 10^8 . At any rate, they had no inhibitions at all as to the accuracy which could be obtained by this method, especially since they were measuring these voltages within a hundredth of a volt.

Anybody who looks at the setup would be a little doubtful about whether the electrons had velocities that were fixed and definite within a hundredth of a volt, because this is not exactly a homogeneous field. The distance over which the alpha particles and electrons were moving along together was only [about] 5 [cm].

Well, in [Davis's] talk, a few other things came out that were very interesting. One was that the percentage of capture was always around 80%. The curve would come



Apparatus used by Arthur Barnes and Bergen Davis in a 1929 experiment at Columbia University. Alpha particles from source S entered the tube through the window W and would either travel straight through to the window Y or be deflected by a magnetic field at M and leave through window Z. Scintillations from zinc sulfide screens beyond both exit windows signaled which path the alpha particles took. Electrons were produced at the filament F, accelerated by the anode grid G and focused toward the anode A. Thus electrons traveled along with alpha particles for a short distance. Barnes and Davis claimed the electrons were captured by the alpha particles when their velocities were equal. If so, the alpha particle would be deflected somewhere between Y and Z, and the scintillation count would decrease. The Columbia scientists later retracted their claims. (From ref. 1.)

along at about 80% and there would be a sharp peak up here and another sharp peak here and, well, all the peaks were about the same height.

Well, we asked him, how did this depend upon current density? "That's very interesting," he said. "It doesn't depend at all upon current density."

We asked, "How much could you change the temperature of the cathode?"

"Well," he said, "that's the queer thing about it. You can change it all the way down to room temperature."

"Well," I said, "then you wouldn't have any electrons."

"Oh yes," he said. "If you check the Richardson equation and calculate, you'll find that you get electrons even at room temperature and those are the ones that are captured."

"Well," I said, "there wouldn't be enough to combine with all the alpha particles, and besides that, the alpha particles are only there for a short time as they pass through, and the electrons are a long way apart at such low current densities, at 10^{-20} amperes or so."

[Davis] said, "That seemed like quite a great difficulty. But," he said, "you see it isn't so bad because we now know that the electrons are waves. So the electron doesn't have to be there at all in order to combine with something. Only the waves have to be there and they can be of low intensity and the quantum theory causes all the electrons to pile in at just the right place where they are needed." So he saw no difficulty. And so it went.

On-site inspection

Well, Dr. Whitney likes the experimental method, and these were experiments—very careful experiments, described in great detail. The results seemed to be very

interesting from a theoretical point of view. So Dr. Whitney suggested that he would like to see these experiments repeated with a Geiger counter instead of [relying on] counting scintillations, and C. W. [Clarence] Hewlett, who was here working on Geiger counters, had a setup and it was proposed that we would give [Davis] one of these, maybe at a cost of several thousand dollars or so for the whole equipment, so that he could get better data. But I was a little more cautious. I said to Dr. Whitney that before we actually give [the counter] to [Davis] and just turn it over to him, it would be well to go down and take a look at these experiments and see what they really mean. Well, Hewlett was very much interested, and I was interested so only about two days after [Davis's] colloquium, we went down to New York. We went to Davis's laboratory at Columbia University, and we found that they were very glad to see us, very proud to show us all their results. So we started in early in the morning.

We sat in the dark room for half an hour to get our eyes adapted to the darkness so that we could count scintillations. I said I would first like to see these scintillations with the field on and with the field off. So I looked in and I counted about 50 or 60. Hewlett counted 70, and I counted somewhat lower. On the other hand, we both agreed substantially. What we found was this: These scintillations were quite bright with your eyes adapted, and there was no trouble at all counting when these alpha particles struck the screen. They came along at a rate of about one per second. When you put on a magnetic field and deflected them out, the count came down to about 17, which was a pretty high percentage—about 25% background. Barnes was sitting with us, and he said that's probably radioactive contamination of the screen. Then Barnes counted and he got 230 on the first count and about

200 on the next, and when he put on the field, [the count] went down to about 25. Well, Hewlett and I didn't know what that meant but we couldn't see 230. Later, we understood the reason. . . .

Well, I don't want to spend too much time on this experiment. I wrote a 22-page letter about these things and I have a lot of notes. The gist of it was this. There was a long table at which Barnes was sitting, and he had another table where an assistant named Hull sat looking at a big scale voltmeter, or potentiometer really. It had a scale that went from one to a thousand volts and, on that scale that went from one to a thousand, he read hundredths of a volt. He thought he might be able to do a little better than that. At any rate, you could interpolate and put down figures, you know. Now the room was dark except for a little light on which you could read the scale on that meter. And it was dark except for the dial of a clock.

[Barnes] counted scintillations for 2 minutes. He said he always counted for 2 minutes. Actually, I had a stopwatch and I checked up on him. [The intervals] were sometimes as low as 1 minute and 10 seconds and sometimes 1 minute and 55 seconds, but he counted them all as 2 minutes, and yet the results were of high accuracy!

Well, we made various suggestions. One was to turn off the voltage entirely. Then Barnes got some low values around 20 or 30, or sometimes as high as 50. Then to get the conditions on a peak he adjusted the voltage to 200 and—well, some of those readings are interesting. [One figure I put down was] 325.01. There he got only a reading of 52, whereas before, when he was on the peak, he got about 230. He didn't like that very much so he tried changing this to 325.02—a change of one-hundredth of a volt. And there he got 48. Then he went in between. [The counts] fell off, you see, so he tried 325.015, and then he got 107 [counts]. So that was a peak.

Well, a little later, I whispered to Hull, who was adjusting the voltage, holding it constant, and I suggested to him to make it one tenth of a volt different. Barnes didn't know this and he got 96. Well, when I suggested this change to Hull, you could see immediately that he was amazed. He said, "Why, that's too big a change. That will put it way off the peak." That was almost one tenth of a volt, you see. Later I suggested taking a whole volt.

