ENGINEERING RESEARCH INSTITUTE
UNIVERSITY OF MICHIGAN
ANN ARBOR

REPORT ON VISITS TO EUROPEAN ELECTRON TUBE LABORATORIES, 1953

University of Michigan
Electron Tube Laboratory

by
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Project 2275

SPONSORED IN PART BY THE U. S. ARMY SIGNAL CORPS
CONTRACT NO. DA-36-039 sc-56714
AND DA-36-039 sc-15450

November, 1954

ABSTRACT

A survey is presented of various European vacuum tube research and development laboratories visited by the author during the summer of 1953. General tube practice and policies, reliable tubes, microwave tubes, and miscellaneous tube subjects are discussed.

Two other chapters appear in a classified supplement to this report.

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REPORT ON VISITS TO EUROPEAN ELECTRON TUBE LABORATORIES, 1953

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Introduction

0.1 Organization of the Material

The material in the following pages constitutes a report on a survey of various European vacuum tube research and development laboratories, and related institutions, made by the writer between July 15 and September 15, 1953.

Chapters 1, 2, 3, and 4 taken together constitute a report that is not classified in the military security sense; however, because rather frank appraisals and comparisons are made, only limited circulation seems advisable. These 4 chapters deal respectively with

(1) General practices and policies;

(2) Reliable tubes ("trustworthy valves");

(3) Miscellaneous subjects, and

(4) Microwave tube material.

Chapters 5 and 6 are separately prepared and circulated.

0.2 Places Visited

The establishments visited and the dates of the visits on the European continent were as follows:

Laboratoire Central de Telecommunications (The research laboratory of the French subsidiary of the International Telephone and Telegraph Corporation), 46 Avenue de Breteuil, Paris VII E.	21 July
Compagnie Generale de Telegraphie San Fils, Research Laboratory, 23 Rue de Maroc, Paris.	22 July
Brown Boveri Corporation, Baden, Switzerland.	4 August
Philips Gloeilampfabrikinen, Research Laboratory, Eindhoven, Holland.	11, 12 August

Royal Institute of Technology, Stockholm, Sweden.	17 August
A. B. Svenska Elektronror, Stockholm, Sweden.	18 August

The establishments visited in England, with dates, are as follows:

Mullard Corporation, Research Laboratory, Solfard, England.	20 August
Mullard Corporation, Trustworthy Valve Laboratories, Mitchum.	20 August
Associated Electrical Industries, Aldermaston.	20 August
Cambridge University, Cavendish Physical Laboratory, and Radio Astronomy Field Station, Cambridge.	27 August
University College of the University of London, Physics Laboratory, Gower Street, London.	28 August
Standard Telephones and Cables, Research Laboratory, Enfield.	31 August
Standard Telephones and Cables, Brymar Valve Works, Footscray.	31 August
Standard Telephones and Cables, Microwave Research and Development Laboratories, Ilminster.	1 September
General Electric Company, Limited, Research Laboratory, Wembly and India Pavilion, near London.	2 September
Admiralty Signals Research Establishment, Portsdown.	3 September
Mullard Corporation, conversation with Mr. Malleson, London.	3 September
Services Electronics Research Laboratory (SERL), Baldock.	4 September
British Thompson Houston Company, Research Laboratory, Rugby.	7, 8 September
Telecommunications Research Establishment (TRE), Malvern.	9 September
Electrical and Musical Instruments, Ltd., Hayes.	10 September

During the period 24, 25, 26 August, I attended a meeting at Oxford University of the Upper Atmosphere Rocket Research Panel, held together with a meeting of the Gassiot Committee, for an exchange of experimental and theoretical information and points of view regarding rocket research toward determining the properties of the upper atmosphere. The discussions at that meeting are not a part of this report.

However, following this meeting at Oxford, the UARRP group adjourned to Cambridge University, and some also to the Physics Laboratory of University College of the University of London. Inasmuch as the observations at these two locations are of some interest in vacuum tube work, comments on visits to Cambridge and University College are incorporated in this report.

