 First Retype, October 14, 1965

Question 16: VB's Progenitors and His Ability to Administrate

1) EARLY HISTORY
2) PROGENITORS

I'm certainly a Cape Cod Yankee. As a matter of fact, my family goes back for seven or eight generations on the Cape, and so does Mrs. Bush's. (X-REF to 638 and 713) Very naturally, most of the crowd were a seagoing outfit.

One grandfather took a ship to the coast of Africa as captain when he was 21. The other, I have heard, was captain of one of the three ships that first traded up the Amazon. My grandmother sailed with this latter. As near as I can make out he ran the ship and she did the business, trading with West Indies. She was a grand person. She lived with us until I went to college, went blind, nevertheless would not quit, and finally got killed.

1) EARLY HISTORY
2) PROGENITORS

It might possibly be that inheritance has something to do with one's characteristics, for for all of the recent ancestors were sea captains, and they have a way of running things without any doubt. So it may have been partly that, and partly association with my grandfather, who was a whaling skipper. That left me with some inclination to run a show, once I was in it.

I was not a particularly husky youngster and I had a series of difficulties: rheumatic fever, which was supposed to leave me with a bad heart but which didn't, although it left me with rheumatism which cursed me for years, so that occasionally I had to drag a leg behind me; typhoid fever, ruptured appendix, and what have you, in addition to the usual children's affairs.

1) EARLY HISTORY
2) FATHER

This had the result, for one thing, that I lost a year out of high school, and I lost a half year out of college. The latter resulted in my taking the master's and bachelor's degrees in three and a half years, and then of course I took the doctorate with one year more at MIT. I can go into some more of this a little later perhaps.

The real key to the situation is that my father, who was a minister, sent two of my sisters through college before I got there. This pretty well knocked out the family funds, so that by the time I got going there wasn't much money around; all of my academic training was circumscribed by the necessity for getting some cash.

Now of course I'm essentially a mild chap, but I'm occasionally charged with being belligerent. If there's anything in the rumor,

1) EARLY HISTORY
2) ABILITY TO ADMINISTRATE

I suppose the psychologists would ascribe it to the fact that I was ill a good deal of the time as a youngster. I personally would be much more inclined to attribute it (if it's true) to the fact that my great-great-grandmother came from County Kerry. It was not unusual for the old skippers to bring wives back from overseas; this kept Cape Cod, for a long time, from being too ingrown.

Now the last part of this question has to do with my ability to administrate. For one thing there's a difference between administration and the practice of executive authority, and I think it's the latter that I lean to. It's utterly impossible, on the basis of self-analysis, to tell how one acquires various skills (if he does) but on this particular point I can make a couple of

comments. There are a lot more chaps in the world who think they want responsibility than there are who really want it or who can handle it when they get it.

1) ABILITY TO ADMINISTRATE
2) EARLY HISTORY
3) FATHER

I'm inclined to think that in a small group, where one chap takes over the leadership almost automatically, it's usually because that

(ital) chap is willing to take the <u>responsibility</u> of leadership. Of course also, the exercise of executive authority, the skill of delegating, and all that sort of thing, undoubtedly stems from an interest in people.

Now here I'm sure that I acquired a great deal from my dad. He was a clergyman, and a very good one. He had in interest in all the people about him, and he understood them pretty well. This didn't mean just his parishioners, and it didn't mean just the upright citizens in the community. He was interested in the

chap who kept the saloon down the street, and he understood him too. He had an uncanny sense of how to work with people of all sorts, and I saw him do it.

1) EARLY HISTORY
2) FATHER
3) JOHN VANNEVAR

In days when it was hardly ever done, my dad worked hand-in-glove with the local Catholic priest, Father Powers, in civic affairs, such as taming saloon keepers. Incidentally, my father (ital) worked his way through Tufts by running a coal business. Students had stoves in their rooms, sometimes three flights up, and he delivered the coal on his back. Tutoring was a much easier way to do it.

Question 17: John Vannevar and how to pronounce the name

My name is mispronounced a large part of the time. I remember one time I received an honorary degree; after the ceremony the president of the college told me he felt he should be congratulated on the care with which he had pronounced my name. Well, I told him he certainly

ought to be congratulated, that I had noticed it very carefully. He'd pronounced it four times, each time the same and each time wrong. I really don't mind, of course, but the name I think really has the Dutch pronunciation which is Vann-EE-var. The family of Vannevars went to England with the Huguenots, and then some of them came to this country. At least, I've always assumed so.

1) EARLY HISTORY

2) FATHER AND JOHN VANNEVAR

Now John Vannevar was a boyhood friend of my father's. They more or less grew up together, and I think my dad protected him in his youth. My dad was instinctively a fighter even though he became a clergyman, but John Vannevar was more retiring. The two went to college together when they were in the divinity school. Both went into the Universalist ministry. Now apparently when John Vannevar was a boy, one or possibly both, of his parents died. To get this story accurately, we have to get at one of my sisters, because I'm sure I don't know it

with any accuracy. (REMINDER) But John Vannevar became almost a brother to my father, and I think he was to a considerable extent taken care of by my grandparents,
* although as I say, I can't detail this.

Well, that's where I got the strange name. If my parents had only given me the full name it would have been excellent, for then I'd have been called John. In fact my sisters usually did call me John when I was a young fellow, but my parents left off the first part and got me into all sorts of trouble doing it. I can't say it's been a real handicap, but it certainly has been a nuisance at times.

1) EARLY HISTORY

2) FATHER AND JOHN VANNEVAR

3) TUFTS

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Question 2: Undergraduate years at Tufts

This question has to do with Tufts and my undergraduate years there. It was the usual sort of experience, except that I was always short of money. My dad put up all he could afford, but

this wasn't very much. I had a scholarship, and that helped. I started in washing dishes, and that sort of thing to piece out the funds. This was kind of tough going until I got onto a really good source of funds. This was tutoring. I tutored individually and I also "held" small classes. It paid out pretty well. I'd have a class in the evening, and everyone who came in would put fifty cents on the barrel-head. Some of these would get the fifty cents back at the end of the hour as a rebate. These were the ones that really didn't have the money to pay; their job was to drum up trade. I can remember one chap who was a great football star of the day, standing in front of the chapel among a group of admiring freshmen and telling them, "Why, you doggone fools, you can't (ital) possibly get by that course without taking Bush's tutoring sessions." So this drove the freshmen into my sessions in good shape. Later

1) EARLY HISTORY
2) TUFTS
3) TUTORING

on I actually had the nerve to tutor in courses I was also taking. (Of course the college didn't know anything about this.)

1) EARLY HISTORY
2) TUFTS
3) TUTORING

But I also worked for the college. When I was a sophomore, freshman mathematics had what was known as a mathematics laboratory. It was a place where the freshmen did problems, and assistants circulated among them, and helped them out when they got stuck. I was one of these assistants that worked in this laboratory, and got paid a pittance. Professor Ransom was head of the mathematics department and a delightful chap. He never was a great mathematician any more than I was a mathematician essentially but he was a fine teacher. One time when I was in the laboratory helping the freshmen, he told me that he was in quite a spot. One of his professors was ill, and another one was away and he had no one to take a particular class next day. So I said to him, "Why don't I take it?" "Well," he said, "You couldn't

possibly control that group of freshmen." I didn't respond, but I knew I could. As a matter of fact I was one of the last men on College Hill that the freshmen would have tried to cut up with. In those days there was such a thing as hazing, and anyone who raised hob with me in a class might possibly have found himself tossed into the reservoir that night. ~~Lamson~~ RANSOM was appalled at my idea and said it would be terrible and, "Why the president certainly wouldn't approve it". I told him I didn't think the president needed to know anything about it. So I taught the class for a few sessions, and got along quite well. There may be something unique here, because not many times has a sophomore taken over a professor's class.

1) EARLY HISTORY

2) TUFTS

At the end of one term, instead of me owing the college money, the college owed me money. So I went up to collect it, and the bursar refused to pay me; said he'd credit it

on the next term bill. I told him I needed the money and I wanted it, that he owed it to me and I proposed to get it. He said, "Well, I can't give it to you, and what are you going to do about it? Why," he said, "you wouldn't sue the college, would you?" I said, "Oh yes I would. That would give me great pleasure, sir." Whereupon after a bit more argument he paid me what he owed me.

1) EARLY HISTORY

2) TUFTS

Certainly I managed football. The way in which football was run in colleges fifty years ago and a bit more, was rudimentary and absurd. A faculty dean was supposed to have responsibility for university athletics but in fact he paid practically no attention to it. Then there was a student manager. One of his main jobs was to get enough money to pay for the team's uniforms and equipment. And the way that he got this money was to play teams from large colleges -- who would pay him a so-called guarantee in order to get the small college to come for what amounted to a practice game. In other words, he sold the services of his team early in the season to get enough money to run it for the rest of the season. There was a nice balance to be obtained; if he took on too many of these good-pay games, he would be left with a team in no condition to finish the rest of the season. I was the student manager (fall of 1912) who was supposed to run the show that way. In the course of the season

[graduated 1913; manager football fall of 1912]

1) TUFTS

2) FOOTBALL

3) CROSS-REF
SPORTS ILLUSTRATED
ARTICLE
SEE VB BIBLIOGRAPHY

we went out and played West Point. This was the game in which Eisenhower twisted his knee, and gave it an injury which followed him all the rest of his life. I one time told Ike that I managed the team that bent his knee, but I don't think he thought it was funny at all.

