

I'm putting quite a lot of stuff into this set of reels that has really nothing much to do with the book. I haven't any objection to getting some of it on record, of course, but it begins to make me wonder how far this book is tending toward a biography and you know my phobia on biographies. On the other hand, there are still a number of things that haven't been treated that may be quite pertinent and I have in mind, for example, the events after the war such as the famous cabinet meeting and the Attlee Conference that had to do with the handling of the atomic energy matters.

[X-REF BACK TO P. 503 AND AHEAD TO PP. 728-730]

While I'm making general comments, the last New York Times Magazine [AM NOTES: EARLY MAY 1965] had an article by Ralph Lapp on the beginnings of atomic energy in which he blew up the Einstein letter. [X-REF BACK TO P. 125 AND AHEAD TO PP. 624-625, 794] As I've said before, I had no knowledge of this letter until the publicity about it came out quite a bit later. FDR never mentioned it to me. The simple fact, I think, is this:

[X-REF BACK TO PP. 123, 125 AND AHEAD TO P. 623]

At the time the letter was written there was no governmental action on atomic energy. On the other hand there was plenty of action all over the place by various groups in private laboratories. All this followed after the meeting at which the fission process became known to Americans. This needed no subsidy; it was going on all over the place. Everybody

1) EINSTEIN LETTER
ON ATOMIC ENERGY

in physics knew about it. A little group got together and said this is an opportunity, and got Einstein to write a letter. [EH to VB: THIS MUST BE LABELED A SURMISE, I THINK.] [VB to EH: OF COURSE] I don't think the letter had the slightest effect on the progress of the work. It went on in private laboratories, it was co-ordinated after a fashion by the Briggs' committee. It had no subsidy and when it began to get to the point where it needed subsidy and needed co-ordination, the Briggs' committee was turned over to NDRC and the thing went on from there.

I think it's probably not the first time that a group saw an opportunity to get in a bit of credit. [EH to VB: WE CAN'T SAY "A GROUP" UNLESS WILLING TO IDENTIFY.] [VB to EH: LOOK, I'M NOT ON THE STAND AS I DICTATE AND NONE OF THIS IS IN FINAL FORM FOR ANY USE.]

1) EINSTEIN LETTER
ON ATOMIC ENERGY

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Question B1: MacDowell

On Question B1. My relations with MacDowell were certainly very interesting ones. The first time I came across him was in the Navy Yard in New York where I was a Sub-inspector of engineering materials during the summer and he was the Materiel Officer. We got along very well and got quite well acquainted, probably because I didn't follow the usual pattern. [X-REF BACK TO PP. 21-23] For example, there was no smoking allowed in the building; hence after having eaten their box lunches all the clerks and sub-inspectors went outside to have a smoke. However, MacDowell smoked in his office and his office was just up above the big room where my desk was. Noticing this, I didn't go out with the crowd one day but sat at my own desk, put up my feet on the desk and lit my pipe. MacDowell walked down through, saw me there, made no comment and went on his way. So I continued the process. After a few days he stopped on the way in and chatted with me. In this way we began to get acquainted.

He was quite startled when I resigned, apparently. I told him that my plans were to go ahead and get a doctorate; he was quite enthusiastic about those plans and said he thought I was dead right to try it.

In connection with this, let me put in one point that's quite off this question. That is my experience with A. G. Webster at Clark University. I think this has got some

- 1) FLASHBACK--BUSH AS
SUB-INSPECTOR AT
NAVY YARD (Repeat)
- 2) Mac DOWELL
- 3) WEBSTER

interesting sidelights on the character of Webster that might be worthwhile having in the record, again with the reservation that we're not writing a biography. [EH to VB: THERE IS NO WEBSTER MATERIAL ON THE TAPE AT THIS POINT.] [VB to EH: IT'S QUITE A STORY, TOO LONG TO WRITE, BUT I'LL ATTACH A SHORT NOTE.] [Briefly, I got a \$1,500 scholarship and went to Clark University to study under A.G. Webster. I found him working on acoustics, in which I was not interested. I also found chaps studying under him unhappy for various reasons. So I pulled out, which was a blow, since I needed the \$1,500, and went to MIT (where I snared \$300.).

[A few years later AGW came to D.C. Jackson for advice as to how to conduct private consultation. I judge he needed income. DCJ talked to him, and then asked me to. I did. I don't think AGW ever found out I was the chap who got disillusioned at Clark and quit.

[A.G. Webster once made a monkey out of A. E. Kennelly -- quite cruelly. Webster was one of those chaps who, once eminent, saw the new physics pass him by. He later committed suicide.

[This is the outline, more if we really want it for anything.] [END WRITTEN ADDITION] [X-REF BACK TO 25, 31]

Now the second time I ran into MacDowell was during the First World War when I was working on antisubmarine devices and he was the secretary of the Navy's Antisubmarine Committee. I had no support from that committee nor did I

- 1) A. G. WEBSTER
- 2) MacDOWELL

have any support from the National Research Council. In fact, there was almost complete absence of a system by which civilian engineers and scientists were brought into the development of weapons. [X-REF BACK TO MILLIKAN & HIS NRC COMMITTEE, PP. 26, 27]

I got my support from Amrad and thus from J. P. Morgan and I got it for two reasons: one, Morgan was anxious to help out no doubt; and second, Amrad had an idea, possibly with my aid, that the thing that would detect submarines during the war would also be useful for finding things underground after the war. Indeed this happened after the war, but only in the connection with minesweeping detectors, mine detectors for land use. The actual thing that I worked on had no use for the finding of ores although at the time that I started work on it, I couldn't be sure but what it might have. [EH to VB: FOGGY. BAD TRANSCRIPT?]

However, that connection with MacDowell was merely that I heard from him what the Navy wanted and so forth and so on. The Navy did cooperate to the extent of making it possible for me to test the devices and so on, most of which was done at New London.

The third time that I ran into MacDowell was in connection with the telescope. [EH to VB: HALE?] Before I joined the Carnegie Institution in Washington, while I was still at MIT, the people at Pasadena who were developing the telescope asked me to come out and consult with them on the

- 1) FLASHBACK--WW I
- 2) MacDOWELL SECRETARY OF NAVY'S ANTISUB. COMMITTEE
- 3) BUSH WORKING ON ANTISUB DEVICES
- 4) SUPPORT FROM AMRAD AND J.P. MORGAN
- 5) TELESCOPE & MacDOWELL

controls -- that is, the devices that would hold the telescope on a star for a long period while a photograph was taken. I went out and spent quite a bit of time.

The man that I worked with principally was Max Mason on whom incidentally there's an excellent biography, a brief one, written by Warren Weaver and in the National Academy records, just published a short time ago. [EH to JK: REFERENCE WANTED. J.K. CHECK WITH VERA.] [VB to EH: I HAVE IT, IF YOU WANT IT. THE ACADEMY PUBLISHES A BOOK OF OBITUARIES FROM TIME TO TIME.] Max was a delightful chap and I worked with him and with the astronomers and so on for a few weeks. The principal thing I had difficulty with was finding out just what those astronomers wanted the telescope to do; what precision they needed and so on, and how far they wanted the whole thing to be automatic.

MacDowell was the engineer on this job so that I worked with him. There was a stress between MacDowell and the astronomers and the whole thing was none too smooth, I'm sure, but my job was sufficiently clear-cut. I ended up by giving them a design -- that is, a set of operating specifications as far as that's concerned and a list of equipment that would fill these, for their controls. Then they wanted a permanent man on the job and I recommended a former student of mine by the name of Poitras whom they hired. [EH to VB: NAME OK?] [VB to EH: YES] He went out to Pasadena and, as

- 1) FLASHBACK--1930's
- 2) BUSH & MacDOWELL
WORK TOGETHER ON
TELESCOPE IN
PASADENA
- 3) MAX MASON

a matter of fact, completely redesigned the affair so that the set of controls finally installed bore very little resemblance to the design I'd suggested. It was less elaborate and less precise, but it proved to be entirely adequate for their purposes.

The point is, I think, that those astronomers did not really want fully automatic controls because they never expected fully automatic controls to be good enough. Every time they take a long photograph some chap sits up there and keeps a reference star on a pair or cross hairs by manual control. However, the controls are satisfactory. Poitras had a strange career after that, but I don't need to go into that.

These are the three ways in which I got in touch with MacDowell. It's interesting that his path and mine crossed in these unusual ways.

- 1) FLASHBACK--1930's
- 2) BUSH & MacDOWELL
WORK TOGETHER ON
TELESCOPE IN
PASADENA
- 3) POITRAS

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Question B2: Tufts and Consulting at Amrad

Well, there's quite an interesting story about Amrad and I don't think it's ever been written. When I finished with my doctorate at MIT in 1916, I had two offers of jobs. One was from the Bell Telephone Laboratories and the other one was from Tufts College. The salaries were about the same -- and not very large. I intended to get married that fall and I didn't have enough income from either job to live on. [X-REF AHEAD TO P. 793] I took the job at Tufts, and I did it, I think, primarily because I thought I could supplement my income. So it turned out. I immediately went to work on doing a bit of consulting and the first outfit I ran into was Amrad. I remember the joy with which I picked up something I could do in my spare time, and earn twenty-five dollars a day at -- which was pretty good money according to me at that time. Of course it didn't stay at that level very long.

Amrad was a strange affair. It was run by a fellow named Harold Power who had been the wireless operator on J.P. Morgan's yacht: Morgan money was entirely behind it. [X-REF AHEAD TO P. 786] Power started in to make amateur radio equipment. Incidentally, I think his outfit out there in Medford was the first one to broadcast generally, in spite of all the story about KDKA in Pittsburgh. [EH NOTES: RESEARCH POINT] I think Amrad was ahead. They were selling radio equipment to amateurs and they began to put some

- 1) FLASHBACK--1916
- 2) BUSH FINISHES AT MIT & TAKES JOB AT TUFTS
- 3) AMRAD -- HAROLD POWER
- 4) VB's FIRST JOB AS CONSULTANT WITH AMRAD
- 5) J.P. MORGAN'S MONEY BEHIND AMRAD

music on the air so that the amateurs would have something to listen to. That was just about all there was to it.

[X-REF AHEAD TO P. 788]

The Amrad outfit was as strange a one as anyone could possibly be connected with. They called me in, I imagine, because they were making some transmitters for the Navy. The theory of those transmitters was not very well worked out anywhere; I tried to work it out and, I imagine, got little progress. But Harold Power was certainly no manager.

Now after a bit we started a laboratory in Medford and began to develop some new radio equipment. I was in charge of that laboratory. After a bit I hired a fellow by the name of C. G. Smith, who was a genius. He was a physicist with no graduate degree, and he was looking for a job. When he came in to see me, I told him about a puzzle I had on my mind at the time, of a way in which to control something. I've forgotten exactly what it was. To my surprise, the next morning he came in again with a very neat solution to the problem. So I hired him on the spot.

Now, he's still around; I've seen him recently.

[1964[±]] In his days with Amrad he was extremely ingenious. The most important thing he did was to produce a gas rectifier which was very much needed at the time when you had to have B batteries, and so forth, for your radio set. [X-REF AHEAD TO P. 789] This could make a B eliminator. It was a very

- 1) BUSH AT AMRAD
- 2) AMRAD EARLY BROADCASTER
- 3) AMRAD MADE TRANSMITTERS
- 4) BUSH HIRES C.G. SMITH

ingenious affair, the making of which involved some knowledge of gaseous conduction all right, and he was a jump ahead of General Electric and Westinghouse and all the rest in getting gas-filled tubes that were useful for control purposes and so on. There was another man there and I'll tell about him later, Al Spencer. [X-REF AHEAD TO PP. 561-563]

Now this tube had patents filed on it and so forth. It was nowhere near ready for production but Power kept insisting that we begin to get it out. Amrad went broke, and J. P. Morgan quit putting up the money. When this was about to occur (and it was quite obvious that it was going to) I proceeded to tell L. K. Marshall [X-REF BACK TO P. 12], who had been my roommate in college. He got together some of his friends that had been in his outfit in France during the First World War who were Boston businessmen and so forth -- quite a notable crowd in fact -- and they were all ready to form a little company to take over the rights on Smith's tube.

