

Question 94: VB Comments on Generals and Admirals
Not Already Covered in Question 93

- 1) POST WWII
- 2) OMAR BRADLEY
...HIS BOOK
- 3) MONTGOMERY

I didn't know Omar Bradley until after the war. Thereafter I saw quite a lot of him; first on the Joint Chiefs of Staff, then he became one of the trustees of the CIW. I got to know him in that way, and became very fond of him. Incidentally, in reading his book [REFERENCE] one gets excellent estimates in my opinion of Eisenhower, Montgomery and George Patton. [X-REF BACK TO P. 173] I think these are fair and well-balanced. Montgomery, you know, got very mad about the book and made some of his bright remarks -- but Bradley gives him credit for his accomplishments, and where he criticises it seems to be well-warranted. [EH to JK: SEE ME ABOUT 1965 BLAST AT MONTY BY BRADLEY.]

This of course brings up the point that I can tell the story of my visits with Montgomery at the Chateau de Courance [VB VERIFIES SPELLING] after the war when I went over on some planning. [X-REF AHEAD TO P. 765] The exercises were being held in order to work out ways of handling an army in the field in the presence of an enemy having A-bombs. Montgomery was making the plans; thus it was that I lived with him for several days in the Chateau de Courance and we had some very interesting occasions.

My comments on Bradley are merely that I think he's a grand fellow and I think his book is accurate. It gives the best impression that I've got anywhere of the way Ike operated

and so on. His handling, for example, of the slapping incident by Patton is, I think, a fine piece of writing. [REFERENCE... THE BOOK]

Now in regard to Montgomery. He has the reputation of being very egotistical and so forth. I can give you another slant on this. One night at the Chateau de Courance I was dining with him, a couple of his generals, men who had been with him in the Alamein and a few others. I think there was one other American present but as I remember, he was a junior. After the dinner, Montgomery said to me, "Doctor, will you give us a text we can discuss?" And I said, "Very well." [X-REF AHEAD TO P. 764] And after a moment's thought, I said, "The heavy tank as used in the last war is now obsolete." Now this was addressed to the greatest tank fighter in history, but Monty pounded the table and said, "That is excellent; we shall discuss that." We did, for an hour or more -- and nobody was pulling any punches. We discussed the advance in antitank weapons and recoilless guns and various other things that had come into warfare and debated whether the tank had reached the limit of its usefulness since it now required heavier armour in the presence of shaped-charge shells -- and so on. It was the kind of discussion one would have where no rank was involved. I think this throws a different kind of light on Montgomery than is usual.

A day or two later I was in his headquarters up at Roquencourt [VB VERIFIES SPELLING] and he was giving a press interview in the other room. I sat in the anteroom with some

1) POST WWII

2) MONTGOMERY AT CHATEAU de COURANCE-"DOCTOR, WILL YOU GIVE US A TEXT?"

of his staff. There was a mike in the room so that we could listen to the conversation; in other words the room was bugged by Monty himself. These staff men were having a fit because they seemed dead sure that Monty would put his foot in it somewhere in the course of the interview. As I listened, I thought he did once or twice. He's quite irresponsible when he gets talking.

Another incident shows him in a different way. At one time I was talking with Monty about something and he said to me, "You know, I think your President ought to know about that. I'm going to be in the States shortly, and when I see him I'm going to take it up with him." I went to Al Grunther and said, "Al, I want to recite a conversation I had with Monty." When I did, he said, "You don't need to worry about that, Van. Monty would never take anything up with the President without clearing it with me first." This is extraordinary because here was Al Grunther who had no great war record but had, as a deputy, the great Montgomery. Yet Montgomery was apparently loyal to him and was careful in his relations not to step out of line.

[This started out to be something about Bradley but I don't think you mind if things get a little mixed up.]

[SMALL CUT HERE ON JIMMY DOOLITTLE]

No, I never knew either Admiral Kimmel or General Strong. I do know that what happened at Pearl Harbor was quite absurd. We had a chap in the Carnegie Institution by the name of Johnson who was out there at the time to install some stuff. He

1) MONTGOMERY'S
ALLEGIANCE
TO GRUNTHER

was working with the Navy on torpedo development and that kind of thing and incidentally can tell quite a story of the time when our submarines at great risk got within range of a ship and fired a torpedo and the torpedo wouldn't go off.

[X-REF AHEAD TO P. 755] [EH to VB: WANT MORE DETAILS.]

[VB to EH: Torpedoes were handled by a special group in the Navy with vested interests. They improved the torpedoes from time to time, but they did not properly compute their work. Hence defective torpedoes were sent to submarines. I can't document this of course. But as I understand it the firing mechanism, on a faster torpedo, crushed before it operated. Hence subs worked for days to get in firing position on a target, made a hit, and got no result. The sub commanders griped (vigorously I judge) to headquarters in Hawaii, who then griped to Washington. Getting no satisfactory progress, the Navy Commander in Hawaii set up his own shop, and reviewed the firing mechanisms. Ellis Johnson, a man from CIW, who after the war headed the Army operations research group, went into the lab and helped. They succeeded in making a change in the firing gadget so that it worked. Johnson could give us the full story I have no doubt.

[This is one of the places where the Navy was hidebound when the war started.] [END WRITTEN INSERT]

This brings up quite a point about ingrowing groups: the torpedo group in the Navy allowed no one else to have anything to do with torpedoes and they did it badly themselves.

- 1) KIMMEL AND STRONG
AT PEARL HARBOR
- 2) TORPEDO DUDS
THE NAVY

Well when Johnson was out there, somewhat before Pearl Harbor, he saw on the dock crated radar sets that had never been uncrated and had been sitting there for a long time. This shows the type of thinking that was being done not merely by Kimmel and Strong but by many others.

One strange thing about Pearl Harbor is this: In the Philippines there was just as great a surprise. We lost a whole lot of planes on the ground and it crippled us, decidedly. Yet in the criticisms that came afterwards, one heard criticism of Kimmel and Strong, but one did not hear similar criticism of MacArthur.

* * *

Admiral Halsey, oh, I've met him and so forth. I think about the only story I can tell about him is that one time in the second battle of the Philippine Sea when Halsey turned his battleships north and completely missed the battle. We came pretty near to having a catastrophe. When that was going on, the Navy, of course, was getting reports hour by hour. I was down in the Navy Department with a good friend of mine, an Admiral [VB ADDS: PURNELL], and we were getting these reports and watching the show with very keen interest. We were talking together about whether our battleship fleet was going to be out of the show completely and I said, "I can't understand how Halsey made that move." He said to me, "Well, didn't you know, Halsey is dumb as hell!" Apparently that was the opinion around the Navy Department.

- 1) UNCRATED RADAR ON THE PEARL HARBOR DOCKS
- 2) CRITICISM OF KIMMEL & STRONG BUT NOT OF MacARTHUR
- 3) WWII -- ADMIRAL HALSEY'S GOOF

Just the way in which men get promoted to high command in time of war depends, of course, upon the judgment of their senior officers. The fact that we had perfectly swell commanding generals in France I think is undoubtedly due to the fine judgment of Marshall in regard to his officers. [X-REF AHEAD TO P. 714] Ike may have come into it and undoubtedly did but I think the real judgment of our personnel was exercised principally by Marshall and that it was awfully good judgment. On the other hand, I don't think King knew his men to anywhere near the extent that Marshall knew his. This was probably because he was such a tough, distant guy: he kept himself so remote that he didn't really get to know them. At any rate, with Halsey we had a fellow who did not have, in my opinion, the mental capacity for the enormous responsibilities he carried.

[SMALL CUT HERE ON GENERAL MARK CLARK]

General MacArthur. Well, I could say a great deal.

[EH to VB: AND I WISH YOU WOULD! WITHOUT SPECIFIC PROMPTING FROM ME, TOO.] I in particular never had any sympathy for MacArthur at the time of the MacArthur-Truman fiasco. I felt strongly, and I think my military friends did also, that for him to defy the President of the United States was unforgivable.

I had no direct contact with MacArthur during the war. I did, however, have this experience: he sent word that he wanted a man attached to his staff, attached to him personally, who was a scientist and who was thoroughly posted on all the recent scientific developments, so that he could consult him

1) ESTIMATES OF KING &
MARSHALL IN PROMOTING
OFFICERS

2) OPINION OF MacARTHUR

on the spot. This looked like a very important assignment. I asked Karl Compton if he'd like to go out; he wanted to and he did. Before he went, he took time to be sure that he was posted on everything recent, on all of the possibilities of production of devices and so on. He went out to the Pacific and when he came back he told me that when he arrived MacArthur received him in a half hour interview during which MacArthur did all of the talking. Thereafter Compton wasn't called in on anything of scientific importance. So this sheds a little light on MacArthur indirectly. [VB ADDS:

[Ed Moreland went also. Compton and Moreland had good relations with many officers, and something may have been accomplished. But MacArthur's message indicated that he, personally, wanted to be brought up to date.

[Most of my impressions of MacArthur came from what I read in the papers.

[I think Truman could have handled his part better. If he had called MacArthur to Washington for a conference on Far East strategy, the General would then have been forced to make a quiet entry, and couldn't have talked. Then if Truman had handled him through the Chief of Staff, it would have been a very different affair.

[MacArthur was dangerous -- the man on a White Horse. Think what would have happened if he had been in the position of Ike or Marshall.] [END WRITTEN INSERT]

I never had, as you can see, great admiration for him.

- 1) BUSH's OPINION OF DOUGLAS MacARTHUR
- 2) COMPTON AS ADVISOR TO MacARTHUR

I have told one story about Mountbatten. He was a great rollicking fellow who undoubtedly had great influence on the esprit de corps in Great Britain. I can't judge his performance out in the Far East and I haven't much more to add to what I've already put in. [X-REF BACK TO PP. 138-139]

I never met Stillwell.

Somervell, I've already mentioned. I think Somervell undoubtedly did a good job on the Service of Supply. Every time his path and mine crossed there was a bit of a fencing match. As I told him once or twice, if my shop was any good whatever it would make continual trouble for him, as indeed it did. He used to tell me that I was more damned nuisance than anybody else in Washington. I don't think he had much direct contact with what I was doing. He merely came into it when some new radar or some new device of some sort was getting into production and thereby balled up all his planning and schedules.

He opposed the Dukw as I've recited [X-REF BACK TO PP. 409, 457] and in general he had no great enthusiasm. Fortunately, as I've said, he had a fine deputy in Styer and Styer made up well for the lack of attention to the new devices by Somervell himself.

- 1) MOUNTBATTEN
- 2) BUSH's RELATIONS
WITH SOMERVELL
- 3) STYER

Question 95: VB more involved in European Theatre than Pacific during WWII

Now on question number 95. I've already told the story of Karl Compton going to the Pacific. As a matter of fact, he got there quite a while before the Japanese surrendered and I think he was back well before that time.

Certainly our interests were primarily turned toward Europe. That was because there wasn't too much that we could do in connection with the Pacific. For one thing, we didn't work as well with the Navy as we did with the Army. We did have a great deal of activity on antisubmarine matters and on the submarines themselves; one thing I've mentioned, the torpedo business. Of course, there were all sorts of things going on on magnetic torpedos and all that kind of business. We also had some activity in connection with mines and so on. But in the Pacific, our principal contributions were the proximity fuze, (which has been mentioned) radar, (of course with its improved forms for use in aircraft) and finally rockets for preparing a beach for landings. And these were quite important:

The difficulty in making the rockets that we ran into was an odd one. It was easy enough to design the things, or fairly easy. They had to be made in very large numbers and cheaply, and we couldn't get the powder to put in them. The British had a mixture that worked well in rockets and we tried to get an American explosives company to manufacture

- 1) WWII
- 2) OSRD DID NOT WORK AS WELL WITH NAVY AS WITH ARMY
- 3) SCIENTISTS CONTRIBUTIONS TOWARD WAR IN PACIFIC
- 4) DIFFICULTY OF GETTING POWDER FOR ROCKETS

this stuff for our experimental purposes. They wouldn't do it. They were afraid of it -- afraid of the dangers of it -- and quite rightly so, it was rather delicate stuff.

I remember this came under Dick Tolman. One day we were having a discussion of this thing around the table [I guess in the NDRC days before OSRD was formed]. Dick was explaining what the powder was like and the difficulties of getting it manufactured. I suddenly tumbled to the fact that we had large chunks of the stuff all around the table and that various people around the table including myself were smoking! We snapped out of that pretty quickly after we realized it. Of course if you touch the stuff off with a match it didn't explode, it merely burned violently. Nevertheless.

So, we couldn't get the powder manufactured by an explosives company. The British, of course, had no chance of supplying us. So we got a university to make it: we got Cal Tech to make the stuff, and to take the responsibility. They set up the manufacturing in a canyon in California which was a convenient place because the canyon had little branches. If you put a mill in one of these branches and it blew up it wouldn't damage the rest of the show. Cal Tech set up machines in these canyons which were completely automatic. You dumped in the ingredients and then you left; after you'd left, the machine started up and did the job. It was principally an extrusion process. One or two of them did blow up but it did no harm whatever of

- 1) WWII
- 2) POWDER FOR ROCKETS
ON CONFERENCE TABLE
- 3) CAL TECH MANUFAC-
TURES ROCKET POWDER

course under those circumstances.

Out in the open we had a bunch of huts, little things ten to twelve feet square, set at intervals. In each hut was a girl and a lathe. She took one of these sticks of powder and stuck it in the lathe and turned it down to size much as you'd turn a piece of steel or wood except that this stuff was easily cut, was more or less like wax. Of course the chips were taken away every little while. Also the two doors were always wide open so that if anything caught fire, all the girl had to do was to step out. And at no time did any of these catch fire that I know of.

Now it was not possible (that is, we didn't think it was, and I guess it wasn't) to make one of those things explode in that way. You might make it catch fire but that's all. At any rate, there were no accidents there. We made a lot of explosive powder there because when we'd got the stuff so that the Navy wanted their bombardment rockets in quantity it was easier to keep that show going as it was than switch over to some company. So Cal Tech continued to manage and we continued to pay the bills.

There was only one accident in this whole affair. One man was killed -- and he was the man whose job it was to enforce safety regulations inside the area. Being in charge, he violated the regulations himself and blew himself up. That was the only casualty.