Then we had lunch. We sat for half an hour in the dark room so as not to spoil our eyes and then we had some readings at zero volts and then we went back to 325.03 [volts]. We changed by one-hundredth of a volt and there [Barnes] got 110 counts. And now he got two or three readings at 110.

The denouement

And then I played a dirty trick. I wrote out on a card of paper ten different sequences of V and 0. I meant to put on a

certain voltage and then take it off again. Later I realized that that [trick wouldn't quite work] because when Hull took off the voltage, he sat back in his chair—there was nothing to regulate at zero so he didn't. Well, of course, Barnes saw him whenever he sat back in his chair. Although the light wasn't very bright, he could see whether [Hull] was sitting back in his chair or not, so he knew the voltage wasn't on, and the result was that he got a corresponding result. So later I whispered, "Don't let him know that you're not reading," and I asked him to change the voltage from 325 down to 320 so he'd have something to regulate. I said, "Regulate it just as carefully as if you were sitting on a peak." So he played the part from that time on, and from that time on Barnes's readings had nothing whatever to do with the voltages that were applied. Whether the voltage was at one value or another didn't make the slightest difference. After that he took 12 readings, of which about half were right and the other half were wrong, which was about what you would expect out of two sets of values.

I said: "You're through. You're not measuring anything at all. You never *have* measured anything at all."

"Well," he said, "the tube was gassy. The temperature has changed and therefore the nickel plates must have deformed themselves so that the electrodes are no longer lined up properly."

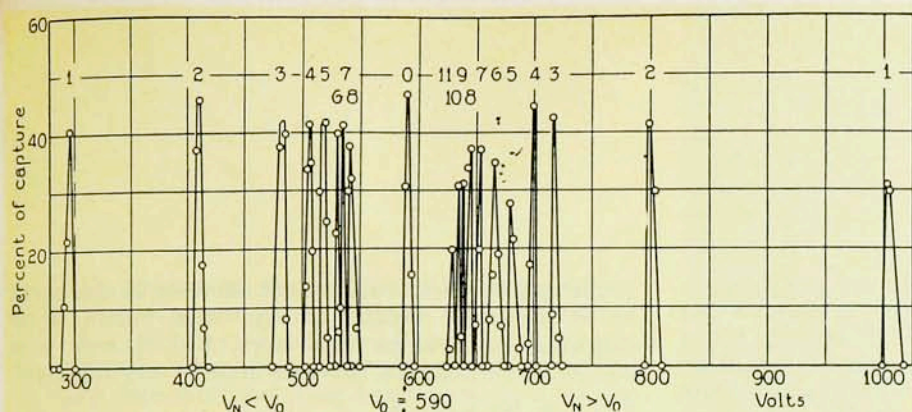
"Well," I said, "isn't this the tube in which Davis said he got the same results when the filament was turned off completely?"

"Oh, yes," he said, "but we always made blanks to check ourselves, with and without the voltage on."

He immediately—without giving any thought to it—he immediately had an excuse. He had a reason for not paying any attention to any wrong results. It just was built into him. He just had worked that way all along and always would. There is no question but [that] he is honest. He *believed* these things, absolutely.

Hewlett stayed there and continued to work with [Barnes] for quite a while, and I went in and talked it over with Davis and he was simply dumbfounded. He couldn't believe a word of it. "It absolutely can't be," he said. "Look at the way we found those peaks before we knew anything about the Bohr theory. We took those values and calculated them and they checked exactly. Later on, after we got confirmation, in order to save time, to see whether the peaks were there, we would calculate ahead of time." He was so sure from the whole history of the thing that it was utterly impossible that there never had been any measurements at all that he just wouldn't believe it.

Well, [Davis] had just read a paper before the research laboratory at Schenectady, and he was going to read the paper the following Saturday before the National Academy of Sciences. . . . And he wrote me that he was going to



Peaks signaled electron capture, according to Barnes and Davis (see the figure on page 39). Plotted here is the decrease in the number of scintillation counts when a magnetic field was turned on, as a function of the accelerating voltage applied to electrons. Fewer scintillations meant that fewer alpha particles were arriving at the screen—because, the researchers claimed, the alpha particles were capturing electrons. Peaks appeared only at certain discrete voltages, whose energies corresponded to the energy levels of electrons in the Bohr theory. Numbers above each peak correspond to the principal quantum number n . The central peak, labeled 0, is at the voltage where the electrons and alpha particles have the same speed. (From ref. 1.)

do so on the 24th [of April 1930]. I wrote to him on the day after I got back. Our letters crossed in the mails and he said that he had been thinking over the various things that I had told him, and his confidence wasn't shaken. So he went ahead and presented the paper before the National Academy of Sciences.

Then I wrote him a 22-page letter giving all our data and showing really that the whole approach to the thing was wrong; [Barnes] was counting hallucinations, which I find is common among people who work with scintillations if they count for too long. Barnes counted for six hours a day and it never fatigued him. Of course it didn't fatigue him, because it was all made up out of his head. He told us that you mustn't count the bright particles. He had a beautiful reason for why you mustn't pay any attention to the bright flashes. When Hewlett tried to check his data [Barnes] said: "Why, you must be counting those bright flashes. Those things are only due to radioactive contamination or something else." He had a reason for rejecting the very essence of the thing that was important. So I wrote all this down in my letter and I got no response, no encouragement. For a long time Davis wouldn't have anything to do with it. He went to Europe for a six months' leave of absence, came back later and I took up the matter with him again.¹

In the meantime, I sent Bohr a copy of the letter that I had written to Davis, asking [Bohr] to hold it confidential but to pass it on to various people who would be trying to repeat these experiments—to Professor Sommerfeld and other people. It headed off a lot of experimental work that would have gone on. And from that time on, nobody ever made another experiment except one man in England who didn't know about the letter that I had written to Bohr.² And he was not able to confirm any of it. Well, a year and a half later, in 1931, there was just a short little article in the *Physical Review* in which [Davis and Barnes] said that they hadn't been able to reproduce the effect.³ "The results reported in the earlier paper depended upon observations made by counting scintillations visually. The scintillations produced by alpha particles on a zinc

sulfide screen are a threshold phenomenon. It is possible that the number of counts may be influenced by external suggestion or autosuggestion to the observer." And later in that paper they said that they had not been able to check any of the older data. And they didn't even say that the tube was gassy.