I had to stick around. For one thing my loyalty was necessarily to the young fellows I had under me at Amrad. I was not going to let the cracking up of Amrad, which seemed inevitable, put them into serious straits. So I was not originally a part of the outfit that Marshall formed. However, I told him about this tube, quite properly, because I was sure that Amrad was going to crack. I thought that if a new crowd picked the thing up it would take care of people

- 1) FLASHBACK--BUSH
AT AMRAD
- 2) BUSH HIRES C.G. SMITH
WHO PRODUCED
GAS RECTIFIER
- 3) AMRAD GOES BROKE

such as C. G. Smith, and so it did. [EH to VB: NOT CLEAR TO ME.] [VB to EH: I stayed with Amrad until it folded. The outfit which Marshall formed later became called Raytheon. I joined this new outfit as a consultant after Amrad's full demise. A number of lab men besides Smith also went with the new company.]

The story of that is very interesting because Amrad did break up and went into bankruptcy. The only assets were the patents that had come out of C. G. Smith's work, one or two of mine possibly, but C. G. Smith's were the principal ones. Marshall went down to New York and saw J. P. Morgan, said he understood Morgan held these assets and he'd like to buy them. Morgan said, "You mean cash?" "Yes sir." "What will you give me for them?" "Fifty thousand dollars," says Marshall. "They're yours," says J.P. and that ended the conversation. (His group incidentally had authorized him to go up to a hundred thousand.)

A few weeks later, Morgan's curiosity apparently got the best of him. He got Marshall to come down again and said, "I'm interested in what you're going to do with these patents of mine that you bought." Marshall told him -- whereupon he sold Morgan fifty thousand dollars worth of stock in the new company. This new company finally turned into the Raytheon Company, now the largest outfit of its kind in New England. [VB ADDS: largest employer in New England] The stock that J. P. Morgan bought at that time went down to his two sons, both of whom, Harry and Junius,

- 1) BUSH AT AMRAD
- 2) L.K. MARSHALL (VB's COLLEGE ROOMMATE) BUYS PATENTS FROM J.P. MORGAN
- 3) L.K. MARSHALL'S GROUP LATER BECOMES RAYTHEON
- 4) J.P. MORGAN BUYS STOCK IN MARSHALL'S VENTURE

eventually became great friends of mine.

I, of course, after Amrad cracked and after the show had been cleaned up, went in with Raytheon, the new outfit, but as a consultant, not as one of the original organizers. Out of that I received some fees for consulting, and very moderate ones, but more importantly, some stock interest in the company. This is the Amrad story as far as C. G. Smith and the tubes which formed the basis for the stock of Raytheon.

- 1) BUSH WORKS AS CONSULTANT TO RAYTHEON
- 2) AL SPENCER'S THERMOSTAT

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[EH NOTES: TIME FLASHBACK HERE] At the bench in Amrad was a chap named Al Spencer who had come down from Maine where he had had, I guess, only a part of a high school education. His father was killed in a mill accident when he was young and he'd been brought up by an uncle or something of the sort. He was a mechanic, but he was a doggoned ingenious one. [X-REF AHEAD TO P. 788] One day he told me about a little thing that he'd built (incidentally on his own time). It was just a thermostatic sheet which was dished a little bit and which would click through like the bottom of an oil can and of course being made of thermostatic metal, it would click through at a certain temperature. [EH to VB: WASN'T IT TWO METALS OF DIFFERENT COEFS. OF EXP., WELDED TOGETHER?] [VB to EH: To make thermostatic metal one welds, or silver solders, two blocks of metal, of different expansion coefficients, together, and then rolls them down to a

sheet of the desired thickness, annealing occasionally as it is done.

[Strips of scrap metal were then used. The dished sheet which would click, was new.

[Marshall made an important invention later. He used two metals which had widely different coefficients at the high operating temperature, but nearly identical coefficients at room temperature. Thus the device would not become overstressed in arriving at the operating temperature. This made the flatiron thermostat possible.] [END WRITTEN ADD]

I told him [Spencer] I thought it was a very interesting thing here and that I'd look into it a bit. So I went to the patent attorneys of the company, stated a hypothetical case and asked whether, under such circumstances, the company had any equity in the invention. They assured me that it did not, after which I felt perfectly free to tell Marshall and his crowd that Spencer had made what seemed to me to be a very interesting invention.

There was a little episode there that was interesting because Spencer, who had no business experience whatever, came pretty near selling the thing out for a song while Marshall and his group were getting around to doing something about it. I stopped him from doing it; in fact, told him if he wanted to sell it I'd give him more than he had been offered. But he didn't sell and pretty soon Marshall's group made him an offer and took him over. They founded a new company and gave him a good husky stock interest in it. This

- 1) FLASHBACK--
BUSH AT RAYTHEON
- 2) AL SPENCER's
THERMOSTAT
- 3) BUSH PREVENTS SPENCER
FROM SELLING HIS
INVENTION FOR A SONG
- 4) FOUNDING OF SPENCER
THERMOSTAT COMPANY

was the beginning of the Spencer Thermostat Company, which was later joined with General Plate Company of Attleboro to form Metals and Controls Corporation which three or four years ago was merged with Texas Instruments. [X-REF AHEAD TO P. 609] [EH NOTES: FIX DATE] [VB NOTES: VERA COULD FIND OUT FROM MY MIT FILES.] Al Spencer had good stock interests; every time that the company got in trouble he'd try to sell out and had to be gently persuaded from doing so.

Now there's one story here that I can't vouch for, I couldn't prove, but I think it's true. At one time the company was in difficulty. Remember that all of these little companies got into difficulties at various times, could hardly meet their payroll and so on. Al Spencer insisted he was going to sell out his stock. Whereupon some of the Boston group bought his stock and then, when the trouble was over and the company was back on its feet, they sold it back to him at the same price. Now I can't vouch for this but I think it's true. If it is, it's one of the most decent ways in which an inventor was ever handled. In whatever event, Al did all right, although of course if he'd hung on until the time when the merger with Texas Instruments occurred he would have done better still. He died before this occurred.

[VB ADDS: The story of merging Metals and Controls into Texas Instruments is a wild one, involving a chap who became, in my opinion, a pathological case (as well as a member of Jehovah's Witnesses) and who nearly wrecked the company.]

- 1) SPENCER THERMOSTAT COMPANY
- 2) PROTECTION OF SPENCER BY HIS BOSTON GROUP
- 3) METALS & CONTROLS
- 4) TEXAS INSTRUMENTS

Again, I became a consultant to the Spencer Thermostat Company and pretty much operated the laboratories and so on. But I was not one of the original organizers so I had none of the stock distributed at the time. Marshall, in this way and with me acting as technical man, set up about six different companies [X-REF AHEAD TO P. 569] of which about two and one-half, let's say, survived. In all of them, of course, he organized by selling preferred stock and giving with it a bonus of common. In doing this he could give himself some of the common without a tax penalty for his promotion. In the Spencer Thermostat case, Al could be given some common for his patent rights. I wasn't in on that sort of thing: my dealing with them was as a consultant. I took some cash [EH to VB: REVISE OK?] [VB to EH: OK] and some common stock as my consulting fees. So I ended up with stock interest in the companies but nowhere near, of course, as much as Marshall had for his promotion, which was quite correct.

- 1) BUSH AS CONSULTANT
TO SPENCER THERMOSTAT
- 2) STOCK DISTRIBUTION
IN SPENCER THERMOSTAT

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This is the Amrad story. Now of course if I were to trace the story of Raytheon through all of its vicissitudes that would be quite a story indeed, but hardly worth putting into all of this business here.

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Question B3: Submarine Detection....Bush's Device

If I were to spin the whole story of World War I it would take ten reels but I think a little about things as they occurred is worth getting into the record somewhere.

I worked on antisubmarine devices, in fact on submarine detection. The story of the submarine at that time was of course a very different one from what it was in the Second World War. Submarines could not then descend to great depths; hence they operated in rather shallow waters. Their usual procedure, when they could hear the propeller of a destroyer or one of the wooden antisubmarine boats or a destroyer chasing after them was to sit on the bottom and turn off all the equipment so that they would make no noise and simply wait until their pursuer went away. These were perfectly good tactics. The U-Boats wouldn't sit down in water that was much more than a hundred feet or a hundred and fifty feet deep, but doing this completely baffled the attack which was conducted by old style depth charges. So there was a need for something which could pick up these submarine boats when they were sitting on the bottom. Several Navy groups went to work on it.

I had an idea and presented it down in the National Research Council to R. A. Millikan who headed the committee on submarine devices, I don't know, it may have been more general than that, of the National Research Council. [X-REF BACK TO PP. 26, 27, 555] Rightly or wrongly I got the

- 1) FLASHBACK--WW I
- 2) SUBMARINE TACTICS & ANTISUBMARINE DEVICES
- 3) VB PRESENTS IDEA TO NATIONAL RESEARCH COUNCIL--MILLIKAN

impression that if I was one of Millikan's boys I'd get some support, but not otherwise. At any rate, I came out of the place completely disgusted, although I may have been utterly wrong. At any rate I got no support from Millikan of the NRC so I went to work on my own.

When I look back on it, I think what a crude idea I was pursuing. What I had was an alternating current bridge circuit; in other words I had a coil or a magnet on the ship which created a magnetic field. [X-REF BACK TO P. 28 AND FORWARD TO PP. 776-777] That field would be distorted by the submarine and I would pick up a departure from normal conditions on a coil, amplify it and put it on a pair of headphones.

The crude thing about it was that I had no theory that would tell me what frequency to use so that I'd have a minimum interference from the interface between water and air and so on. (I had in fact very little support of any kind that I can remember.) I just guessed at the proper frequency. When I needed a filter I did get help from the Bell Laboratories who promptly made one for me. But they had no interest in the idea except to give me a lift which was very decent of them indeed.

I can remember a hundred things that happened. When I was working away on the thing and needed a filter, I went to see the great Pupin down at Columbia University in New York. When I went into his office he proceeded to superpose some blank sheets of paper over several things on

- 1) FLASHBACK--WW I
- 2) BUSH'S ANTISUBMARINE DEVICE RATHER CRUDE
- 3) BELL LABS HELPS BUSH WITH A FILTER
- 4) THE GREAT PUPIN DOES NOT

his desk he'd been working on, which was a bit of an insult. [X-REF FORWARD TO P. 777] I asked him questions about the design of a filter for my purposes. It became evident that one of two things was true. Either the great Pupin did not know how a filter worked or, knowing how, he proceeded to deceive me as to its method of operation while I was working on a war project. I think it was the first one. Pupin is given a lot of credit for originating the electric filter and I think quite erroneously. He had some patents on it. I don't think they were particularly good ones, but he did, and the Bell System bought them. There was an aftereffect on this that was an interesting one.