In general, I had very satisfactory clearances everywhere in England. At the Compagnie Generale de Telegraphie San Fils in Paris there was plenty to talk about for the entire day on scientific matters and questions having to do with the reasons for functioning or malfunctioning of various devices, without getting very much involved in classified matters. The general feeling there seemed to be that they were willing to tell me anything that was appropriate for them to tell the Evans Signal Laboratory at Belmar, or the Raytheon Corporation. Because of this fact much of the material discussed with this group in Paris is included in the classified portion of this report. At all other places on the Continent, the Laboratoire Central de Telecommunications (the I. T. and T. Unit), in Eindhoven, Brown Boveri, and in Sweden the discussions were all on an unquestionably unclassified level.

There are a few cases where the problem seemed to be primarily one of commercial rather than military security. This seemed to be true at Eindhoven, and at one or two places in England. However, this appeared to offer no very serious obstacle to very valuable interchanges of technical ideas.

In general I did not take notes during conversations, except to jot down on occasion specific numerical values of interest. It seemed more satisfactory to keep the conversation centered around particular items long enough so that the essential facts were clarified in one's mind sufficiently to enable later recording of the information and points of view.

From time to time there will appear in the report my own evaluation of comments made by the men being visited. In some cases where the discussion was actively participated in by several parties, to the extent that it was difficult to state whose ideas were which, there may be stated ideas that are mine rather than being attributable to personnel of the laboratory being visited. I believe the context will indicate when this is true.

The schedule of visits in England was laid out by Lt. Col. Herbert M. King, the Signal Corps representative at the United States Embassy in London. Col. King did an excellent job of translating into tangible dates, places, and available people the needs expressed to him in the correspondence channeled through the Signal Corps as to the purposes of the visit. The secretariat of the British Committee on Valve Development (a British organization set up during World War II, and continuing much as in those days to supervise research and development on vacuum tubes) was also very helpful in providing the final contacts.

Perhaps it should be commented here that the British Committee on Valve Development (the CVD) has genuine authority, backed up by financial resources, which permits it to a large degree to control the channeling of funds into the development of vacuum tubes for military purposes. There is of course also a substantial amount of research and development work done with private money, that is, by industrial concerns with their own money, in certain areas, but apparently not very much in the microwave area. The work on trustworthy valves appears to be largely done with government money, and its course has to a considerable degree been directed through the CVD. However the CVD maintains close contact with industrial opinions.

It is rather difficult to understand the reasons why some things are being done the way they are in Europe, in the electron tube industry. In an effort to help clarify the situation, or at least outline the major phases of the confusion, there have been included as Chapter I various comments dealing with international commercial policies, international standards, and a few remarks on research financing and the television broadcast patterns. These comments will indicate to some extent the influence of the NATO organization.

CHAPTER I

Commercial and Organizational Policies and Standardization

1.0 Nature of Material

In this chapter there are reported, arranged according to place and date, various comments of a non-technical nature, relative to commercial practices, organizational policies, television plans, the relation of standardizations to NATO requirements, and some strictly political comments, growing out of a visit to various European vacuum tube research and development laboratories between July 15 and September 15, 1953.

1.1 Laboratoire Centrale de Communications (LCT)

(The Research Laboratory of the French subsidiary of the International Telephone and Telegraph Company). 46 Avenue de Breteuil, Paris.

Tuesday, 21 July, 1953.

1.1.1 Personnel contacted. At LCT the personnel contacted included:

Mr. E. M. Deloraine, Technical Director for the International Telephone and Telegraph Co.

Mr. Georges Chevigny, Assistant Director of LCT, and in charge of a large portion of its work, including all vacuum tube work.

Mr. Beckman, responsible for vacuum tube work under Chevigny's general supervision.

Dr. Gerard Lehman, responsible for work on servomechanisms, and I believe also magnetic amplifiers at LCT, also, Chief Engineer of "Societe des Serromecanismes Electronique", Ter Rue to Chanez, Paris.

1.1.2 General comments on I.T.T. activities in Europe. As mentioned in Section 4.1.2, I had become acquainted with Deloraine, Chevigny, and Lehman in connection with their u h f tube work in the U. S. during World War II.

One of the very important economic factors controlling activites of I. T. and T. throughout Europe, and particularly at LCT, is that the NATO organization calls for interchangeability of tubes between military equipment made by the various NATO countries. Agreement has been reached to standardize on U. S. tube types in NATO equipment. Because of the dollar shortage, the French particularly do not want to buy American-made tubes to use in NATO equipment. Therefore they are as completely as possible arranging to build, in France, the U. S. tube types needed. As discussed in Chapter 4, LCT is carrying an important part of this program.