1) TUFTS

2) EISENHOWER

3) SPORTS ILLUSTRATED ARTICLE: BIBLIOGRAPHY

Question 40: Changes in science and engineering education from between post WWI and post WW II, studies at and after Tufts

Tufts College was a fine place in those days, and it still is. It was particularly good for my purposes because it was flexible -- it was very far from rigid as what follows will show pretty clearly. I was supposed to be studying as an electrical engineer, and the head of that department was a Professor Hooper, who was a grand old man. I suppose he probably wasn't very old, but he seemed to me old in those days. He gave a course in the mathematics of circuits, based on a textbook that was about the only thing electrical engineers had in that field in those days. When it came time for the course to start, I visited him and I told him I had the

book during the summer, and thought I could probably handle what was in it fairly well. He said, "Sit down, boy," and right then and there gave me an examination on the book, passed me, and then went to the faculty and got permission to give me credit in the course without having to take it. Tufts was flexible enough so that this sort of thing could be done with great informality.

1) TUFTS

2) SPENCER THERMOSTAT DIGRESS. THIS RECURS LATER

Tufts had some grand teachers too; Hooper was one of them. Now later on, we had some excellent courses in mathematics with Professor Ransom and Professor Dillingham. I roomed -- I think I roomed -- for a while with L. K. Marshall, later the founder of the Raytheon Company (X-REF AHEAD to 559). He had a good head in mathematics, and he also had a good head in physics. When the Spencer Thermostat Company was just getting underway, it was his key invention that made it possible to manufacture the expensive thermostats in quantity so that

they would stand operation at very high temperatures. (THIS LAST SENTENCE IS THE BASIS FOR PART OF QUESTION B14, REEL 9) This made their introduction into the flatiron possible, and it was this which really made the company go. After the first war Marshall became primarily a promoter, and he was responsible for starting quite a few things. I was working with him at the time. I was the technical end of the show, and he was the business and promotional end. But when we were in college his interests were mathematics and mathematical physics. We took a course together in dynamics, and I think we were the only students in it. This course in dynamics led to some of my experiences later in connection with graduate work which I'll recite in due time. [E1 to Y B: When?] Now Professor Ransom was very much interested in non-Euclidean geometry. And instead of just puzzling about it by himself, he got Professor Dillingham interested also, and they set up a course. They set it up quite formally, entered in the catalog and available for credit. I elected

1) SPENCER THERMOSTAT DIGRESSION. THIS RECURS LATER

it, and I was the only student who did. The result was that we had sessions -- just the three of us. We studied a textbook, in German, and went over some of the work of Lobachevski and Bolyai and others. One of the three of us would go to the blackboard and demonstrate a theorem; the other two would sit back and criticize him. For example one chap would get up and draw three lines on the blackboard and say, "Let this be a * triangle with all its sides parallel in pairs and proceed to try to demonstrate the area of that triangle. It was magnificent experience; both men were full of enthusiasm, and they knew the underlying mathematics well enough so that we could go along rapidly.

1) TUFTS
2) MATHEMATICS

There was another course, in which we studied Minkowski's four-dimensional vector analysis, the space-time affair. This was much tougher going, and I'm not at all sure I got it thoroughly grasped, even at the end of the course. But this was before the days of

Einstein's general relativity. When general relativity appeared and I listened to men expound on it, I found that most of the people I listened to were not talking about general relativity at all; they were either talking about non-Euclidean geometry, or they were talking about the space-time concepts of Minkowski. In fact even today I get no great bang of enthusiasm out of general relativity. I don't think physicists generally feel it's by any means the last word. In the American colleges of those days I doubt there was very much good work on the borderlines of mathematics and physics. We were still leaning on Europe. If anyone really wanted to study in those subjects, the thing for him to do was go and take his doctorate at Heidelberg or somewhere of the sort. The atmosphere in

1) TUFTS
2) MATH
3) NON-EUCLIDEAN
4) X-REF SCIENCE PAUSES

American colleges was pretty drab for anyone who was really keen to try and get in touch with some of the newer things. This was certainly not true at Tufts in mathematics for the simple reason that Ransom and Dillingham made it very different. Ransom is still out there at Tufts and I see him occasionally. Dillingham went to the Naval Academy and I saw him there. They are both great men. We'll get some more on this general business of the quantum jump as you call it, later on. [EH to VB: When?] [VB to EH: whenever you ask for it.]

1) TUFTS

2) EDUCATION, U.S. IN TEENS OF CENTURY

I think between WWI and WWII interest in and proficiency in physics and mathematics advanced gradually in this country. WWII itself, and the intense interest in the applications of science, gave it the push which made the extraordinary change that followed the war.

Question 3: Recollections of years and job with General Electric

1) GENERAL ELECTRIC INTRODUCE

Question 3 has to do with recollections of years and jobs with General Electric. There are plenty of recollections, mostly bad ones. When I graduated from Tufts in 1913, I went with the General Electric Company as a test man. I wanted experience. I was aiming to be an electrical engineer, and I had to get in touch with the practical side of things. I spent just about a year at GE; worked first in Schenectady, second in Pittsfield. The place was crude indeed. (QUERY: Which place? Both?) It was also dangerous. The safety precautions for the test men were mostly mythical. We handled high voltages and high currents with the contempt that young chaps will usually have for that sort of thing. We'd string a 2300 volt wire across an iron floor covered only with the insulation one would find on a 220 volt line in a house. I remember one man stepping on one of those wires. He didn't get burned or shocked, but the impact

of the explosion under his feet made him lame. 1)GE We had quite a few burns and flashes and shorts. Things are altogether different now, of course, but in those rough days the test men got little protection and even less instruction. We got a lecture once a week, and they were stupid. I remember one old treasurer -- he'd been retired some years -- who told about how the company had bought some insulation, found it was full of metal scraps, and managed through an agent to sell it to their competitors. This was much to the discomfiture of the heads of tests and much to the joy of the test men generally. We acquired no loyalty to the company whatever; we thought it was crudely managed and crudely run and these impressions have never entirely left me. My starting salary was $11.20 a week. I managed to live on it not too uncomfortably. After I'd been with the company for quite a while, it was discovered that my salary should have been $14 a week, since I had joined with a M.S. degree. The company

paid me my back excess salary all in one lump, which didn't do me any good, because the rest of the test men knew about it, and most of the excess went for a beer party. 1) GE

But this did have an interesting result later. The Test Department had a General Superintendant of Tests, and a General Foreman of Tests, then foremen for the various tests in various buildings -- and then assistant foremen. There was a vacancy for the post of assistant foreman in Building Eleven. So they looked around for a chap to make assistant foreman. When they looked at my name they found I was getting $14 a week: hence I must have been with the company for over a year. So I was made an assistant foreman, when, as a matter of fact, I'd only been with the company three or four months. This was fine by me; it gave me all the better experience. I had a group of men under me, most of them quite green -- one chap from Harvard who'd never studied anything concerned

with electrical engineering at all but who worked out excellently. We tested a whole lot of equipment of all kinds. I stayed quite a while in Schenectady, mostly with rotating machinery. Then I went over to Pittsfield to get some experience with transformers. The safety precautions in Pittsfield were worse than they were in Schenectady, and the management was certainly no better. We had one test man who'd gotten his promotion because he was related to one of the engineers somehow, and had a little pull -- or at least so we all thought. Whenever he went toward a switchboard a yell would go up that every test man got to know. It meant, Stand Clear of All Circuits -- so that no matter what this chap did, he couldn't blow somebody else up. 1) GE (ital)

After I'd been at Pittsfield for a while, they had a fire; it burned up the cables that supplied the test floor. So the company put some of the test men to work repairing the cables, and fired the rest. I was one of those that was fired without notice. So I went up to my boarding house and told my landlady I'd been fired, and lit out for Boston. This experience of course endeared me to the company. Many many years later, I met Owen Young in the White House at one of Eisenhower's dinners, and in the course of conversation told him I'd worked for the company once. He asked me why I'd left the company and I told him, "Because I got fired". "Well," he said, "at least we showed sense in one instance," and that was that. (X-REF FORWARD to 709) But it was a good experience just the same.

1) GE

2) Fired!

Question 1: 1919, Boston Police Strike
Woodrow Wilson's illness, VB's first year at MIT as Associate Professor

Now we come to Question 1, which is all about the Boston Police Strike and Woodrow Wilson's illness and so forth. I think the principal point here is that students in the last generation or two have grown up. They're far more mature than we were in our college days. We paid very little attention indeed to what was going on in the world. Of course we were excited about the Boston Police Strike when it came along, but that was local. In general, we paid very little attention to national affairs, and in fact no attention at all to international affairs. I graduated from college in 1913, and the war in Europe was certainly a topic that you'd have thought we'd be discussing right and left, but this was not so. In undergraduate gatherings there was hardly any discussion whatever of that sort of thing. Now part of this no doubt, was due to

1) NO NATIONAL AFFAIRS

2) NO INTERNATIONAL AFFAIRS

the fact that the faculty didn't pay very much attention either.* We had no courses that brought this sort of thing up. Our economics, for example, was the old-fashioned stilted economics, which was repulsive but didn't really get down to brass tacks on the current world either (X-REF FORWARD to P. 93) I don't remember taking any courses that would bring up international affairs. In recent years I've written a number of things about the matter of engineering education, and I've emphasized that the engineer needs to pay attention to men as well as to things. The engineer utilizes science to produce things that the public needs or wants. He stands between science and industry in many cases. He has to deal with men in companies, on boards of directors, in government, and this is just as essential to

1) NO INTERNATIONAL AFFAIRS

2) EDUCATION OF ENGINEERS

*A foreshadowing of the attitude of J. Robert Oppenheimer prior to the Second World War. EH

his success as it is that he should be able to apply it. We had no such concept in the early days.