- 1) WWII
- 2) CAL TECH MANUFACTURES ROCKET POWDER

I had a great deal to do with some of the admirals operating in the Pacific. One of the finest ones was "Ching" Lee. I don't know whether the story has ever been told; if it hasn't it should be. He was headed for Guadalcanal with a division of battleships. The Japanese fleet was north of there; it was fully expected that they would descend shortly. At that time the only thing standing between the Japanese fleet and a U. S. transport group was a group of PT boats under the command of a young lieutenant commander. Fortunately the Japs did not move down until Lee arrived there. He went up through the straits at night and he called the skipper of the PT boat on his phone in full clear; in other words, not in code: he spoke right out. When he got him on the line he said to him, "This is Ching Lee speaking. Do you know who I am?" The boy said, "Yes sir." Lee said, "All right, I'm coming through and you boys get the hell out of here." So the PT's proceeded to run ashore and the battleships went up through the straits. A day or two later they engaged the Japanese fleet with the results you know. (I knew Ching Lee in connection with the Joint Research and Development Board of the Joint Chiefs of Staff on which he was a member for a while.)

Lee told me the story also about how, after that night battle, he had a rendezvous with the battleship, Indiana. It was agreed that they would meet at a certain point and Lee was lying there in the semi-light waiting to be joined,

- 1) WWII
- 2) ADMIRAL CHING LEE
HEADING FOR
GUADALCANAL
- 3) CHING LEE'S
RENDEZVOUS WITH
THE INDIANA

when a battleship began to close on him. As soon as they sighted it, Lee's ship began giving recognition signals, but got none back. They tried all sorts of ways of recognition. The battleship came on; they trained their main battery on it. Only when it got near enough so that they could make out some of its details did they recognize that it was the Indiana rejoining them. The Indiana had been so plastered with light stuff [EH to VB: GARBLE?] [VB to EH: LIGHT STUFF MEANING SECONDARY BATTERY] that all her radar had been knocked out; nor could she give any recognition signals. This was before portable radars had been put on the big ships so that if all their radar was knocked out they could bring up one from below for that purpose. [X-REF BACK TO ADM. KING P. 467] Now this seems to be a rather elementary sort of thing on a battleship yet that was the circumstance. It came pretty nearly being disastrous at that time. (Lee was training some of the units for the invasion of Japan up on the coast of Maine when he was killed by a heart attack.)

- 1) WWII
- 2) CHING LEE's
RENDEZVOUS WITH
THE INDIANA
- 3) PURNELL & JAVA

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I've spoken of Purnell who was on the Policy Committee on the atomic energy matter. He had been through the whole show at Java where we got the spots knocked out of us. It had been a rugged thing indeed. And he told me a great deal about it when we were relaxed in various ways. [? to ? : ARE

YOU INTERESTED?] [EH to VB: I HOPE YOU WILL FEEL LIKE
ENLARGING ON PURNELL & JAVA.] [VB to EH:

[I couldn't today reproduce any of Purnell's stories
of the Java affair. The story of some of the clumsy things
we did in the Pacific has never been really told to my
knowledge. We were caught with inferior forces under
conditions where we couldn't just clear out.

[The Halsey story, the loss of the two cruisers due
to failure to use radar properly, these things were just
as bad as Pearl Harbor in some ways. Morison doesn't, I
think, really tell them, for he avoids treating new devices
in all of his I've read, and I suppose he wouldn't care to
knock the Navy too hard. Also I suppose it would do no
real good to tell the sad story now.] [END WRITTEN COMMENT]
I knew a lot of the Navy that were operating out there, of
course, in one way or another. But, as you say, our eyes
were turned toward Europe as in truth they should have been
under the policy that had been completely adopted.

Incidentally speaking of the China-Burma-India theatre
there's one thing that we ought to pin down sometime. A new
type of radar was shipped out there to help in getting the
planes that were lugging stuff into China over the hump..
[X-REF FORWARD TO P. 767] They were having a hell of a time
going through the pass in the mountains. A special radar
unit had been built for explicit aid in that. I never got
the story completely but there was a chap who went out with
it. I think it was one of Hartley Rowe's men, but I'm not

- 1) WWII
- 2) MISTAKES IN THE
PACIFIC THEATRE
- 3) RADAR INSTALLED
FROM WHEELCHAIR

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sure and I've forgotten his name. At any rate, when his plane landed in India it cracked up and he busted his back. They put him in a cast and he sat in a wheelchair and in that condition supervised the installation of the radar and its introduction into use. I think we ought to get that chap's name some time. Probably Palmer Putnam knows it.

[EH INDICATES N.B.] [VB NOTES: I THINK PALMER PUTNAM COULD FILL IN ON THIS IN SOME DETAIL. IF NOT, SOME OF THE RADAR MEN MIGHT.]

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Question 96: Wartime Scientists: Szilard, Fermi, Rabi, Bohr, Urey, von Neumann, etc.

As you've seen on the atomic energy matter, I handled the relations with the Army, with Congress, with the President and so on. [X-REF BACK TO P. 364] Which kept me plenty busy. Conant handled the internal affairs with the scientists directly before we turned the thing over to the Army; then we acted in concert with Groves after that. So Conant had far more contact with the scientists than I did.

One thing I can remember that's worth telling about: Arthur Compton was doing some very important work in Chicago, of course in this connection. He used to come in my office once in a while and tell me how he was getting along and how he was going about it. He would say that he was doing this because he wanted to be sure that he was handling the matter in the way that I wanted him to handle it. Now this, coming from Arthur Compton, was really something because he knew perfectly well that I didn't know enough about atomic physics to be any judge. But I certainly appreciated it.

I saw very little directly of Szilard and Fermi and the rest of the group: most of the contact there was with Conant. I did see Rabi quite often but that was because of other connections and I've recited one or two things about him. [X-REF BACK TO PP. 308, 310, 372, 422]

Bohr was a problem. He was kept under wraps; he went by an assumed name and all the rest of it. Poor old Tolman

- 1) WWII
- 2) RELATIONS WITH SCIENTISTS
CONANT HANDLED
INTERNAL AFFAIRS

had to handle it. Bohr was not, as far as I know, of any great help. The reason for that, of course, was that during the war and after the atomic energy affair got well under way the problems were not those of fundamental new physics; the problems were engineering problems and applied physics.

[VB to EH: THERE IS SOME ON BOHR IN THE TIZZARD BIOGRAPHY.]

There was some science of course but there were highly able men to deal with it; Bethe, for example, was the fellow, I think, who calculated the size of the explosion. He was as capable to do that as Bohr was. So was Fermi. So Bohr was not in a position to help on the fundamental stuff much and he certainly was not the type of man who could help on the applied aspects.

One time well along in the war, Bohr got the idea that he must see the President about some plan that he had for the end of the war, or the peace terms or something. He explained it to Tolman and Tolman told me the problem he had before him and I didn't understand what he was driving at. I saw the President one day and I said to him, "You know, I'd appreciate it, Mr. President, if you'd see Bohr for a few minutes." I told him who he was, and what an eminent scientist he was. I said, "He's very anxious to see you and I think, in Scandinavia at least, after the war it would help toward good will if you'd personally talk to Bohr." The President said, "What's he want to see me about?" And I said, "He's got some kind of a plan and I think it has to do with peace but I'm not sure." He said, "Do you think

- 1) WWII
- 2) BOHR WAS NOT OF GREAT HELP
- 3) BOHR'S MEETING WITH THE PRESIDENT

I'll be able to understand him?" And I said, "No, I don't think you probably will." He said, "Fine, bring him in." I did and Bohr got off his stuff and went away perfectly happy. I don't think that the President really understood him at all.

The story of Urey is a difficult one to tell. [EH INDICATES N.B. THIS PARAGRAPH] It's one of the things that I might put into the record but never would publish. Urey was, of course, a very able physicist, but he was a strange fellow individually. In connection with the atomic energy project, before he'd gotten very far, I began to get stories about his carelessness in talking with people who had no right to the kind of information they were getting. Urey was just unstable in certain ways. Another thing: he'd sit in committee with all the rest of the atomic scientists, with Conant presiding; all would agree to a course of action -- and then the next day Urey would oppose it. This made Conant quite irritated.

At the time when the Manhattan District took over there was a whole new system of clearances. My system of clearances had applied before that but of course the Manhattan District's own system came into effect. For some time I refused to clear Urey. Moreover, I sat down with him and told him why not: that there was no question whatever about his loyalty, but there was merely the question of whether he was so emotionally unstable that it might be dangerous to get him in too deep. He took it very well and

- 1) WWII
- 2) UREY TALKED TOO MUCH AND WAS UNSTABLE ABOUT DECISIONS

thereafter he behaved very well. After a fairly short time, I cleared him. But he was that kind of a guy. I think if you follow his full history, you'll find that's typical. He gets going on anything and then gets so emotionally charged up that at times he becomes quite unreasonable.

Johnny von Neumann. I knew him in connection with computing machinery and I knew him after the war when he was doing his great work on the theory of games. He was about as likable a chap as you could imagine.

There is just one short thing about him. I was riding in a Pullman one day in the lounge car after the war and I hadn't looked about me before I sat down; I was reading something and it had my full attention. From across came von Neumann. He sat down aside me and introduced himself. Well here was the man who, in my opinion, was the most able mathematician in the country in many ways and he felt that he needed to introduce himself to me. That's a type of modesty one can't help liking.

He certainly, if he had lived, would have been a great factor in the development of analytical machinery. He was, even as it stands, but had he lived longer he would have had a still more sparkling career. He not only had a profound knowledge of mathematics, he had in addition, ingenuity, an insight, a judgment that was extraordinary.

- 1) WWII
- 2) JOHNNY von NEUMANN
VERY LIKABLE

Question 97: Wartime Politicos: Ickes, Henry Agard Wallace,
Acheson, Vinson, etc.

I haven't said much about Jimmy Byrnes. I didn't come into contact with him a great deal until the time of the conferences after the war, particularly the Attlee Conference. I remember well his leaving the White House after that conference at midnight with a straw hat on sideways, jaunty, not a worry in the world, Secretary of State of the United States just leaving a very important international conference. He was just a fine Southern gentleman enjoying his associations. [X-REF FORWARD TO P. 734]

- 1) IMPRESSIONS OF PEOPLE
- 2) JAMES BYRNES
- 3) HAROLD ICKES
- 4) HENRY WALLACE

Harold Ickes, I'm glad to say, I never crossed swords with. I think I probably would have if I'd met him.

There is quite a story about Henry Wallace. I knew Henry before the war. In fact, when I lived in the Wardman Park Hotel he had a suite over mine and sometimes we'd take a walk together on a Sunday morning. He was a delightful individual to mix around with. I remember visiting his Beltsville show where we took great joy in kidding some of the people who were raising turkeys and what not. He and I played the game back and forth for the fun of it. He had a good knowledge of biology; he was a good fellow to reason with, and all of that.

Then during the war the first place I ran into him was this: [EH NOTES: HOLD YOUR BREATH UNTIL P. 489] just as the atomic energy thing began to have very large implications,

when it had become pretty clear that the thing could be done, and so forth, I said to the President one day, "Mr. President, I'm carrying quite a load of responsibility on this thing because it's a matter requiring strange judgments. I'd feel a little bit more comfortable if I had someone else, some group to report to, so that I wouldn't be bothering you any more than necessary as this thing goes along. He said, "Very well, let's have a policy committee." He named Mr. Stimson, General Marshall, Jim Conant, Henry Wallace, and of course myself. I said to him, "How about Mr. Knox?" He looked at me with one of his strange smiles and said, "No, I guess not, not now." I think that he was afraid that Knox was inclined to talk too much. At any rate, that's what he did.

I don't think that there ever was any formal appointment of that committee. It certainly never met but after that whenever I had a report of any consequence to go to the President, I'd go to each of these individuals beforehand, tell him what was in it and talk to him about it. With Mr. Stimson I'd be likely to go into it in some detail; with General Marshall I'd just mention it, he wouldn't show much interest and I wouldn't go much further. I also took it up with Henry Wallace. Then one day I went in with a report to the President. When I gave it to him I said, "Mr. President, your policy committee has approved this with the exception of Mr. Wallace who is not available." He grinned at me and said, "Well, Henry's out West making political speeches. I don't think we need to worry about him." That

1) WWII

2) FDR DROPS WALLACE
FROM ATOMIC ENERGY
POLICY COMMITTEE

was my cue and I never went near Wallace on atomic energy matters after that.

That is the way Roosevelt operated on such things. He wouldn't say flatly, "Now we'll drop Wallace off the committee," or anything like that. He'd just give an implication and expect me to catch it. So that was that. There were no hard feelings between Wallace and myself as a result. But I think that was the time when Roosevelt had decided that he would not continue to regard Wallace as the man to take the Vice Presidency -- and that's how Harry Truman became President of the United States. [EH NOTES: MUCH REFERENCE TO SOURCE MATERIAL NECESSARY HERE (this whole page)].

I saw very little of Dean Acheson during the war -- just occasionally on some things. But after the war I worked with him on several things. One was on the committee set up by Acheson to prepare the atomic energy matter to go to the United Nations. The President had agreed that that would be the procedure after taking the matter up with the Cabinet. Then there had been the Attlee Conference and the British and the Americans had agreed on that course of action. Acheson set up the committee and we worked pretty hard at the thing. Oppenheimer was a member. [X-REF AHEAD TO P. 736] I can't recite all of the members; there were six or eight of us. Acheson met with us some times and there the whole procedure was worked out, with a lot of thinking as to Russian reaction and all the rest of it. Then, when that report was submitted, Bernie Baruch was called in as the man who was to take the thing to the United Nations.

- 1) WWII & POST-WWII
- 2) FDR DROPS WALLACE FROM ATOMIC ENERGY POLICY COMMITTEE
- 3) ACHESON'S COMMITTEE TO PREPARE ATOMIC ENERGY DATA FOR U.N.