Many years later Campbell [EH to VB: NAME OK?] [VB to EH: YES] of the Bell System, who really did the work on filters, had his papers gathered and published [EH to JK: SOURCE WANTED] [VB to EH: I HAVE THE VOLUME] by the Bell Laboratories. I was asked to write a foreward on them, which I did giving credit to Campbell for the origin of the filter with a good deal of enthusiasm. When I'd finished this the Bell System said to me, "But you didn't mention Pupin." I said, "No, I wasn't writing about Pupin; I was writing about Campbell." They said, "Pupin ought to be mentioned." And I said, "Not by me." They said that they thought this was not going to be quite satisfactory so I told them that they could do one of two things; they could either publish my foreward without changing a comma in it, or they could throw it out the window, and I didn't

- 1) FLASHBACK--WW I
- 2) BUSH GOES TO PUPIN FOR A FILTER
- 3) BUSH FORWARD FOR BELL LABS BOOK ON CAMPBELL AND THE FILTER

care which one they did. [X-REF AHEAD TO P. 778] They published it without changing a comma and then they added an introduction by Colpitts in which he gave credit to Pupin. [EH to VB: I'VE LOST YOUR THREAD.]

There was another episode when Raytheon was under way. The great combination of AT&T, General Electric, Westinghouse and RCA clamped down on it and nearly put it out of business by utterly improper methods and in violation of the Sherman Act. [EH to VB: WHAT YEARS ARE WE TALKING ABOUT HERE?] [VB to EH: 1927 OR THEREABOUTS.] It's interesting that after this contention with the great telephone system, I later on became a member of their board of directors. Of course, after that, when I mentioned the time that AT&T was in combination with GE and Westinghouse and so forth, the people inside of AT&T quite frankly admitted that this had been a very serious mistake on the part of the company. It's quite an interesting yarn that I had those many contacts with the telephone company. [VB ADDS: They tried to hire me in the Raytheon battle, and I went on their board. I also made OSRD contacts with them.]

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Now to get back to the First World War, I got the stuff to work on a wooden submarine chaser, the Navy then insisted that it must be able to work on a destroyer. I protested that this was much tougher and I doubted that it could be done. But they insisted and I worked for six months

- 1) BUSH FORWARD FOR BELL LABS BOOK ON CAMPBELL (Continued)
- 2) AT&T, GE, RCA, and WESTINGHOUSE COMBINE AGAINST RAYTHEON
- 3) FLASHBACK--WW I
- 4) BUSH'S ANTISUBMARINE DEVICE

on trying to put it on a destroyer with no success whatever. Then the Navy changed their minds again and said they wanted it for a wooden chaser. When that was decided we got to work making stuff. A lot of a hundred [?] instruments was put into production by Amrad. Three of these went overseas to American submarine chasers and were installed. In tests they detected some British submarines but then the war ended and no set ever detected a German submarine. [X-REF BACK TO PP. 27, 28 and AHEAD TO PP. 766-767]

That was my First World War experience and I think [it] probably forced into my mind pretty solidly the complete lack of proper liaison between the military and the civilian in the development of weapons in time of war. If there'd been any such thing as NDRC in operation at that time, the problems would soon have been thrashed out, and a competent committee, made up of military men and civilians, would have decided that the stuff ought to be rushed into wooden ships and that the conversion for iron ships was absurd. But there was no such thing, so I was stuck.

Now one other point: toward the end of the First World War there was another group working on this same problem. (I didn't know anything about them.) They were trying to pick up stray currents from the submarine by electrolytic effects, you know, with a bronze propeller and a steel hull by trailing a wire which had some non-polarizing electrodes on it. I didn't even know they were working on it until one day down at New London I went aboard a craft, and saw the

- 1) FLASHBACK--WW I
- 2) BUSH UNAWARE OF ANOTHER GROUP WORKING ON SAME PROBLEM--ANTISUB DEVICES

work. I asked them what they were doing and they told me. I asked what troubles they were having and they said, "The stabilizing of the galvanometer on shipboard sufficiently so that you can get proper sensitivity." I said, "Why don't you use a pivot instrument. Western Instruments has just gotten out a very sensitive microvolt meter of this form, a millivolt meter." They had never heard of it. They got one and put it on the deck and it promptly proceeded to work all right.

The point is that this was a lot simpler thing than the thing I was developing. It wasn't quite as good in some ways because my affair gave a signal when the seeking ship was immediately over the submarine and theirs gave the signal later, but it was utterly simple, cheap to build, could have been put into effect in quantity in a hurry. But it also never did arrive. I think if I had known about that system and known that there were non-polarizing electrodes that would work that way, which I didn't, I probably would have abandoned my own efforts, because the other way was the way to do it. But neither idea got into effect and the reason was that there was no centralizing group to bring together such things, and compare them.

[VB ADDS: Ernest Fox Nichols was also working on magnetic submarine detection, using a dipping needle affair. One day he came aboard a yacht I had my stuff installed on.

- 1) FLASHBACK--WW I
- 2) ANTISUB RESEARCH THAT
DUPLICATES OR OVERLAPS
SHOWS NEED FOR NDRC
TYPE OF ORGANIZATION
- 3) ERNEST FOX NICHOLS
ALSO WORKING ON
SUB DETECTION

I was adjusting the rig by detecting a locomotive that went by a hundred yards or so away. When he found I could do that rather positively, he told me his affair could never equal it when operated on shipboard, and I believe he abandoned his program.

[Note the complete absence of correlation between similar efforts.] [END WRITTEN ADDITION]

- 1) FLASHBACK--WW I
- 2) ERNEST FOX NICHOLS
ALSO WORKING ON
SUB DETECTION

* * *

Question B4: Clients & Consultations in Early Years

I judge the reason you want to go into this and a number of similar things is not for the purpose of biographical material (for if you are finagling me into the biography it would [EH NOTES: WILL!] be damned hard finagling), but rather to get some lessons out of it if possible. I think there are some to be found.

1) BUSH's EARLY CLIENTS
AND CONSULTING WORK

I was consulting, of course, before I went to MIT and then after that as well. Most of my consultation was in connection with companies that Marshall organized and where I was the technical man. There were as many as six of these. [X-REF BACK TO P. 563] Three of them survived. One was Raytheon, another was Metals and Controls. There was a third one [EH ASKS: WHICH?] that survived but which didn't grow large. A year or two ago I had a letter from a fellow who wanted to buy my stock in the company. I told him that I didn't own any stock in the company but later discovered that I did. It is one of the things that we started that just went into a moribund small state. But a batting average of two out of six is pretty good. The same people put money into all of them so that nobody got stuck. We, Marshall and I, of course worked very closely in those days and I went into everything he went into. [EH INDICATES QUERY] [VB to EH: WHAT QUESTION?] In addition I had a couple of very important jobs (for me, that is) with Stone and Webster

[X-REF BACK TO P. 533] and a couple of other outfits.

The interesting thing here to my way of thinking is this: in order to have a successful commercial venture or an innovation that gets someplace you need three things present; the inventor, the promoter, and the manager. Now let me give you an example. Way back in very early days, seven or eight of us (Marshall was not one of this group) [X-REF AHEAD TO P. 595] found a chap named Doble who had a bright idea for testing insulators. It was a clever idea, not requiring very much money. High-voltage insulators on transmission lines give a warning before they fail; they begin to make a noise. So Doble got a highly insulated tube with which you could listen to the things. It was a clever stunt and it filled a need. Some seven or eight of us put up a thousand dollars apiece to get him started. He started all right, and then he got still some more ideas, and began to do some business.

He was a good inventor, he was a reasonably good salesman with the public utilities (although he talked too much) but he was not a manager. I was one of his board and after talking to him I went into the board meeting one day and said, "What this company needs is a manager. We've got a good technical man, a good engineer and our sales are all right, but we haven't got a manager and we'd better get one." Well they disagreed with me so I said, "All right, run it your own way, and no hard feelings, but let me get off the board." Which I did.

- 1) BUSH's EARLY CLIENTS AND CONSULTING WORK
- 2) INGREDIENTS FOR A SUCCESSFUL COMMERCIAL VENTURE
- 3) DOBLE ENGINEERING CO.

Well, about seven or eight years ago [EH to VB:
i.e. PERHAPS 1957?] [VB to EH: YES] I got a check for the preferred dividend of the Doble Engineering Company for the year, I think it was 1924. In other words, that company had defied all laws of gravity; it had neither grown nor had it gone busted for many, many years. After that, they paid up all their arrears on their preferred, and they're now paying common-stock dividends and the company is finally getting on its feet. I think this is the best example that I ever saw where everything was present to do something really worthwhile, except good management. Now we can say this about Raytheon too and I'll come to that.

I read just recently Jim Conant's latest book in which, rather laboriously, I think, he outlines the difference between the deductive and the inductive types of thinking in science, in law and so on. He's got a chapter in there on inventors and in it are two fallacies; one, he falls into the old, old fallacy of saying that Bell invented the telephone and Edison invented the electric light and so forth. Actually, of course, many inventors were involved in both of these cases and particularly so in connection with the electric light. Conant also completely ignores the function of the promoter.

Edison, for example, was a very good inventor, a still better promoter and a very poor experimenter. [X-REF BACK TO P. 525] Some of his experimentation was crude to

- 1) DOBLE ENGINEERING CO.
- 2) EVEN CONANT BELIEVES OLD FALLACIES ABOUT EDISON, ETC.
- 3) THOMAS A. EDISON

say the least. When we talk about the Edisonian Method, which means try everything without any theory to guide you, just hit and miss, we're talking really about very poor experimentation. But Edison was such a good promoter that he got away with it no end. Later management came in. He had good management, not himself but through people that he tied in by his promotion.

- 1) INGREDIENTS FOR A SUCCESSFUL COMMERCIAL VENTURE
- 2) EDISON
- 3) C.G. SMITH--RAYTHEON
- 4) SPENCER THERMOSTAT AND MARSHALL

Now the Raytheon Company illustrates this very well. When it was small and everything was precarious and so forth, its invention was good. We had C. G. Smith, who kept having ideas, and when he was young he had many. Interestingly enough after that first young splurge he kept on with Raytheon for another forty years or something of the sort, thirty at least, and didn't produce a single new invention of any importance. He had one flair which then disappeared as far as his creations were concerned. Although he continued to live a happy life, I'm sorry to say that he did not handle his stock in Raytheon in a way so that he made any real money, and he had some [EH to VB: SOME WHAT?] [VB to EH: STOCK.]

Raytheon in its younger days got along well but when it began to grow it did not go well. The trouble was that Marshall, who was a very good promoter, very good indeed, was not a manager. He didn't know it and that of course made things still worse.

He was also a good inventor. The Spencer Thermostat was successful because of an invention of Marshall's.

The Spencer Thermostat, the clicking disk went into flatirons and produced the first automatic flatiron. [X-REF BACK TO P. 561] Before that there had never been a thermostat that would operate flatiron temperatures successfully. Marshall got out an invention on a combination of methods which had a small difference of expansion coefficient over a wide range until they got up to a high temperature and then had a wide difference of coefficient of expansion. [EH to VB: FOGGY. BAD TRANSCRIPT?] [VB to EH: SEE NOTE ON P. 561] [JK to EH: There is a total confusion as to who invented the Spencer Thermostat -- Marshall, or Spencer. See page 561, 573. Straighten out Spencer & Marshall cards.] [AM to EH: See P. 562.] With this thing one could take a thermostatic disk all the way up to flatiron temperatures without over-stressing it and there at high temperatures it had good activity.

Now this is as clear-cut and ingenious an invention as I ever saw. I kind of [rather] think that if Marshall hadn't invented it, I might have done so myself within a short time, but Marshall did it and by doing it proved that he had an inventive instinct. But he was not a good manager. One of his faults, and a very serious one, was that he did not hold his board of directors in a way that made them really useful and that kept his reputation with them.