To me, [it's] extremely interesting that men, perfectly honest, enthusiastic over their work, can so completely fool themselves. Now what was it about that work that made it so easy for them to do that? Well, I began thinking of other things. I had seen R. W. Wood and told him about this phenomenon because he's a good experimenter and doesn't make such mistakes himself very often—if at all. [Wood was a physicist from Johns Hopkins University.] And he told me about the N rays that he had an experience with back in 1904. So I looked up the data on the N rays.⁴

N rays

In 1903, Blondlot, who was a well-thought-of French scientist and a member of the Academy of Sciences, was experimenting with x rays, as almost everybody was in those days. [René-Prospér Blondlot, shown in the photo on page 42, was a physicist at the University of Nancy.] The effect that he observed was something of this sort. I won't give the whole of it; I'll just give a few outstanding points. He found that if you have a hot wire, a platinum wire or a Nernst filament, or anything that's heated very hot inside an iron tube, and you have a window cut in it and you have a piece of aluminum about $\frac{1}{8}$ of an inch thick on it, some rays come out through that aluminum window. Oh, [the window] can be as much as 2 or 3 inches thick and [these rays can still] go through aluminum but not through iron. The rays that come out of this little window fall on an . . . object [that is barely illuminated by a light source], so that you can just barely see it. You must sit in a dark room for a long time. [Blondlot] used a calcium sulfide screen which could be illuminated with light and gave out a very faint glow which could be seen in a dark room. Or he used a source of light from a lamp shining through a pinhole and maybe through another pinhole so as to get a

faint light on a white surface that was just barely visible.

Now he found that if you turn this lamp on so that these rays that came out of the little aluminum slit fell on the piece of paper that you were looking at, you could see [the paper] much better. Oh, *much* better! And therefore you could tell whether the rays would go through or not. [Blondlot] said later that a great deal of skill is needed. He said you mustn't ever look at the source. You shouldn't look directly at it. He said that would tire your eyes. Look away from it and, he said, pretty soon you'll see [the piece of paper], or you won't see it, depending on whether the N rays are shining on [it]. In that way, you can detect whether or not the N rays are acting. [See the figure on page 43.]

Well, he found that N rays could be stored up in things. For example, you could take a brick. He found that N rays would go through black paper and would go through aluminum. So he took some black paper, wrapped a brick up in it and put it out in the street and let the sun shine through the black paper into the brick. Then he found that the brick would store N rays and give off the N rays even with the black paper on it. [You could] bring [the brick] into the laboratory and hold it near the piece of paper that you were looking at—faintly illuminated—and you could see [the paper] much more accurately. Much better, if the N rays are there, but not if [the brick was] too far away. . . .

Well, you'd think he'd make such experiments as this—to see if with ten bricks he got a stronger effect than he did with one. No, not at all. He didn't get any stronger effect. It didn't do any good to increase the intensity of the light. You had to depend upon whether you could see it or whether you couldn't see it. And there, the N rays were very important.

Now, a little later, [Blondlot] found that many kinds of things gave off N rays. A human being gave off N rays, for example. If someone else came into the room then you probably could see [the faintly illuminated paper]. He also found that if someone made a loud noise that would spoil the effect. You had to be silent. Heat, however, increased the effect—radiant heat. Yet heat itself wasn't the same thing as N rays. N rays were not heat because heat wouldn't go through aluminum. Now he found a very interesting thing was that if you took the brick that was giving off N rays and held it close to your head, it went through your skull and it allowed you to see the paper better. Or you could hold the brick near the paper; that was all right too.

Now he found that there were some other things that were like negative N rays. He called them N' rays. The effect of the N' rays was to decrease the visibility of a faintly illuminated slit. That worked too, but only if the angle of incidence was right. If you look at [the paper] tangentially you found that the [N' rays] increased the intensity. [The intensity] decreased if you looked at [the paper] normally and it increased if you looked at it tangentially. All of which was very interesting. And he published many papers on it—one right after the other. Other people did too, confirming Blondlot's results. There were lots of papers published, and at one time about half of them were

confirming the results of Blondlot. You see, N rays ought to be important because x rays were known to be important, and alpha rays were, and N rays were somewhere in between, so N rays must be very important.

Enter R. W. Wood

Well, R. W. Wood heard about these experiments—everybody did, more or less. So R. W. Wood went over there. At that time Blondlot had a prism, quite a large prism of aluminum, with a 60° angle. [He also] had a Nernst filament with a little slit about 2 mm wide. There were two slits, 2 mm wide each. The beam [from the filament] fell on the prism and was refracted and he measured the refractive index. . . . He found that it wasn't monochromatic, that there were several different components to the N rays. . . . He could measure three or four different refractive indices, each to two or three significant figures, and he was repeating some of these and showing how accurately repeatable they were, showing it to R. W. Wood in this dark room.

Well, after this had gone on for quite a while and Wood found that he was checking these results very



René Blondlot in the robes of the French Academy of Sciences. The French physicist believed he had found a new form of radiation, which he named N rays to honor the University of Nancy, where he worked. (Photo courtesy of Irving Klotz, Northwestern University; and the French Academy of Sciences.)

accurately, measuring the position [of the beam on] the little piece of paper within a tenth of a millimeter, although the slits were 2 mm wide. Wood asked him about that. He said: "How? How can you, from just the optics of the thing, with slits 2 mm wide, how can you get a beam so fine that you can detect its position to within a tenth of a millimeter?"