I've known Bernie Baruch pretty well and I've had some very nice relationships with him, but on this occasion he proceeded to announce that he was going to make [EH to VB: WORD?] [VB to EH: PUT TOGETHER, REVISE, I DON'T KNOW.] the plan. These fellows who'd worked for a couple of months like the devil were not overjoyed at the idea of Baruch taking their report and making a radical revision of it. When I next saw Bernie and I said, "Look Bernie, this is no go. These fellows have done a lot of work on this and they've got a good plan, a good report. What the hell, how are you going to improve on it unless you get a similar group together?" He said, "Now Van, don't worry about that. I have my fellows at work with me and I have to feed them a little something once in a while to keep them happy." [EH to VB: WHO DID BB MEAN BY "MY FELLOWS"?] [VB to EH: THE CADRE HE ALWAYS PULLED IN (AND PAID) WHEN GIVEN A JOB. DON'T REMEMBER ANY NAMES NOW.] So I circulated around and told the gang that had been working on Acheson's committee, "You don't need to worry, Bernie really isn't going to revise that report." And he didn't. But it did become known as the Baruch Plan. And it wasn't the Baruch Plan; it was the plan prepared by Acheson's committee.

And Bernie did another thing: he told the press (or at least the press quoted him as saying) that Conant and I were going to be his aides as he proceeded to get "his" report ready and take it to the U.N. So I said, "How come?" to Bernie and he said, "Well, you are, aren't you?" I said,

- 1) POST WWII
- 2) BARUCH TAKES OVER
ACHESON REPORT

"You mean that I'm going to report to you?" "Why sure," says he. I said, "You know a damn sight better than that. I never worked under you." "Oh, alright" says he, and things went along as before. He'd never take offense at that kind of a thing. By the same token, I never would work under Bernie Baruch, fine chap that he is and all that. He just is not the kind of a guy that I'd want to serve with, let alone under.

Fred Vinson I didn't see until after the war. Then when he was Chief Justice of the United States he automatically became Chancellor of the Smithsonian Institution. I was a Regent: in fact for some time I ran the doggone thing when things were going to pot under the old secretary. After Vinson came in as Chancellor I had very nice relations with him indeed.

When Leonard Carmichael came to Washington to head the Smithsonian, I put on a dinner for him. I got Vinson there and Vinson gave a most humorous sort of a speech. Incidentally, I had every man and his wife that was important in science and government at that dinner. I worked a little trick being able to read a card at six feet -- that is I could at that time. I introduced these chaps to Carmichael one after another, gave their names, their positions, and some of their history in so doing. A number of them afterwards said to me that they couldn't imagine how I could possibly do that with some forty men. They didn't know that

- 1) IMPRESSIONS
OF PEOPLE
- 2) BARUCH (Cont'd)
- 3) FRED VINSON
- 4) DINNER PARTY WHEN
CARMICHAEL BECAME
HEAD OF SMITHSONIAN

Reel 8-A
Page 493

I had a stack of cards and a guy that moved them over as I went along and that I was really reading the stuff as I went.

1) FRED VINSON

Vinson was a pleasant fellow. His influence on the Smithsonian was excellent. I got to know him pretty well and we used to joke together. He was not a great Chief Justice in the sense that Chief Justices are usually measured. I've worked with Stone and Stone was not, in my opinion, a great Chief Justice either. But I wish today that we had either one of them back.

Question 98: Atabrine, Perkin's Mauve, & German Attack
on British Trade

Well, of course, there were a number of things that sparked the modern splurge of science and its applications. The German success with the synthetic drugs was one of these. There were others. The advent of radio and the fact that it brought in a lot of amateurs who recognized for the first time what waves and the ether were like and so on. But this question reminds me of a bit of a story.

During the war we had a group in the medical committee working with the National Research Council looking for better ways of treating malaria than was available through Atabrine. Bob Loeb headed this up. It was primarily centered in the Research Council but it was OSRD'S money and in the last analysis I controlled it. We had university groups all over the country synthesizing new materials; we had a center for evaluation; we had groups trying the stuff out on fowl malaria; we had a bunch of volunteers in penitentiaries willing to be guinea pigs. In this way we went through fifteen thousand organic compounds before we hit on one that was better than Atabrine. By the time they hit on it, the war was nearly over. When they hit on one, they also hit on two or three others; the modern treatment of malaria comes from the things they had developed.

Now in the course of this, when it was going well, a chap named Woodward who had been a star student at MIT (in

- 1) WWII
- 2) NRC & OSRD SEARCH
FOR MALARIA
TREATMENT DRUG

fact, I think he took his doctorate in three and a half years after entering the place or something of that sort), had become a professor of organic chemistry at Harvard and he did synthesize quinine for the first time. But, by that time, there was no need of it. One, Atabrine was better; two, in accordance with Woodward's synthesis the quinine would have cost fantastic amounts and was entirely uneconomical so that it was of academic interest only. But this didn't prevent Ralph Ingersoll in his newspaper, called PM, from attacking Conant and me and saying that we were suppressing this great advance on the synthesis of quinine because the pharmaceutical companies had plantations where they were raising quinine; that we were protecting them and presumably were doing so for something handed us under the table. His stuff was libelous of course. But both Conant and I disregarded it.

Some of the young fellows in the office were bothered that I wouldn't reply. I told them, "There's something you don't understand about this. If an outfit like PM attacks you in such an extreme manner, you can sit back and feel comfortable. In the first place, nobody will copy them and in the second place nobody will believe them." And that was just about the size of it. The thing blew over, nobody did pay much attention to it. And as an example, I never got a question on it in any hearing in Congress.

- 1) WWII
- 2) INGERSOLL's PM
ATTACKS BUSH AND
CONANT ON QUININE

Question 99: Some paths, when taken, occlude later free choice: eg. Steam engine for automobiles

The story of the steam automobile was a very interesting one. When steam automobiles were in their prime, when the Stanley Steamer was built, when I owned one and so forth, the steam car strikingly out-performed the gasoline car. Now, two things happened. The Stanleys held the patents which pretty well controlled this situation. There were competitors, White for example, but the Stanley Steamer people had things pretty well in hand. For a long time they were very progressive and developed the car. They put on a condenser on the engine, for example; before that you had to run a few miles and get out a bucket and bail some water out of a stream to keep going. But the Stanleys got old and they ceased developing; they stood still, and nobody else could move. The Stanleys didn't, as far as I know, license freely under their patents and by the time the principal patents ran out, the show was pretty well over. [X-REF AHEAD TO P. 639] We'll have to check this: [N.B.] I couldn't document it. I have that impression or I had it many years ago that this was the situation. But I couldn't prove it.

1) FLASHBACK --
STANLEY STEAMERS

The second thing that happened was this. The gasoline engine developers did get legislation enacted in various places which prevented an open flame in a garage. This was fairly easy to do because of course town fathers generally were afraid of fire. But so far as the technical aspects

went, it would have been possible to prevent flame hazards for one could have enclosed the flame as in a miner's lamp except when it was in use -- or one did not need to have the flame in the garage. After the fire went out on a steam automobile, you could run it for many miles on the steam still in the boilers. In fact, with my car I used to leave it in the garage overnight, come in the morning, run out of the garage on the steam that was still in the boiler and fire up after I was on the road. Then of course electric ignition was possible, and there were various other things that could have been done. So that it was possible for a steam car to be developed that wouldn't have any open flame in the garage. But this was the thing that finally killed the Steamer off. By the time other people got working with flash boilers and so on, it was too late.

Now the same sort of a thing is true of the hot air engine, and in this I was considerably involved. The hot air engine in its early days was an extraordinary engine. It was simple; it would last forever. Its only trouble was that it had too little output per pound or per cubic foot. Before the war, I tried to modernize it. I got as far as filing patents and assigning them to MIT and I built a little model but I didn't get very far with it. I've told this story elsewhere. [X-REF BACK TO P. 299-301] Somebody else did that very thing and made an engine that was some good.

[EH to VB: REFERENCE] [VB to EH: PHILLIPS, NOW JOINED
BY G.M.]

1)FACTORS IN THE
DEATH OF THE
STANLEY STEAMER

2)HOT AIR ENGINE
ALSO DOOMED (Repeat)

I believe today that a hot air engine could be built which would be comparable to the internal combustion engine in power-per-pound or power-per-cubic-foot of engine, that it could have a better efficiency than the gasoline internal combustion engine and a much longer life. But the trouble is this: the gas car has become so embedded in our life and ways that the automobile industry cannot afford to make the change. It would cost many millions of dollars to develop the hot air engine to the point of good performance. Even if it was done, the cost of converting the factories would be enormous. Nor is there incentive, for the gas car people can sell all their automobiles as it is. If they improved them a little they wouldn't sell any more and they wouldn't make any more profit. So they have no incentive.

[X-REF BACK TO P. 299]

No one outfit in the automobile industry in this country can step out ahead of the others technically because of the patent pool arrangements of that industry under which, if they have an invention they have to license everybody else. This fixes things so that anyone who makes an invention of any importance pertaining to automobiles has only one customer. He can give it to them at their price or he can let it rot. I know this because I had a development of an external combustion engine which I've just told about and I also had a development of a free piston engine. I think the free piston

1) PATENT POOL
ARRANGEMENT OF
AUTO INDUSTRY

engine would also beat the present engines but it's not possible to get it developed in this country for automotive use.

Now this goes back to our patent system, for one thing. There was a time when the patent system worked very well in this country: when a single inventor alone developed a gadget and some company would start in and manufacture it under an agreement with him. Today when devices in great use are manufactured by enormous companies having hundreds of patents, it's pretty tough for anybody to try to break into this. This is not the place to go into the whole question of how the patent system might be modernized. I have written on this as I've already remarked. [N.B.] [X-REF BACK TO PP. 291-294]

We've had a great change in the way technical innovations have come about. The day of the lone inventor is not quite over but it has been greatly changed. I've seen it happen even in my lifetime that a lone inventor could have a bright idea that people could grab and run with and do a lot with. The Spencer snapping disk [X-REF AHEAD TO P. 561] was one of these. But in general, advances in technology are made today by big teams of individuals in laboratories where it's not so much a spark of ingenuity that counts, it's a knowledge of physics and mechanics and painstaking work in development.

- 1) PATENT SYSTEM SHOULD BE MODERNIZED
- 2) LABORATORY TEAMS REPLACE THE LONE INVENTOR'S SPARK OF INGENUITY

For example, take the transistor and compare it with the thermionic tube. In the days when the thermionic tube was developed [X-REF AHEAD TO P. 601] we did have a Fleming valve which one chap by the name of Fleming just happened upon. [VB ADDS: REALLY EDISON FOUND THIS OUT--AND DID NOTHING ABOUT IT] He found that where you had a filament, a collector and a vacuum, electric current would flow one way and not the other. That was just one man's bright observation which he could demonstrate very readily. Then along came de Forest who had the idea of sticking a third electrode in there to control the thing. [X-REF AHEAD TO P. 600] He didn't know any physics to speak of but he hit on that idea, got a patent on it and away he went. Then Langmuir had an idea of making the vacuum very high. I don't think this was invention at all: I think the patent office was entirely incorrect in issuing a patent on it. But the existence of this patent modified the entire course of development of thermionic tubes [X-REF AHEAD TO PP. 499 & 601] and modified the arrangements between RCA, General Electric, the Telephone Company and Westinghouse. They made great deals on the basis of what I think was an invalid patent.

Then came along the development of the transistor. What happened? A group in the Bell Telephone Laboratories, a whole group of many men, studying solid state physics, developing some other abstruse theories of the flow of electrons through semiconductors and the flow of holes in the opposite direction (a bizarre idea in itself), found a whole new field of

- 1) THERMIONIC TUBE--
FLEMING VALVE,
deFOREST & LANGMUIR
- 2) TRANSISTOR OUT
OF BELL LABS

physics in which they developed all sorts of interesting relationships. It soon became inevitable that the transistor, so called, could be made into a control device. So the whole art of transistors evolved out of the great teamwork operation. Incidentally, since the development occurred in the Bell Laboratories it was licensed freely and became available to the whole "electronics" industry. It was not hard to get the transistor into the manufacture, and it promptly came into very wide use.

It is quite true that when a development becomes successful and gets into wide general use, its very existence blocks to a considerable extent the development of a product which might be somewhat superior if fully developed. Some competitive company can't afford to take the chance of spending a whole lot of money to try to break into the field unless the field is exceedingly attractive commercially. So a successful development blocks, to a considerable extent, and quite independent of the patent system, developments in other parallel paths. Then, of course, there's another thing: the way in which the American public leans on slogans, trademarks, advertising is in itself a barrier to the advent of new devices.

We saw a breakthrough on this thing right after the war. During the war the inventions made in connection with military devices in the universities (and there were hundreds of them), were patented and in that way made freely available to anyone

- 1) TRANSISTORS OUT OF BELL LABS
- 2) FINANCIAL SUCCESS BLOCKS PARALLEL VENTURES

after the war, since the patents were in government hands and hence to be freely licensed without royalty. Now this had to be: otherwise there would have been utter chaos in sorting out who owned what, for example, in the Radiation Laboratory. But it was done. [X-REF BACK TO PP. 69, 159] In these days the attention of everyone was on military matters. There wasn't time to think about commercial applications. After the war any number of young chaps who had been working on one thing or another saw ways in which new instruments or new gadgets could be built that would have an appeal to the public or an appeal to manufacturers, and hence have a sale.

Moreover, during the war a whole lot of young fellows had learned to step out on their own, had learned to take direction of important things and make decisions. After the war a whole lot of these young fellows did not go back into a humdrum life as employees of great organizations; they stepped out and set up their own companies. They were very often helped if they could devise a gadget which the government would buy for its military purposes and quite a few got going in that way. Hundreds of new little companies sprang up all over the country clustered around Cleveland, around Stanford University on the Peninsula, and around MIT on Route 128 and so forth. Many of these companies have prospered and grown to be a reasonable size.

- 1) WWII & POST WWII
- 2) GOVERNMENT PATENTS ON MILITARY DEVICES
- 3) GOVERNMENT AS THE CUSTOMER OF MANY POST-WAR COMPANIES

I think personally that one of the principal safeguards we have against over-concentration of power in great industrial complexes is the continual advent of young, highly flexible, courageous, small companies. [X-REF BACK TO PP. 97-98] Such a small company can cut rings around the big ones. We certainly experienced this in the early days of Raytheon and Metals and Controls and so forth. We were very vulnerable; we came near going broke many times; we had plenty of vicissitudes; the big companies took cracks at us.

Raytheon was nearly put out of existence once when a move by the combine resulted in cutting off all the customers. Raytheon, whose name was something else at that time, on one day had a factory turning out rectifier tubes at a fine profit with many orders for many months ahead -- and the next day all of its orders were cancelled. But both companies survived, and the primary reason was that they could make a decision. They could get into a field, they could develop something and begin to sell it by the time the fourteenth vice president of one of the big companies had been consulted and had objected. The big companies have enormous inertia and the little ones, if they're fast on their feet, can cut rings around them and make a place for themselves.