Eventually, Marshall and I broke. This was due to two incidents. The first one occurred during WW II. I think it's hardly worthwhile to go into it, but MIT had contract

- 1) SPENCER THERMOSTAT
- 2) MARSHALL HAD AN INVENTIVE INSTINCT
- 3) MARSHALL A POOR MANAGER

with OSRD. It made a subcontract with Raytheon and did not carry across the patent clause as it was supposed to do. Raytheon made some inventions and then refused to assign them as far as military purposes were concerned although it was working under a sub-contract. If it had been working on a prime contract it could have assigned the military rights and kept the commercial rights. But it was working on a development contract with the university where a complete assignment was called for.

Now I had to protect myself because I owned stock in Raytheon. When I first became Director of OSRD I wrote to FDR and told him I had stock holdings in two companies which might become involved, so should I sell this stock? In return I got a letter from the Attorney General [EH to JK: WHO WERE FDR ATTORNEYS GENERAL 1933-1945?] [VB to EH: BIDDLE] which undoubtedly FDR told him to write (and which he undoubtedly wrote without any enthusiasm certainly) in which he said that inasmuch as my contracts were all low-profit contracts there was no need for me to sell. Nevertheless I leaned over backwards to be sure that no favoritism was shown either to Raytheon or Metals and Controls. My instructions in my office were such as to protect me against that.

In spite of this nobody saw this situation developing until it was full blown and then I had to get really rough and insist on assignment. In fact, I had to go so far as to tell MIT that unless the thing was settled properly, it was their fault, they'd made the slip in the contract,

- 1) WWII
- 2) BUSH & MARSHALL
BREAK DUE PARTLY
TO PATENT PROBLEMS
OF OSRD & RAYTHEON

that I'd have to turn this thing over to the Department of Justice. When this was done, finally Marshall agreed to a procedure. We set up a scientific committee which examined into the thing and determined what patent rights properly should be assigned and what ones were properly the property of Raytheon. The thing was settled on that basis. I was protected, and I think Raytheon got all the commercial rights to which it was properly entitled -- those which it had developed on its own and not those which it developed on government funds passed through MIT. However, this caused a complete break at the time between Marshall and myself.

[EH to VB: WILL YOU RECITE THE OCCASION ON WHICH YOU WITHHELD \$1 MILLION MORE OR LESS FROM MIT?] [VB to EH: IT WASN'T 1 MILLION, IT WAS 7 MILLION. I THINK I'VE TOLD THE YARN SOMEWHERE. IF NOT, I'LL DO SO LATER. BUT I THINK IT IS ALREADY ON TAPE.]

After the war I got together with Marshall and went over with him the plans for Raytheon in the postwar period. He refused to tell me anything. I pressed it but got no real information. His plans seemed to me, as far as he said anything, to be grandiose and unrealistic. So I turned around and I sold my Raytheon interests. I don't know how much this had to do with the fact that a little later on his board of directors also got annoyed at finding that the company had taken steps that they knew nothing about it until it was a fait accompli which apparently was what

- 1) POST WWII
- 2) BUSH & MARSHALL
SPLIT COMPLETELY
OVER RAYTHEON'S
POSTWAR PLANS

occurred. At any rate Marshall was moved out of the presidency whether on his own initiative or not I don't know. That made the break pretty complete. [EH to VB: THIS SOUNDS DAMNED INTERESTING, BUT IT REALLY DOESN'T CONVEY MUCH INFORMATION. ARE YOU WILLING TO ELABORATE?]

[VB to EH: ON MARSHALL AND RAYTHEON, I REALLY CAN'T add much, as I kept entirely out of Raytheon affairs from the time I joined CIW until now.

[Marshall and his family have gone nuts on the subject of the Bushmen in Africa. They have been over there many times. His daughter has an article on the subject in the National Geographic. I suspect this may be a case of seeking culture with a capital C, and that Mrs. Marshall may be the moving spirit. Today Marshall, as far as I know, is out of contact with all his old-time friends.] [END WRITTEN ADD]

After that they got in Charlie Adams to run Raytheon and for the next many years their company was pretty badly managed. They had a highly fluctuating government business and every time it went down, they would proceed to lay off personnel with the result that they got a very bad reputation in New England among people that they might hire.

The promoter is often frowned upon but he performs a very essential function and it's probably more polite to call him an entrepreneur. If you have the combination of some new ideas which have a market, and you have good promotion to

- 1) POST WWII
- 2) MARSHALL LEAVES RAYTHEON
- 3) CHARLIE ADAMS REPLACES HIM
- 4) RAYTHEON EARNED BAD REPUTATION BY POOR MANAGEMENT

Reel 9-A
Page 576

get the solid industrial company on its feet, and then you have good management, you can go places. The trouble with Raytheon is that it has not had the latter thing after it grew to large size. I think it did have good management in the days when it was dodging the big companies and operating in a highly precarious way where lightness on its feet was exceedingly important. When it came to be a large company and one with a lot of government business, then I think that it has been managed poorly.

- 1) RAYTHEON's
POOR MANAGEMENT
- 2) PROMOTER-ENTREPRENEUR
IN BUSINESS

* * *

Question B5: Topic of Ph.D. Thesis...Electrical Engineering
of the day..."first class error"... "60 cycle man"

I don't remember the title of my doctorate thesis.
What it was was simply this: I took some of the things that
Heavyside developed and tried to bring them up to date and
make them useful -- in particular one formula that he had
that applied to transients in electrical circuits. Now I
didn't claim of course to have originated this. I merely
picked it up from Heavyside, attempted to prove it and show
the way in which it could be applied. [X-REF BACK TO PP.
32 and 58]

It's true that in those days practically all of
the attention was on heavy currents; the weak current or the
light current technology was not developed appreciably at MIT
or as I can judge, anywhere else in a university. The
problems were primarily steady-state problems; transient-state
problems did not come to the forefront until we got into the
field which is now called electronics. I was early in seeing
that the transients had got to be handled. True there were
some transient problems in connection with power engineering
but they were not the prominent problems and what I did
really didn't amount to a great deal.

The trouble with my thesis really was that while
I thought that I'd proved this formula of Heavyside's, I
really hadn't. I found this out when later Wagner in Germany
published an excellent proof which showed me at once that

- 1) FLASHBACK--MIT
- 2) BUSH's DOCTORAL
THESIS UPDATED
HEAVYSIDE's WORK
- 3) "1st CLASS ERROR"

mine was quite fallacious and quite weak. This was strange because Wagner's proof was in terms of functions of a complex variable, which was a field of mathematics I had studied and which I might readily have applied to that problem. Of course no one in the Department of Electrical Engineering knew the mathematics or used the mathematics that was necessary for this purpose, and there was no one in the Department of Mathematics that knew the problems of electrical engineering. So I was a sort of a fish out of water. I managed to put something together that appealed to the electrical engineering department, although looking back on it today I realize that it was an exceedingly weak attempt. With a little steering I might have done something that was far more sound..

Later on, of course, I developed this same field and published a book on it. This book became used in many universities where they were tackling for the first time Heavyside's operational circuit analysis which was what I'd called the book. Again this book was not a profound thing by any means. It took Heavyside's approach, put it into form where it could be used, and proved a few things. Primarily it was what was ordinarily called heuristic, that is, if a thing worked why that was good enough. In other words if the new mathematical treatment presented there gave sound results in a number of cases where one knew the answer, one merely assumed that it could be used safely without proof in conditions that were at all similar. So again I did not do a profound thing by any manner of means. The mathematics that

- 1) MIT STUDENT DAYS
- 2) DOCTORAL THESIS
"EXCEEDINGLY
WEAK ATTEMPT"

I used, however, was a step beyond what was done at that time by electrical engineers and also a step beyond the point where mathematicians had entered into the picture. [X-REF BACK TO P. 516]

Now there was one book that preceded me in this field that was more profound than mine but did not become useful. After I got through, other books were published that did both, notably the first book by Barnes of which I think I've spoken before. The field today has moved so far that I can't pick up a paper on the subject and read it. It's gone way beyond what I did and I today have not got the vocabulary or the background to stay with it. Fortunately I never tried to after I got through teaching the graduate course on the subject at MIT. [X-REF BACK TO P. 517]

[VB ADDS: One primary reason I built the differential analyser was to solve some of the equations of transients in electric circuits.

[Ralph Booth and I published what I think was the first paper that attacked mathematically the behavior of a long transmission line when subjected to a heavy switching jolt. It showed pretty well that a proposed line to bring power down from Canada to New York would be unstable. This caused quite a fuss, as the line had been designed by G.E. and Westinghouse engineers. The line was not built. This was when I was consulting for Jackson and Moreland, who were in turn consulting for Boston Edison, New York Edison, Philadelphia Electric.] [END WRITTEN ADD] [AM NOTES: PAPER

- 1) VB's THESIS
- 2) HEAVYSIDE's OPERATIONAL CIRCUIT ANALYSIS
- 3) BOOK WITH BARNES
- 4) PAPER WITH BOOTH

Reel 9-A
Page 578-A

PRESENTED FEBRUARY 1925 & ABRIDGED INTO ARTICLE "POWER SYSTEM 1) CIRCUIT BEHAVIOR
TRANSIENTS" IN JOURNAL A.I.E.E., MARCH 1925. COPIES IN FILES]

However, MIT was a pioneer in using rather advanced
mathematical methods for the analysis of circuit behavior.

* * *

Question B6: AC versus DC and Rise in Status for DC

This is an interesting situation. Of course in the days when electric lighting was first introduced, DC was the thing that was used because of a number of conditions -- and also because power did not need to be transmitted very far and two hundred and twenty volts of direct voltage would suffice. One of the primary points of course was that the DC motors were flexible. The AC motors of the time were quite rudimentary and when used at all were of constant speed. Then came greater distances and this alone forced alternating current into its own, because with its transformers one could step the voltage up and then down again. One could not do this with DC and long distance transmissions had to be at high voltage. So AC moved in, and ever since has been used despite that the AC motor is not nearly as flexible a thing as the DC motor. For some purposes the AC motor is ideal but where variable speed is called for it leads one into great complications.

1) AC versus DC

Now today the situation is again changed, because today one can step up and down with DC using the various new devices; rectifiers and so on. In fact one can do this: one can generate alternating current (which is the easiest thing to do), rectify it, transmit it as DC at high voltage and then use it at the other end to create alternating current again. This is not done much but the reason is that the other

techniques have become thoroughly embedded. If the rectifiers and the thyratrons and the transistors and so on had all been available in the early days, I have no doubt that DC would have persisted. It's not that Thomson thought correctly and Edison incorrectly; it's rather that the nature of the physical problems involved forced the solutions in spite of many who dug their heels in the sand.

Of course we still have a DC district in New York and it's a bit of an anomaly but the point there is that it works all right. It would take a lot of money to reconvert it so why change it? Ultimately it will probably be changed over unless, indeed, rectification becomes so prevalent that it will still be DC, but handled by rectifiers at the powerhouse instead of being handled by machines.

- 1) AC versus DC
- 2) THOMSON & EDISON
- 3) DC in NEW YORK CITY DISTRICT WORKS FINE

* * *

Question B7: Elihu Thomson & GE, MIT's Years of Poor Administration, KTC as a Physicist.

I can't swear that Elihu Thomson wasn't properly taken care of in the light of his contributions. I merely remember that when he died I was appalled when I learned of the size of his estate. After all these years I can't tell you what the figures were but I know that he was not a wealthy man because I remember talking with people about whether his widow had adequate funds and so forth. Now there may have been something that I didn't know about. Perhaps there was a big trust somewhere that I never heard of or something of the sort. At the time I know I felt very annoyed because it seemed to me that some of the other people made a lot of money out of GE. Incidentally, one of them was Caryl Haskins' father who did very well indeed. It seemed to me there was something wrong if Thomson who had been very important in the early days (The Thomson-Houston Company and so forth) didn't come out very well indeed. At the time I certainly was convinced that he did not. But I have nothing to back that up with except just a vague memory. [X-REF BACK TO P. 46]

[You're right, I was wrong. It was ten years when MIT had a very bad administration. I imagine it merely seemed like twenty years.]