The vacuum tube activities at LCT in Paris consist of:

(a) Late research and development on small uhf and microwave space charge control tubes, that is, triodes and tetrodes, discussed in Chapter 4.

(b) Development and manufacture of large power tubes, that is, tubes at the 100 kilowatt, 200 kilowatt, etc., level, for broadcast use in all radio bands.

(c) Research and development on gas tubes.

Mr. Deloraine is Technical Director for all of the I. T. and T. activities, including subsidiaries in Germany, England, and the United States. Thus I was told that Mr. Scott of Standard Telephones and Cables in England is responsible as to technical matters to Mr. Deloraine, although he is responsible as to business and managerial matters to the officials of Standard Telephones and Cables in London.

It became fairly obvious that there is rather good coordination between the work of LCT in Paris and of Standard Telephones and Cables in London. For example, Mr. Chevigny found it very convenient to arrange, while I was in his office, a telephone call to London which permitted me to talk to Mr. Scott and discuss with him my interests, and plans that could be made for my subsequent visit in England. The telephone conversations that went on at that time between Mr. Chevigny and Mr. Scott were of a very informal nature, such as take place between people who have rather complete trust in one another, with a good understanding of what each one could feel free or could not feel free to say to the other.

Mr. Chevigny has now what is primarily an administrative position, being no longer intimately involved in the technical aspects of vacuum tube work. Lehmann has the responsibility for some servomechanism and related applicational problems. He is also Chief Engineer of a small company organized to promote the use of some servomechanisms patents held by I.T. and T. in France. Thus Lehmann is no longer working on vacuum tubes, although he has become much interested in solid state problems. My conversation with Lehmann turned out, although I had not expected this, to be an outgrowth of previous association with him, rather than directly because of any present interest in vacuum tubes. I found conversations with him very interesting and although of a general rather than specific nature, valuable. He seemed very anxious to convey to me various points of view regarding servomechanisms and magnetic amplifiers applicational matters that he and some of his associates held in France.

Although Chevigny is no longer working directly on vacuum tube problems, his responsibilities include supervision over the vacuum tube work, so that he is very well informed as to the objectives and accomplishments of the vacuum tube program. Mr. Beckman, responsible under Chevigny for the tube work, showed me around the LCT vacuum tube research and development laboratories.

As to I. T. and T. activities in Europe as a whole, traveling wave research and development is being carried out in Ilminster, and klystron development largely by Mr. A. H. W. Beck at Enfield (I visited both places later), both in England. I had no indication that any extensive magnetron work is being done by I. T. and T. in Europe at any of these laboratories, although I believe some attention is being given to magnetrons at Enfield, primarily from the cathode standpoint.

1.2 Brown Boveri Manufacturing Company, Baden, Switzerland

Tuesday, 4 August 1953

1.2.1 Personnel contacted. The following men took part in showing me around at the Brown Boveri plant:

Dr. Theodore Boveri, a director of the company, and in charge of engineering and manufacturing. He is a son of the man who founded the company.

Mr. Kesselring, a relatively young man in charge of the electron tube work, possibly also of some of the electronic equipment work.

Their "reception engineer", a man whose name I do not remember, about 55 to 60 years of age, with a long background of engineering experience, who has the primary responsibility of seeing that technically-minded visitors like myself see what they want to of the plant and the engineering staff personnel.

Two assistants to Dr. Ludi, who is in charge of the microwave tube work and is the inventor of the "turbator", an interdigital microwave magnetron. Dr. Ludi was out of the city on the day of my visit.

1.2.2 General comments as to Brown Boveri activities. The Brown Boveri plant at Baden employs about 7000 people. The most important products are of course electric power generation equipment. They manufacture water wheel generators, steam turbine generators, transformers, transmission line equipment, high voltage circuit breakers, lightning arrestors, etc. Of course their market is chiefly outside of Switzerland. They sell a great deal of equipment to Latin America, and to other countries in Europe.