In the last twenty years we have seen this happen in extraordinary fashion in many places. It may not happen again to the same extent in the absence of war. But we

- 1) RAYTHEON and METALS & CONTROLS
- 2) FLEXIBILITY OF YOUNG COMPANIES vs INERTIA AND RED TAPE OF BIG ONES

Reel 8-A
Page 503

can be sure of this; that during the last generation a very large number of men in this country have learned the joys of independent operations as contrasted with the somewhat routine operations in a great organization.

- 1) POST WWII
- 2) DELIGHTS OF INDEPENDENT OPERATIONS

* * *

Question 100: Coffee Break to Evaluate Questions
and Responses to Date

1) EXAMINATION OF
BBS TO DATE

2) EH NOTES: ALL OF
THIS TO BE REVIEWED

I think as you listen to the reels up to now you'll find a couple of dozen places where I've mentioned something that we haven't gone into and you'll want to come back at me with further questions. And in addition, I think that you'll find that in many places I've glossed over a point rather rapidly where you'd like to have me dig in.

There are, of course, a whole lot of things that we haven't come to yet. For example, in the postwar affair, the famous Cabinet meeting where Truman made the decision on the postwar handling of the bomb and the Attlee Conference with all of the strange things that occurred there. [X-REF AHEAD TO PP. 553, 728-730] We haven't gone into, for example, the extraordinary episode of Archambault and Normandy and the German radio set on which incidentally, I think a chap by the name of Peterson wrote an article in the Saturday Post but it's nevertheless worthwhile to go into it again.

We've said a little bit about the conferences that I had with Al Grunther's team in France after the war. [X-REF AHEAD TO P. 765] I doubt if there's any use in my going into any of the things that happened in the relationship between science and government after I stepped out of the picture nine years ago. I took the point of view that when I'd stepped out, I'd step out and not muss into it. For two reasons, one because of Bob Loeb's caution and second,

because, well I can tell it in the form of a story.

Lewis Strauss called me up after I'd been away from Washington for a year or two and he wanted me to head some kind of a committee in AEC on something. I said, "Lewis, I used to be a general, and I'll be damned if I want to be a sergeant." Now I think that has been my point of view, that when a man's had a crack at some of the top things it's not a very pleasant thing for him to fade out and do lesser things. In fact, I might give you, I think, quite a bit of a picture of my point of view on, well I don't know as I want to say growing old, but on stepping out of things before being pushed out. We might have kind of an interesting conversation on that because I've thought about it a good deal. In fact, whether we use it or not, the art of retiring is worthwhile taking a bit of a crack at. [X-REF AHEAD TO PP. 515-518, 768]

So I think we're going pretty well as we stand and we don't need to revise the method. As I look over the questions from 101 on, I am a bit appalled by some of them because they are getting quite a long ways away from brass tacks. I think we want to try to get back to explicit things as far as possible but I'll know better after I've gone through the next ten or a dozen.

I do notice one thing that as I go along on this business the answer to a question occupies much more space than it did when I first started in. I imagine this is just because I'm

- 1) COFFEE BREAK
- 2) "DAMNED IF I WANT TO BE A SERGEANT"
- 3) ALL OF THIS TO BE REVIEWED

Reel 8-A
Page 504-B

getting a little more at ease in talking to a damn machine
instead of an individual. [EH to VB: AND IT'S ALL TO THE
GOOD!]

Question 101: OSRD's Committee on Medical Research
the Progenitor of the NIH

I really had very little to do except very indirectly with the establishment of the National Institutes of Health. During the war for the first time there was government support of medical research. All the support previously had been pretty picayune, but during the war, through the Committee on Medical Research of OSRD, and with great aid from the medical division of the National Research Council and all of its committees, a program of research was set up which was completely subsidized. [X-REF BACK TO P. 210] It devoted itself entirely to matters of interest in connection with the war. I got the recommendations of the Committee on Medical Research periodically, and as I approved them, they resulted in contracts.

Now there is an important point here, namely that in all of OSRD's contracts with universities we endeavored to pay the full cost, that is, we paid overhead. This was the first time, I think, that government grants for research had included overhead. When I put this into effect I was visited by a group from the Bureau of the Budget and from the General Accounting Office and so forth who protested that this was not cricket; that I was subsidizing the universities, that I was giving away money. Of course I said that I was merely paying costs. If I hadn't had the full backing of the Appropriations Committee of the House, I

- 1) WWII
- 2) OSRD's COMMITTEE
ON MEDICAL RESEARCH
- 3) PAYMENT OF OVERHEAD
IN OSRD CONTRACTS
WITH UNIVERSITIES

might have been in trouble on this. But all I had to do at the next hearing before the House Committee was to make a few remarks along these lines which got into the record. Since no question was raised by any member, I evidently had the support of the committee in the way I was handling things. The Bureau of the Budget did not try to argue with the committee.

There's another interesting thing here that comes in only incidentally. In regular peacetime operations, a budget for a department is made up under the control of the Bureau of the Budget and hence under the control of the President. When the head of an agency appears before the committee, he is not supposed to tell them what the budget did or try to boost the thing beyond the budget recommendations. If he did do so, he was in trouble with the Bureau of the Budget. But there's a very easy way around this, and it was particularly easy in wartime. All one had to do was to start to say something and then stop and say, "But, wait a moment, I probably shouldn't go into that," and the committee would know exactly why. They'd proceed to ask questions and, of course, one is supposed to answer the questions and hence the whole thing would be brought out. In some such way, one could get support for what was needed. [EH to VB: UNCLEAR]
[VB to EH:

1) HOW TO MANOEUVRE
BETWEEN BUREAU OF
THE BUDGET & HOUSE
APPROPRIATIONS COMM.

[There is always a bit of a contest between the Bureau of the Budget and the Appropriations Committee of the House,

and the Agency is in between. If it asks for money or authority beyond what has been authorized by the Bureau, it is in trouble on the basis that it is not following the orders of the President through the Bureau. But the Committee wants to use its own judgment, not be tied down by the Bureau. Hence, if it finds a point where it feels the Agency is being cramped, it will open up the point by questions. The Agency can't volunteer on the point, but is bound to answer questions.

[In time of war the control of the Bureau breaks down. Smith, Director of the Budget, tried a number of times to get control of my financial operations in one way or another. He had no success, for I had the backing of the Committee.

[For example, on paying overhead, all I had to do was mention this in my testimony before the Committee. When there was no objection from any member, the Bureau laid off.] [END WRITTEN ADDITION]

Now this process of granting full costs to universities, particularly in medical research, opened up great vistas; without any push on my part and very little contact with it. In the postwar period it [EH to VB: WHAT?] [VB to EH: SUPPORT OF MEDICAL RESEARCH.] flourished and blossomed and the National Institutes of Health was one of the results that came out of it. So my connection with it was quite indirect although I think I did set the pattern which has been followed ever since -- with a great deal of argument

- 1) WWII
- 2) GETTING LARGER APPROPRIATIONS (Continued)
- 3) PAYMENT OF OVERHEAD ON UNIVERSITY WAR-TIME CONTRACTS (esp. MEDICAL RESEARCH)

as to just how the overhead is figured. But the paying of overhead is an accepted principle today.

Now sometime I ought to tell you about Newton Richards' appearances before the Appropriations Committee [X-REF BACK TO P. 407 & AHEAD TO PP. 518, 769] and in particular the time that he made the remark about his age [X-REF AHEAD TO P. 770] and a few other little things like that. [VB ADDS: In one of the last appearances, a Committee member asked Richards, "Will all these things you are doing tend to increase the span of human existence?" Richards, who was weary and getting old, replied "God forbid." This went into the formal record. When Richards reviewed the record he crossed it out. When I reviewed, I restored it. So, as far as I know, it is still in the printed records of the Committee.] [END WRITTEN ADDITION] [AM to EH: THIS STORY ALSO TOLD IN VB LETTER TO GEORGE MERCK, 10/29/47, FILE 1751]

The Committee on Medical Research was never short of money; I was usually urging them to spread out and spend more, for I had no difficulty whatever in getting it from Congress. [X-REF AHEAD TO P. 768] The amount of money they used was picayune compared to the money being spent on weapons. I did have a little difficulty with them in keeping them on things that were directly connected to the winning of the war. I also had to be pretty careful that the outfit under my command did not simply pick up from other agencies of government research projects which those agencies wanted to carry out but for which they couldn't get the money. In

- 1) WWII
- 2) NEWTON RICHARDS:
"GOD FORBID!"
- 3) OSRD's COMMITTEE ON
MEDICAL RESEARCH
FRUGAL WITH MONEY &
INCLINED TO WANDER

other words, I couldn't be a cat's-paw for them. If I were I would soon be in trouble with the committees of Congress, and I'd be in trouble all over.

The only real show I had with the Committee on Medical Research was when they recommended a program on bovine mastitis, because I was pretty sure this was a program that had been started in the Department of Agriculture and that they were anxious to pursue, but on which they were short of funds -- so that they got the Committee on Medical Research to pick the program up. When it came to me as a recommendation, I replied with a very courteous letter in which I said that of course I would never attempt to overrule their professional judgment on a project and so on, but that in my judgment this did not come within the definitions of the scope of the Committee on Medical Research affairs since it did not directly affect the progress of the war. With this I declined to approve that item.

They came back at me with it all over again and a bit more explanation, and it went back and forth four or five times. They never did get their money and they never got the program on bovine mastitis moving. This made some stress between me and the Committee, but of course I had to stick to it because I knew perfectly well that if I was bailing out the Department of Agriculture, I'd very soon hear from the Bureau of the Budget and various other groups. Otherwise my relations with them went all right and I got to know the medical people pretty well.

- 1) WWII
- 2) OSRD's COMMITTEE ON MEDICAL RESEARCH & BOVINE MASTITIS

One thing that helped me, of course, was this: the Committee on Medical Research was not made up entirely of M.D.'s. I had a couple of men there who were Ph.D.'s; the other members were very distinguished medical research men but handpicked to be men of broad understanding and men who had fine relationships with the medical group and with biologists, and so on. Baird Hastings of Harvard was one of these. He had a mission to Russia which I ought to tell you about some time [QUERY] and another one was Dochez, a perfectly delightful individual. [EH to VB: OF JOHNS HOPKINS?] [VB to EH: YES] Lou Weed was also a member of the committee and as I've said he was inclined to be troublesome. But apparently Newton Richards managed him without difficulty. Of course the fact that I was ready to back up Newton undoubtedly helped. I didn't have to back up Newton very often.

I did have one show with a chap at Harvard who was doing work on blood fractions, gamma globulin and that sort of thing, who was very irritated about a ruling of the Committee and who finally appealed to me. He was a troublesome cuss. His name has escaped me for the moment; I'll probably get it in a minute. [QUERY] He had been going around the town growling about the Committee and generally raising the devil. When he came in to see me all I had to do was say, "Now I don't know very much about this whole affair, and certainly I don't know anything whatever about

- 1) WWII
- 2) BUSH'S RELATIONS WITH COMMITTEE ON MEDICAL RESEARCH

the professional aspects of it. But I do know people fairly well and you can be sure of one thing: when Newton Richards makes a ruling I'm going to back him up completely -- and in the last analysis, I have the money." That ended the argument; I didn't hear anything more out of him.

In general I didn't have to work very hard to back up Richards because everybody had such high regard for him. And since he had Chester Keefer as his administrative officer [X-REF BACK TO P. 403] , things went smoothly. Between Chester and Irvin Stewart, the two kept all of the business aspects of the thing thoroughly in order.

- 1) WWII
- 2) BUSH's RELATIONS
WITH OSRD's COMMITTEE
ON MEDICAL RESEARCH

* * *

Question 102: What began Bush's interest in fields which led to chairmanship of Merck-Sharpe & Dohme?

There's quite a lot to this one. During the war we had a very serious problem on biological warfare. Of course we couldn't ignore it because we didn't know what the Germans might be doing. We had to study it intensely to be alert; hence we had a program on biological warfare. We didn't have the slightest intention of developing it for use but we did have the fullest intention of developing it so that we would know what was possible, what the Germans might do, so that we could move reasonably rapidly if they introduced it. In the light of some of the things they did in their concentration camps, we couldn't be sure how far Hitler would go in the direction of such matters.

Now there's a natural aversion of men generally toward biological methods in warfare, and it's as old as history. You'll find, in some of the old sieges [EH to VB: B.C. OR A.D.?] [VB to EH: BOTH, I THINK.] , that some general in the field rigged up a catapult and tossed decaying carcasses of horses into a besieged city. But I think you'll find also that this practice was frowned upon; that it was regarded as being outside the pale of military men and so on.

During the last two wars there'd been talk of biological warfare, which indeed could be very terrible. We might say, I think, today, that if it were not for the presence of atomic bombs people might be thinking about biological warfare

- 1) WWII
- 2) BIOLOGICAL WARFARE

intensely. They're not, I'm pretty sure; they're not in any country including Russia. [EH to VB: HOW ABOUT CHINA?]
[VB to EH: I DOUBT IT. THEY ARE TOO VULNERABLE, WITH THEIR CONCENTRATED POPULATION.] The A-bomb is so completely a weapon of mass destruction that it's hardly worthwhile to think of any others.

But during the war we had this problem, and I couldn't put it in my Medical Committee's division: if I had, my medical people would have all left me. The Army was in somewhat the same position. I talked it over with Stimson and by some rather indirect methods, we finally got the activity placed under Paul McNutt.

[I'll continue on the other side of this tape.]

Paul McNutt was a strange person. When I visited his office for the first time, on the wall behind him were a lot of framed certificates of one sort or another, degrees and what have you, all made out to Paul McNutt. On another wall was a great portrait of Paul McNutt. Over in a corner on a pedestal was a bust of Paul McNutt. I don't think that Paul McNutt ever knew that he had a group on biological warfare under his organization. [? QUERIES: WHAT AGENCY EXACTLY?]
I don't know how it got the money; Stimson looked after that. In some way or other the money was supplied and the work went forward. George Merck handled the thing; he headed up the group and they had some laboratories and so forth. George Merck did this at great personal risk without

- 1) WWII
- 2) PAUL McNUTT
AN EGOTIST
- 3) GEORGE MERCK HANDLED
BIOLOGICAL WARFARE
UNDER McNUTT's
AGENCY

END FIRST SIDE REEL #8

the slightest doubt. If it had become known, he would have been severely criticized in medical circles -- and quite unfairly. Moreover his pharmaceutical company was dependent on the good will of the medical profession; George was personally a somewhat sensitive fellow who couldn't have stood up well against a scurrilous attack. Yet he took the risk. Nothing happened, as we know. No biological warfare was used by anyone. So the whole thing didn't amount to much, although it was a precaution that had to be taken.