Now I ought to bridge the time between my undergraduate and graduate years. I left Tufts in '13, went to the General Electric Company, and got fired the next spring (1914). Thereupon I went to work for the U.S. Government as a subinspector of electrical materials in the New York navy yard. I got this post through a civil service examination which I took when I was still in college, and for the fun of it, or rather to see what the thing was like. One day at Tufts I saw some students with slide rules about to take a trolley, headed for a civil service exam. So I joined them. I made no preparation for it. The exam as far as I remember was more concerned with

1) U.S. NAVY INSPECTOR

2) SPUR-OF-THE-MOMENT CIVIL SERVICE EXAM --TOOK THE TROLLEY

one of them, and simply rang for him or called gently for him, nothing happened. If I yelled, (ital) "Hickey!" so that you could hear me all over the big room, then he'd come running. Some of the other inspectors didn't care too much for me. They'd take things from the file of the job that I was supposed to be on. But it was amusing rather than annoying because I was only temporarily on the job anyway. I used to take an inspection slip, a long blue form that enabled me to walk all over the Navy's ships. Of course, once in a while I'd get caught and be thrown off a ship. I usually said something about looking for some item in the wrong place. Anyway, I came by some ideas about navy ships and submarines and one or two other things that were not supposed to be in my curriculum. The subinspectors were grouped in a number of sections, and the head of my section was a very nice chap. Then there was (ital) a chap named Huey who was the head of all the

1) U.S. NAVY INSPECTOR

subinspectors, and above him a materiels officer who was in charge of the whole works. The provisions were that when Huey was out of town, the head of my section took his place. Well, the head of my section took sick, so I took over his work. While I was doing this, Huey went out of town, apparently not knowing about this, so I took over his job, too. With these three jobs, I was reasonably busy for a while. Of course I didn't cover everything those fellows were supposed to do, but I held down their offices, at least for a while, with no great difficulty. I remember the materiels officer coming in -- his name was Macdowell, and I had lots to do with him later in connection with submarine warfare. (X-REF AHEAD TO PAGES 27, 554-556) In came Macdowell and said, "Where's Huey?" I said, "He's out on the West Coast." "Where's so-and-so?" meaning the head of my section. I said, "He's sick." "Oh," he said, and

1) U.S. NAVY

walked out, and that was that. He never inquired as to how I happened to be sitting in Huey's office. Since there were "no smoking" signs about, the subinspectors, at lunch time, went out in the yard for a smoke. Macdowell stayed in his office and lit his pipe. So I stayed at my desk and lit mine. He passed me a number of times without comment. Then he joined me. After that we sometimes chatted at lunch time. Obviously I had some queer drag, so the other subinspectors ganged up on me. But also, when I took over Huey's desk without being instructed to do so, they regarded it as quite normal. This sort of thing was undoubtedly one of the reasons why the rest of the subinspectors in my outfit thought I was a bit of a menace in some ways. At the end of this experience of some months, I resigned and went on to other things.

1) NAVY

1) BACK TO TUFTS

Now I think I've got the time schedule a bit balled up here, but we can get it straight later. (GE - 1913; Navy - 1914; Tufts, math instructor - 1914-1915; Ass't professor, electrical engineering, Tufts - 1916-1917)

When I left the General Electric Company, suddenly, I went back to Tufts and did some teaching. And I think I've put the Navy experience in the wrong place. I remember that I walked into the office of Dean Wren and told him I wanted a job. He said he didn't have any job that I'd want, and I asked him, "When do I report?" He repeated, "I haven't got any kind of a job that you'd want." I asked, "Would it be all right if I go to work Tuesday morning?" After quite a bit of this he finally said, "Okay, okay, you asked for it."

I didn't know what I was going to teach until I showed up. Then I found that among my duties was teaching a group of women a

course in elementary mathematics. I got this job because the fellow who had been teaching it had left. The girls had been raising the devil with him and he was a bit shy. (I confess I have never been very shy.) My principal difficulty was somewhat different. I had the Dean of Women's daughter in the class, for one thing; for another I'd come fresh from Pittsfield where I'd been bossing a gang of Italians erecting transformers; my stock of profanity had been enriched by some Italian phrases which were non-academic. But the girls weren't shy either -- not even in 1915. [Taught math at Tufts in 1914-15] They would toss a pants button into the air so it would fall near me and then watch it roll across the floor. The closer it came to me the better -- that was the game. I told them to have their fun; mine would come later, because I didn't think any of them were going to pass the course. After that we got along pretty well. Of course my aim of that day was to get to MIT and do some

1) TUFTS

2) TEACHING

graduate work. I was still short of funds, of course. I got a $1500 fellowship at Clark University and was delighted. My idea was to go down to Clark, in Worcester, and study mathematical physics or really the mathematical handling of things in engineering, particularly circuit theory and the like, under Arthur Gordon Webster. I had enormous admiration for him, for he'd written a classic book on dynamics. I still turn to it at times; it remains a grand piece of work.

1) BRIEF FORAY TO CLARK

When I saw him at Clark I was appalled to find he was working on acoustics, and insisted that if I was going to study with him that I was going to work on acoustics too, a subject in which, at that time, I was not interested at all. I also met one or two of the other graduate students, and found one poor fellow who'd been working away for a year or more at a problem, with very little aid or consultation from anybody, including Webster.

1) BRIEF FORAY TO CLARK

The final straw, however, came about this way: I was just about to start classes and was standing in the corridor with two or three other students, when along came a chap whose name I've fortunately forgotten. He had hair about six inches long, and as he walked down the corridor it flapped on his shoulders. I asked the other students, "For Pete's sake, what's that?" And they said, "That's Professor Blank." Well, I was scheduled to take a mathematics course under Professor Blank, and this was just too much. So I went back to my boarding house, checked out, took the train for Boston and resigned my fellowship by mail. This left me in quite a spot, because I had been counting on that $1500. But I got a $300 scholarship at MIT and went on there.

That's how my association with MIT began -- an association which lasted, in one way or another, for almost fifty years up to now.

But in this question you want the story of how I went on to the MIT faculty. Well, while I was teaching at Tufts I was also doing some consulting for a little outfit called Amrad, the story of which is also interesting. (X-REF AHEAD TO PAGES 557-561; 783-796) When American entered the First World War I went to work on submarine detection. Out of this I learned a great deal about how not to conduct civilian research on war matters. In fact what I learned then probably had a great deal to do with the way in which I went about things during the Second World War, for the relations between civilians and the military were terrible. The only civilian war research I ever came in contact with was conducted under a committee of the National Research Council headed by Robert A. Millikan. When I conferred with him about my proposals on submarine warfare, he intimated that if I became one of his loyal lads, everything would go well. But there seemed to

1) MIT BEGINS
2) RESEARCH AMRAD
3) FLASH AHEAD TO WWI

[X-REF ahead to Pp. 555 and 564]

be a bar to support if I was going to be really independent. This may not have been what he intended to say at all, but it was certainly the impression he gave me. So I had nothing more to do with the committee of the National Research Council.

1) FLASH AHEAD TO WWI

2) BACK TO NAVY SUBJECT

Thus I didn't spend government money. Instead I spent J. Pierpont Morgan's money, because he was supporting Amrad, and submarine warfare and detection became part of an Amrad program. I may have gone a little too far in intimations that the same equipment that detected submarines during the war could be very useful in detecting ore bodies after the war. Well, the (ital) same general line of work <u>did</u> become useful in detecting ore bodies, but not until a generation later. However, it was all sufficiently interesting to the company so that we went ahead with the program on Morgan money. I had my relations with the Navy Board on Antisubmarine Devices, the secretary of which was the same Macdowell that

I'd worked under in the Navy Yard in Brooklyn.

1) CONTINUED NAVY DIGRESSION

To make a long story short, I developed a piece of equipment that worked. (X-REF FORWARD TO PAGE 567) It could have been developed quite a lot earlier, except that the Navy requirements were cockeyed, and there was no civilian-military consultation on what would be really useful at all. The Navy simply stated what it wanted, and that was that. After losing six months we finally got something built and working, with one hundred sets into production. I got three of these sets into Britain. There they detected some British submarines. They never detected any German submarines becuase the war had ended. I think the gadgetry was all right for its day, but it was pretty crude stuff. I could tell quite an elaborate story about its development. (X-REF FORWARD TO PAGES 565 AND 776, 777) But this all occurred before I had any intention of going to MIT. I taught at Tufts the next winter, and then in the fall of '19 I went over to MIT.

D. C. Jackson was the head of the Electrical Engineering Department, and I owe him a great deal. In a strange way I was very fond of him. He was a fire-eater. The story around his department was that if you wanted to see him, the thing to do was throw your hat into his office; if it stayed in, you could go in after it. He had nervous indigestion, and this made him pretty belligerent. Inside his department he was rough on everybody. Outside, he defended it totally and claimed he had the greatest department in the country, bar none.

1) MIT

2) D.C. JACKSON

Well, I went to see him, and he told me he wanted me to join the staff. One of the first things he said after we got through the preliminaries was that he didn't know whether he ought to make me an assistant professor or an associate professor. I told him I thought I had a formula which could resolve his doubts; put me in as an associate professor. He did. This saved me quite a few years of climbing up the ladder, I

[EH to VB: Please remember Bush/Timbie throwing Phi Beta Kappa Keys — and Timbie's took — in Charles River]

imagine. I intended to continue my outside consultations, and he read me quite a lecture that the Institute's affairs must come first; that none of my consultation should be on anything that wasn't dignified and that wouldn't advance me professionally; that I mustn't overdo it and so on. When he finished, I said I thought the solution was very simple: that all I needed to do was follow his example. This was a pretty good example, because he was the head of a consulting firm in Boston, and was known throughout the country as a consulting engineer. Well, he couldn't very well object to my formula. So I joined the department, and thus began the great experience of my life.