This department in which electronic equipment is manufactured is relatively recent and is staffed with relatively young men, but is obviously a significant activity. One of their important electronic-type products is carrier-current communications and related equipment for power companies. I am sure they also make military electronic equipment, but I was not shown any of it.

One fairly recent product they are quite proud of is the air blast high voltage circuit breaker, for use on high voltage transmission lines. They have sold a number of these in the United States. Since returning to the U. S. I have discussed this product with a number of electric power company engineers, and find that Dr. Boveri's statement as to the presence in the United States of a number of these units were entirely correct. Thus the Brown Boveri air blast circuit breakers are sold in the United States in competition with circuit breakers of different kinds but for the same purpose, manufactured by the General Electric Company, the Westinghouse Company, the Allis Chalmers Manufacturing Company, and some smaller companies.

Of course one of Brown Boveri's very important electronic productions consists of large mercury pool rectifiers, many being of the steel tank continuously pumped type. They manufacture these up to ratings of several thousand kilowatts, the important customers being the chemical industry. Many of these were supplied to the German chemical industry before the war.

At one time the Allis Chalmers Manufacturing Company in the United States had a patent license agreement with Brown Boveri. I am told by people in the United States that this arrangement no longer exists, but there still does exist rather a close personal relationship between individual Allis Chalmers engineers and Dr. Boveri. My introduction to Dr. Boveri was provided by an Allis Chalmers man whom I have come to know through activities in the American Institute of Electrical Engineers.

1.2.3 Dr. Boveri's comments relative to the Russians. While Dr. Boveri, the reception engineer, and I were walking together through one of the aisles where some rather nice electrical equipment assembly was being done, I asked Dr. Boveri, "Can the Russians do this kind of work?" His English was not really fluent, nor did he understand my English too well, but when he understood my question he answered it partly by telling a story.

Prior to World War II Brown Boveri used to sell quite a bit of equipment to the Russians, but there was one aspect of such sales that was unsatisfactory to them. They would deliver a piece of equipment, maybe a waterwheel generator, or a turbine generator, or circuit breakers, to the Russians, and would then never know where the apparatus went, and none of the Brown Boveri people would ever see it again. In contrast, in case of sales in western countries, they know where each unit goes, and one of their service engineers will help the customer in putting it into operation the first time. Also, if anything happens to make a unit need repair, their engineers and repairmen are sent out to make the repairs, not so in a shipment to communist Russia, even before World War II.

However, on one occasion, about 1938, the Russians did ask their assistance in making repairs to a steam turbine, so they sent an engineer and repair crew to the location in Russia--he did not say where it was. When these men came back they had so many derogatory comments to make about Russia that the management had to insist that they not talk too much, lest word of the talk get back to the Russians, who would then say to Brown Boveri that the Brown Boveri management was telling its men what to say, as anti-Russian propaganda.

Remember, these are comments made in 1953 by the man in charge of engineering and manufacture for Brown Boveri, a Swiss concern, regarding the situation as it was in about 1935-1940.

In particular, the repair crew commented as follows:

1. It surprised them to find that in Russia you could not just walk up to a railroad ticket office and buy a ticket to go where you wanted to; you first had to ask for permission to go, and 95 per cent of the time the permission would not be granted. This was strange for men used to living in a free country.

2. The Russian mechanics were completely incompetent, by Swiss standards.

3. The Russian engineers were very theoretical, with very little understanding of practical matters. Dr. Boveri said that his men said to the Russian engineers, "go back to your school, we'll make it work".

Dr. Boveri then said that of course he does not know how good Russian engineering competence is today, but it was obvious from the way he spoke that he would have great difficulty in believing that the Russians can as yet compete on anything approaching equal terms with the Swiss as to engineering competence in the electric power generating equipment field.

When speaking of the Russians, Dr. Boveri said, "We have no dealings with them now."

He was quite emotional in his comments regarding the Russians--he talked so rapidly it was hard to understand him.

Although I cannot be completely sure of his precise wording, he said to me at one time, "the world cannot continue to be part free and part Communist". Judging not just by the words he used, but by his manner and the way things were said, it seemed evident that he is completely in sympathy with the western world, and is strongly anti-communist. It is very difficult for me to imagine his tolerating any engineering activity in his organization directed toward strengthening the Russian military position. He has rather an impulsive and expressive personality; he has very definitely technical rather than business attitudes and reactions.