I didn't say much about Karl Compton as a physicist, and one could hardly compare Karl and his brother Arthur for a number of reasons, one of them being that Karl didn't stick

- 1) ELIHU THOMSON'S ESTATE SEEMED TO INDICATE NO PROFIT FROM GE WORK
- 2) TEN YEARS OF POOR ADMINISTRATION AT MIT
- 3) KARL COMPTON

to physics, and Arthur did. There is one thing that I remember very well, however. You know that Millikan made his great reputation on the basis of his measuring the charge on an electron by the oil-drop method. He received the Nobel on that account, and because of some other things he did, but this was his great achievement. I remember one time talking to Karl about this and saying to him, "Look, when you were at Princeton didn't you measure the charge on an electron by an oil drop method or by a very similar thing?" He said, "Yes, yes I did. It was nearly the same thing as the oil drop method used later, different in some respects, but my work was quite crude compared to Millikan's. He was the man who did the real job of making it precise." This is Karl Compton's modesty and also his inclination to think the best of everyone. He knew I didn't care much for Millikan's curves and he was not exactly defending Millikan, but he came pretty close to it.

The point is that as far as I know Karl Compton's work on the measurement of the electron by methods at least very similar to the oil drop method was prior to Millikan's work and published before Millikan's work. Yet Millikan, as far as I know, did not refer to Karl's work in his own publications.

The fact that Karl could be generous in his estimation of this was utterly characteristic of him. Now I never got so far as to go back and read Karl's paper to see just

- 1) KARL COMPTON DID NOT STICK TO PHYSICS AS HIS BROTHER DID
- 2) COMPTON'S MODESTY-- HE DEFENDS MILLIKAN'S NOBEL

Reel 9-A
Page 582

how far he anticipated Millikan, but I haven't any doubt that he gave me an accurate statement of it; that while both were using similar methods, Millikan's work was a precise job.

This reminds me also of the story I told you about too. People who think that scientists are always striving to get the last ounce of credit for everything that they do just haven't known the right physicists.

* * *

1) KARL COMPTON DEFENDS
MILLIKAN'S NOBEL IN
CHARACTERISTIC
MODESTY

Question 8B: Differential Analyzer....Norbert Weiner....
Digital & Analogical Machines....Motion of
A Ship In a Seaway....Problem Solving.

What do you expect me to do, give you a course in
integral equations? I hardly think I can make the thing
clear but let's try a bit of it.

The differential analyzer solved total differential
equations of a somewhat limited scope. Many of the problems
of physics are not covered by ordinary or total differential
equations even when non-linear. Many of the problems of
physics are covered by partial differential equations or
integral equations. Now the integral equation is not merely
one that involves an integral, because this can be turned
over into a differential equation; it's an equation that
involves an integral with a variable on a parameter under the
sign of integration. Lots of problems fall into that type.

The general way in which such problems are handled
today is by digital machines. The digital machine performs
by doing simple numerical calculations, which of course it
does very fast indeed. It can solve almost anything because
if you want to do calculation enough you can get at almost
any equation. The digital machine solves a partial differen-
tial equation by splitting things up into a lot of very small
parts and so on. I don't need to go into that.

Over a considerable scope any problem that can be
stated in terms of partial differential equations can also
be stated in terms of integral equations. There are quite a

1) DIGITAL MACHINES
HANDLE INTEGRAL (OR
PARTIAL DIFFERENTIAL)
EQUATIONS

few things that come into integral equations anyway. One is correlation; a lot of statistical theory comes under this form. Way back after I'd got the differential analyzer moving it became apparent that there had to be a machine approach to this whole affair of partial differential equations or integral equations. I tried to go at it through the integral equation method. I wanted a machine primarily that would evaluate instantly a value of an integral with the parameter under the sign of integration. I had a scheme for doing this which was an optical one and it could be done. When Norbert Weiner and I were talking it over, probably at lunch, he suggested an alternative form of the optical system that would be better and more precise. I adopted his suggestion and of course gave him credit for it when I published it.

[X-REF BACK TO PP. 55-56]

Harold Hazen and I worked the thing out. We published it in the Franklin Institute Journal and then we solved one problem on it. This will give you an idea of what kind of a problem you could solve: Take a ship in a seaway and assume that we know all about the ship -- its shape, distribution of mass, and everything we need to know. We also know the size and shape of the waves. As the waves pass the ship, they induce certain forces on and in the hull, and the hull proceeds to oscillate in the waves. Now we've neglected forces due to inertia of the water; we took into account only forces due to buoyancies -- which made the thing an approximation, but not too bad a one.

- 1) WEINER SUGGESTS
OPTICAL SYSTEM
VARIANT FOR
BUSH'S MACHINE
- 2) HAROLD HAZEN
- 3) DEMONSTRATION PROBLEM:
MOTION OF A SHIP IN
A SEAWAY

Now you can see how this leads to an integral equation: the waves put forces on the ship to which the ship responds, but as soon as the ship responds, a different set of forces appears so that one finds himself chasing his tail around in a circle. This is the sort of problem that leads to a set of partial differential equations or to an integral equation. In this case, very nicely, to an integral equation.

The first time I became acquainted with integral equations was when I was a student at MIT. A fellow by the name of Volterra in Italy had published a paper which intrigued me so much [X-REF BACK TO P. 55] that I learned enough Italian to read some of his other papers. He was dealing with the interaction between fish in the seas around Italy and the predators on these fish. It's a very similar thing to the relationship between foxes and rabbits up in the Arctic. If the predator gets very plentiful the number of the victims decreases, whereupon the predator begins to starve so that the two populations oscillate around one another. This is the kind of thing that leads to an integral equation. Volterra solved some of these problems by the use of integral equations, and this was what intrigued me.

We did not have a precise machine; in fact it was pretty crude. [X-REF BACK TO P. 60] Moreover the ideas in that machine could never be made precise in the form we were talking about. Hence when the digital machine came along and

- 1) FLASHBACK--
STUDENT AT MIT
- 2) VOLTERRA'S WORK
ON PREDATORY FISH
PROBLEM USING
INTEGRAL EQUATIONS

became powerful and was able to solve the same problems, but to do so precisely, this optical machine became obsolete at once. That was the end of that.

However it was interesting while it lasted and it opened up a field of mathematics that I think has not been too well explored for I think that integral equations have a lot more uses than those usually made of them.

Now the differential analyzer was an analogue machine because in a way what one did was this: he considered a physical system where he wished to get a solution. He set up a mechanism which would behave in the same way as the physical system to be studied (which would be an analogue, in other words) but which could be measured and which would perform slowly enough so that accurate measurements could be used. This is an analogue machine obviously, and the differential analyzer was of that type.

The digital machine on the other hand does no such thing. By simple arithmetic processes it proceeds to calculate arithmetic solutions. Not quite by cut and try, it substitutes a difference equation for a differential equation and computes that step by step, gradually refining its procedures. It would be of no good whatever except for the fact that arithmetical operations can be performed at exceedingly great speeds by the use of transistors and so on. Since they can do this, it is the best approach to almost every problem you'll find except problems where relatively unprecise

- 1) DIFFERENTIAL ANALYZER WAS ANALOGUE MACHINE
- 2) DIGITAL MACHINE USES SIMPLE ARITHMETIC PROCESSES
- 3) THE USES OF BOTH MACHINES

solutions are adequate and where rapid exploration of a system is desired. When that is the case, differential analyzers are still used. They're made today electrical rather than partly mechanical; they are made so that one can plug in different elements and so on. They are used in engineering work quite a bit and for that purpose. You do the exploration and then if you need a precise solution, you go on to the digital machine.

Now most people working in this field classify all machines into these two types. That is utterly incorrect. In addition to the two types, there is a third type which is neither digital nor analogue and which is being grossly neglected. The simplest illustration I can think of for this is a slide rule. This is obviously not digital; neither is it analogue in the true sense. It's a way in which one solves problems by using logarithms and measuring them in terms of distances. It's not classifiable.

Now the affair for solving integral equations I talked about is also not an analogue machine, and not a digital machine. It's not analogue because it doesn't follow the equation. It's a way in which the equation can be instantaneously solved -- or rather one term of the equation can be instantly solved so that combinations may be readily made; successive approximations to get at a solution. This is not an analogue machine by any manner of means.

1) THIRD KIND OF MACHINE
(NEITHER DIGITAL NOR
ANALOGUE) SUCH AS
THE SLIDE RULE

2) USES OF DIGITAL AND
ANALOGUE MACHINES
(Continued)

I believe that today people have become so wedded to digital machinery that they think in those terms only and that they are neglecting enormous chances to do things better by other means.

To be specific, a few years ago I suggested to Wiesner at MIT and to the people in the National Science Foundation a special machine for doing weather analysis. Weather analysis today, weather prediction, is done by the use of digital machinery. One simply takes a model of the atmosphere and observes present conditions. The digital machine, using equations that govern the flow of masses of air, extrapolates from the present picture to one twenty-four hours ahead.

The trouble with this is that the model used has to be a simplified model; it will thus, for example, leave out differences in the heights of land in various places, or leave out heat generated by precipitation. In one way or another it's a simplified model, and experience shows that even with a simplified model the machine can do a pretty good job of prediction. However: if one made the model more nearly approach the actual conditions in the atmosphere -- in other words if one introduced more of the real effects, the machine could still solve the equations, it could still predict. But even with a good machine this would take so long that it would be hardly worthwhile. In fact if one went to extremes the machine could hardly keep up with the actual weather.

- 1) PEOPLE TODAY TOO
WEDDED TO DIGITAL
MACHINERY
- 2) BUSH's SUGGESTION TO
WIESNER FOR A WEATHER
MACHINE NEITHER
DIGITAL NOR ANALOGUE

I proposed a machine, which incidentally was not an analogue machine and not a digital machine, which had this property: that every time one complicated the model, instead of lengthening the time of solution he merely complicated the machine that did the solution. I think it would work; in fact, I'm almost sure of it. I didn't work it out in detail. I simply said, "Here's an idea. If some bright chap would like to go to work on it I'll be glad to help him and I won't be hovering around looking for credit."

But nobody has started on it that I know of and I haven't pushed it because I've had other things on my mind. If I were twenty years younger, I'd make one. The reason, I think, is that people have gotten to think so much in terms of digital machinery that they don't think in any other terms. They get horrified if you try to suggest that some other thing can do a better job.

In the particular instance of weather prediction one does not need the precision of the digital machine because the data itself is very far from precise. One does need to handle a very complicated thing in a very short time. The people doing the job merely lean on the fact that digital machines are getting to be faster and faster; undoubtedly they will soon be able to handle very complex models. They do not turn around and say, "Is there a machine which sacrifices precision in terms of speed?" I really think that if

- 1) BUSH's SUGGESTED WEATHER MACHINE (Continued)
- 2) PEOPLE TODAY TOO WEDDED TO DIGITAL MACHINERY (Cont'd)

Reel 9-B
Page 589-A

the special machine I was talking about was made it could do the twenty-four hour prediction in a few minutes, which would be very convenient. However I haven't pushed it and I probably never will.

1) WEATHER MACHINE
(Continued)

* * *

Question B9: Conversations with Eisenhower (Columbia Univ.)

[EH NOTES: circa 1949] I suppose Columbia handled its job of getting a new president about as badly as it could be done. They had a committee of their trustees and also a committee of their faculty. Various people were considered for the post and usually learned about it by reading about it in the newspapers. I know I was one of these and Senator Fulbright was another. There were several more; so many that we began talking about a new society made up of those who were considered by Columbia for its presidency.