1) MIT

2) D.C. JACKSON

Question 4: VB's doctoral dissertation; relations with Kennelly; Budget rumpus with D.C. Jackson

Your question 4 pretty well covers the waterfront. Let me start with Dr. Kennelly.* He was an exceedingly interesting individual and I think it's worthwhile to say a few things about him, not because we may want to use any of it, but merely because there's an interesting story here. Kennelly was trained as an electrician. There was really no engineering education in Britain of his day. He worked, I believe as a telegrapher. How he ever worked his way up to a great reputation is a mystery. When he died I had to write his biography for the National Academy of Sciences, which took some doing under the circumstances. He came to this country and finally became a full professor of electrical engineering at Harvard. Now the British have only recently learned what

1) MIT

2) Kennelly

* Footnote needed to identify Kennelly

an engineer really is, and how important engineers are in their national affairs. For a long long time the engineer in England did not quite belong. Kennelly with this background was always exceedingly interested in recognition of any sort, and this was the cause of some of his troubles. Now he did accomplish some very interesting things. He took some of the mathematics of circuits, particularly the mathematics of long transmission lines, and put it in a form which engineers could really use. In doing this, he didn't create any new mathematical treatment, which was all in the literature, but practically inaccessible to engineers in the form that it was written. Kennelly made it intelligible and real so that engineers could use it in their daily affairs, and they did. He was highly regarded for this, and quite rightly so. But this did not satisfy him. He must be recognized for his scientific accomplishments too,

1) MIT

2) KENNELLY

and these were really quite few and far between. Now as an example, and a rather sad one; one time, when I was a student at Tech and Kennelly, of course, was supervising my thesis, he wrote a paper on electric circuit theory. He'd had in it a formula, a useful formula, and as usual he applied it in an interesting manner. The only difficulty was that he claimed this formula as his own: it was not. He showed me his paper. I was appalled, but said nothing. I went to the library and got out the paper of Oliver Heavyside, opened to the place where Heavyside treated that very formula, and then showed it to Kennelly. It was exactly the same thing except for the notation. Kennelly wouldn't admit this at all. He still insisted that his was different and new, and of course I couldn't protest very heavily. He presented that paper at an engineering meeting in Boston.

1) MIT

2) KENNELLY
cf: OLIVER HEAVYSIDE

3)

A.G. (Arthur Gordon) Webster attacked him on it, and attacked him in a very cruel manner. It was totally unnecessary, but it was quite typical. The last chapter of my relations with Arthur Gordon Webster, just before he committed suicide, is quite a story -- which is not recorded anywhere. (REMINDER)

1) MIT
2) KENNELLY
3) WEBSTER
4) JACKSON

The most interesting episode between Kennelly and me occurred when I was finishing my doctor's thesis. He discovered that I was going to finish the work for my doctorate in one year of residence, and he didn't think this was right. That spring I took four oral exams, and I had concluded that I could finish that year. Some time before, Jackson had discovered what my plans were, but all he said was, "Bush, I think if you go on with this plan it'll be too much and you'll wreck your health." "Well", I said, "Professor Jackson, suppose I do this: I'll come in to see you once in a while, as I do anyway, and you look me over; if you think

I'm getting into poor physical shape, you tell me so and I'll quit." "Okay," he said, "that's a deal." So Jackson had it thoroughly in mind.

1) MIT

2) JACKSON & KENNELLY

Kennelly, however, took the step of trying to increase the scope of my thesis. I protested. He insisted. Whereupon I said I wanted to take it up with the head of the department, and he said, "We'll go in and see him together. Having said this, he went in to see Jackson himself. And I knew this, because the girls in the office were all friends of mine. So when I went in for the three-party interview, I was a little stirred, to say the least. Jackson opened the discussion by saying, "Now Bush, I want to assure you of one thing before we proceed. Any agreement that's been made with you by this department will be carried out fully." And when he said that, I got up and said, "Professor, that's all I want." And he said,

"Sit down!" So I sat down. He listened to Kennelly's side of the thing, overruled him, told him that while Kennelly could judge the way in which I covered the scope of my thesis, he could not, under the circumstances, increase the scope. There was a slight difficulty in this because in my thesis I was handling a branch of mathematics that Kennelly had never himself studied, so his judgment of how adequately I covered the scope of my thesis was a little lame, to say the least.

1) MIT

2) BUSH THESIS

This brings me to another part of your question, which is the question of my doctoral thesis. The principal thing I hope about this is that no student will ever fish it out of the file and look at it. In the first place, it is an exceedingly crude job viewed from today's point of view. (X-REF FORWARD TO PAGES 58 and 576-577) In the second place, there's a first-class error in it. I didn't discover that error, nor did anyone else, until shortly after I graduated. But a chap by

the name of Wagner published a paper on the same subject and when I saw it, I knew very well I had gone off the rails. So for a number of reasons I hope nobody sees that thesis. The branch of mathematics I was using was one that engineers didn't use at all, at the time. So I worked with the mathematics department and particularly with E.B. Wilson. As I say, my treatment was pretty crude, and I hope and pray nobody will ever look at that thesis.* Certainly it has no pertinency whatever in regard to the way in which circuits are treated today.

1) BUSH THESIS

2) KENNELLY

[EH to VB: When and what subjects?]

* * *

Coming back to Kennelly: I think when he tried to stop me on my doctorate program, he thought he was doing it for my own good. He was a kindly man, there's no doubt about that. But after I'd been on the staff for two or three years, ~~Kennedy~~ Jackson one day tackled me, and said he wanted me to take charge of the graduate work in the department and of the graduate research labora-

* Still on file, MIT Library [VB says: "Yes, I haven't swiped it."]

tory. Now Kennelly ~~Jackson~~ was listed as the head of that affair in the MIT catalog. So I asked Jackson what was going to happen in that regard, and he said Kennelly was going to remain the titular head of the graduate work, but I was going to run it. I asked him how I was supposed to do that, and he told me that was my problem. Shortly after, I thought I might as well have a thorough showdown on it. If I was going to have one, I might as well have it on my own ground, so I dispensed with the services of a girl who was acting as a stenographer in the graduate research laboratory, whom Kennelly had hired -- a girl that should have been, let us say, encouraged to leave some time before. In fact, a good old professor of physics had come to me and reported that he went to the research laboratory and discovered the girl sitting in a student's lap, which was not, of course the sort of experimentation intended for that laboratory. I called the student in and told

1) MIT

2) KENNELLY, ETC.

him that I had heard it said that he and the girl were found in somewhat close juxtaposition, and he said, "Professor, I don't know what juxtaposition means." "Well," I said, "suppose we call it propinquity," and he said, "Professor, I don't know what that means either." (ital) Well, I said, "Son, you know damn well what I mean; now how about it?" And he said, "Well, Professor, I think there's some truth in the rumor." That particular student, incidentally, made quite a success of his later career. I just told the gal that I didn't like the way she typed her letters, and she went to another part of the institution and got hired over there. This is typical of the way in which the institution operated at that time. One department, in hiring a girl, would not think of asking another department how she'd gotten along. But she got along all right, and I greeted her one day after she'd made the move, and we didn't seem to have any hard feelings. But the point was, I'd fired

1) MIT

2) KENNELLY, ETC.

Kennelly's stenographer. He not only did not get irritated over it, he never said a word to me about it. He never took it up. In other words, Kennelly was quite content if he was listed in the catalog as being the titular head of graduate work in electrical engineering. He was quite contented if somebody else ran it.

1) MIT

2) KENNELLY

3) FIRST MENTION OF McKAY TRUST

4) S.W. STRATTON

* * *

When MIT and Harvard joined their engineering efforts, under the McKay Bequest, Kennelly came over to MIT, but he insisted on being a professor at MIT as well as at Harvard. And when he finally retired, he insisted that he must be Professor Emeritus at both places. (Samuel Wesley Stratton was president of MIT at that time). Both acceded to his request. But then Kennelly insisted he must have an office at MIT. (We wouldn't want to put this anywhere into the record

except as an amusing affair, but) I told Stratton that what Kennelly wanted was not an office but his name on a door. I told him I had a door in the department where I could put his name, and he said, "What's behind it?" and I said, "A switchboard, but Kennelly'd never find that out." Stratton said he didn't think that would be a good idea, so I told him that all right, I'd put his name on my door. "Well," Stratton said, "Won't he be an inconvenience?" And I said, "No, he won't ever come in." Stratton wanted to know why, and I said, "He can't stand tobacco smoke, and he can't stop me smoking, so I'm quite sure he won't come into the office." And he never did. The point is that he had to have his name on the door.

1) MIT

2) STRATTON & KENNELLY ANECDOTE

* * *

Now I don't want any of this to indicate in any way that I didn't appreciate Kennelly.

There is a great deal being put onto this set of tapes that has nothing whatever to do with our present project. But on the other hand, I suppose it's just as well to get some of these things down, for use in some other connection perhaps, and merely as a matter of interest.

One of these things is my relationships with D.C. Jackson. Jackson was an extra-ordinary individual and I owed him a great deal. I've already told how he supported me in my doctorate work and how he took me into the department. But the story goes much further than this. He was a tough egg. He dealt with his own people in a highly direct and rugged manner sometimes, because he had a stomach ulcer or for some other reason, without much discrimination. Mediocrity, he couldn't tolerate. The only way to get along with a man of Jackson's type, was to stand up

1) MIT

2) DC Jackson

3) NOTE REPETITION
D.C.J.

to him. If you stood up to him thoroughly he would gain respect for you; and you could become friends and work together. But if you took his unjust wallops without coming back you were sunk. Sometimes you encounter a man who makes rugged attacks because of some inferiority complex or some other over-compensation. With a chap like that, if you come back hard, he'll usually wilt. Jackson was not that type at all, not by any manner of means. He was proud of his own accomplishments, but he had no really deep respect for anyone unless the other fellow could give him as good as he sent. I had this exemplified very early in my relations with him. He put me in charge of the first course in electrical engineering given to sophomores. I had three or four instructors, and Jackson took one of the sections himself. Each week I outlined the work with the instructors for the next week.