1.2.4 The Boveri family and the origin of the Brown Boveri Company. Dr. Theodore Boveri, whom I talked to, is a son of the Boveri who helped start the Brown Boveri business in 1891. At that time Theodore Boveri's father and an Englishman named Charles Brown were working in an engineering capacity for the Oerlikon Company (a Swiss engineering concern that is still active, particularly in the manufacture of military equipment). Charles Brown was a young man, age 25 or so, who had come from England, with English engineering training. Charles Brown and Boveri decided to attempt to initiate a business of their own in Switzerland. It took Boveri some five years to work out the financing. I gathered this financing was partly arranged through Brown, but that a very considerable part of it came from Boveri's wife's relatives. Of course, from its beginning in 1891, the Brown Boveri Corporation has grown substantially and is now considerably larger than Oerlikon, as to size of business activity.

Dr. Theodore Bover is a director of the Brown Boveri Company, and has charge of engineering and manufacturing. His brother, a banker in Zurich, is Chairman of the Board of Directors. Dr. Boveri has been in charge of engineering and manufacture for only a few years, had previously been working towards this position by assisting an older man who is now 73 years old and has practically retired. I judged Theodore Boveri to be perhaps 50 years old.

He told me of having spent a number of years in the United States at the time, perhaps 20 or 25 years ago, when the commercial connection between Allis Chalmers and Brown Boveri was active. It was at that time that he became acquainted with the people in the Allis Chalmers company who gave me an introduction to Dr. Boveri. He was quite willing to talk about his earlier history and about the earlier commercial relationship.

1.2.5 German competition. Dr. Boveri commented that their business had been excellent since the close of World War II (he made no comment in regard to conditions during World War II). They had had, 1945-1952, practically no competition in Central Europe, because of the complete destruction of the German manufacturing plants. However, within the last year or so they have begun to experience competition from two German companies, the A.E.G., and Siemens. These were of course the two important German electrical power equipment manufacturers before World War II. His comments indicated that A.E.G. has excellent engineers and are doing a good job in coming back, but that the really vigorous competition comes from Siemens. It seems that before the war the Siemens plant was in Berlin. Because of the difficulty in operating in Berlin at the present time, Siemens have obtained permission and capital to build a plant in Western Germany. They are, however, also rebuilding their plant in Berlin. Siemens seem to be plunging very heavily in the way of capital investment, in almost a gambling spirit. I asked Dr. Boveri if he did not think this might lead to over-expansion and disastrous results. His comment was that there seemed to be a philosophy in Siemens that after you have perhaps 100,000 people in your employ the government cannot allow you to go bankrupt. The implication here is of course of existence of a rather close connection between big business in Germany and the government of Western Germany.

It is interesting to note in passing that I heard somewhat confirming comments in Sweden. The comment in Sweden was that it would probably be necessary in the next few years for the Swedes to reduce their standard of living, because the appearance of German competition could reasonably expect to take some of their foreign markets away from them. Thus both the Swiss and the Swedes have had very distinct commercial benefits since World War II from the weakness of the Germans.

I was much interested to find that Dr. Boveri was so much more interested in German competition than he was in British competition. I am sure the British competition is very real, but it seemed to be a standard thing that he was used to, and could anticipate the effects of, whereas the resurgent German competition was something whose effects were going to be very difficult to evaluate. Of course part of my feeling is that along with the resurgence of the German manufacturing will be resurgence of German needs for the products of manufacture; much of the new German manufacture will overall be absorbed in Germany. This may not occur in all individual establishments, but taken as a whole, there is bound to be a great deal of this.

1.2.6 <u>A new employee services building</u>. They have just completed a seven-story new building, not a small building, which I was told was devoted entirely to employee services. By this is meant cafeteria service, meeting rooms for professional and social groups, presumably an auditorium for talks both technical and non-technical, recreation rooms for off-duty recreation, and so forth.

Dr. Boveri's comment in regard to this building was that activities of this kind were necessary to maintain good employee relationships in Switzerland at the present time. Of course, I suppose one aspect of this provision is that it is a means of using the profits from their successful commercial activities for the past several years, since World War II, to give them some long term advantage in a more highly competitive market. I suspect that in various other ways they have been putting profits back into the business in order to strengthen their future position.