Blondlot said: "That's one of the fascinating things about the N rays. They don't follow the ordinary laws of science that you ordinarily think of." He said: "You have to consider these things all by themselves. They are very interesting but you have to discover the laws that govern them."

Well, Wood asked him to repeat some of these measurements, which he was only too glad to do. But in the meantime, the room being very dark, R. W. Wood put the prism in his pocket and the results checked perfectly with what [Blondlot] had before. Well, Wood rather cruelly published that.⁵ And that was the end of Blondlot.

Nobody accounts for the methods by which [Blondlot] could reproduce those results to a tenth of a millimeter. Wood said that he seemed to be able to do it but nobody understands that. Nobody understands lots of things. But some of the Germans—Pringsheim was one of them—came out later with an extremely interesting story. They had tried to repeat some of Blondlot's experiments and had found this: One of the experiments was to have a very faint source of light on a screen of paper. To make sure that you are seeing the screen of paper you hold your hand up and move it back and forth. And if you can see your hand move back and forth then you know it is illuminated. One of the experiments that Blondlot made was that the [illumination] was made much better if you had some N rays falling on the piece of paper. Pringsheim was repeating these in Germany and he found that if you didn't know where the paper was, whether it was in front of or behind your hand, [the experiment] worked just as well. That is, you could see your hand just as well if you held it in back of the paper as if you held it in front of it. Which is the natural thing, because this is a threshold phenomenon. And a threshold phenomenon means that you don't know, *you really don't know*, whether you are seeing it or not. But if you have your hand there, well, of course, you see your hand because you *know* your hand's there and that's just enough to win you over to where you know that you see it. But you know it just as well if the paper happens to be in front of your hand instead of in back of your hand, because you don't know where the paper is but you *do* know where your hand is.

Mitogenetic rays

Well, let's go on. About 1923 there was a whole series of papers by Gurwitsch and others. [Alexander Gurwitsch was a professor at the First State University of Moscow.] There were hundreds of them published on mitogenetic rays.⁶ There are still a few of them being published. I don't know how many of you have ever heard of mitogenetic rays. They are rays that are given off by growing plants, living things, and they were proved, according to Gurwitsch, to be something that would go



N rays could enhance illumination of a dimly lit object, or so Blondlot claimed. Among other evidence, he published the above photogravure, made by exposing film to a small luminous spark. The darker exposure (right) was made in the presence of N rays, mysterious radiation emanating from a hot wire. (From *Comptes Rendus*, 22 February 1904.)

through quartz but not through glass. They seemed to be some sort of ultraviolet light.

The way they studied these [rays] was this: You had some onion roots—onions growing in the dark or in the light—and the roots would grow straight down. Now if you had another onion root nearby, and this [first] onion root was growing down through a tube or something, going straight down, and another onion root came nearby, [the first onion root] would develop so that there were more cells on one side than the other. One of the tests they had made at first was that the root would bend away. And as it grew it would change in direction, which was evidence that something had traveled from one onion root to the other. And if you had a piece of quartz in between [the change would occur], but if you put glass in between it wouldn't. So this radiation would not go through glass but it would go through quartz. [See the figure on page 45.]

Well, it started that way. Then everything gave off mitogenetic rays—anything that remotely had anything to do with living things. And then they started to use photoelectric cells to check it, and whatever they did they practically always found that if you got the conditions just right, you could *just* detect [mitogenetic rays] and prove it. But if you looked over those photographic plates that showed this ultraviolet light you found that the amount of light was not much bigger than the natural particles of the photographic plate so that people could have different opinions as to whether it did or did not show this effect. The result was that less than half of the people who tried to repeat these experiments got any confirmation of it, and so it went. Well, I'll go on. . .

Symptoms of sick science

The Davis-Barnes experiment and the N rays and the mitogenetic rays all have things in common. These are cases where there is no dishonesty involved but where people are tricked into false results by a lack of understanding about what human beings can do to themselves in the way of being led astray by subjective effects, wishful thinking or threshold interactions. These are examples of pathological science. These are things that attracted a

Symptoms of Pathological Science

▷ The maximum effect that is observed is produced by a causative agent of barely detectable intensity, and the magnitude of the effect is substantially independent of the intensity of the cause.

▷ The effect is of a magnitude that remains close to the limit of detectability or, many measurements are necessary because of the very low statistical significance of the results.

▷ There are claims of great accuracy.

▷ Fantastic theories contrary to experience are suggested.

▷ Criticisms are met by *ad hoc* excuses thought up on the spur of the moment.

▷ The ratio of supporters to critics rises up to somewhere near 50% and then falls gradually to oblivion.

great deal of attention. Usually hundreds of papers have been published on them. Sometimes they have lasted for 15 or 20 years and then they gradually have died away.

Now here are the characteristic rules [see the box above]:

▷ The maximum effect that is observed is produced by a causative agent of barely detectable intensity. For example, you might think that if one onion root would affect another due to ultraviolet light then by putting on an ultraviolet source of light you could get it to work better. Oh no! Oh no! It had to be just the amount of intensity that's given off by an onion root. Ten onion roots wouldn't do any better than one and it didn't make any difference about the distance of the source. It didn't follow any inverse square law or anything as simple as that. And so on. In other words, the effect is independent of the intensity of the cause. That was true in the mitogenetic rays and it was true in the N rays. Ten bricks didn't have any more effect than one. It had to be of low intensity. We know why it had to be of low intensity: so that you could fool yourself so easily. Otherwise, it wouldn't work. Davis-Barnes worked just as well when the filament was turned off. They counted scintillations.