I became acquainted with George Merck under those circumstances, and got to admire him greatly. Since I did admire him and because I had become a close friend of his, when he suggested that I go on the Merck board, I readily did so. [EH to JK: FIX DATE] [AM to EH: ONLY CLUE SO FAR IS ANNOUNCEMENT WAS MADE BETWEEN OCT. 28, '59 and NOV. 23, '59.] There I became very interested indeed in medical research; I mixed into it deeply and helped reorganize it. Then we had a problem, because the chiefs of the company had become old. George was really not controlling anything; he was chairman of the board, but he really didn't control it. The president of the company had run it well when it was small but he didn't know much about running such a thing when it became big. I tried to help reorganize the research program and I got into quite a show.

This brings up the incident, which I think I've told you about, [EH NOTES: YES] of the time when I wrote out

- 1) WWII
- 2) MERCK & BUSH
BECOME FRIENDS
- 3) BUSH GOES ON
MERCK BOARD

some primary rules for organization. [X-REF BACK TO P. 65 AND AHEAD TO PP. 537-538, 771-772] The president of the company approved them and sent them to all of his vice presidents in spite of the fact that he violated nearly every one of them occasionally. We had to reorganize and the board was in a fix. There had been a merger between Merck, and Sharpe & Dohme. This had not been fully digested so that the board was made up of two groups. When it came to reorganization, we urged the men then in charge to give us a recommendation for turning it over to young men. They ducked the issue until we had to take action, so a committee was set up. The committee was in just the same fix as the board itself. Finally the committee turned it over to me and I took it after I'd said to them, "Now if I'm going to do this job, I've got to have assurance in advance that you'll back me up when I make recommendations unless I'm completely out of line." They assured me they would.

I spent an entire summer on it working with Orville ^CShell [VB NOTES: SPELLING OK] who was counsel for the company. We interviewed some thirty men in various positions of responsibility throughout the company. We interviewed some of them several times and I finally got to a solution. We had two very good men and either one of them could have run the company adequately, although they were very different in their approaches. They complimented one another excellently. I proposed to make one of them president and the other one executive vice president. The problem here

1) REORGANIZATION
OF MERCK

Attachment to P. 513

AM to EH: In a letter to Mr. Merck (March 3, 1950) VB analyzes the structure of the company. It looks as if Mr. Merck was president, not Ch. of the Bd., and the "president" VB speaks of in this reel was a Mr. Kerrigan (James J.) who seemed to run everything but without any actual title"I am not at all sure that his proposal would be that he [Mr. Kerrigan] be established as president, while you stepped to chairmanship of the board, or that he be established as executive vice president while you remain president, with unqualified authority and all the furbelows and ambiguities removed."

In April 1953 the stockholders approved the merger of Merck with Sharpe & Dohme. VB joined Merck in late '49 or early '50; so the 'merger' he mentions must have been between Merck and somebody else...or maybe he's got the time telescoped.

Also from Merck files: Date of VB and Schell interviews was summer of 1955. And correct spellings of names are from Merck files #1749, viz; Schell, Sarett, etc.

was to keep the man who did not make the presidency. This was also accomplished and this team has gone on ever since.

I think the company got as fine management as it could get because Jack Connor [EH NOTES: SEC. COMM. 1965] is a very broad-minded public-spirited chap who carries out the traditions of George Merck himself in that regard. His sidekick is a rather tough egg of whom most of the men are quite afraid. They're afraid of him because his mind is so keen that he sees through things in a hurry. The combination works beautifully. Henry Gadsden is the second in command [now president]. [AM to EH: H.W. GADSDEN IS CORRECT SP.]

When George Merck died suddenly, it was natural that I should step in as chairman of the board where I served for some years. I was, of course, merely chairman of the board; the chief executive officer was Jack Connor. My principal function was to handle the board's committees and that sort of thing, and of course, preside at meetings. Before board meetings, I'd sit down with Connor and Gadsden and on some point that was not completely agreed to between them, we'd thrash it out right there. This helped, of course, to put things through the board in orderly fashion. Gadsden was an exceedingly good soldier. In some of those conferences he would oppose a thing until he got into the board and then he'd either keep quiet or support it. The combination of those two men has worked well ever since. That's how I got into the Merck outfit and why I've got so very much satisfaction out of it.

- 1) REORGANIZATION OF MERCK
- 2) JACK CONNOR BECOMES PRESIDENT
- 3) HENRY GADSDEN BECOMES EXECUTIVE V. P.
- 4) BUSH BECOMES CHAIRMAN WHEN MERCK DIES

There's another phase of this thing, and I think maybe this is a good place to go into it. I have had for many years, in fact, since I first started my professional career a great horror of being in a subject, in a field, in an activity where I stayed too long -- and hence became surrounded by young men who could handle it better than I could, who knew more about it, while I was regarded as the old fossil. [X-REF BACK TO P. 504 & AHEAD TO 516-518] I have never allowed myself to get into that position. This is one of the reasons why after the war I went into the pharmaceutical industry, where certainly I was not supposed to know anything whatever and hence would not be embarrassed by my ignorance. And also, where I could have the fun of learning new things. I completely stepped out of the computer field where I'd started in and been a bit of a pioneer. I stepped out, and didn't touch the field for years while young fellows went on with it and cut rings all around what I'd been thinking of doing.

* * *

The first experience I had on this I believe was back in my days at MIT soon after I'd joined the staff. I had been giving a graduate course on circuit theory based on Heavyside's mathematics. I wrote a book on it which is a pretty elementary and crude affair judged by today's standards, but which was then unique and which gave some pretty good ideas to the youngsters. They'd work. [X-REF AHEAD

- 1) ON ENTERING NEW
FIELDS & AVOIDING
"FOSSILIZATION"
- 2) FLASHBACK -- BUSH
ON STAFF OF MIT

TO P. 578] The only trouble was that my stuff wasn't any more rigorous than Heavyside's had been. It was heuristic, if you know that term. I was interpreting and extending Heavyside's approach.

I taught this course for several years and it was much sought after, so that I had quite a group of graduate students likely to be mathematically inclined. I had two assistants, Barnes and Murray Gardner. One spring, when they were making out the final exam for the course I looked over their draft and suddenly realized that these two fellows had now gone further than I, Barnes in particular. I had been paying attention to a dozen things while they were paying attention to one, and they'd stepped beyond me. So I called them in and I said, "Gentlemen, this is the last year I'm going to teach this course. Next fall, I turn it over to you. I wrote a book on it which is now rapidly becoming obsolete. You will write the book to bring the subject up to date."

They didn't believe me at that time, no doubt, but that's exactly what I did. And in fact, Barnes did write the book which superseded mine, not in popularity, but in rigor. [X-REF AHEAD TO P. 578] That field has gone to the point where I can't pick up a paper on it today and read it. The language is different from what I used, the reasoning is entirely different, the mathematical basis is now sound where once it was fragmentary and unsound.

1) FLASHBACK--BUSH
ON STAFF OF MIT

2) BUSH IS OUTDONE BY
HIS ASSISTANTS AND
TURNS COURSE OVER
TO THEM

This is an instance of a general thought that I've had all my life -- that I would never be caught in the position where I was getting old and the people around me thought I was fading out of the picture, but where I didn't have the sense to get away. This is why I insisted on retiring from the Carnegie Institution in Washington at the statutory age which, incidentally, I'd put into the by-laws myself. It's why in the time after retirement, I paid attention to the pharmaceutical industry and thus to a lot of biological research and then my own work which I did for the fun of it. I went into high-pressure hydraulic machinery, which was certainly a good far cry from electrical engineering. I think many a man would save himself a lot of grief if he'd arrange to fade from the picture before any considerable number of people around him think it's time for him to go.

1) CONTINUATION OF
GETTING OUT BEFORE
YOU'RE FOSSILIZED
THEORY

* * *

Question 103: Empiricism and Accident as Forces
Affecting Discovery

This question, to a certain extent, reminds me of a story about the two Jews which you undoubtedly know, which ends up by one fellow saying, "And besides, I didn't make the fifty thousand dollars, I lost it." The reason for this is that it wasn't a tranquilizer and many of the other things are a little off the mark, but I recognize the story you're thinking about.

The man involved was [EH to VB: SPELLINGS OF SERAT, KENDALL & HENCH?] [VB to EH: SARRETT, I BELIEVE] [AM to EH: FOUND THEM -- KENDALL, HENCH & LEWIS H. SARETT.] Lew Sarett, the man who synthesized cortisone and hence made the whole program on steroids move. Kendall and Hench received the Nobel for this. They used the material that Sarett produced and proved that it had a beneficial effect on rheumatoid arthritis. [X-REF AHEAD TO P. 769] The really tough job, that is of making the stuff available, was done by Sarett. We started this work during the war; we hadn't finished it by the end of the war. We started it because we heard that the Germans were using this particular hormone for their air force pilots to give them longer endurance or something of the sort. The story was probably incorrect.

This is the thing that Newton Richards started to tell about the Committee on Medical Research when he picked out the only unsuccessful thing in the group that he was working

- 1) WWII
- 2) SARETT SYNTHESIZES CORTISONE
- 3) NEWTON RICHARDS
--CMR

on much to my discomfiture. [X-REF BACK TO PP. 407, 507, & AHEAD TO PP. 769-770] [EH to VB: FOGGY: TRANSCRIPT?] [VB to EH : Richards, before Appropriations Committee had a list of ten or twelve programs of CMR. When they turned to him they asked him to pick out one of these. He picked the only one that had gone sour. I tried to steer him onto another one; some of the members caught on and kept him going on the same one, no doubt because they enjoyed watching me try to get out of the pocket.] After the war, Sarett completed the job and he's a great fellow. He's one of the most competent organic chemists in the country, without doubt.

Well, Merck was developing steroids some years later and it had made and tested five hundred of them, with radicles tacked on in various places. They had not only synthesized these things, but had put them through various tests mostly with animals, but some of them with humans. Their file of cards presented a beautiful detective problem. You could trace a bit how one radicle did one thing and another radicle another, how changes in structure of the molecules affected certain side effects, but not others. The correlations, of course, were not by any means exact; you couldn't write them down as a formula, but by study, you could get some intuition as to where it made sense to move.

Sarett, going over this with me one day, said, "We've come to the conclusion that if we put a methyl radicle in one position," and it may have been the ortho, "we ought to

- 1) WWII & POST WWII
- 2) "SARETT MOVES THE METHYL RADICLE" STORY

get such-and-such effects from this judging from this experience." They tried it, and it worked. It became one of the better steroids, quite an advance over previous ones from the standpoint that it had less side effects. Some months later -- this was all in the laboratory still; it hadn't got into production -- Sarett told me with a great grin, "You remember what I told you about this thing? Well," he said, "when we finally got it to working, we reversed and instead of synthesizing we analyzed and we discovered that we had the radicle on in the wrong place." This is the kind of thing that one speaks of when he uses the word serendipity. They were alert all right and they profited by a mistake. Of course they went ahead and put the radicle in the right place -- and found that the effects were equally good, but there was no advantage in doing it.

In addition to empiricism and accidents, and the alertness of the research man toward the unexpected and all that sort of thing, I think there's one more factor that ought to be mentioned. That is the ability to proceed by arguments based on balance of evidence. Now, of course, most scientists proceed on the basis of rigorous mathematics. Thus, the chap delving into the nucleus of the atom and so forth works on the basis of quantum theory and various other formulations that have been developed -- all mathematical -- he leans on these and from them predicts and then searches.

- 1) POST WWII
- 2) "SARETT MOVES
THE RADICLE"
- 3) FORCES & FACTORS
IN DISCOVERY

In certain fields of research, one cannot arrive at any such basis of judgment; this is particularly true of biological medical research. One cannot have mathematical formulas and hypotheses that have been formulated and tested and accepted; one can only have balance-of-evidence. The story about the five hundred cards [X-REF BACK TO P. 519] illustrates this. No one could have written out on that basis an exposition of how the structure of the molecule and all of the biological effects were interrelated. By no means. One had to proceed by balance-of-evidence.

Now of course, other professions do this very decidedly. The analyst in an investment house has, of course, the data on companies before him but he has to consider also other things: the probability of good results coming out of their research laboratories, the quality of management, the trends in the industry, the possibility of economic setbacks in the country, and many other things. When he does this analysis he's not arguing from strict premises and by definite logic; he's arguing on the basis of balance-of-evidence. It is not generally recognized that scientists proceed in both ways and that both are exceedingly important for success. The mistake often made in this connection is the use of the wrong method in the right place.

Areas of research are moving gradually, of course, from the empirical to the stage where balance-of-evidence controls and finally, into the rigorous logical form. But they're

1) BALANCE-OF-EVIDENCE
as RESEARCH TECHNIQUE

moving very slowly. In the life sciences, the simpler things have already moved over there. Genetics, for example, and the story of the inheritance through the genic system, has been so well sorted out that one has now pretty well-proved hypotheses on which he can lean. But the chap who's working, for example, on a new drug to produce a certain effect in a human (a diuretic, for example) is generally still operating from balance-of-evidence. And he will do that for a long time to come.

Certainly people in biological research and particularly in the pharmaceutical industry are operating on a basis of cut and try, but this is not the whole story by any means. I can give you one example. One of the most important products that Merck brought out while I was with them was diuril. In fact, it was so successful that in the first year of its introduction, its sales went to something like fifty million dollars. It filled a great need.