1.3 Philips Gloelampfabrikenen, National Research Laboratory, Eindhoven, Holland

Tuesday, 11 August 1953

1.3.1 <u>Personnel contacted</u>. The comments on general policies and European international television practices contained in this chapter were obtained from:

Dr. J. H. A. Kleynen, who was my host and has charge of their microwave research.

Dr. Bruining and his associates, interested in cathode ray tubes.

A man whose name I do not remember, interested in television equipment research.

1.3.2 The World War II situation at Eindhaven. Dr. Kleynen stated that during World War II the Eindhoven plant was operated under German management, who attempted of course to achieve results useful to the German war effort. The Dutch saw to it that many things went wrong, or results were found useless, or effort was wasted. At least once or twice some of the senior workers were shot in an effort to encourage the rest.

1.3.3 General objectives of research at Eindhoven. It appeared to me that the general objectives of vacuum tube and related research at Eindhoven were quite strongly oriented toward needs of the communications arts--radio, television, radar, aids to navigation, etc., telephone switching, telegraphy, etc., with but little attentions to industrial electronics and related problems. The contrast with Brown Boveri in this respect was very marked.

1.3.4 International television broadcasting agreements and practice in Europe. Prior to being shown the work on television tubes, I had an opportunity to discuss the general international pattern of television in Europe as seen by Philips today. I was shown a map of Europe with existing or prospective television stations marked on it. These markings are as agreed on at an international conference held in 1950. It was stated there is to be another international conference like this in 1954. The general plan that is being worked on in this conference, and in other ways, is to try to arrange matters so that the same program can be shown simultaneously everywhere within the "European area".

On the map there were of course no dots indicating television stations east of the Iron Curtain, although there was one in Berlin. The European area is described as including Holland, Belgium, Western Germany, Switzerland, and Italy, I cannot be sure about Spain. Notice the omission here of France, England, and Russia.

I was told that, as to television Europe is divided into four areas, as follows:

(a) The British area (the United Kingdom and Northern Ireland, do not know about Southern Ireland). In this area the television used is 441 lines per frame, 6 megacycles band-width, and a sound channel that is amplitude modulated.

(b) France. I was told that in France they use 819 lines per frame or thereabouts, with perhaps 9 megacycles bandwidth.

(c) Russia, about which I was given little information, except that they use the same number of lines per frame as the European area (I believe 625).

(d) The European area, including Holland, Belgium, Western Germany, Switzerland, and Italy, cannot be sure about Spain. I am afraid I missed recording the number of lines per frame used in the European area. However, it is, I believe, different from that used either in England or France, probably 625 lines per frame.

Throughout Europe, all television reception is done on a set licensing basis, as is done for radio in England. I was told that the British Parliament has recently, after prolonged debate, passed a bill which permits to some degree advertising support of television.

1.4 The Mullard Company, Mitchum, England.

Thursday, 20 August, 1953

1.4.1 Personnel contacted. The people dealt with at Mitchum were as follows:

F. M. Walker, about 50 years of age, the number 2 man in the engineering staff at the Mitchum plant where the tube reliability engineering development work is being done. The number one man was away at the day of my visit. Mr. Walker seemed fully competent to discuss the situation.

Dr. Roberts, in his early thirties, a Ph.D. in what we call engineering mechanics.

A man around 35 - 40 whose name I do not recall, from Mullard's London office, who picked me up at the hotel in the morning and accompanied me on visits to Mullard's Research Laboratory at Solfard and their reliable tube activity at Mitchum, and appeared well-informed on policy matters.

1.4.2 Position of the Mullard Company as a manufacturer of tubes. I was accompanied on the trip to Solfard and Mitchum by a representative of the Mullard Company headquarters office in London. He gave me considerable detail as to the general pattern of Mullard's activities. Some of the ideas presented at that time will be recorded here, partly as forming a background for the later conversation with Mr. Malleson on September 3rd.