▷ Another characteristic thing about them all is that these observations are near the threshold of visibility of the eyes. Any other sense, I suppose, would work as well. Or many measurements are necessary—many measurements—because of the very low statistical significance of the results. With the mitogenetic rays particularly, [people] started out by seeing something that was bent. Later on, they would take a hundred onion roots and expose them to something, and they would get the average position of all of them to see whether the average had been affected a little bit. . . . Statistical measurements of a very small effect . . . were thought to be significant if you took large numbers. Now the trouble with that is this. [Most people have a habit, when taking] measurements of low significance, [of finding] a means of rejecting data. They are right at the threshold value and there are many reasons why [they] can discard data. Davis and Barnes were doing that right along. If things were doubtful at all, why, they would discard them or not discard them depending on whether or not they fit the theory. They didn't know that, but that's the way it worked out.

▷ There are claims of great accuracy. Barnes was going to get the Rydberg constant more accurately than the spectroscopists could. Great sensitivity or great specificity—we'll come across that particularly in the Allison effect.

▷ Fantastic theories contrary to experience. In the Bohr theory, the whole idea of an electron being captured by an

alpha particle when the alpha particles aren't there, just because the waves are there, [isn't] a very sensible theory.

▷ Criticisms are met by *ad hoc* excuses thought up on the spur of the moment. They always had an answer—always.

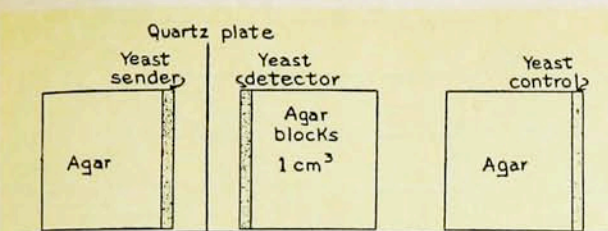
▷ The ratio of the supporters to the critics rises up somewhere near 50% and then falls gradually to oblivion. The critics couldn't reproduce the effects. Only the supporters could do that. In the end, nothing was salvaged. Why should there be? There isn't anything there. There never was. That's characteristic of the effect. Well, I'll go quickly on to some of the other things.

Allison and isotopes

The Allison effect is one of the most extraordinary of all.⁷ It started in 1927. There were hundreds of papers published in [journals such as] the *Physical Review*, the *Journal of the American Chemical Society*—hundreds of papers. Why, they discovered five or six different elements that were listed in the Discoveries of the Year. There were new elements discovered—alabamine, virginium. A whole series of elements and isotopes were discovered by Allison. [Fred Allison was at the Alabama Polytechnic Institute.]

The effect was very simple. There is the Faraday effect, by which a beam of polarized light [is rotated when it passes] through a liquid which is in a magnetic field. The plane of polarization is rotated by a longitudinal magnetic field. Now that idea has been known for a long time and it has a great deal of importance in connection with light shutters. At any rate, you can let light through or not depending upon the magnetic field. Now the experiment of Allison's was this. [See the figure on page 47.] They had a glass cell and a coil of wire around it and [they had] wires coming up here, a Lecher system. [There was] a spark gap so that a flash of light came through [the lens] and went through one Nicol prism and then another one. You would adjust [the second Nicol's prism] with a liquid like water or carbon disulfide or something like that in the cell so that there was a steady light. If you had a beam of light and you polarized it and then you turned on a magnetic field, why you see that you could rotate the plane of polarization. There would be an increase in the brightness of the light when you put [on] a magnetic field. magnetic field.

Now they wanted to find the time delay, how long it takes [for the Faraday effect to occur]. So they had a spark, and the same field that produced the spark induced a current through the coil. By sliding this wire along the trolley of the Lecher system, they could cause a compensating delay [in the second cell, where the field was



Detection of mitogenetic rays. These rays were believed to be a form of ultraviolet radiation, emitted by biological materials, that influenced the growth of other biological materials. The experiment shown here was one way to test whether mitogenetic rays from the yeast on one block of agar would go through the quartz and affect yeast on the facing block. (From ref. 6.)

reversed]. The sensitivity of this thing was so great that they could detect differences of about 3×10^{-10} seconds. By looking in they could see these flashes of light, the light from the sparks, and they tried to decide as they changed the position of this trolley whether it got brighter or dimmer. They set it for a minimum and measured the position of the trolley. They put in this [second] glass tube a water solution and added some salt to it. And they found that the time lag was changed. . . . They got a change in the time lag depending upon the presence of salts.

Now they first found—very quickly—that if you put in a thing like ethyl alcohol you got one characteristic time lag, and with acetic acid another one that was quite different. But if you had ethyl acetate you got the sum of the two. You got two peaks. So that you could analyze ethyl acetate and find the acetic acid and the ethyl alcohol. Then they began to study salt solutions and they found that only the metal elements counted, but they didn't act as ions. That is, all potassium ions weren't the same, but potassium nitrate and potassium chloride and potassium sulfate all had quite characteristic different points, which were a characteristic of the compound. It was only the positive ion that counted and yet the negative ions had a modifying effect. But you couldn't detect the negative ions directly.

Now they began to see how sensitive it was. Well, they found that any [concentration of] more than about a 10^{-8} molar solution would always produce the maximum effect. You'd think that that would be kind of discouraging from the analytical point of view, but no, not at all. And you could make quantitative measurements to about three significant figures by diluting the solutions down to a point where the effect disappeared. Apparently, it disappeared quite sharply when you got down to about 10^{-8} or 3.42×10^{-8} in concentration, or something of that sort. . . . Otherwise you would get it so that you could detect the limit within this extraordinary degree of accuracy.

Well, they found that things were entirely different—even in these very dilute solutions—in sodium nitrate from what it was with sodium chloride. Nevertheless, it was a characteristic which depended upon the compound, even though the compound was dissociated into ions at those concentrations. That didn't make any difference, but it was fact that was experimentally proven. They then went on to find that the isotopes all stick right out like sore thumbs with great regularity. In the case of lead, they found 16 isotopes. These isotopes were quite regularly spaced so that you could get 16 different positions and you could assign numbers to those so that you could identify them and tell which they were. Unfortunately, you

couldn't get the concentrations quantitatively; even the dilution method didn't work quite right because [the isotopes] weren't all equally sensitive. You could get them relatively but only approximately. Well, [this effect] became important as a means of detecting elements that hadn't yet been discovered, like alabamine and elements that are now known and filling out the periodic table. All the elements in the periodic table were filled out that way and published.