[X-REF BACK TO P. 75]

Columbia was in a bit of a turmoil anyway and the man who went there was going to be up against a tough job at best. One thing that was going on, incidentally, was that Joe Barker was angling for the post internally. This will come up if you ask me about how I became vice president of MIT. [X-REF AHEAD TO PP. 615-616]

Ike and I had offices near each other in the Pentagon at that time and I saw him quite frequently. I never have understood Ike fully and I've seen him under various circumstances. He's always been very cordial and our relations have been fine. On the other hand, while I had a great deal to do with him while he was chairman of the Joint Chiefs of Staff and so on, after he became President I hardly saw him. Some of this was no doubt due to the fact that he leaned on Louis

- 1) POST WWII
- 2) COLUMBIA UNIVERSITY
CHOOSES PRESIDENT
- 3) BUSH NEVER UNDERSTOOD
IKE COMPLETELY

Strauss heavily and certainly Strauss didn't care to have me around if he was getting along well without me. [X-REF AHEAD TO P. 708]

Well, when Ike began to consider going to Columbia I tried to help him out and one thing that I did was this. Devereaux Josephs was then President of Carnegie Corporation, a man of considerable influence in educational circles in New York, naturally. I got him down and he and I lunched with Ike and discussed the program. After we came out Dev Josephs said to me, "You know, I'm appalled. That man hasn't got the slightest idea of how a university is really run and the deuce of it is, he thinks he has. I believe he's going to get into trouble."

In another conversation I remember saying, "Ike, what are you going to Columbia for?" And he said, "Well, I want to get back to have close relationships with American youth." "Well," I told him, "if you become President of Columbia you won't see an American youth once in six months." I was appalled, too, because Ike didn't have any idea of what he was facing, or how to face it.

Now you've prefaced this question by noting that I've had executive posts in three areas, administrative posts, if you prefer. They are decidedly different. I don't think very many people really grasp how a university runs. [We'll come to some of this in other connections.] For one thing, in a university the chief executive doesn't pass out orders except on matters of building repairs or something

- 1) IKE'S NAIVETY ON HOW A UNIVERSITY IS RUN--CONFIRMED BY DEV JOSEPHS
- 2) FUNCTIONS OF CHIEF EXECUTIVE IN A UNIVERSITY

like that. To the faculty he doesn't issue orders and the faculty is not controlled by a pyramidal organization. The acts of a faculty member are controlled by his own ambitions and his own conscience and also by public opinion among his colleagues. [X-REF BACK TO P. 280 and AHEAD TO PP. 661, 664] This is very important. I'll have something to say about this no doubt when we come to the professors' union.

[X-REF AHEAD TO PP.660A-666]

Ike once said to me that he thought he'd get along well at Columbia if he got on a good working basis with his board of trustees. Then if there was an obstructionist in the academic organization he could take care of him. I said, "Ike, you mean that if you've got a dean who's an SOB, you can fire him?" And he said, "Why, yes, something of the sort." I said, "Well you might fire him Ike, but he wouldn't leave." That was about as far as Ike got into a grasp of how the show went. Fortunately when he went to Columbia things went reasonably well for the short time that he was there although he must have been surprised at what went on about him.

* * *

I had many sessions with Ike on various things: one when I posted him on the German V-bombs, buzz bombs; another was when I tried to get him to take care of Groves

- 1) IKE's CONCEPTION OF A UNIVERSITY PRESIDENT
- 2) BUSH's WARTIME RELATIONS WITH IKE

in what I thought was a proper way. We were on good terms and I think understood one another. We went before the Patterson Committee of the first Hoover Review of Organization. We were testifying about the Joint Chiefs of Staff. I went on the stand and said that the show didn't work and never could be expected to work in the form it was in. I continued this with examples under questioning for quite a while. Ike sat there and listened to me and then he went on the stand and told the group that everything was lovely. They believed Ike and they didn't believe me.

The point is, I'm quite sure that Ike knew that the Joint Chiefs did not operate well. When he became President he understood completely, I'm sure, that we didn't have a single war plan, we had three of them. We didn't have real unification at the planning level yet he never did anything about it. The Joint Chiefs were put in to order and made to operate in a reasonable way only when we got to Kennedy and MacNamara. [X-REF FORWARD TO PP. 704, 815]

Why is it that Ike who has great devotion to his country, great feeling of his responsibility, ducked that issue? Now you can say the same thing about what happened during the recent Republican Convention. [EH to VB: INADEQUATE.] [VB to EH: WHAT MORE DO YOU WANT?]

1) THE PATTERSON COMMITTEE
IKE'S DEFENSE OF THE
JOINT CHIEFS & BUSH'S
STAND AGAINST THEM

* * *

Question B10: Assigning of Patents....Tufts

This is Question B10. I never made any money out of patents directly. One time I was before the Senate's Anti-monopoly Committee and someone asked me how many United States Patents I had in my name. I said, "I don't know, I haven't counted them, maybe twenty." One of the senators said, "Did you make any money out of them?" The chairman, Senator O'Mahoney said, "You don't need to answer that question Doctor." And I said, "I'd like to answer it. The answer is no." [X-REF BACK TO P. 67]

Well, I never did make any money out of them directly. I assigned them, of course, where I was consulting and I assigned them to the company they pertained to. I never signed contracts to do this; I merely did it. The fact that I had no contract to do it left me in the position where if there was any doubt as to whom this or that affair belonged I could do the judging rather than to have some sort of a hassle. [X-REF BACK TO PP. 67-68]

Now you've got some questions -- to what companies? Well, the early beginners were Raytheon and Metals & Controls and various other outfits I was connected with; of course later and in connection with any research that I did there. At MIT, for example, I assigned to MIT. I assigned them some things that I think are pretty good although they never came out to amount to anything.

- 1) BUSH ASSIGNED
PATENTS TO
PERTINENT COMPANIES
-- MIT, RAYTHEON,
METALS & CONTROLS, etc.

When you assign a patent, you sell it -- or, as in my case, give it away. To assign merely means that a man or a company to whom you sell it stands in your place as the owner of the patent. You can sell it either on the basis of outright cash or some sort of a royalty basis. When you assign, however, you abandon your rights completely in return for whatever terms you put into the assignment contract. Of course, you can also keep the patent in your own hands and license either exclusively or non-exclusively on a royalty basis.

What's the quid pro quo? Well, it could be anything under the sun. For example, I now have some patents on hydraulic machinery and I've assigned them to Stewart Warner, and Stewart Warner has to pay me royalties. Incidentally they have to start paying minimum royalties this year. That's the quid pro quo in that case. Back in the days of early consulting, however, there was no explicit quid pro quo because the consulting fee covered the matter. Of course in my case and with companies that were just formed and so forth, I not only got a small consulting fee, I also got a stock interest by options and by stock payments in lieu of a fee and so on. This method of handling things is the usual affair among consultants. A consultant doesn't go in and get all the secret information concerning the company and turn around and make an invention in their field and then consider that he owns it. He turns it over to them, quite properly.

- 1) VB EXPLAINS
"PATENT ASSIGNMENTS"
- 2) THE ETHICS OF
PATENTS BY CONSULTANTS

When I was at Tufts, I invented, if you wish, a machine that would draw a profile and I used it as a master's thesis, I believe. [The machine is still in existence in an attic in Provincetown.] I filed a patent application on it and in due time the patent issued. The patent has since expired of course. I never got any money out of it and I never got any agreement with anybody concerning it.

But out of this experience I certainly did learn a good deal about how patents are obtained, what they amount to, how you handle them and how difficult it is to go anywhere with an invention. I learned, in fact, a very important thing I've already recited: namely, that an invention standing alone is not very likely to be successful. It has to be joined up with a promoter and either the inventor himself has got to be a promoter or he's got to be associated with someone who is. And finally, it takes management and this comes about in most cases if the promoter succeeds in interesting some large company to take the thing over.

[X-REF BACK TO P. 570] In my profile machine case I didn't do any of these things but I did learn, let's say, how not to do it.

The gadget itself (i.e. the profile machine) is still in existence in an attic down in Provincetown where my sister lives. What it was was a little box (incidentally, I built the thing myself down in the laboratory) which had a pendulum in it that was oil-damped and an integrator. Mind you, this was back in 1913 when I built a servo-actuated

- 1) FLASHBACK--TUFTS
- 2) THE PROFILE MACHINE
- 3) VB LEARNS ABOUT PATENTS

integrator. It was a great many years after that the same idea came into the differential analyzer. This was a servo-controlled integrator because it had a disk which rotated and two little rollers that rested on it. These two rollers picked off the vertical and horizontal distances that the thing travelled. The box was swung in a little frame between a couple of bicycle wheels and one pushed it around.

Now that kind of a thing can't drive much without the little rollers slipping. Hence the principal roller was driven by a spring which was kept wound up by a sort of a slipping clutch thing. If the roller was lifted off the disk it would just barely rotate under the influence of this spring. Hence the disk could guide it rather than driving it and this made it have some precision. The machine wasn't any too darned bad. It made its record on a roll of paper with a pen. I could run the thing around the block and it would come back to the same elevation at which it started. I even ran it across rough fields and it would still hold its precision pretty well. Running it down the middle of a street, if I went over a manhole cover the record would show the little bump of the manhole cover. So it wasn't a bad gadget at all. I thought the thing had a lot of use; for example in making contours before roads were built and so on. It was a lot better than getting out with a level and an ordinary surveying rig. But I couldn't interest anybody in it although I tried in a weak sort of a way to do so.

- 1) FLASHBACK--1913
- 2) THE PROFILE MACHINE

Question B11: Unobtrusive Patents....R.R. car knuckle-coupling

Why, railroad car knuckle-coupling was a very fine idea and it certainly was successful. I judge it made a lot of money for somebody although I don't know who. But I can give you another side of the thing you're talking about here. Knuckle-coupling which is old as the hills [EH to VB: DATES FROM 1880's, I THINK.] has been used right up to the present day. It's one of the reasons why railroad cars shake up their freight. It's one of the reasons why there are all sorts of activities today to try to prevent damage to the freight due to the joggling way in which one handles a long line of cars by backing up the engine to take out all of the slack and then starting ahead bumpety-bump down the line -- to say nothing of other things that they do to cars which are absurd.

One of the things that's being worked on today is a coupling that has shock absorbers in it and this should have been done forty years ago. It ties right in with the idea that the greatest improvement ever made by the railroads themselves was when they put a slot in the toilets of the parlor cars into which one could drop old razor blades.

[EH to VB: VAN: I REMEMBER A VERY FUNNY STORY OF the prize offered at a Master Mechanics Convention in Chicago about 1885; prize for coupling which had to successfully withstand drawbar pull of X-pounds; had to stay coupled on

- 1) RAILROAD CAR
KNUCKLE-COUPLING
- 2) MECHANICS CONVENTION
COUPLING PRIZE

gradient of Y degrees and on a curve of Z radius (sharp).

All entries by one flunked. Just before prize was awarded to winner it was discovered that winner could not be un-coupled except by four sledge-hammers. Prize withheld.

[A better coupling by A.D. 1900 and roller bearings for freight and passenger car axles by 1915 could have saved the railroads.] [END EH ADD]

Most of the improvements on the railroads at least in my time have come from outside the railroads themselves. [EH NOTES: G.M; ELECTROMOTIVE CORP.] This is probably due to the fact that it has been generally felt in the railroads that the men who hold the higher positions must come up through the roundhouse. Railroads have not had research laboratories and they haven't brought into their organizations young keen individuals who could take a fresh look. This happens to all old organizations of course if they don't look out. It happens particularly to railroads because the railroads were over-regulated. [EH to VB: WHAT AN UNWORTHY REMARK!]