1) MIT

2) D.C. Jackson

On one of these occasions I gave them instructions on how to handle some particular circuit problem, and the instructions I gave were at variance with the treatment in Jackson's own book on electrical engineering which he had written with his brother. So he hit the roof. He got hold of me, he told me my treatment was all wrong, that it wasn't rigorous, and so on. Instead of arguing the technical matter with him, I told him that since he was head of the department he could obviously take the course out of my hands, where he had put it, but that as long as I remained in charge I was running it. And as long as he was teaching a section in that course, he was going to take his orders from me as to how it was run. It's easy to see what kind of an explosion this could cause,but Jackson was completely fair. I think he recognized the logic of my position, anyway. I heard no more protest whatever from him during the course.

1)MIT

2)D.C. JACKSON

One day, a dozen or so students came into my office -- all members of D.C. Jackson's section. They explained to me that they were getting a great deal out of Jackson; his accounts of organization of the public service industry were very revealing, he was in close touch with trends in the industry, and it was very much worthwhile. "But," they said, "we have to pass an examination at the end of this course, and we're not getting any of the course material, we're just getting some very interesting stuff that has nothing to do with the subject matter." I told them that since they were reasonable young engineers, they'd undoubtedly held a caucus before they came to see me; had put themselves in my position, examined the matter from all angles, and decided to ask me to take some action -- an action I could properly take and which might solve their problems. "So," I said, "just what action do you recommend." I got some grins out of

1)MIT

2)D.C. JACKSON

the crowd and after a little hesitation, they started to go out. But I called them back and I said, "Now I'll propose a solution. I'll change my section so that it doesn't intervere on the schedule with yours. I always welcome visitors to any section I teach. I don't treat them any differently from the regular section members when they come in, and I call on them for an explanation or the like. But I never object to visitors." From then on, until the end of that course, I had the men of two sections in my class every day. I don't think Jackson ever knew this.

1) MIT

2) D.C. JACKSON

3) JACKSON & EDGERTON

A far more violent sort of episode occurred when one of the young instructors ordered a machine for a piece of research -- a dynamo -- and ordered it wrong. The man that did this was Harold Edgerton, the man who later made ultra high-speed photography and flash photography possible, and who's still at it. A very distinguished individual (now a member of the National Academy of Sciences). At the time he told me about his mistake and how he and some

of his friends went to work evenings, rewiring that machine to get it back to what it was supposed to be if it hadn't been ordered wrong. In some way or other Jackson heard about this. So he called me in, and promptly wanted to know who'd make the mistake. I told him that I was taking care of it, that came within my scope, there'd been an error, but it was being corrected properly. But still Jackson wanted to know who had made the error. I told him that if I told him who had made the error, I knew what he'd do: He'd call the victim in, jump all over him, and this would be exactly the wrong thing. I wasn't going to have that done to one of my men; and I ended up by saying I'd be damned if I'd tell him who it was. He then told me in no uncertain language that he didn't like the way I was running the graduate research program anyway, and a few more things like that. So I told him that if he didn't like the way I was running the research program, he could wind it around his waist and go jump in the lake, or some equally polite suggestion.

1) MIT

2) JACKSON & EDGERTON

When I went home that night I said to my wife, "Well, Phoeb, I guess we're all through at MIT, by the looks; where'd you like to go now? The Pacific coast, maybe?" But when I went back next morning, Jackson had something else to take up with me. I went to his office, he greeted me pleasantly, we chatted about it, went to lunch together, and no one would have known that anything bad ever happened between us. The other subject was never mentioned again.

1) MIT

2) "ALL WASHED UP"?

Some three or four months later at a research meeting, one of the young men, speaking about that particular research and referring to the machine, said, "You remember this was the machine Edgerton ordered wrong." Jackson was sitting there when this occurred. He never said a word. He was cooled off by that time.

* * *

Later on, of course, I jumped over his head and became Dean of Engineering, whereupon presumably instead of my reporting to him, he reported to me. This put on quite a strain as you might well suspect. Yet there was no jealousy or anything of the sort on his part. In fact, since he regarded me as one of his boys, he was very proud to see me advanced, and he said so all over the institution. We worked together all right, but with occasional flares of temperament. I remember one time when I was in my office, with Karl Compton, and in came Jackson to protest about a budget item. He proceeded to tell me that I'd got the arithmetic all wrong, that I'd got the thing balled up, that it didn't make sense, and a few more things. I sat there and didn't say a word until he finally got it all off his chest and went out. Then Compton said to me, "Van, I don't see why you did that that way. It's quite an extraordinary thing for a depart-

1) MIT

2) JACKSON IN LATER YEARS

ment head to jump on the Dean of Engineering in that language and in the presence of the President of the instution, but you didn't seem to take offense at all." "Well," I said, "the reason is, I know the old boy pretty well, and I know exactly what'll happen. Since I didn't say anything, Jackson will go back and examine that recommendation with great care. He'll find out that he made a distinct error, that he was wrong, and that when I was sitting there smiling at him, I knew all about it, but said nothing. And about tomorrow morning, he's going to meet me casually somewhere, and he's going to say, 'Oh by the way, Bush, I find I was all wrong on that point yesterday -- let's forget it.' I'll say, 'Sure, I forgot it already.' And that'll be that." And that's exactly what happened.

1) MIT

2) JACKSON IN LATER YEARS

* * *

1) MIT

2) JACKSON EULOGY

One of the toughest things I ever had to do was deliver the eulogy at the memorial service for D. C. Jackson. (EULOGY IN FILES) His widow and family were in the audience; so were a lot of men from the department and from his consulting firm who had known the old fellow very well indeed. I was in a tough spot, because I could either be unkind or I could be hypocritical. I managed to work it out, but I never worked harder on anything in my life, to try to pay a proper tribute to a very interesting great man, who was very kind to me and a great friend, but who nevertheless was one of the toughest chaps I ever worked with.

* * *

Question 5: MacLaurin's death; Administrative Committee of Norton, Talbot and E.B. Wilson, the non-active Elihu Thomson; Ernest Fox Nichols; Samuel W. Stratton; 1920 to 1930 at MIT

There's not such an awful lot of material here that is pertinent to what we're now doing.

Some of it perhaps is worth putting on the record for some MIT historian or the like. I think the real point of the questions here is that the progress, the prosperity of a great university depends enormously upon the caliber of the man who leads it. And of course this again means that the prosperity depends upon the care and effectiveness with which the Board of Regents, or the Corporation or what have you, exercises its primary function of choosing the president. This has certainly been exemplified at MIT during my experience. Dr. Maclaurin (Richard C.) was a great man. He built the institution in its present form not only by the way in which he guided it, but also by the way in which he attracted money, specifically the money from George Eastman which made the new Institute possible. I had very little to do with him, of course. I started my graduate study in the fall of '15, when we were still over on Boylston Street, and finished up as we

1) MIT

2) ON CHOOSING PRESIDENTS

were getting into the great new buildings across the Charles River, As we all know, Maclaurin died very shortly after he had made clear the great gift from George Eastman. Then followed ten years of very bad administration. Primarily I think the reason for this was that the Corporation of the Institute became largely dominated by one man, namely Gerard Swope, who had other things on his mind. Swope didn't have time to get into institution affairs closely enough to do a really good job there. Nevertheless he dominated, and the rest of the Corporation allowed him to. I'm sure that kind of thing couldn't happen today with the present caliber of the Corporation. It is one of the great dangers of a big public institution of any sort that many of the trustees will regard themselves merely as ornaments, or sources of funds, and they

1) MIT

2) GERARD SWOPE

leave the running of the show to some one of their members who drives the carriage from the back seat. I've seen it happen elsewhere. This may not have been the full reason, but at any rate, we had quite a series of queer shows. We had the institution run by a committee; Norton, Talbot, and E.B. Wilson. They had Elihu Thomson* as an Acting President. Now Elihu Thomson was a good engineer, and he was a very attractive chap who'd done fine things for the General Electric Company. When he died, Mrs. Thomson was left with, I understand, very meager support. (X-REF AHEAD TO PAGE 580) Elihu Thomson did not accumulate any fortune. This always annoyed me since I was a great admirer of Thomson as a man. He was also one of the pioneers in getting the General Electric Company really going, so it further annoyed me that the other people who were pioneers, who helped get the company set up and into its modern form, did not see to it that Elihu Thomson was properly taken care of in the light

1) MIT
2) INTERREGNUM
3) ELIHU THOMSON

* note attached

Add Note From Carol Young from margin of Page 46:

Cf."Elihu Thomson" file in Bush Papers: contained therein is a copy of VB's Paper before the American Philosophical Society in April 1938, "Elihu Thomson, His Interest in Education, His Work as a Teacher."

Also letter 2/28/39, from Mrs. Thomson thanking V.B. for his commemorative paper.

of his contributions. But at any rate, while he was named Acting President, he didn't act. At least I never saw him do so at any time that I was there. We do have to remember of course that I was a young professor who didn't come in contact with the central administration. We had the episode of the choice and inauguration of Ernest Fox Nichols, who fell ill and resigned and had no effect on the place whatever. (1921) I never knew Ernest Fox Nichols well. When I was working on submarine detection during the First World War he was, too. I remember that very late in the program, he got in touch with me, and found out what I was doing, and stated quite frankly that he thought the way I was going was more promising than his. He certainly wasn't a difficult person, or a man of great exaggerated ego. But I never had much contact with him. After that, we had an Administrative Committee at MIT again. Finally the Corporation brought in Samuel Wesley Stratton.