But a little later, in 1945 or '46, I was at the University of California. Owen Latimer who is now head of the chemistry department there—not Owen Latimer, Wendell Latimer—had had a bet with G. N. Lewis in 1932. [*Gilbert Lewis was also a chemist at Berkeley.*] He said: "There's something funny about this Allison effect, how they can detect isotopes." He had known somebody who had been down with Allison and who had been very much impressed by the effect, and he said to Lewis: "I think I'll go down and see Allison, to Alabama, and see what there is in it. I'd like to use some of these methods."

Now people had begun to talk about spectroscopic evidence that there might be traces of hydrogen of atomic weight 3. It wasn't spoken of as tritium at that time but as hydrogen of atomic weight 3 that might exist in small amounts. There was a little spectroscopic evidence for it, and Latimer said: "Well, this might be a way of finding it. I'd like to be able to find it." So he went and spent three weeks at Alabama with Allison. Before he went he talked over with G. N. Lewis what he thought the prospects were, and Lewis said, "I'll bet you ten dollars you'll find that there's nothing in it." And so they had this bet. Latimer went down there and he came back. He set up the apparatus and made it work so well that G. N. Lewis paid him the ten dollars. He then discovered tritium and he published an article in the *Physical Review*⁸—just a little short note saying that using Allison's method he had detected the isotope of hydrogen of atomic weight 3. And he made some sort of estimate as to its concentration. to its concentration.

Well, nothing more was heard about it. I saw [Latimer] then, 7 or 8 years after that. I had written these things up before, about this Allison effect, and I told him about [my observations concerning pathological science] and how the Allison effect fit all these characteristics. Well, I know that at that time at one of the meetings of the American Chemical Society there was great discussion as to whether to accept papers on the Allison effect. There they decided, no, they would not accept any more papers on the Allison effect, and I guess the *Physical Review* did too. At any rate, the American Chemical Society decided

that they would not accept any more manuscripts on the Allison effect. However, after they had adopted that as a firm policy, they did accept one more a year or two later because here was a case where all the people in the faculty had [made up] twenty or thirty different solutions. . . . They had labeled them all secretly and they had taken every precaution to make sure that nobody knew what was in these solutions, and they had given them to Allison. He had used his method on them and he had gotten them all right, although many of them were at concentrations on the order of 10^{-6} molar. That was sufficiently definite—good experimental methods—and it was accepted for publication by the American Chemical Society, but that was the last.⁹ You'd think that would be the beginning, not the end.

Anyway, Latimer said: "You know, I don't know what was wrong with me at that time. After I published that paper I never could repeat the experiments again. I haven't the least idea why. But," he said, "those results were wonderful. I showed them to G. N. Lewis and we both agreed that it was all right. They were clean-cut. I checked them myself every way I knew how to. I don't know what else I could have done, but later on I just couldn't ever do it again."

I don't know what it is. That's the kind of thing that happens in all of these [cases]. All the people who had anything to do with these things find that when [they] get through with them, [they can't account for them]. You can't account for Bergen Davis saying that they didn't calculate those things from the Bohr theory, that they were found by empirical methods without any idea of the theory. Barnes made the experiments and brought them in to Davis, and Davis calculated them up and discovered all of a sudden that they fit the Bohr theory. He said Barnes didn't have anything to do with that. Well, take it or leave it, how did he do it? It's up to you to decide. I can't account for it. All I know is that there was nothing salvaged at the end, and therefore none of it was ever right and Barnes never did see a peak. You can't have a thing halfway right.

Extrasensory perception

Well, there's Rhine. [*Joseph B. Rhine was a parapsychologist then at Duke University.*] I spent a day with Rhine at Duke University at the meeting of the American Chemical Society, probably about 1934. Rhine had published a book and I'll just tell you a few things. First of all, I went in and told Rhine . . . the whole story [I have just told you]. I said these [traits of pathological science] are the characteristics of those things that aren't so. They are all characteristics

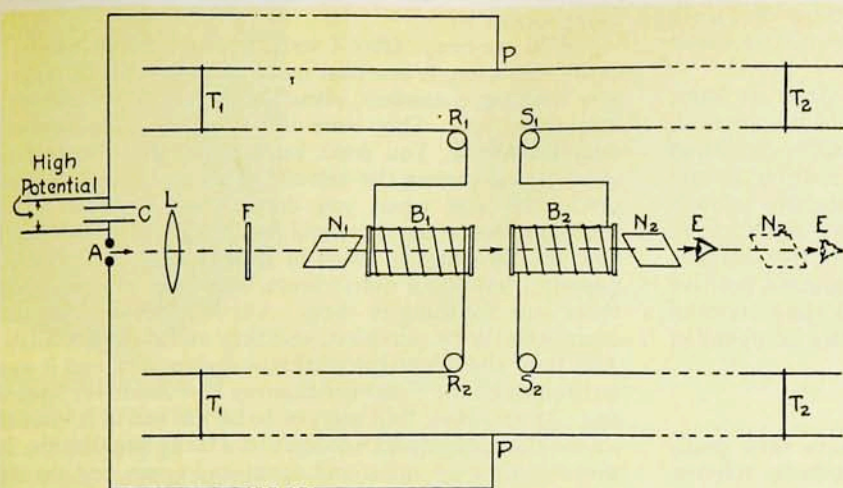
of your thing too. He said: "I wish you'd publish that. I'd love to have you publish it. That would stir up an awful lot of interest." He said: "I'd have more graduate students. We ought to have more graduate students. This thing is so important that we should have more people realize its importance. This should be one of the biggest departments in the university."