Now, the full story of this is fascinating. Some twelve years before, the group in the medical research laboratory that Merck calls the Merck Institute -- where they work on rather fundamental things, had a new theory of the action of the liver and kidney and so forth. This theory was quite contrary to usually accepted ones. The Institute worked on it for years on and off; they tried to check it in various ways and made some progress on it. Then, much later, they developed a product for an entirely different

- 1) RESEARCH MOVING FROM EMPIRICAL TO BALANCE-OF-EVIDENCE TO RIGOROUS LOGIC
- 2) MERCK'S DIURIL IS RESULT OF LOGICAL REASONING, NOT "CUT AND TRY"

purpose and some bright chap whose name I can't recall now, said to himself, "Well, now wait a minute. If this theory is sound, this product ought to act in such and such way. In fact it should act as a diuretic." They tried it and it did. This was a case not of empiricism, but of good logical reasoning based on a theory which they tried to test and based on a hypothesis which they followed up -- in this case successfully. It's quite a far cry from simple cut-and-try.

In fact, when there are millions of organic compounds that can be synthesized, the chap who merely tries the shotgun methods of Edison in that field is bound to be thoroughly disappointed. Fragmentary as the hypotheses, the theories, have to be, unless one is following a line of reasoning he is not going to get anywhere in that exceedingly complex field. I don't mean that accidents don't happen. In fact there was an accident on diuril as you see; but I do mean that unless the crowd has its reasoning behind it, unless it is following a clearly patent line of thought, it will not get anywhere by just trying this and that.

* * *

I don't need to enlarge, I think, very much on Boss Kett; he was essentially a gadgeteer. As a speaker, he was delightful. Some of his wisecracks were exceedingly brilliant and he was much sought after for that reason. After he was famous he was put on all sorts of committees; I served with him on one or two and he never did any work on them. He and

- 1) LOGICAL REASONING
ESSENTIAL TO
FRUITFUL RESEARCH
- 2) BOSS KETT

Sloan set up the Sloan-Kettering Institute. I understand that it was primarily Sloan's money; I don't know this, I've heard it. I wouldn't be at all surprised if Kett contributed very little but had no hesitancy in taking a good part of the credit. I never had, as you can see, very much use for Kett. I liked him -- you couldn't help liking him -- but I had no admiration for him.

He had contempt for the scientific method which, of course, he didn't understand. He was always the practical chap who could do things while the theorists were wandering around in the ether. Of course he did do things. But he did very simple things, where the job was almost done for him because of the way in which the need developed and the place in which he was located. It was thus he accomplished the things that made him famous, such as the self-starter for the automobile -- a good job, but one that was done by the reasoning of a garage mechanic with a bit of ingenuity but nothing else behind it. The day when that kind of thing would work is pretty well over; we don't get gadgets made that way anymore.

The thing I objected to about Kettering was that he didn't hesitate to expound with his wisecracks this general point of view. I think it discouraged a lot of youngsters who might otherwise have gotten into reasoning on their affairs instead of simply the attitude that is usually held by the radio ham of trying this and that.

- 1) KETTERING A
GADGETEER WITH
CONTEMPT FOR THE
SCIENTIFIC METHOD
- 2) SLOAN-KETTERING
INSTITUTE

I think the argument that I've been giving you about the procedure from sheer empiricism through arguments on balance-of-evidence and finally to the point where one can follow proved theories is something that hasn't been expounded well enough as yet. [JK NOTES: FURTHER STATEMENTS?]

1) RESEARCH PROCEDURES
& MATCHING MEN TO
DISCIPLINES

No, I don't think we need to name Kettering, who after all is dead, but I think we might at some point put in a word or two [JK NOTES: FURTHER STATEMENTS?] about what makes sense for the young fellow who's thinking of going into science or its applications. For men differ as well as fields and the youngster who's thoroughly mathematical in his natural approach and who gets thrown into a field where balance-of-evidence controls is likely to be very unhappy -- and vice versa.

* * *

Question 104: Kettering Continued...Edison...and
Advice to Youngsters

This is question number 104. Now Edison, of course, was a promoter par excellence, one of the best. He was also an excellent gadgeteer and he also had the facility of seeing where there was a public demand for something. This combination was what made Edison. [X-REF AHEAD TO P. 572] Now, as a research man, even as a trial-and-error man, entirely an empiricist, I don't think he was so hot. He had such other qualities that he succeeded in spite of the rather clumsy way that he went about his experimentation. The history of what he did on the filament for the electric lamp shows this. He tried all sorts of foolish things.

Now, I got a personal slant on this one time from a chap whose father or uncle had worked in Edison's laboratories as a mechanic. I acquired a small planer, a little bit of a thing. It was quite evidently made for planing bamboo into thin slices which Edison used in making his experimental filaments. I proved this because when I took it apart, I found shreds of bamboo in the thing. I gave it to the Smithsonian Institution where it probably still is. It was a good relic of Edison's work, but the point about it is this. That machine, a thing only eight inches long or so, four-by-four in cross section, cost four or five times as much to make as it should have. It had curlicues and

Bamboo planer also related in VB letter to Albert Merck
2/9/54 in CIW File 1748.

- 1) EDISON AS GOOD
GADGETEER BUT POOR
RESEARCH MAN
- 2) BAMBOO SHAVINGS IN
EDISON'S PLANER

finishes and various embellishments utterly unnecessary in a shop machine. It would do its work, and undoubtedly it worked well enough to plane those small things, but a man who really knew machine work, who really understood how you work metals and wood, would have made some sort of a thing that would have done the job but which would have taken a whole lot less work to put together.

One of the marks of a good experimenter is that he gets his results without being diverted or putting in unnecessary furbelows. So that I think that even as an experimenter, as an empiricist, Edison left something to be desired. This was certainly offset by the fact that he had all the other qualities and his promotion was shown beautifully by the way he organized the electric light companies.

Another thing of course, the old Edison bipolar machine. The old generator that was run by a steam engine and delivered direct current, was a monstrosity. Edison put enormously long field poles on it. Today field poles are made with a form of about a rectangular cross section. Edison's form was about ten times as long as it was thick. I understand that he did this because he read that Henry, in making magnets, had found that to make an electric magnet that was very powerful you needed to have it long compared to its cross section. But there's an enormous difference between a magnet that's going to operate in the air and with no return ion path and the field pole of a machine which has an ion path

- 1) EDISON A GREAT PROMOTER BUT NO EXPERIMENTER
- 2) EDISON'S BIPOLAR MACHINE WAS CLUMSY

throughout except at a small air gap. Edison didn't grasp this, and apparently didn't even try other forms to see. To this extent, his machines were pretty clumsy.

Of course you find throughout all the history of inventions many times where a man who is an excellent promoter gets the credit. Take Samuel Finley Breese Morse who is given credit for inventing the telegraph. Well, Faraday, and Henry in this country, had built electromagnets and connected them together with batteries so that you could make something move when you touched a switch. Everybody knew about this and all that Morse did was to string a long wire and use that kind of a gadget and make a telegraph. There's no invention involved really. But he was a promoter. And you'll find in many places that this is the case.

* * *

Well, I don't know what I'd say to a young scientist today or a young engineer. If I gave any lecture at all it would be rather a long one. One of the things I've emphasized a great deal is versatility. Another thing that I've emphasized to the men who were inclined to be theoretical in their approach is the necessity in getting a barnyard grasp of things.

When I had my graduate classes in rather advanced circuit theory (that is, advanced as of that day), I used to have them attended by a whole lot of chaps whose entire

- 1) SAMUEL MORSE ALSO
A PROMOTER MORE
THAN AN INVENTOR
- 2) FLASHBACK--MIT
- 3) "BARNYARD GRASP"
FOR YOUNGSTERS

approach, entire inclination, was to be mathematical. I did two things to get them in the proper frame of mind. At a class session very early in the course I'd pile on the front table a bunch of resistors, coils, condensers, and things. I'd pick up a resistor and hand it to one of the fellows and say, "What's the resistance of that unit?" I wouldn't tell him I only wanted the order of magnitude of course, but that was it. It might be an iron grid. The chap that knew something about the practicality of things would tell me and some other fellow would look completely bewildered. I'd do the same thing by giving them a coil and say, "What's the inductance of this thing?" The chap could see roughly how many turns the coil had and what air gap was involved, and so forth, and he could get a rough idea in a moment. I did this to get the boys into the frame of mind that the practical common sense approach has to go along with the theoretical if a lad is going to get anywhere.

Another thing. We were working out all sorts of mathematical ways of examining the transients in circuits, that is what would happen when the switch was closed or opened, in some detail. I'd draw a complex circuit on the board and say to somebody in the class, "What will happen when I close this switch?" Three times out of four the fellow tried to get out a piece of paper and start to compute something. I'd say, "No, wait a minute, look at it. What's going to happen? I don't mean to draw

- 1) FLAHSBACK--MIT
- 2) BUSH TECHNIQUES FOR BUILDING COMMON SENSE IN THEORETICALLY INCLINED STUDENTS

a curve on the chart of where the current is going to pile up, but is the current going to increase indefinitely and what's going to happen?" After a bit of that, I could get those fellows to look at the thing from that point of view.

Now another thing that I think that needs to be emphasized to young engineers is versatility and flexibility of approach. When I was a young consulting engineer, I made a rule with myself that I would never say to anyone who wanted me to do a job that that is out of my line. The result was that I had jobs that were, to a considerable extent, metallurgy. I had some that were more in the nature of mechanical engineering. I don't remember off-hand where I had a job that was strictly an electrical job. But I got into very interesting work in that way.

Another thing that I did, of course, was to make sure that my fees were larger than anybody else's among the young group in the department. Once in a while a chap would want me to do a job. When I got started toward doing it he'd then ask the fee and then he'd say, "Well, that's pretty high." And I'd say, "Well, I think it is myself, with this job. As a matter of fact this isn't really very much of a job, it's pretty straightforward business as far as I can see. There are a number of very good chaps in the department that can do this job excellently, and I can recommend several of them." Of course,

- 1) FLASHBACK--MIT
- 2) YOUNG ENGINEERS
NEED VERSATILITY
& FLEXIBILITY
- 3) VB RESOLVES NEVER
TO TURN DOWN A JOB
NOT IN HIS LINE
- 4) VB ASKS HIGH FEES
& GETS INTERESTING
JOBS

the result was usually that I got the job anyway.

By taking that point of view, I got the really interesting jobs before I got through -- quite a few. Of course with some of them I was on the basis of complete retainer, as in the early days of Raytheon and Metals and Controls and so forth. I also had Stone and Webster and various other groups that called me in on interesting things, and incidentally helped me piece out my income.

I suspect that much of Kettering's success came because he was able to get bright young fellows to work for him. Of course, that's one of the real qualifications of a successful research man and we ought to give Kettering credit for it.

* * *

Going back to my consulting experience though, there's one thing that I'd like to recite that's interesting. The Boston Edison Company was building a steam power plant which was going to use superheated steam at higher pressures and temperatures than had been used before. They called me in and they had two problems. One of them was the question of strains in their pipe system due to expansion at the very high temperature which I solved without very much trouble. Then they had another problem that involved some couplings for their pipe and they had designs and samples from two or three firms. They wanted to know whether these would be adequately strong at the

- 1) VB AS CONSULTANT:
HIGH FEES AND
INTERESTING WORK
- 2) KETTERING'S SUCCESS
DUE TO BRIGHT YOUNG
MEN AROUND HIM
- 3) BUSH AS CONSULTANT
TO BOSTON EDISON

operating temperatures and pressures.

It wasn't too simple a question because of the different expansions of the different metals, possible loosening of the couplings under temperature, and so on. So they'd like to get a test at higher pressures and higher temperatures than would be found in use, just to be dead sure that they didn't have a catastrophe of some sort. I looked the thing over and said, "Yes, I can see how to test one of those." They allowed that they only wanted to test one of them, the one they'd already selected as probably the best, and I said, "Sure, I can test that, I've figured out a way to do it and I think that I can do it." They said, "How long will it take you?" "Well," I said, "I can't tell offhand but I think that unless I land into something I can do it in two or three weeks." They said, "Go ahead."

So I got it set up in the laboratory and started to test it and soon discovered that my method of testing was no good whatever. So I sat down and devised another way of doing it. By the time I had the equipment put together and got the doggone thing tested, it took me at least a couple of months of a very hot summer. Whereupon, I billed them for three weeks work. That's the way, I think, you have to do it if you're going to get any reputation whatever. As it was, I got the reputation of doing a tough job very readily. If I'd told them that I'd had a hell of a struggle doing it, my evaluation would have slipped

- 1) BUSH AS CONSULTANT
TO BOSTON EDISON
- 2) THE PIPE-TESTING
EPISODE
- 3) ETHICS, FEES, &
REPUTATION

badly, particularly since I'd told them I knew how to do it.

I had a good many interesting experiences in those days as a consulting engineer. I found, for one thing, that when you go into a company, brought in as a young consulting engineer, you're likely to be about as popular as a skunk at a lawn party with the permanent personnel. And quite naturally so. It's interesting how it's possible to get on terms of understanding with them.

I remember one incident in particular. Stone and Webster called me in. There had been a failure at a penstock out in California that had washed a piece out of a side of a mountain and killed a couple of people. They were putting a very similar penstock into use up in Oregon and they wanted to know if I would go to work on it to see if I thought it was going to be safe. Well, to tell the full story would be quite a job because it strung out and it was a tough one. I finally got the darned thing worked out so that I was quite sure it was safe.

When I first went in among the Stone & Webster engineers and in their drafting room and so forth, the atmosphere was distinctly hostile. So I say down by myself and figures out a bunch of questions. The next day among other things I said to them, "Now you people, of course, have figured the combined stresses on this pipe at this joint where we can have three kinds of stresses.

- 1) BUSH AS CONSULTANT TO STONE & WEBSTER ON PENSTOCK PROBLEM
- 2) HOSTILITY OF THE ENGINEERS-IN-RESIDENCE TO CONSULTANTS
- 3) HOW BUSH HANDLED IT

We'll have our pressure stresses of course and then the two types of stress that you can get from the thermal expansion due to the change of temperature." They said, "Now wait a minute: the water that goes through this pipe doesn't change in temperature more than two or three degrees, summer or winter. It comes from way up in the mountains and largely out of snow fields, so there isn't any temperature difference to take account of." I said, "I understand you're going to connect it up sometime in August when it's good and hot. Then you're going to fill it full of ice water. What're you going to say about the thermal stresses then?" Well, faces fell all around the place because they had apparently completely forgotten that particular thing.

They went on for a minute and started to sidetrack and so on, and I said, "Wait a minute. I brought this up because I was figuring it over last night and I don't think we need to worry about it. I figured the thermal stresses and they're not going to increase your total stress very much. I just brought it up to see whether I'd figured it correctly, whether I'd checked your figures. Why don't you take my figures, go over them and see if I've made any errors."