1) MIT

2) ELIHU THOMSON

3) ERNEST FOX NICHOLS

I had a great deal to do with Stratton because Jackson would go away on his consulting business and leave me in charge of the department. He was away for a year one time. Stratton had no more knowledge of how to administer a university than a babe in arms. In fact he didn't have very much sense on anything, so far as I could find out. The great mystery is how he ever became the director of the great National Bureau of Standards. Perhaps this is explicable in terms of Washington's strange political moves at times, but how he became president of MIT I don't know.

1) MIT

2) S. W. STRATTON

S.W. Stratton was a bachelor and lived with a sidekick called Morris Parris. A great deal could be said about that, but probably shouldn't be. The way he ran the Institute was a caricature of administration. I remember once I put in a budget recommendation; in connection with it took up the case of a man named Gardner, who was a professor, now

retired. When it came to Stratton's desk, he changed the arithmetic on it. Gardner was being transferred from a 9 months to a 12 months basis. Stratton not only changed the arithmetic, but notified Gardner. Through faulty arithmetic he inadvertently gave the man a very substantial increase in salary which wasn't called for. When I called this to his attention, he said, "Oh well, just change it." I said, "It isn't quite so easy, Mr. President; you've got a contract with this man. You've notified him of a very substantial increase. Now you've got the contract, and I don't see what you're going to do about it. Moreover, having done this to one man in the department, how are you going to explain it to the rest of the department?" That folded him up fairly well, but I said I'd see what I could do, so I walked down to Gardner's office and said, "Did you get a notice?" He laughed

1) MIT

2) S.W. STRATTON

and said, "I sure did," and I said, "Well, give it back to me and I'll get it correct -- I got the arithmetic balled up." So I went back into Stratton's office and said, "Well, you're dealing with a very reasonable man, and a man who's very loyal, and quite willing to overlook the whole business. Here's the contract and we can correct it." Stratton said, "All right, I'll correct it."

1) MIT

2) STRATTON

So a couple of days later in came Gardner who said, "I got the notice back again, and look at this time." He gave it to me; this time the arithmetic had been corrected so it gave him a substantial cut in salary. So I just changed the figures on the slip, and changed it on the books without taking it up with Stratton again. This was typical of what was going on all the time. There were two troubles, of course. In the first place, Stratton never should have dealt with such matters anyway. He should have delegated them. In the second place, he was unbelievably

clumsy in handling things of this sort. As an individual, when he was entertaining, he was delightful. But he had no capacity to be president of an institution.

1) MIT

2) KARL T. COMPTON ARRIVES

How fortunate it is that Karl Compton came to MIT! After ten years of perfectly terrible administration, which nearly wrecked the place, we've now had 30 years of excellent administration. And Karl Compton started this. His great success stemmed from just one thing. He was one of the most likable men I ever knew. Everybody at MIT was so thoroughly loyal to him in every way, that he could do anything. He had good sense, he was a kind individual, and he was not a bloated egotist. He was generous, he delegated easily, he always supported his subordinates in a kindly manner, and he was a great fellow. He turned the Institute around and started it on a proper path. Shortly after he'd arrived and got going, I saw Gerard

(ital)

Swope, and he said to me, "Did we make any mistake this time?" and I said, "You certainly didn't, but you made a honey the time before." Which ended that conversation.

1) MIT

2) KARL T. COMPTON ARRIVES

Interestingly enough, one of the first times, perhaps the first time, I saw Karl Compton, he issued an edict about consultation at the instance of Swope. (X-REF FORWARD TO PAGES 616-617) This was completely contrary to the understanding I had when I joined the institution, which incidentally, I had in writing. So I went into Karl's office. I'd never seen him before. I told him I was out, that I simply wasn't going to work for an institution that abrogated its agreements unilaterally and without consultation. I also wasn't going to stay at an institution that had so little sense that it tried to clip down to nothing proper consultation on the part of its engineering professors.

1) MIT & COMPTON

2) THEN CAME THE WAR

So that was my introduction to him, not ordinarily the sort of thing that would lead to a great friendship. But it did. The reason was that Karl met that issue in such a kindly manner, and with such obvious willingness to look at all sides of things that I changed my tune completely. The result of that conversation, or the result of that and a whole lot of other things, was that I became Compton's vice president and deputy (two years later) served in that capacity for some five or six years. (X-REF FORWARD TO PAGES 617-618) (VB became KTC'S vice president in 1932. In this year he also became Dean of the School of Engineering) It was one of the pleasantest episodes of my life. I was never more fond of a man than I was of Karl Compton.

Then (1939) I went to Washington, along came the war, and Karl Compton came into my organization in government. We worked

1) BUSH & COMPTON

together for some further years, having reversed our roles where he was boss and I was his deputy, to the point where I was the boss and he was one of my organization. Things were just as harmonious in one case as in the other. Moreover, if someone could have taken down the teletype record of the conversation between the pair of us on some problem, I'm sure he could not have told whether it applied to the first period or the second.

There are various ways that men become great leaders. Karl became a great leader simply because he was such a fine individual. There was nothing more to it than that. He was just such a delightful, lovable man that everybody wanted to follow him and no one would hurt him if they could possibly avoid it. I could of course fill out any number of things along these lines, but this thumbnail sketch is certainly all that we need now, and probably far more than we need for our present purposes.

Question 6: The state of science and engineering in U.S. at that time.

1) EXECUTIVES AND ENGINEERING

I can testify a bit in regard to engineering in this country, and particularly at MIT, but I'm hardly in a position to comment very well on the situation in science. In the days after World War I and right through to the time that I became an executive at MIT, I was primarily an engineer, although I've been accused of being a scientist more recently. I think that's more reflected than integral in my general career. I was an engineer, and I was engaged in engineering, teaching engineering, and practicing it by managing little research laboratories. Working with L.K. Marshall in the promotion of some new industrial efforts took me into all sorts of questions of organization, promotion and finance -- particularly finance because we occasionally went broke -- all of which is engineering. (VIDE: Spencer Thermostat, earlier) Now as far as the status is concerned, I think we can say this: the first

war did not produce any great innovations in methods of warfare except, of course, for poison gas, and the very early beginnings of the tank which were not properly utilized. The reason that more was not done was because there was no organization in this country or anywhere else, for that matter, that brought the real talents of the country to bear along such lines. Hence after the first war, there was no great splurge of interest in science as there was after the second war. (QUERY: EH to VB! MIT's enrollment rose hugely in 1919! Not science but engineering was then attracting thousands of young men) It might have occurred had there been one or two striking innovations that were really scientifically based. Chemical warfare was not thus based. It merely utilized existing gases that were known to be toxic, and the methods of hand-

1) FLASHBACK - WWI

2) ENGINEERING

ling them were the methods by which they'd been handled right along. The tank was new, it's true, but it got so little distance that it had very little influence. Of course, if the tank had been used to effect, we would have had mobile warfare instead of the terrible situation we had in France.

1) BACK TO MIT

2) CIRCUIT THEORY AND MATHEMATICS

Now at MIT, the situation was something like this: no one in Electrical Engineering was interested in modern methods of going at circuit theory (then my primary interest) except myself, and of course some disciples among the younger men. In the Mathematics Department, there were one or two men who had the mathematical strength to tackle such a thing -- E.B. Wilson, notably, and of course Norbert Weiner, although his interests were quite elsewhere in those days. The trouble was that there was no corresponding response over in the Department of Electrical Engineering, and most certainly not in other engineering

departments. In Germany, things were happening, and the early steps toward modern attack on circuit problems and the like, had been taken. The type of mathematics used -- theory of the functions of a complex variable -- was a subject that I had studied. I don't think anybody else in the Electrical Engineering Department had ever studied it. And I don't think anyone in the Mathematics Department had ever applied it. So there was a great gulf. The trouble was, of course, that engineering at that time had not yet recognized that new forms of analysis were ready for its use. The reason that I went into analytical machinery (which is usually termed computer machinery today, but which is much more) was just this: I was trying to make some progress in computing the performance of strange circuits, notably transmission lines in the transient state. I very promptly ran into the situation that the mathematical equations could be set up, but the process of solving them was so laborious that it took more man-hours than

1) MIT

2) DIFFERENTIAL ANALYZER

anyone could possibly expect to devote to them. So I built a very crude breadboard sort of Differential Analyzer to see if the doggone thing would work. (X-REF AHEAD TO PAGES 584-585) The ideas that were involved there, of representing the differential equation by so many connected mechanical parts, was entirely novel to us at the time. Long after I'd gotten going on it, I discovered that in an appendix in the book by Thomson *and Tate (DYNAMICS) Tate had very much the same idea,(X-REF AHEAD TO PAGE 266) although he had never built anything. The building was really the tough part of this problem, because one had to get precision and ease of connection and a few other things like that. A more interesting aspect of the thing perhaps was this: when I was a student, the use of integral equations for approach to physical problems was in its infancy. The only work

1) MIT

2) DIFFERENTIAL ANALYZER

*CY NOTES: AM UNABLE TO FIND. SUGGEST LOOK IN NATIONAL CYCLOPEDIA OF AMERICAN BIOGRAPHY, VOLUME B, 1927 OR SOME SIMILAR REFERENCE.

* VB to EH: MY COPY IS NOW LOST.