There are dozens of stories about patents that show the queer ways in which the system works. One of the allegations is that companies suppress inventions. That is, if they're manufacturing something and a better thing comes along they buy the patent on the new thing and put it in their safe and continue to manufacture the old thing. Well, there's nothing in this. It may occasionally happen, but not much. The reason is that a company that's got any sense knows that it's in a very vulnerable position if it tries to

- 1) MECHANICS CONVENTION
COUPLIN PRIZE
- 2) RAILROADS NOT FAMOUS
FOR SELF-IMPROVEMENT
- 3) COMPANIES SELDOM
SUPPRESS INVENTIONS

do such a trick. [EH to VB: YES BUT WHAT ABOUT YOUR OWN inability to improve automotive engines because of no Detroit incentive? End result just as lousy, n'est-ce pas?]

[VB to EH: THE FACT THAT AUTOMOTIVE COMPANIES DO not stretch to introduce new engines is a matter of patent pooling, and the enormous cost to a new company trying to enter the field. What I'm talking about in this last paragraph is something different. It's been alleged many times that companies patent things, or buy patents concerning them, in order to suppress them. They won't make the device and no one else can. I've never seen a clear example of this.

[True, the railroads were over-regulated. They still are. Note a decision only a few days ago to prevent two railroads from merging. It would admittedly save money. No question of the evils of monopoly are involved, since these changes are regulated. Certainly railroad managements have been stupid. Of course in the early days piracy was rife. But that doesn't blind me to the fact that management by bureaucracy doesn't work for the public interest either. We have a good example in the public utilities today, enough regulation to protect the public against monopolistic exploitation but not a regulation in detail to stifle initiative completely.] [END VB ADD]

[REPEAT FROM TOP OF THIS PAGE] do such a trick. Because if there's one way to round a situation, there are probably half a dozen.

- 1) COMPANIES SELDOM SUPPRESS INVENTIONS
(Continued)
- 2) RAILROADS SUFFER FROM OVER-REGULATION AND POOR MANAGEMENT

I think the best example of this that I know of is the old vacuum tank that was used on automobiles. This was a complex device. A can up under the hood had a float valve in it. When this float valve dropped to a certain level it caused the vacuum from the manifold to such gasoline out of the tank and into the little can. Then when the can was full the float valve went up and stopped this operation. All the time the little can which was set above the carburetor fed gas into the carburetor by gravity.

This was used on all automobiles for years and it was an infernal device; it had numerous ways of getting out of order. I had it happen on my car where the valve would quit functioning and it would keep on drawing gasoline out of the tank and flood the whole place. It was an expensive device and all the rest of it.

The company that manufactured this made no changes in it whatever over the years; it just went on manufacturing and selling the same thing. One fine day some chap made a reasonably decent little gasoline pump that was attached to the engine and the days of the vacuum tank were suddenly over. The company that made the vacuum tanks one day was running a factory full blast and the next day had no business. This is bound to happen to any company making a single device covered under patent that sits tight and thinks it has the world by the tail.

- 1) COMPANIES CANNOT AFFORD TO SUPPRESS IMPROVEMENTS
- 2) DEMISE OF THE INFERNAL VACUUM TANK

Of course the way to avoid it is the way the United Shoe Company avoided it. They had a monopoly of shoe machinery so complete that when a new device appeared, its inventor had only one customer: he'd have to sell it to United Shoe at their price. [X-REF BACK TO P. 292] Quite properly the courts have broken up this kind of thing. While the single patent quite properly can create a monopoly for a limited period there's nothing in the patent statutes that talks about the combination of many patents and improvement patents. The courts have held that a combination of patents in restraint of trade is something that is frowned upon.

The courts I think have done very unwise things in regard to patents largely because they don't understand the patent system. But on this particular point, certainly it is true that the perpetuation of a patent monopoly by the acquisition of the new things coming out is certainly not in the public interest. It's one of the things that I put into my reports [EH NOTES: GET REFERENCE] wherein I dealt with possible modernization of the patent system.

- 1) SINGLE-PATENT MONOPOLY
LEGAL FOR A LIMITED
PERIOD
- 2) MULTI-PATENTS MONOPOLY
A RESTRAINT OF TRADE
(UNITED SHOE)

* * *

Question B12: deForest; "Edison Three-Electrode Effect"

DeForest really made an innovation. He did put a third electrode in, and he did get control so that he made a real contribution and an important one. He was the fellow who originated the three-element thermionic tube. [VB ADDS: I'VE SEEN AN ARTICLE RECENTLY (in Scientific American I think) on deForest which puts even this statement in doubt. Apparently someone earlier put in a third electrode but didn't do much with it.] [X-REF BACK TO P. 499]

Edison noticed (and I don't believe it's folklore, I think it's probably true) that when he had another electrode inside the bulb with his lamp filament, he got a little current in that other electrode. I don't know whether he put a voltage on it or not but whether he did or not he would have found a voltage present because the evaporation of electrons from his filament would have resulted in some of them landing on his other electrode.

So Edison had a two-electrode affair and did nothing about it. Fleming, however, took this two-electrode affair, realized that it was a good rectifier, and used it as a detector in radio sets in the days when rectifiers were used for that purpose. Lee deForest did something real. [X-REF BACK TO P. 499]

Following this, a long time later the hard-vacuumed tube came along. It came along merely because good vacuum pumps by now were made. The early tubes did not have a hard

- 1) deFOREST's 3-ELEMENT THERMIONIC TUBE
- 2) EDISON DROPS THE 2-ELECTRODE MATTER
- 3) FLEMING USES THE 2-ELECTRODE TUBE AS RECTIFIER IN RADIOS

vacuum and hence they were not particularly good tubes. When the hard vacuum came along various people found out that you can make a better tube that way.

The theory of the tube prior to that time very often introduced the presence of a little gas into the discussion. But, theory or not, when the hard vacuum came along due to the manufacture of better vacuum pumps, hard tubes were bound to be made in the laboratory. They were bound to be made and various people were bound to find out that they were better. This happened and it happened in the General Electric Company and in the Bell Laboratories.

The General Electric filed a patent under the name of Langmuir and the Bell Telephone under the name of Arnold. It soon appeared that the interference might go either way and it also appeared that a valid patent was likely to issue. How the Patent Office could possibly conceive that there was invention involved in getting a better vacuum when people had been trying for generations to get better vacua is more than I can understand.

The fear on the part of the Telephone Company that the patent might issue to Langmuir and that this might interfere with their whole repeater business for long-line communication was the reason, apparently, that AT&T joined in with RCA, General Electric and Westinghouse in a cross-licensing program which incidentally very nearly put the Raytheon Company (then operating under a different name) out of business. This was certainly a violation of the Sherman Act.

- 1) G.E. & BELL TEL. FILE FOR PATENT ON HARD VACUUM TUBE
- 2) SHERMAN ACT VIOLATION: AT&T, GE, RCA, AND WESTINGHOUSE IN CROSS-LICENSE MOVE

I didn't have a great deal of respect for the patent department of the General Electric Company at any time. Part of this was due to an experience that I had. At one time while I was at MIT I made an invention and filed a patent application on it connected with power cables. It wasn't much of an invention but at any rate I thought I had something that was some good. I made some samples and one time I talked to Gerard Swope and said that I had a thing there that I thought was pretty good. He referred me to his people in Schenectady and I talked with them. They were not interested. But when my patent was about to issue I found myself in interference, in a seven-party interference, and three of the parties as I remember it were General Electric employees. Their patent applications were filed shortly after I had visited Schenectady and were based on memoranda in their files.

Well, they didn't know that I had filed a couple of years before ahead of the time that I spoke to Gerard Swope. So when the cards were all on the table, I won the seven-party interference without any contest because my filing date was ahead of their alleged date of conception in their memoranda. So I got a clear patent.

I made a deal with a crowd that was going to develop it and they were due to pay me some royalties and they didn't pay them, so I brought suit against them. The suit came up in the Philadelphia court. When the opposing lawyer came in he and the judge chatted together and the court admitted all

- 1) FLASHBACK--MIT
- 2) V.B. versus G.E.
IN SEVEN-PARTY
PATENT INTERFERENCE
- 3) SUIT re. PATENT IN
PHILADELPHIA COURT

of the testimony of my opponent and ruled out most of my testimony. And the court promptly found against me. It was so balled up that my Philadelphia attorney refused to take a fee on the basis that I really hadn't had a trial. So I didn't get anywhere except to get a good deal more experience about how one handles a patent and also a bit of instruction as to what can happen in the Philadelphia courts and why it's probably unwise for an outsider to go after the scalp of a Philadelphian under Philadelphia law, at least as things operated at that time.

1) LEARNING ABOUT
PATENTS

An experience in this world in regard to the ways in which things are actually happening often is very expensive. This is particularly true of course of a chap that has a keen inclination to learn how to play poker -- which I never did. But the experience that I got was not expensive. It was not expensive except in my devotion of a good deal of time to things that didn't pan out. It wasn't expensive because the property that I lost was patent property that I had also created. So I'd recommend that anyone who needs to learn about the patent system ought to make a few inventions when young and try to follow them up.

Incidentally I once gave a lecture on the patent system at MIT to a bunch of freshmen that you might find amusing if we get time to put it on a film. [JK to EH:WANT THIS?] [EH to JK: YES] [VB to EH: I FEAR IT IS LOST, BUT I COULD TELL HOW I CAME TO GIVE IT.]

Question B13: G.E.'s Charles Proteus Steinmetz

Oh yes, when I was a test man with General Electric Company in Schenectady, Charles Proteus Steinmetz was the great name. There was a lot of publicity as you know, about the rules for smoking in the plant that applied to everyone except Steinmetz and so on. [EH to VB: AHM: V BUSH SMOKED WHILE A SUB-INSPECTOR FOR USN AND COMMANDER MacDOWELL.]

[VB to EH: BUT I WASN'T ADVERTISING THE NAVY DEPARTMENT.]

That was part of the old, old game of taking an individual's name and blowing it up for company publicity. I can make my comment about Steinmetz in this way. He wrote a book about circuit theory. In the book there was nothing new when I read it, whatever. And there were many places where it was badly stated, or else incorrect. I never regarded Charles Proteus Steinmetz as a genius, and I don't know of anything that he created in the field of electrical engineering that was really unique.

There's a bit more of the story about General Electric's patent department that I ought to get into the record although I'm pretty sure that it shouldn't get into print. During the war I worked up two kinds of contracts: one was entered into with the nonprofit organization; and another one was entered into with industrial units. They naturally differed quite a bit. Each one of them contained a patent clause and I created these patent clauses for the first time. The principles involved have been followed

- 1) FLASHBACK--BUSH
AS G.E. TEST MAN
- 2) STEINMETZ NOT
A GENIUS
- 3) PATENT CLAUSES IN
WARTIME CONTRACTS

pretty much since in Government contracting in regard to research.

When Irvin Stewart and I got out the patent clause for use with industrial units, every company that we were dealing with accepted them without question, except the General Electric Company and General Motors. We didn't mind the situation on General Motors because we didn't have any idea of contracting with them anyway. But with the General Electric, yes, we probably would and should. So we minded.

Down came some of the patent counsel of General Electric to protest to me about the patent clause in the proposed contract. [X-REF AHEAD TO P. 725] The fellow leading this group was a chap who was head of the patent department of General Electric Company when I had my amusing experience on the interference and when I'd been trying to sell the cable patent to GE back in the old days. He was still around. He sat across from me in my CIW office in 1530 P. Street and orated at length about the unfairness of the patent clause and its absurdity and the fact that I didn't really understand and so on. I just let him go on and on and sat back and listened. After a while he said, "What's more, the Commissioner of Patents agrees with my position." And I said, "Well that's interesting. In fact, that's fine. You just get a letter from him saying that and I'll change the patent clause, and that concludes our conference."

