

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

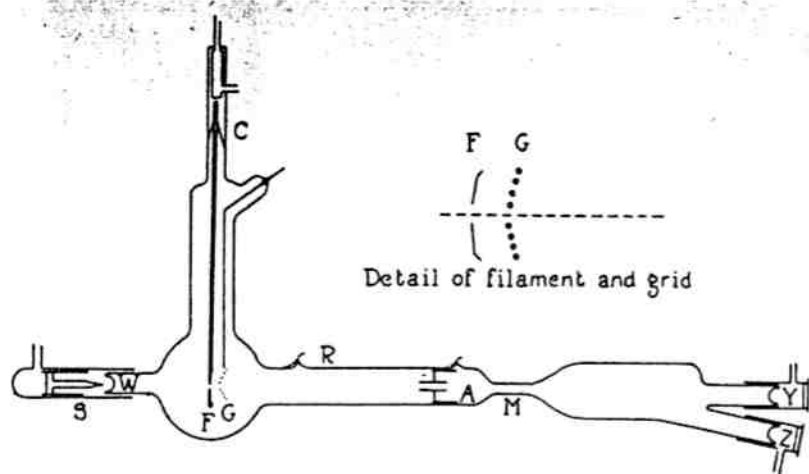
it was going to have after it settled down into the orbit.

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 10^6 . 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