1) MIT

2) DIFFERENTIAL ANALYZER

3) USES

that I could find on it was written by a man named Voltera in Italy and had never been translated. (X-REF AHEAD TO MORE COMPLETE VERSION ON PAGE 585) I had to struggle with Italian in order to read it. It was not hard to do, of course, for the amount of Italian language involved was very small: most of the stuff was in equations. Following out this thing, I made a machine at MIT working with Harold Hazen*, and with a very important suggestion on it from Norbert Weiner,(X-REF AHEAD TO PAGE 584) in which I attempted to set up a mechanism to solve integral equations. We set it up and we made it work. We solved one problem on it, (the motion of a ship in a seaway) and we published it. (X-REF AHEAD TO MORE COMPLETE VERSION ON PAGE 584) Nobody paid the slightest attention. It was far ahead of its time. It was by no means precise -- it was intended for exploratory purposes primarily -- but it was interesting because the integral equation approach is in many problems equivalent to

* CY notes: re Harold Hazen ... see Bush Papers

the approach through partial differential equations. Now I didn't find anybody in the Engineering Department at MIT except Hazen (now Dean of the Graduate School) who was interested in integral equations or, in fact, knew what they were. Today, similar problems are handled entirely by partial differential equations on which the digital computer works excellently. Moreover, this gives precision, which my approach did not. I think we can sum things up by saying that the little attempt we made here was before its time, that if it hadn't been, it might have been a way of going at exploratory things in a rather fascinating manner. I growl once in a while, these days, that the digital machine has become so successful it has made people forget there are other types of approach that are at least fully as intriguing for the mathematics employed and the method of going about it. The point is that MIT,

1) MIT

2) DIFFERENTIAL ANALYZER

and I judge the rest of the engineering fraternity throughout the country was just beginning to wake up and get going on some of the things where the mathematical approach pays off. Although I can't testify directly on it, I think that somewhat the same sort of latency was present in science. We had some pioneers and they were very good men. But, taking the thing generally and across the country, my impression is that we were decidedly following and not leading, that we weren't even following very well, and that the really great things in science were happening in Germany and England, principally in England. The U.S. made the mistake, a very thorough one,of neglecting science through all those years. The English made a comparable mistake in neglecting engineering. (X-REF AHEAD TO PAGES 780-783) They led in science particularly in the field of atomistics with Rutherford and J.J. Thompson and a host of others. But the engineer had no real status

1) MIT

2) EDUCATION

3) US VS. ABROAD

in Britain. The men who were doing great things in science did not carry their affairs over into application. The engineers in Britain were not thoroughly in touch with science, and that condition lasted for a very long time. In fact, it was one of the things that cramped the style of Britain after World War I when the great surge came in the applications of science to engineering affairs. This is being cured in Britain now, but they still have a long way to go. They recognize the situation completely, they're getting onto it, they're making great progress. You'll find that the British agree on this point: that they lagged in this field and must catch up.

1) SCIENCE VS. ENGINEERING

One other thing ought to be added. I remarked a while ago that my doctoral thesis was a very crude affair. (X-REF BACK TO PAGE 32 AND FORWARD TO PAGE 576)-- so crude that I shudder when I think of it. It was so crude because I

had no one to lean on. There were people in the world who were doing things along these lines, but of course I was not in touch with them except through their published papers, and these came along a year or two after they had done their work. There was no one at MIT, with the exception of E.B. Wilson, who had any (ital) interest whatever in what I was doing <u>and</u> the mathematical background to talk with me about it. Wilson helped me and helped me a great deal; (X-REF FORWARD TO PAGE 382) but I needed a far more constant companion; someone to whom I could turn to from day to day. But there was no one.

1) MIT

2) VB'S DOCTORAL THESIS

Question 7: Bush Differential Analyzer -- what electronic advantages over world of Babbage?

1) DIFFERENTIAL ANALYZER

2) PASCAL; BABBAGE

Question 7 follows very directly from what I've just been saying. The first Differential Analyzer was a breadboard affair made out of wood and junk parts. The second one, supported by the Rockefeller Foundation was again mechanical (QUERY) but a considerably better machine. It was precise in a mechanical sense, but of course not at all precise in terms of what can be done on a digital machine. (X-REF AHEAD TO PAGE 262) When we look for the sources of this sort of thing we think of Charles Babbage, but we also ought to think of Blaise Pascal. Pascal, (1623-62)very early, made an adding machine. (X-REF AHEAD TO PAGE 265) Involved in it were nearly all the ideas which are today involved in the desk computer, key computer type of thing.

(ital) What has happened since Pascal is not the addition of great ideas, but the refinement of mechanisms.

When Charles Babbage (X-REF AHEAD TO PAGE 265) started his work, he undoubtedly knew of what Pascal had done, that is, I should judge he must have. Also, the machine he contemplated, and partially built was in the direction of the digital machine, not in the direction of an analogical one such as the Differential Analyzer. (EH TO VB: For elementary purposes would you say that the sentence "The digital machine counts; the analogical machine measures" made a proper distinction? VB TO EH: They begin this way and then both do much more.) Since he speculated on all kinds of things, he probably speculated on this, although I've never seen it. He did go right ahead on what might have been a digital machine if he had had the adequate mechanical skills and help which came along a lot later. As it was he bit off far more that he could chew. His ambition to build a very comprehensive

1) DIFFERENTIAL ANALYZER

2) PASCAL; BABBAGE

machine did not really make sense. If he'd built a few simple machines first, for explicit applications, he could have had success, and one success could have been followed by another. But he aimed too far, and he bogged down. The proper approach to the problem was, of course, electronic, but this approach was not open to anyone until the twentieth century. Just before World War II, in fact before I left for Washington, Sam Caldwell and I built a new Differential Analyzer that was partly electrical and partly mechanical. (X-REF AHEAD TO PAGE 264) This was a far better machine. It had some use during the war -- in fact it did a lot of work on ballistic tables and the like. It was far more flexible, and it could handle much more complex equations. But about the time it really came into use, it also rapidly became obsolete because of the far greater progress made in digital machinery. Thereafter the digital machines took over. (X-REF AHEAD TO PAGE 585)

1) DIFFERENTIAL ANALYZER

2) DIGITAL VERSUS ANALOG

I never had anything to do with the digital machine development directly. Of course I was in touch with various people who were working on it, and gave suggestions if I could and so on, but I wasn't directly involved. The great success of the digital machine came about for two reasons. First, a lot of very ingenious work was done on circuits: first, thermionic tube circuits, second, on transistors, and also on storage devices and a whole lot of other things that go into such a machine. The great progress came because the thermionic tube became available in quantity at very low prices, and then the transistor still cheaper, became even more able to do the needed things. This made the digital machine go ahead at great speed.

I had one very amusing experience on this that I think is worth reciting. There is a tendency among the British to think that we Americans become enamored of very fine instruments -- so much enamored that we spend

1) DIFFERENTIAL ANALYZER

2) DIGITAL VERSUS ANALOG

3) ANAL. BY MACHINE

undue time in building them and manipulating them, and that in so doing we sometimes lost sight of what we're trying to do in the first place.

1) ANAL. BY MACHINE
2) BRITAIN & U.S.
3) HARTRY

I think there's something in the accusation. I went to Britain at one time, and there discussed this whole problem with Hartry of Manchester and Bragg at Cambridge. Hartry had worked very closely with me. He became very interested in the Differential Analyzer, came over here and spent some months. We gave him our drawings, and we got him to use the machine so that he became acquainted with its possibilities and also with its defects. He went back to Manchester, and there built a machine, and put it into use. This was the beginning of such machinery in Britain. I was very fond of Hartry. He visited me here, and I visited him there, and he was a grand fellow to work with. When Cambridge gave me

an honorary degree, (1950) I was his guest and we had a wonderful time together. He's died since. When Hartry and I were talking with Bragg at Cambridge, Bragg was then working on the location of the hydrogen atoms in the hemoglobin molecule, and he was approaching part of this problem by machine analysis. For this, he was using a digital machine that Hartry had assembled at Cambridge. I call it a machine out of courtesy, and because it worked, but it looked as though someone had driven up with a truck, discharged a whole lot of surplus electrical equipment into a corridor, which had then been piled up and more or less connected. In fact, it looked like the wrath of God. On the other hand, just at that time, Harvard was getting going on digital machinery, and this equipment was downright beautiful, with nice chromium-plated handles and what have you -- all panel-assembled, and an excellent affair. No doubt far better to use in the

1) ANAL. BY MACHINE

2) ENGLAND, BRAGG,ETC.

long run. But the real point was that while the machine at Harvard was not yet really ready to tackle tough problems, the machine at Cambridge was tackling them and getting results. There's a lesson here that we have never completely learned in this country. We are, I think, inclined to overdo the equipment end of things. We rush into great machinery, and sometimes we rush too fast. We get a laboratory with a lot of equipment in it, and then spend too much time making the equipment work, maintaining it, and so forth, and not enough time sitting around a table and really thinking about where we are and where we may be headed.

1) ANALYSIS BY MACHINE

2) BRITISH VERSUS U.S.

* * *

Question 8: K.C. Compton/Bush relationship prior to 1935

I've already answered a good deal of Question 8, and I've reviewed rather briefly some of my relations with Karl Compton. There is one part of this thing which you bring up, though, that is worth some comments. One of the most difficult jobs in the world is to act as second-in-command in an organization. There are not very many men who acquire the skill well. The difficulty, of course, lies in the fact that the second-in-command has got to get things done, and yet he must avoid at all cost any appearance of trying to compete with his chief. Moreover, he may have ideas as to how things should be done, and he should insist on having the proper opportunity to present these to his chief. He also must have the attitude so well acquired by military men, that after a proper airing of views, the big chief

1) COMPTON

2) BEING A DEPUTY

decides. The rest of the crowd then get in line and put his decision into effect, whatever prior opinions they may have held. This is difficult for many men to do, and it's particularly difficult under the freedom of academic life. I've been fortunate in having had one man exceedingly skilled in this regard: Paul Scherer, who backed me up at the CIW during my entire tenure. He had the skills and was so thoroughly liked by the staff that things went very easily indeed. Of course I had the same kind of excellent backup whenever I was in a military situation, because there, the officers learn it plenty. For example, in handling the Research and Development Board of the Joint Chiefs' Organization during the war. The Board, of course, accomplished very little because it had no authority except when it was unanimous, and when it disagreed it

1) CIW

2) BEING AN EXECUTIVE

1) BEING AN EXECUTIVE

2) IN ACAD., BUSINESS, AND GOVERNMENT

3) FAILINGS OF BUSINESS SCHOOLS

could only refer the thing to the Joint Chiefs who would disagree in exactly the same way. (X-REF FORWARD TO PAGES 357 AND 805) But it served as a place where things could be discussed, and sometimes quite frankly. I had three men there as secretaries to that outfit, and every one of them was excellent -- particularly excellent in regard to the points I've just been making. One of them was Pete Cox (who received a medal from the Institute of Chemistry which I presented). [This happened two weeks after VB read the first transcription.]