Well, we went on with our conference and toward the end (mind you I was with a bunch of junior engineers), just before I left, I said, "Oh, by the way, gentlemen, I

- 1) BUSH AS CONSULTANT
TO STONE & WEBSTER
(Continued)
- 2) HOW BUSH HANDLED THE
S & W ENGINEERS

don't think anybody but the group here ought to hear about this thing, do you?" With that I left. Well they knew perfectly well I meant that I never would mention the subject to any of their chiefs. After that, we got along gloriously. And this was very important because I never could have done the job if I hadn't had their collaboration.

Almost all the answer to this question is along this line because if an engineer is going to be successful at all, he's got to learn to work with men as well as with things. And there we go back to Edison. Edison certainly had that characteristic. He made men mad at him no doubt, but he had a way of getting them to do things he wanted them to do, in the line of promotion. Kettering had it also. Kettering, with his breezy manner and his very attractive wisecracking, got people to like him, got people to work with him. I think the young chap who's getting into engineering today needs to be reminded time and time again that he'd better study the ways of men as well as the laws of physics. I think a lot of youngsters fail to do this even though they have formal study in the humanities.

One of the finest courses that ever was at MIT was taught by Alexander Magoun who was forced out of the place after I left because he did damn fool things as well. He didn't have any darned sense and everybody was laying for him. When I got away they got him out of there in a hurry.

- 1) HOW BUSH HANDLED
STONE & WEBSTER
ENGINEERS (Cont'd)
- 2) YOUNG ENGINEERS
MUST STUDY MEN AS
WELL AS PHYSICS
- 3) EDISON & KETTERING
WORKED WELL WITH
MEN
- 4) ALEXANDER MAGOUN

I used to protect him when I was there and I protected him because I thought he did some of the finest teaching I ever saw. I can describe some of it to you if you'd like to have me do so sometime. [JK to EH: DO YOU?]
[EH to ALL: NO! I WROTE TWO BOOKS WITH MAGOUN AND BY 1932 I KNEW ALL I WANTED!] [VB to EH: OH HO! WHAT THE HELL DID YOU DO THAT FOR?] His course in the humanities [EH NOTES: THE WORD IN THOSE DAYS WAS "HUMANICS"!]
was exactly the kind of thing to get youngsters to think about their human problems, not in terms of subtle psychological approaches or anything of the sort, but in the very terms in which they'd have to think of them when they got to be operating engineers. [X-REF AHEAD TO PP. 539-541]

The scientist of course is in a somewhat different category, for a scientist can be very successful and have very little to do with his fellow man. If he is really brilliant almost anything will be tolerated from him and the people around him will do everything they can to ease his way because of his great value. But it's not so with an engineer.

- 1) MAGOUN'S HUMANICS COURSE AT MIT
- 2) YOUNG ENGINEERS MUST STUDY MEN AS WELL AS PHYSICS
- 3) NOT SO SCIENTISTS

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Question 105: Memex in Medicine.....Is
American Medicine Lab-Happy?

Every art has its underlying techniques. Every art that's worth a damn requires on the part of the practitioner meticulous care with the details of the technical methods he employs. Now, I realize that modern painting cuts away from all of this but it doesn't disturb my judgment that only when modern painting is careful of its techniques does it contain anything that is really worth looking at.

In science, in medicine in particular, this is true. Medicine is an art, but it has its underlying techniques. The development of automated methods for getting at data has done great things for business. It does not exercise judgment: it merely enables the man who must exercise judgment to exercise it in the light of explained, coordinated, oriented facts on which he can base his judgment. In the same way, in medicine, there's no doubt in my mind that if the man who is going in to see a sick patient where it is a bit subtle, where the diagnosis is by no means straightforward, if he had before him instantly and without a lot of trouble the total experience on a dozen such cases written out in detail for his perusal, with all the symptoms, the prognosis, the treatment and the subsequent development, he would be in a far better position to advise that patient or to treat that patient successfully.

[X-REF BACK TO P. 347]

- 1) EVERY ART HAS
TECHNIQUES
- 2) EXTENSIVE DATA
COULD IMPROVE
DIAGNOSTIC MEDICINE

This does not mean that he needs to abandon his art. You might just as well say that because a man has excellent pigments and has learned how to mix them and has beautiful brushes that he can then be a painter. Not at all. Neither can a physician really be a physician unless he rises above his techniques and applies his art.

1) ART & TECHNIQUES
IN MEDICINE

Now there is, as you say, today a tendency for American medicine to be lab-happy. But I've seen plenty of examples of places even in the last year or two where medical men failed to use the techniques about them. I had one right in my own family where just a bit more laboratory work would have saved a heck of a lot of trouble. The physician did not have the sense to get the data that he needed before he began to use his judgment. I'd make a long speech on this if I didn't think that it was pretty obvious.

* * *

Question 106: The Scientist-Engineer as Humanist....
C. P. Snow and Jacques Barzun

One of the difficulties in spelling out the fact that the engineer needs to touch both things and men in his study and his experience is that when you come to define what you mean by 'men' you don't have the language available. 'Humanist', 'Liberal Arts subjects' -- some of those words do not convey exactly what I think should be in mind. Now, I believe thoroughly that where many young engineers and young men who take training in business management fall down is that they pay attention to the complex parts of the subject and fail to pay attention to the simple things. I've had a couple of illustrations of this, and I might spell a couple of them out.

One is in connection with the Merck company and I've told a little of this story already. [X-REF BACK TO PP. 65 & 513 AND AHEAD TO PP. 771-772] The fellow who was managing their research department didn't know anything about management. I tried to help him out. He'd been all right when the thing was small; he was no good when it became complex. Of course Merck, today, spends twenty million dollars a year on research so it's a big show. I tried to discuss the matter with him and I couldn't get to first base because he was always on the defensive.

One day I wrote out a list of very simple principles, the most simple ones in connection with management that I

- 1) IMPORTANCE OF THE
SIMPLE ASPECTS
OF MANAGEMENT
OVER THE COMPLEX

could write down -- for example, on how one delegates and how one deals with a subordinate without going through his chief, and how the chief of an organization gets his information and things like that. I sent it to this fellow and said to him, "I judge that you agree with all of these, but let me know and if you do, let's have a bit of a conversation about it." He agreed with them all and he called me up and asked me if it was all right to pass them on to the president of the company. I said, "Certainly." The president of the company told me he thought they were excellent and he sent them to all of his vice presidents. The joker about this is that both of those men violated those principles very often and neither one of them recognized that they did.

The subsequent history of the thing is also interesting. That list finally got to Harvard and it was put in the papers used by one of their courses. I guess it was printed for that purpose. [EH to VB: HOW DO WE FIND? CAN'T DISCOVER IN BIBLIOG.] [VB to EH: IT ISN'T THERE. DOUBT IF WE CAN FIND THEM NOW.] [AM to EH: I THINK I'VE GOT IT! NOT IN ORIGINAL FORM, BUT PROBABLY CLOSE. WILL ATTACH.]

Now the Harvard Business School, the great Harvard Business School, apparently was not previously paying attention to such simple things as that on which you could readily write out a whole book. [X-REF BACK TO P. 65 AND AHEAD TO P. 654]

1) VB's LIST OF BASIC
PRINCIPLES OF
MANAGEMENT AT MERCK

I believe many a student fails to see that it is the down-to-earth grass roots types of relationships which he needs to know all about, especially early in his career. Of course some youngsters come by it naturally and some never can acquire it. It's a part of their general personality, their youthful experiences and so on, but it is quite possible to study it and to acquire it in that way and this is very seldom done.

Now the other part of the story is the story of Alexander Magoun's course in Humanities [EH to VB: HUMANICS was the word, alas.] at MIT. An old chap [VB NOTES: NICKERSON (illeg.)] gave MIT some money to set up such a course, and Magoun was put in charge of it. Well he was an erratic cuss; he used to give sex lectures and go altogether too far and make people mad. He would teach psychology in his classes when he'd never studied it, and economics when he'd never studied that, and so on. He got a lot of the people at MIT pretty sore at him. As I've said, they finally got him out. But his class was really something. Let me recite only one episode.

He didn't allow visitors in his class but he couldn't very well keep me out. And I watched this one. When the class opened some papers were passed around which gave the essential facts of the problem and the problem was this; the girl in the factory who was head of the local union

- 1) STUDENTS NEED TO LEARN BASICS OF HUMAN RELATIONSHIPS
- 2) MAGOUN'S COURSE IN HUMANICS TRIED TO DEAL WITH THIS

was raising the devil on company time and so forth. On the other hand she was exceedingly popular and the management had been ordered by their owners, this being a mill where there was absentee ownership, to get rid of her. When the class opened, two of the class were on the platform behind desks. One was acting as the manager and the other as his assistant manager and they were about to have an interview with this girl. In came the girl, an actress from Boston who was a friend of Magoun and who readily took this opportunity to have a little fun. She came in and they began discussing with her her activities in the union and how she had violated company rules and so on. She made monkeys out of the pair of them -- quite easily, in fact.

At the conclusion of the hour, the whole crowd got together and started discussing things informally with everybody standing around. In the next class they discussed in detail what mistakes these two young fellows had made who were the manager and the assistant manager; where their approach had been wrong; where they had failed to understand the motivations of the girl and so on. This was great stuff. The students ate it up and they learned a whole lot.

One other class had a somewhat similar background. The two chaps were going to try to work out a deal with the head of the local labor union. The chap who came in

- 1) MAGOUN'S HUMANICS
COURSE AT MIT TRIED
TO GET AT "HUMAN"
ASPECTS OF
MANAGEMENT

to represent the head of the local labor union was indeed the head of a labor union in a textile mill down in Rhode Island. As the discussion got going and got a little hot, he rather forgot that this was artificial and merely a play. He began to pound the table and swear at the boys and the whole party got to be quite a show. I think that youngsters in that class learned things about public relations [EH to VB: EXCEPT THAT I DON'T THINK THIS IS THE RIGHT EXPRESSION.] [VB to EH: HOW IS "HUMAN"?] human relations in an intimate personal way faster than I ever saw it done anywhere else. There is no such thing at MIT today.

People didn't like Magoun; I protected him. At one time we got a new Dean of Humanities; Karl Compton appointed him without saying a word to me about it for the very simple reason that he knew that if he did say anything to me about it, I'd object. When this guy got going, he came into my office one day and said that we must get rid of Magoun at once and I told him, "Now wait a minute, Magoun's my man and you leave him alone." The new Dean, being a timid soul, did just that. So I protected Magoun but after I left for Washington they threw him out. Incidentally, he's been quite successful since, advising public relation groups in industry and he's had a good deal more income since he left than he had before.

- 1) MAGOUN's MIT COURSE DEALT WITH "HUMAN" ASPECTS OF MANAGEMENT
- 2) BUSH PROTECTED MAGOUN AT MIT

Reel 8-B
Page 542-A

The thing that I'm getting at through all of this is that I think there is a failure in our educational system pretty generally to teach in a really convincing, thorough manner the simple aspects of human relations. I could give plenty of other instances.

1) FAILURE OF OUR
EDUCATORS TO TEACH
HUMAN RELATIONS

* * *

Question 107: Thermodynamics and Shakespeare & C.P. Snow

I'd be very interested to know how C. P. Snow would define the Second Law of Thermodynamics. The broadest definition of it that I've ever seen was in Eddington's book which made such a popular splash a long time ago.

[1928] When I was in Tufts, I studied Thermodynamics under a chap who didn't know any Thermodynamics and when I tried to enter Tech, Harry Goodwin wouldn't sign off my Thermodynamics. He said that I hadn't had adequate preparation in it. I told him that the man I'd studied under didn't know any Thermodynamics but that he wasn't trying to enter Tech. I was, and that I'd be quite willing to take an examination in the subject, whereupon Harry Goodwin signed it off.

When I was at Tech on the staff, Thermodynamics was taught in three places, by the chemical engineers, by the mechanical engineers and by the physicists. Anyone taking the text or the notes on any one of these would not have recognized that he was having anything whatever to do with the other two. That's all been cured. But Thermodynamics is a subject that has been thoroughly balled up for a long time.

Now I wouldn't say that an educated man needs to be familiar with the details of the Second Law of Thermodynamics, but I do think that unless an educated man has

1) FLASHBACK--THREE
DISPARATE COURSES
AT MIT IN
THERMODYNAMICS

Reel 8-B
Page 543

got a grasp of the broader aspects of the law, what it means not in terms of equations and so forth, but what its general significance is in the evolution of the world, there's something missing from his general understanding of his place in the world. In the same way, of course, I'd say that I don't think an educated man has to have a scholar's (or even an actor's!) familiarity with the works of Shakespeare, so that he can quote at will and ad lib. But unless he can understand and appreciate the subtle way in which Shakespeare understood the relations between people, again there's something missing.

In this sense I think your statement is a very good one on this strange subject

1) AN EDUCATED MAN
SHOULD KNOW WHAT
SHAKESPEARE AND
NEWTON WERE UP TO

* * *

Question 108: "Science" has no antonym. Should it?
How about "Humanist"?

The English language is singularly lacking when we come to discuss the thing we have here. I rather think that Humanist is as good an antonym for Scientist as we have, although its present meaning is not exactly what we ought to have in mind. I don't think there is any really good word to use. This is in contrast between Scientist and Humanist; we're in still more difficulty when we try to talk about the engineer. There is no opposite here that fits into both fields or should. It's entirely possible, of course, that as the language alters, "Humanist" will come to have just the meaning we'd like to have it have.

The thing I think is missing is the difference between a man who studies the world of men from a complex standpoint -- from the standpoint of the economist, the psychologist and so on -- and the chap who understands men in the way that the farmer down in Vermont understood men who had the little passage with the automobilist who was headed for the town and who finally remarked to the automobilist, "Well, at least I'm not lost." This down-to-earth grass roots understanding of men has no really good word I know of to describe it, and it is the thing that I think gets left out of the equipment of a sad number of people.