Well, I won't tell you the whole story with Rhine because I talked with him all day. He uses cards which you guess at [before they are turned] over. You have extrasensory perception. You have 25 cards and you deal them out face down, or one person looks at them . . . on the other side of a screen . . . and you read his mind. The other thing is for nobody to know what the cards are, in which case they are turned over without anybody looking at them. You record them and then you look them up and see if they check, and that's telepathy, or clairvoyance rather. Telepathy is when you can read another person's mind.

Now a later form of the thing is for you to decide now and write down what the cards are going to be when they are shuffled tomorrow. That works too.

All of these things are nice examples in which the magnitude of the effect is entirely independent of magnitude of the cause. That is, the experiments work just as well when the shuffling is to be done tomorrow as when it was done some time ago. It doesn't make any difference in the results. There is no appreciable difference between clairvoyance and telepathy, although if you try to think of mechanisms for the two, it should be quite different. [It's rather difficult to think of a mechanism] . . . to get the cards to telegraph you all the information that's in them as to how they are arranged and so on, when they are stacked up on top of each other, and to have it given in the right sequence. On the other hand, it is conceivable that there may be some sort of mechanism in the brain that might send out some sort of unknown messages that could be picked up by some other brain. That's a different order of magnitude—a different order of difficulty. But they were all the same from Rhine's point of view.

Well, now, [I've told you a few of the things that I know about Rhine]. There are many more I could [tell] you. Rhine, being in quite a philosophical mood, said "It's funny how the mind tries to trick you." He said: "People don't like these experiments. I've had millions of these cases where the average is about 7 out of 25." You'd expect 5 out of 25 to come out right by chance and on the grand average they come out, oh, out of millions or hundreds of millions of cases, they average around 7. Well, to get 7 out of 25 would be a common enough occurrence but if you take a large number and you get 7,



Apparatus for studying the "Allison effect." Cells B1 and B2 were filled with liquid. When a current flowed through the coils of wires surrounding these cells (from R1 to R2 and from S1 to S2), the resulting magnetic field rotated the plane of polarization of any light passing through the cells, because of the Faraday effect. Discharge of the capacitor C produced a flash of light from the spark gap at A and, at the same time, sent current through the coils. The light was focused by lens L and polarized by the Nicol prism N1. Cell B1 rotated the plane of polarization, and cell B2 was moved until it produced an exactly compensating rotation: At that point the time for light to travel between the cells just equaled the time lag in the Faraday effect in the two cells. Allison claimed that the time lag, as measured by the position of cell B2, was a unique characteristic of each isotope present in solution in the cells. This effect became a way of identifying new isotopes and elements—until it was recognized as pathological science. (From ref. 7.)

well you doubt the statistics or the statistical application or, above all, what I think of, and what I want to give you reasons for thinking, is the rejection of a small percentage of the data.

Before I get into what Rhine said, [I'll first] say this [about] David Langmuir, a nephew of mine, who was in the Atomic Energy Commission. When he was with the Radio Corporation of America a few years ago, he and a group of other young men thought they would like to check Rhine's work. So they got some cards and they spent many evenings together finding how these cards turned up and they got well above 5. They began to get quite excited about it and they kept on, and they kept on, and they were right on the point of writing Rhine about the thing. And they kept on a little longer and things began to fall off, and fall off a little more, and they fell off a little more. And after many, many, many days, they fell down to an average of 5—grand average—so they didn't write to Rhine. Now if Rhine had received that information, that this reputable body of men had gone ahead and gotten a value of 8 or 9 or 10 after so many trials, why, he would have put it in his book. How much of that sort of thing [goes on]? When you are fed information of that sort by people who are interested—how are you going to weigh the things that are published in the book?

Now, an illustration of how it works is this. [Rhine] told me: "People don't like me." He said: "I took a lot of cards and sealed them up in envelopes and I put a code number on the outside and I didn't trust anybody to know that code. Nobody!"

[A section of the speech is missing at this point. It evidently described some tests that gave scores below 5.] "...the

idea of having this thing sealed up in the cards as though I didn't trust them and therefore to spite me they made it purposely low."

"Well," I said, "that's interesting—very interesting, because you said that you'd published a summary of *all* the data that you had. And it comes out to be 7. It is now within your power to take a larger percentage, including those cards that are sealed up in those envelopes, which could bring the whole thing back down to 5. Would you do that?"

"Of course not," he said. "That would be dishonest."

"Why would it be dishonest? The low scores are just as significant as the high ones, aren't they?"

"They proved that there's something there just as much, and therefore it wouldn't be fair."

I said: "Are you going to count them? Are you going to reverse the sign and count them or count them as credits?"

"No, no," he said.

I said: "What have you done with them? Are they in your book?"

"No."

"Why, I thought you said that all your values were in your book. Why haven't you put those in?"

"Well," he said, "I haven't had time to work them up."

"Well, you know all the results. You told me the results."

"Well," he said, "I don't give the results out until I've had time to digest them."

I said: "How many of these things have you?"

He showed me filing cabinets—a whole row of them. Maybe hundreds of thousands of cards. He had a filing

cabinet that contained nothing but these things that were done in sealed-up envelopes. And they were the ones that gave the average of 5.

Well, we'll let it stand at that. A year or so later, [Rhine] published a new volume of his book. In that, there's a chapter on the sealed-up cards in the envelopes and they all come up to around 7. And nothing is said about the fact that for a long time they came down below 5. You see, [Rhine] knows, if they come below 5, he knows that it isn't fair to the public to misrepresent this thing by including those things that prove just as much a positive result as though they came out above [5]. It's just a trick of the mind that these people did to try to spite [him] and of course it wouldn't be fair to publish.¹⁰

Flying saucers

I'm not going to talk about flying saucers very much except just this. A flying saucer is not exactly science, although some scientific people have written things about them. I was a member of General Schwartz's [*This name is uncertain.*] advisory committee after the war, and we held some very secret meetings in Washington in which there was a thing called Project SIGN. I think it was s-i-g-n. Anyway, it was hushed up. It was hardly even talked about. It was the flying saucer stuff, concerned with gathering the evidence and weighing and evaluating the data on flying saucers. And [General Schwartz] said: "You know, it's very serious. It really looks as though there is something there."