* * *

The business schools in the country teach some things very well indeed. I think they've failed to teach some elementary things. I can give you some points on this without much trouble, because I've seen them plenty of times. As a matter of fact, I'm one of the few men, I think, who has headed organizations in business, in government, and in academic institutions. (ital)

1) BEING AN EXECUTIVE

2) IN ACAD., BUSINESS, AND GOVERNMENT

3) FAILINGS OF BUSINESS SCHOOLS

The problems are exceedingly different. My conversations with Eisenhower when he was going to Columbia might make some interesting reading, even if we didn't want to use it, for rather obvious purposes. (X-REF FORWARD TO PAGES 590-591) I one time wrote a document for the Merck people that became utilized at the Harvard Business School.(X-REF FORWARD TO PAGES 513, 537-538, 772) Its history showed pretty well that leaders in industry, some of them at least, and teachers in Harvard Business School, taught and taught and taught about some of the fine points and forgot some of the very elementary ones. (X-REF FORWARD TO PAGES 539, 654) One of the elementary ones is the relation of the deputy to his boss. This I don't think gets spelled out. Many such things don't get spelled out. I've been fortunate in having very good deputies. Certainly this was true when I was chairman of the Merck organization and Jack Connor was running it, although this relationship was by no means that of deputy to chief. Jack was the chief executive officer.

* Note to Carol Young from ?: "Is there correspondence as well as conversation?" CY answers: "No"

I was chairman of the board. The relation of two men in these positions is also a subtle thing which hasn't been spelled out well. However, we're talking about something else here, and I think this covers your Question 8 sufficiently.

1) BEING AN EXECUTIVE

2) MERCK YEARS

Question 9: Patents held by VB and in what fields. Patents and inventions in general.

Your Question 9 reminds me of the time that I went before the so-called Monopoly Committee of the Congress, when there was a bit of a battle on. I went under rather strange circumstances. The Committee had given the Commissioner of Patents, Conway Coe, a couple of days without interruption to explain the patent system to them. The Committee in general was rather hostile to the patent system. Coe asked me to go on as his first witness after he got through. I had small doubt what was likely to happen to

1) CONGRESSIONAL TESTIMONY

2) PATENTS

me under those circumstances, for the members of the Committee had been forced to keep quiet while Coe was presenting, and when suddenly released, they were bound to light on the next witness. I never minded that sort of thing particularly, I guess, and sometimes enjoyed it, so I agreed to Coe that I'd go on. Sure enough, things happened in a hurry. As soon as I got on the stand, they began to ask me about the beginnings of Raytheon, and Spencer Thermostat Company that I'd been involved with in the early days, and which Marshall and a little group and I had set up. One of the Committee members said to me, "Now could you do that same sort of thing today?" and I said, "No." He said, "Why not?" and I said, "Because you people sitting on the other side of the table have made it nearly impossible." I made that remark about 9:30 or 10:00 in the morning and I didn't get off the stand till 5:00 that night -- but

1) CONGRESSIONAL TESTIMONY

2) PATENTS

1) CONGRESSIONAL TESTIMONY

2) PATENTS

I certainly got the attention and interest of the Committee. They soon asked me if I held any U.S. patents and I said, "I've held a lot of them." They said, "How many?" and I said, "I don't know, I've never counted them -- perhaps twenty." And one of the members said, "Did you ever make any money on them?" (X-REF FORWARD TO PAGE 593) The chairman interposed: "You don't need to answer that question, Doctor." I said, "Why Mr. Chairman, (ital) I'd like to answer it. The answer is no." Now of course I evaded the question somewhat. I never did make any money out of patents directly, because I almost always assigned them. (X-REF FORWARD TO 593-595) I assigned them to companies I was consulting for, and I assigned them to companies I was helping to build up from little beginnings into what might amount to something. Some of these were helpful along the way.

If the Committee had asked me if I'd made any money out of patents indirectly, I'd have had to say yes. I don't know today how many patents I've had issued in my name. I do know that occasionally there've been patents issued in somebody else's name on inventions that I've made, but that's a slightly different subject. As a matter of practice, I've always assigned inventions, and only in recent years have I ever kept patent rights personally; Since I've retired I've done that for the fun of it on one or two things. When I was a consultant I assigned to my clients, of course. I never would sign a contract with them agreeing to do so, but I always did it. And this was a much better way, because if I had two or three clients, I was the guy who could decide to which one a thing belonged. When I became an executive in academic affairs, I assigned the patents to the organization I was with as a matter of just plain decency.

1) PATENTS
2) ASSIGNMENTS
3) CONSULTING

1) PATENTS
2) ASSIGNMENTS
3) CONSULTING

So I assigned some things to MIT, and MIT never made any money out of those. I think perhaps one or two might have been worth something if the war hadn't interrupted, but, as things were, they never did. I assigned one or two to the CIW. One of these was a thing that I think might go somewhere. Unfortunately the patent that they issued on it was pretty limited -- so limited that I don't think an industrial concern would really be justified in going ahead on the basis of it.

There an interesting point here. I've long been pretty deeply involved in patent matters. I've been on a couple of committees, and a presidential appointment that was supposed to recommend modernization of the patent system. I've testified before committees of Congress, and I've known the Commissioners of Patents and worked closely with them. During the war I had quite a patent organization in my shop, for we had to

1) PATENTS

take care of the situation that might have come out post-war if Tom, Dick and Harry all over the country had filed patents on things that came out of the war research program. (X-REF FORWARD TO PAGES 159 AND 501) We had to protect the public interest more properly, and try to avoid chaos by taking proper steps. With all this connection with the Patent Office, of course, all the patent examiners know that I've been deeply involved and that I've been close to Commissioners of Patents. Hence when the examiners see a patent application with my name on it, they lean over backwards to be sure that they don't show any favoritism. This is exactly correct; but they certainly get rough at times. One of the times they really got rough was on the thing I assigned to the CIW. I never blamed them, of course, a bit. In fact I'd be very disturbed if I found any other kind of attitude among the

examiners. Let me say in passing that while I've been in touch with the Patent Office in one way or another for perhaps fifty years -- I filed one patent when I was an undergraduate at Tufts [on an interesting gadget you ought to ask about] -- (X-REF TO PAGES 595-597) I have never seen an instance where there was the slightest question as to the integrity of the patent examiner. They have a fine reputation which they've earned completely.

1) PATENTS

Question 10: Washington associations prior to 1939; Chairmanship of NACA

I'll take only one part of Question 10 because we're getting toward the end of this reel. I became chairman of NACA in 1939. (VB was Chairman of NACA from 1939-1941)* This was an extraordinary organization; it was, of course, the forerunner for NASA. It was an interesting form of governmental organization. It operated laboratories, but it operated them

1) NACA

2) FIRST TIME IN WASHINGTON

* Note Hilard Young

under the control of a committee, which was made up of representatives from the military services, some other governmental bureau chiefs and the like, and a considerable number of independent citizens, some of them from industry, some of them quite independent, as I was. This committee selected and supported its own director of research. I was quite opposed to the form of NASA when it was set up. (X-REF FORWARD TO PAGE 258) I thought at the time it would have been much better to have continued the old NACA type of organization, although I have to admit that I had no concept at the time as to the amount of money that NASA was going to be spending.

1) NACA

NACA was started way back in the dark ages of aeronautics. It did fine work. Governmental laboratories were necessary because no individual firm in the field of aeronautics could afford the expensive research

that was necessary to advance the whole field. 1) NACA Ames had been the chairman for years. (X-REF FORWARD TO PAGE 260) He began to fail physically and he had to be replaced and the necessity came very suddenly. So they picked on me to take over the chairmanship. I always maintained that they did so because I knew nothing whatever about aeronautics, which was quite true, but I took it over as an emergency thing. When the war came along I moved out of it, and Jerry Hunsaker succeeded me. He was the man to run the show, because he really did know something about the subject, having been one of the chaps, you know, who was very instrumental in the dirigible program, and who was, I think one of the NC4 fliers to go around the world and so on.

NACA did something for me. It taught me a great deal about governmental organization -- the operation of governmental laboratories -- and it taught me still more about relations

with Congress. (X-REF AHEAD TO PAGE 261 RE LEARNING ABOUT GOVERNMENTAL OPERATIONS) The first time I ever appeared before a Congressional committee, I made a mess of it, and I took a beating. (X-REF AHEAD TO PAGES 262, 631, and 697) (QUERY: cf PAGE 67) Fortunately I had at my right hand a man named John Victory, a very skilled fellow who'd been with the organization for years as secretary, in fact since its inception. He knew his way around Washington, and he coached me. I learned a lot about what not to do before Congressional committees. I also learned a lot about how the government operated more generally: the tough inter-relationships between governmental agencies in the city of Washington, and all that sort of thing. There's no question that this stood me in good stead when I got immersed up to my neck in the whole affair later on. There are a whole lot of incidental stories about how NACA operated, some of the jams we

1) NACA

2) TESTIMONY BEFORE CONGRESS

got into and so forth, but this is probably rather off the track for here.

So we'll close this particular reel with the fact that there's very little in here that's pertinent to the thing we're trying to do, but some of it's interesting background, and I just hope it doesn't wear your ear out as you sit down and listen to it.

END REEL 1-B