- 1) WWII
- 2) PATENT CLAUSES IN WARTIME CONTRACTS
- 3) G.E. OBJECTS TO V.B.

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They all went out and when they'd done so, I called up the Commissioner of Patents. His name was Conway Coe and he and I were great friends. We had a good laugh over it because Conway Coe himself had drawn up the patent clause to which the General Electric Company objected. (They weren't very clever, because Conway Coe was a member of NDRC at that time, as I remember.

- 1) G.E. OBJECTS TO VB's
WARTIME CONTRACTS
PATENT CLAUSES
- 2) CONWAY COE WAS
COMMISSIONER OF
PATENTS

* * *

2
Question B14: Spencer Thermostat....New York Test....Spencer Company....Raytheon Company....Key Invention.

The story on the Spencer thermostat I've told, here and there, to a considerable extent. The excursion to New York was one that L.K. Marshall and I made soon after the affair was developed and we were trying to get some customers. We went down to make a demonstration. We only had one model and we went down on the Fall River Line, I judge. In the morning in the hotel before the meeting we thought we'd better try the hickey out and be sure that it was operating well. We forgot that the hotel was in the DC area, and the thermostat would only operate on AC. So when we plugged it in, it promptly burned the thermostat up. Whereupon we made our apologies and took the train back to Boston.

The story of how the thermostat finally came into use is very interesting. It was the first thermostat that would operate a flatiron successfully, and it obviously had a very good market. There were about four or five flatiron manufacturers. They had all been together under the nichrome patents. The nichrome patents [VB NOTES: NO HYPHEN IN NICHROME] had expired but they still kept up their relationship pretty clearly. We approached all of them, I guess, or most of them, with the idea of getting them to manufacture the Spencer type of iron on a royalty basis. We got nowhere. It was quite a while before we tumbled to the fact that probably they'd gotten

- 1) NEW YORK DEMONSTRATION OF SPENCER THERMOSTAT
- 2) SELLING THE SPENCER THERMOSTAT TO THE FLATIRON COMPANIES

together and had just agreed that no one of them would take the thing exclusively, that anyone who happened to be the one to pick it up from us would proceed to license the others. This of course gave us a beautiful situation where we had no customer, because no one of them could afford to pick it up and pay the cost of taking it over and then pass it out to all of his competitors on the same basis on which he would be manufacturing it.

We thought that was the case. Of course we couldn't prove anything. But we started a counterattack. We spread some rumors that J. P. Morgan was backing the Spencer Company and that we were going into manufacture. This wasn't too hard to spread as a rumor because people knew that J. P. Morgan had been one of the backers of the Raytheon Company and it was the same group involved in Raytheon and Spencer.

Our directors, our stockholders, were people who moved around a good deal, such fellows as Jeff Coolidge, Bill Claflin and so on, and the rumor got around pretty well I think. Then we approached the Graybar Company on a proposition that when we got into manufacture they would handle the distribution. We had some tentative negotiations with them and the story went around pretty well. When we got that going well, we again tackled the Westinghouse Company. They seemed to be much more open to reason.

I remember when Marshall was in Pittsburgh talking to them and I was in the office in Medford -- well in Cambridge, I guess it was, by that time. The conversation between us

1) SELLING THE SPENCER
THERMOSTAT TO THE
FLATIRON COMPANIES
---WESTINGHOUSE---

would go something like this. Marshall would call up and say that they've made such and such a proposition, they want to have such and such a clause in the contract and I'd say, "Well, I'll get in touch with the other chaps on the board and I'll let you know what they think about it. Why don't you call me again in fifteen or twenty minutes; I think I'll know by then." He'd call again and I, not having moved out of the office or talked to anyone else, would say, "We all think it's the bunk. Apparently Westinghouse doesn't realize how well we're set up here now and I personally don't think we ought to abandon our plans." We'd have quite a conversation thinking, perhaps erroneously, that since he was talking from the Westinghouse Plant it would be strange if someone wasn't listening in.

We'd put on a pantomime of that sort. After a while they signed up with us; they manufactured the things and paid us royalties. Before we got through they paid us quite a lot of royalties. As soon as they had made the contract they released a publicity statement in which they implied that they paid a million dollars for the patent. They hadn't paid anything to amount to anything by that time, but that story went around and it got talked about as the million-dollar thermostat.

Before they got through paying us royalties they paid us a good deal more than a million dollars in royalties. That put the Spencer Thermostat pretty well on its feet and it did pretty well thereafter. [X-REF BACK TO P. 562] After

1) SELLING THE SPENCER
THERMOSTAT TO
WESTINGHOUSE

2) L. K. MARSHALL

a while it combined, merged with a group in Providence, Grinnell and that crowd that had the General Plate business where they made gold plate for jewelry and that sort of thing. That formed Metals and Controls.

There are a couple of stories about that that are worth telling. The man who handled this show down at General Plate was old Russell Grinnell. When he presided at a meeting, he sat up very straight in the chair and presided with great formality. He was a good old New England businessman. There were two things that happened that I thought were perfectly swell: one, when we entered into the agreement we had an issue of preferred stock in the combined company. The Providence crowd picked up their share but the crowd in the Spencer outfit didn't have enough money in the Spencer treasury to pick up theirs. So the Spencer stock was put in escrow and our obligation on the preferred stock was to be retired as we got dividends.

This would have been fine except that the great depression came along, and in the middle of the great depression things came to a head. We had a meeting about it. The situation then was such that if Russell Grinnell merely sat tight he could take over the Spencer interest because the Spencer group could not meet their obligations. The stockholders of the Spencer group were disinclined, under the pressure they were all under at that time, to put up any more money to bail them out.

- 1) SPENCER MERGED WITH GRINNELL TO FORM METALS & CONTROLS
- 2) RUSSELL GRINNELL OF GENERAL PLATE & THE PREFERRED STOCK OF THE SPENCER GROUP
---THE DEPRESSION---

So we met. Russell Grinnell opened the meeting and said, "Gentlemen we have an agreement between the Cambridge group and the group in Providence. When we entered into it, we felt all of us that it was an entirely fair and equitable agreement. As things stand today none of us would enter into such an agreement and we would not think it was a reasonable one. I think therefore that we ought to discuss this matter as though the agreement was now being entered into, as though it didn't exist." In other words, having the Spencer Company completely in his hands he said, "I will not take it over by that kind of means." And that's your good old New England businessman at his best.

The voting power was equally divided at that time between the Providence group and the group in Boston. When Russell made his speech and after we had worked out a plan that took the Spencer group off the hook, I spoke up and said, "Mr. Grinnell, the voting power in this company has been equally divided up to the moment. I merely wish to assure you that from now on the voting of the stock that I hold personally is entirely in your hands." In other words he said, "I won't take over the company by reason of a misfortune that none of us could have foreseen," and I replied by saying, "Very well, as another gesture of this sort you now control the company." Which he did and it went on from there with the General Plate crowd or rather Grinnell himself controlling.

- 1) METALS & CONTROLS
BOARD MEETING ON
SPENCER STOCK
DURING DEPRESSION
- 2) GRINNELL'S ETHICAL
& GENEROUS BEHAVIOR
- 3) VB RETURNS THE FAVOR

This went on when old Grinnell died and Rathburn Willard was president of the company. Everything went well until Willard began to slip and got into rather a pathological state. Then after a great struggle (and this was after I'd come back to New England) we finally merged the show with Texas Instruments.

As an incident in the thing, Marshall opposed that merger and opposed it rather vigorously. Although he was not on the board and he was not in the group, it was quite difficult to get the merger through over his opposition.

[X-REF BACK TO P. 562] [JK to EH: I DON'T THINK THIS CAME UP BEFORE. MENTION OF MERGER ON P. 562 BUT NOT OF MARSHALL'S OPPOSITION.] I think also that immediately after the merger he sold out his interest. I'm not sure of this but I rather think he did because he had opposed the thing and felt that it was not a sound merger. If he did he sold out before the stock went up and it went up ultimately to almost four times its original value. It's now back down to about where it was at the time the merger was completed. This is a little side issue on the type of vicissitudes that small companies go through.

At the present time I understand that the Metals and Controls Division of Texas Instrument is the most profitable division they have, that it is going great guns and that they are very proud of it. All it really needed was good management. It did not have it while Willard was beginning to slip and things were going bad. Since then

- 1) RATHBURN WILLARD
SUCCEEDS GRINNELL
- 2) METALS & CONTROLS
MERGED WITH
TEXAS INSTRUMENTS
- 3) MARSHALL OPPOSED THE
TEXAS-METALS MERGER

I've moved out and I no longer own very much of the stock of Texas Instrument. But it was quite a show. Having been in the early days of both Raytheon and Spencer Thermostat as I sit today I've got no interest in Raytheon and not very much in Spencer Thermostat and that very indirectly.

Spencer died some time ago. He had a brother. The brother's name was Percy and Percy was just as bright as Al was although a very different individual. Percy worked for Raytheon and during the war he developed the strap magnetron which is a great advance in magnetron affairs. [EH INDICATES QUERY] He was working with some of the top-line physicists and they all respected him. Here was a man who came up the hard way and who had learned his physics as he went along. He worked with some of the top people on magnetrons and made a key invention in that field.

One of the most pleasant things that I ever had happen to me was when the University of Massachusetts gave Percy Spencer an honorary doctorate. It was well deserved. He became executive vice president of Raytheon; he's now retired. He's a very likable fellow. I can tell you a rather interesting story about one time when Percy doped the gallon of water from the Great Salt Lake with a bit of cesium. But that will have to come in at some other time. It's really got not very much to do with our narrative except as we may want to detail the possibility in this country of a

- 1) VB's STOCK IN RAYTHEON AND SPENCER
- 2) PERCY SPENCER'S STRAP MAGNETRON

chap making his own way and doing so by being ingenious.

[JK to EH: WANT THIS?] [EH to JK: YES.] [JK to EH:
HAVE IT ON ADDITIONAL REEL.]

1) PERCY SPENCER, THE
SALT LAKE WATER, &
THE CESIUM

[VB to EH: BRIEFLY. In Raytheon we got out a thermionic tube which contained cesium metal, which made it work. Cesium was scarce. We had six or eight pounds in the safe, at \$200 a pound, but didn't dare go into manufacture on that, even though it took only a few cents worth per tube. We tried to get a hook on some in Paris Maine [?] etc. where there are pockets containing cesium, and found G.E. had this source tied up. They evidently had a cesium tube also. Someone had the idea that there might be enough cesium to be recoverable in salt lakes (since we could afford several hundred dollars a pound). So we had a bottle come on from the Great Salt Lake. A quick test showed a trace. So a bunch of us, excited, gathered around, boiled some down, tried a spectroscope, and Lo, here were bright blue lines from cesium.

[But it was too good, and we were suspicious. So we had a hogshead come on, with the thought we would be delighted if someone salted that; more tests, no cesium.

[A year or two later, after progress of the art [? ILLEG.?] had made the cesium tube obsolete, I was working at his bench with Percy Spencer. With no preliminaries, I said, "Perc, how come you salted that bottle of Salt Lake water?" "Well," he said, "since you know, I'll tell you.

Reel 9-B
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I was working on a tube and had some cesium in a little saucer. You put the bottle down right side of me. Then Marshall and C.G. and some more of you acted so excited that, before I thought, it occurred to me I ought to give you a hand, so I dumped a bit in. Then you got so stirred up I didn't dare to tell you." [END ADDITIONAL MATERIAL]

Percy also lived a rather tough life for a long time, his wife was slipping badly. Today he's got three bright sons all doing well; he's remarried and he's a very happy chap.

1) PERCY SPENCER & THE
CESIUM IN THE SALT
WATER (Continued)

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END REEL #9