Well, afterwards I told him this story here. I said that it seems to me from what I know about flying saucers they look like [pathological science]. Well, anyway, it ended up by two men being brought to Schenectady with a boiled down group of about 20 or 30 best cases from hundreds and hundreds that they knew all about. I didn't want them all. I said to pick out about 30 or 40 of the best cases, and bring them to Schenectady. [I promised that we would] spend a couple of days going over them. . . .

Most of them were Venus seen in the evening through a murky atmosphere. Venus can be seen in the middle of the day if you know where to look for it—almost any clear bright day, especially when Venus is at its brightest—and sometimes it has almost caused panic. [There has been] traffic congestion in New York City when Venus is seen in the evening near some of the buildings around Times Square, and people thought it was a comet about to collide with the Earth, or somebody from Mars, or something of that sort. That was a long time ago. That was 30 or 40 years ago. Venus still causes flying saucers.

Well, they only had one photograph or two photographs, taken by one man, [that puzzled me]. It looked to me like a piece of tar paper when I first saw it, and the two photographs showed the thing in entirely different shapes. I asked for more details about it. What was the weather at the time? Well, they didn't know but they'd look it up. And they got out some papers and there it was. It was taken about 15 or 20 minutes after a violent thunderstorm out in Ohio. Well, what's more natural than some piece of tar paper picked up by a little miniature twister and being carried a few thousand feet up into the clouds? And it was coming down, that's all. So what could it be? "But it was going at an enormous speed." Of course the man who saw it didn't have the vaguest idea how far away it was.

That's the trouble. If you see something that's up in the sky, a light or any kind of an object, you haven't the vaguest idea of how big it is. You can guess anything you like about the speed. You ask people how big the Moon is. Some say it is as big as your fist, or as big as a baseball. Some say as big as a house. Well, how big is it really? You can't tell by looking at it. How can you tell how big a

flying saucer is?

Well, anyway, after I went through these [cases], I didn't find a single one that made any sense at all. There was nothing consistent about them. They all suffered from these facts: They were all subjective. They were all near threshold. You don't know what the threshold is exactly in detecting the velocity of an object that you see up in the sky, when you don't know whether it's a thousand feet or ten thousand feet or a hundred thousand feet up. But they all fitted in with this general pattern, namely, that there didn't seem to be any evidence that there was anything in them. Anyway, the men [on the committee] were convinced and they ended Project SIGN. And later the whole [project] was declassified, and it was written up by the *Saturday Evening Post* about 4 or 5 years ago. At any rate, that seemed to be the end of it. But of course the newspapers wouldn't let a thing like that die. It keeps coming up again and again and again, and the old story keeps coming back again. It always has. It's probably hundreds of years old anyway.

Epilogue (R. N. Hall)

Pathological science is by no means a thing of the past. The search for some record of Langmuir's lecture began in 1965 out of curiosity about two phenomena—the photo-mechanical and electromechanical effects—that were being reported with increasing frequency in papers from a number of laboratories around the world. The experiments that were described in these publications conformed to the first 5 characteristic symptoms of pathological science precisely as Langmuir had outlined them. Further work¹¹ disclosed the subjective nature of these observations, and helped to bring this field of investigation to its final stage—the decline toward oblivion. Many readers will recall subsequent examples of phenomena that exhibit some of the characteristics of pathological science listed by Langmuir and that may be of a similar nature.

References

1. Eight months after the visit of Langmuir and Hewlett to Columbia and this exchange of letters, Barnes published a paper on the Davis-Barnes effect in *Phys. Rev.* **35**, 217 (1930).
2. H. C. Webster, *Nature* **126**, 352 (1930).
3. B. Davis, A. H. Barnes, *Phys. Rev.* **37**, 1368 (1931).
4. R. Blondlot, *The N-Rays*, Longmans, Green, London (1905). J. G. McKendrick, *Nature* **72**, 195 (1905).
5. R. W. Wood, *Nature* **70** (1904); *Phys. Z.* **5**, 789 (1904). W. Seabrook, *Doctor Wood*, Harcourt Brace, New York (1941), ch. 17.
6. A. Hollaender, W. D. Claus, *J. Opt. Soc. Am.* **25**, 270 (1935).
7. F. Allison, E. S. Murphy, *J. Am. Chem. Soc.* **52**, 3796 (1930). F. Allison, *Ind. Eng. Chem.* **4**, 9 (1932). S. S. Cooper, T. R. Ball, *J. Chem. Ed.* **13**, 210 (1936), also pp. 278, 326. M. A. Jeppesen, R. M. Bell, *Phys. Rev.* **47**, 546 (1935). H. F. Mildrum, B. M. Schmidt, Air Force Aero Prop. Lab. report AFAPL-TR-66-52 (May 1966).
8. W. M. Latimer, H. A. Young, *Phys. Rev.* **44**, 690 (1933).
9. Langmuir may have been referring to the paper by J. L. McGhee, M. Lawrentz, *J. Am. Chem. Soc.* **54**, 405 (1932), which contains the statement "In December 1930 one of us (McGhee) handed out by number to Prof. Allison twelve (to him) unknowns which were tested by him and checked by two assistants 100 percent correctly in three hours." See also T. R. Ball, *Phys. Rev.* **47**, 548 (1935), which describes additional tests in which unknowns were identified.
10. See, for example, G. R. Price, *Science* **122**, 359 (1955) and replies on 6 Jan. 1956. M. Gardner, *Fads and Fallacies in the Name of Science*, Dover (1957).
11. R. E. Hannemann, P. J. Jorgenson, *J. Appl. Phys.* **38**, 4099 (1967); R. N. Hall, *Proc. 9th Intl. Conf. Physics of Semiconductors*, Moscow (1968), p. 481.