- 1) "HUMANIST" IS AS GOOD AN ANTONYM FOR "SCIENTIST" AS WE HAVE
- 2) FARMER "DOWN IN VERMONT": "AT LEAST I'M NOT LOST"

Reel 8-B
Page 545

I think my dad had a wonderful grasp of it, and I think part of the acquisition of the grasp is this: that he always could have sympathy and understanding with men that he regarded as very far from being perfect morally. That didn't trouble him a bit apparently, as long as the guy was reasonably honest and not a hypocrite. I think a great part of the thing I can't define and can't state in one word is an understanding, not of men of one's own group, one's own status in society, or one's own field of interest, but the understanding of men of all sorts and types, without the paraphenalia of Freudism or anything of the sort, with nothing but just a bit of common sense. I don't know anywhere where that is really driven home.

1) VB's FATHER HAD A
GRASP OF THE BASIC
NATURE OF PEOPLE

* * *

Question 109: Physicists Talk to One Another Off
In a Corner of the Scientific Kitchen

I one time wrote something about the modern tower of Babel to the effect that science was getting fragmented into specialties to the point where a man writing a paper would be writing it for only a half a dozen other men, and where the language and the jargon of each group was being made so complicated and so bizarre that they could be understood only by chaps who were working in the identical field. We've got a lot of that.

The worst sinners, in this regard, are the ones whose science is the most elementary in the sense that it is most distant from precise formulations. Take the psychologist as an example. [EH to VB: TALKED TO ANY "SOCIAL SCIENTISTS" LATELY? THEY MAKE PSYCHOLOGISTS SEEM LUCID --FOR THE S.S. BOYS HAVE EVEN LESS TO DEFINE.] [VB to EH: I AVOID TALKING TO S.S.'s WHEN I CAN.] They've developed an enormous jargon because they had to have something they could talk about and they had to have words to talk about them with. They weren't too much disturbed if they couldn't define those words in any way that made sense. Of course, logic can be used in any science only in connection with the definitions that are precise and fixed. That is the trouble with a whole lot of science which is still on the borderline of becoming definite in its formulation and its thought.

- 1) OVER-FRAGMENTATION
OF THE SCIENCES
- 2) JARGON & OBSCURE
WORD DEFINITIONS
esp. IN PSYCHOLOGY

On the other hand, there is a good deal being done on bridging the gaps between disciplines. I think the job that Gerard Piel has done in the Scientific American is perfectly swell. He's not only put in there articles for the good of the layman, he's got articles that are good for scientists and that are read by scientists, read for the purpose of keeping reasonably abreast of what's going on in other areas. He's managed to get people to write properly and clearly, and this has been no small part of his accomplishment.

There are also people that appear once in a while who have special skills in interpreting a field so that other scientists can grasp it readily and hence keep from getting too far apart. I suppose Karl Gavrow [YAVROW? illeg.] was the greatest one of these, writing at the Bell Laboratories. He had no ambition to be a research scientist in the usual sense; he was quite happy to thus serve in clarifying areas. The reason, of course, that these men are so important is because a discipline that gets completely isolated misses all the opportunity that comes from advances in adjacent areas. This applies not merely in new instruments and the like but really in new methods of thought which are missed when a fellow gets too far out on a limb.

- 1) BRIDGING THE GAPS
BETWEEN SCIENTIFIC
DISCIPLINES
- 2) GERARD PIEL OF
SCIENTIFIC AMERICAN
- 3) KARL GAVROW
OF BELL LABS

Darrow

There is another point on Question 109 which may perhaps belong elsewhere and that's the question of what a youngster does faced by the broad range of science to avoid becoming superficial on the one hand or over-concentrated on the other. This has to do with the whole story of the babble of voices at the present time. I made a talk some months ago to a bunch of college students where I tackled this problem and it seemed to make quite a hit with them. Vera can get it for you if you haven't got it already. [EH to VB: TITLE? WANT.] [VB to EH: I DON'T KNOW WHETHER IT STILL EXISTS.] But I think it's worth looking at. The youngster that's interested in science today is likely to do one of two things: he's likely to specialize to an extent where he misses the whole broad range of science entirely, or on the other hand, he's likely to browse around and never dig in deep anywhere. I think you might find some thoughts in that talk which was published and which I think Vera can get. [EH NOTES: WANT]

1) THE SCIENTIFIC
TOWER OF BABEL &
HOW STUDENTS
REACT TO IT

* * *

Question 110: Disadvantages of Living in an Affluent Society -- as, Diminished Services.

I read Ken Galbraith's Affluent Society for the second time not very long ago. I found a lot to agree with. At the same time I found quite a bit to quarrel with, principally what he left out rather than what he put in. He treated only one side of a rather complex story, it seemed to me. But I can comment on this business of the affluent life from another point of view which you went around in your question.

A great many people in the world work because they love to work and they'd be very unhappy if they didn't work. It may be quite possible that as the working hours become shortened, as productivity increases due to automation and to various other things, men will find themselves unhappy with too much leisure. The other group of men in the world, the larger part, work merely because they have to. They don't want to and they work only when they need things. They'll certainly work to acquire reasonable shelter, food and clothing, but will they work in order to get a color TV instead of a black and white TV? I rather doubt it.

I think one of the great problems before us is not to satisfy human wants, but to find human wants to satisfy. As we proceed, the ones that we next touch such as a motor-driven toothbrush are a bit foolish and trivial and not

- 1) THE AFFLUENT SOCIETY
- 2) FINDING HUMAN WANTS TO SATISFY

the kind of thing that men will want to sweat to acquire. This may make quite a puzzle because, of course, as men get to the point where they've got what they need and merely work to acquire luxuries, they're going to be very choosy as to where they work and how. We've got the beginnings of this right now and as we go on, we'll get more and more of it.

Now the trouble, of course, is that the call today is for men of skill. Men of skill are the very ones who readily acquire sufficient income and hence become prima donnas and choosy. The ones who really need to work in order to acquire a reasonable standard of living haven't got the skills with which to do it. I don't know how we get around this one. The advent of more attention to automation, for automation has been going on for a couple of generations, is making work for skilled personnel and removing the opportunities for unskilled personnel.

[X-REF BACK TO P. 221] I haven't any answer, but it seemed to me that Galbraith rather ducked some of this business. I suppose on the whole we ought to welcome a world in which men don't have to work so hard, but we can certainly over-do it and we may be on the verge of just that.

- 1) THE AFFLUENT SOCIETY
- 2) FINDING HUMAN WANTS TO SATISFY
- 3) MEN OF SKILL ARE IN DEMAND & BECOMING PRIMA DONNAS
- 4) AUTOMATION FAVORS THE MEN OF SKILL

[EH to VB: VAN: I wonder why your social analyses so often seem to me to leave something out. "It is the disposition of men to huddle in cities," said Pareto (or somebody) but the problems raised by large aggregations of people living in areas of congestion (by definition, cities) don't seem to interest you much.

1) NEW YORK CABBIE

[Let's take a case: a New York City taxi-driver. The Census Bureau classifies him, at best, as semi-skilled. Actually he is highly skilled. And although he is a "professional" as opposed to "these Goddam Sunday drivers" whom he hates and scorns, he is not a professional man in anyone's eyes.

[Harrassed by the cops, treated like a bum by the cab-riding public, periodically held up, robbed or killed by hoodlums or hop-heads and fighting a losing battle with increasingly intolerable traffic, he hates you and me (his riding public) and as soon as he has made "enough" as his day's pay (which he admits isn't bad) he takes his cab back to his garage, saying "I've had it!" And he has.

[Well, you say, he can upgrade himself to a better job. Sure. But somebody will have to drive this city's 14,000 cabs -- which aren't enough -- so long as the social, political and engineering disgraces of public transportation, accomodation and housing (slum areas where the cabbie gets murdered for the day's fares he's collected) go uncorrected. Your page 548 seems over-placid to me,

sir. Our explosive cities are a bigger threat today than "the bomb", in my belief, which I wish I could sell you a 10% share in.]

[VB to EH: STRANGE. Around Boston I find the cab drivers a pleasant lot on the whole. Often the conversation, when there is one, is surprising. A few days ago I had a cabbie who has an electric organ at home, and we discussed why it had some advantages over a pipe organ. Phoebe usually has nice things to say about the cabbie when she gets home from a ride. Many exceptions, of course. Maybe the trouble is that you live in New York.

[Of course the cabbie is highly skilled. He is also often a professional man, in the sense that he is independent (owns his own cab) ministers to public needs, etc.

[You touch here a point on which I am much interested. I haven't written on it much, for one reason because I haven't thought it through to the point where I have much to say.

[I did write, and send you, a while ago a piece in which I contrasted an expert toolmaker, and a banker, much to the advantage of the toolmaker.

[The chap who often gets my goat is the dentist. Many dentists are just mechanics (I'm not talking about the ones that handle the really tough jobs, such as the chap who gets slapped in the jaw with a shovel). Yet, because they took a course and get called doctor, they are in a separate [[? Illeg.]] social category than your cabbie, who has to

- 1) BOSTON CABBIE
- 2) DOWN WITH DENTISTS

Reel 8-B
Page 548-C

be more skilled.

[O. Henry wrote some excellent material on the City.

[I know it's a cruel [[crude? cold? ILLEG.]] and heartless place, when viewed from the cab rider, by you and me. I wonder if it is when viewed by the clerk or the garage mechanic. I'm afraid it is, but O. Henry painted another picture well. In what I've seen of the country (not much) I'm sure there are two contrasting pictures that can be painted.

[In other words, no lack of interest -- just puzzlement.] [END ADDITIONAL MATERIAL]

1) SOCIAL INJUSTICE
A PUZZLEMENT

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Question 111: Yerkes at Yale and his Chimpanzees

I had a little to do with Yerkes and his chimpanzees because this was being supported by the Carnegie Corporation among others. It was evidently getting into a range where not much was being accomplished. People who had been backing it were withdrawing and the thing was in financial difficulties. The Corporation asked me to look it over and for a while I had a bunch of chimpanzees on my hands. I visited the place, of course, and talked with the people. It soon became evident that the whole trouble was that there was no sound leadership. Yerkes had been very ingenious in his early days but I think he'd gotten a bit into a rut. We got a new director, and we got the money, and I got the chimpanzees off my hands.

[X-REF BACK TO P. 79 AND FORWARD TO P. 625]

Some of the puzzles that they are up against intrigued me no end. I think we are much more likely to find answers through the work of such a chap as Purnell [EH to VB: VAN: THE GENT IS WILDER PENFIELD.] [VB to EH: THAT'S WHAT I SAID. CAN'T THE DAMNED TAPE TELL THE DIFFERENCE BETWEEN PURNELL AND PENFIELD?] Penfield up at McGill's Neurological Institute who's doing perfectly marvelous work. One of his stories is in the very recent issue of the Atlantic and it's excellent. He's finding out about how the brain works to a very extraordinary extent.

- 1) YERKES AND HIS CHIMPS AT YALE
- 2) PENFIELD AT MCGILL

Working with the chimps is the other approach, but there one merely finds that he gets up against inherent limitations. The chimp can go a certain distance and then he can't go any more. Well, I see it in my own pair of poodles. They understand probably fifty English words. They sit around and cogitate; there's no doubt of that. They do pretty good character analysis, in my opinion, in the way in which they try to work gags on me with some success. But how is it that understanding a lot of words, they never tried to imitate them. They could; they might not make good imitations but they could at least make different kinds of sounds meaning different kinds of ideas.

They haven't the part of the brain that is needed for abstract ideas to the extent that they can thus associate words and ideas and speech. They can associate words and ideas, but not with speech. This is their limitation and it's a limitation that evolution has put on them. They have not formed that piece of brain which is necessary for the purpose. Neither have the chimps and after one has discovered this, I don't think that there's very much to be gained by trying to see how well you can make a chimp make change. You can do pretty well and it's amusing, but I don't think it gets at the fundamentals

On the other hand, I think that Penfield with his exploratory probes on a conscious human being can find out just that kind of thing and can find out what may be necessary to improve what is now present. Which is, of course, the real fundamental idea.

- 1) BUSH'S POODLES
- 2) INTELLECTUAL
LIMITATIONS OF
THE CHIMPANZEE
- 3) PENFIELD'S WORK
ON HUMAN BRAIN
MORE USEFUL THAN
TRAINING CHIMPS

Question 112: Killian Committee Report on Scientific
Manpower....The Moon Shot

I have the report in full here at the Cape and I propose to get at it. Thus far, I've only skimmed it and read some of the reviews and so forth. [EH to VB: I AM SURE YOU WILL WANT TO AMPLIFY NOW.] [VB to EH: NOT RIGHT NOW.] I'm inclined to think that the committee pulled its punches, that it stopped short of laying down positively some of the facts that it hints at and skirts around. I don't know whether this is because of Jim Killian or because he was blocked by some of the people on the committee. I've seen Jim pull his punches on this very subject and I've seen him avoid taking a really downright stand. I may find that in the report when I read it. But I've got to read the report, of course, before I can answer this question to any real extent. I can, however, make a comment on why I wrote the Times letter the way I did.

There's a lot of argument around popularly that anyone who opposes the moon shot would have opposed Columbus setting out for America. Of course it's a rather shallow argument. Nevertheless, when I wrote the thing to The New York Times I thought I might wake some of these people up by pointing out to them how they might personally get into one whale of a jam with their neighbors and so forth by forcing ahead a program that resulted in a catastrophe with fifty million people watching. [X-REF BACK TO P. 358] I did that particular plug in order to try to get into the minds of a few

- 1) KILLIAN COMMITTEE
REPORT ON
SCIENTIFIC MANPOWER
- 2) MOON SHOT LETTER
TO THE TIMES

people that are arguing about the thing in a soft-headed way that there is something in this that may affect them personally. And, of course, I had in mind Jim Webb.

My real argument, of course, about the moon shot is that we're inclined in this country to follow fads and slogans and so forth. When we do, we forget all about the economic aspects of the thing. The moon shot is costing so much that the amount of scientific knowledge that will be acquired is utterly negligible compared to the cost. It's also diverting a whole lot of our scientific and technical manpower away from industry where it might be very useful in our competition with the Russians for world trade and so on.

So that you're right, my main arguments are along the lines of the economic aspects of the thing. I put in one plug on the other side in the hope that I'd startle a few people that have been thinking only of the rosy side of the affair.

Now this takes me to the end of all of the questions I have before me; it also takes me to the end of Reel 8. We quit here with the full expectation that it won't be long after you read what has been put on tape before I get some very explicit ones as you go through and see a point which might be interesting but on which I've merely touched. So this is the end of the present series.

1) BUSH'S OPPOSITION
TO MOON SHOT --
MONEY & MANPOWER

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END REEL #8