I was told on August 20th en route to the plants about the experience of Mullard during the World War II. It seems that during World War II, because they had up to that time been a subsidiary of the Philips Plant at Eindhoven, and it was known that the Philips plant was in the hands of the Germans and being operated by German sympathizers, no clearances were given for doing military research or in any way involving Mullard in manufacturing which required using secret or confidential information. However, Mullard did carry on very extensive manufacturing of standard tubes, and for that matter of new tubes when it came to the point that they were so widely known and the question of security had been so completely compromised that there was no harm in anyone knowing what they were doing.

Therefore, during the war Mullard had no research laboratory. Of course a part of the history of this was that the research work, and in fact much of the engineering, had been done at Eindhoven.

Very early in the war, by government action, the financial tie between Mullard and Philips at Eindhoven was broken off; so that the management became entirely British. That put the Mullard people entirely on their own, engineering-wise. They found it entirely possible to move ahead to do a very excellent job of manufacturing. They were the largest producers (at least so they claim) of standard vacuum tubes in England during World War II.

I was also given some statistics as to Mullard's own statement about present volume of production of vacuum tubes by various concerns in England. This is factual information, obviously obtainable from other sources, so it is probably correct. They report that at the present time there are being made in England annually about 40,000,000 vacuum tubes, and that the percentages made by different producers are about as follows:

Mullard	60%
Mazda	15%
Standard Telephone and Cables	15%
Remainder	10%

The above includes the Edison Swan operations, I believe under the Mazda listing, these being made for British Thompson Houston and Metropolitan Vickers. There is some combination into a joint enterprise making Mazda tubes; this is not clear, as I do not understand the British commercial pattern. However, the important thing is that Mullard people state, probably correctly, that they make about 60% of the 40,000,000 vacuum tubes made in England per year.

I was also told that there is also about the same total tube manufacture on the Continent, of which Philips at Eindhoven makes about 50%, France about 25%, the rest being scattered. Thus this implies the total vacuum tube manufacture on the European Continent outside of Russia is about 40,000,000 tubes a year, 20,000,000 of these being made at Eindhoven, about 10,000,000 in France, and the rest scattered elsewhere.

The Mullard Company was successful during the war both commercially and in supporting the military, and in demonstrating their loyalty. Their separation from the Philips group at Eindhoven was made permanent after the war; Mr. Malleson said quite a bit more about this as reported below.

Some three or four years after the end of World War II the Mullard people were successful in making representations that they should be given clearances, and as a result there has been established, relatively recently, perhaps in 1951 or 1952, a research laboratory at Solfard.

This very strong commercial position of Mullard, as indicated by the above figures, together with their only relatively recent entry into classified work, explains to some extent why they have been very active in their pursuit of the "trustworthy valve" program.

1.5 Mullard Company

Discussion with Mr. Malleson, over the dinner table in London.

Thursday, 3 September, 1953

1.5.1 Personnel contacted. This section reports on a discussion with Mr. Malleson, who has charge of the division of Mullard that deals with military activities and with industrial activities, but does not handle commercial radio or television tubes.

1.5.2 Commercial comments by Malleson. As a result of the discussions with the man who was my guide and host on August 20th, I was called by telephone a week later and invited to have dinner with Mr. Malleson. As it worked out, I had had my dinner before he could come, but I joined him while he had dinner in a London restaurant, and we then discussed a great many things about British commercial policies in regard to vacuum tubes. As the ground covered was in some respects the same as on the trips from London to Mitchum and Solford previously, it is little uncertain in some cases which of the comments that I record here were said by Malleson, and which were said by my guide on that day, who worked for Mr. Malleson.

However, they all stem originally from the same general source. On the evening occasion in London, Mr. Malleson did most of the talking, and seemed very much interested in conveying to me his points of view.

Mr. Malleson stated to me that he has the responsibility for all Mullard valves and tubes used outside of the entertainment field. This includes gas tubes, and all tubes for government equipment. Thus the reliable valve engineering is part of his responsibility.

Mr. Malleson feels that Mullard has made important contributions toward encouraging European standaridization on 7 pin and 9 pin sockets, of U. S. design. I asked him to tell me what are the reasons why England, and to a large extent the European continental countries also, have standardized on American bases and ratings, particularly in miniature tubes. He mentioned two of the important reasons, as follows:

(a) When American-made electronic products are sold outside the dollar area, people want to be able to buy replacement tubes without spending dollars for them, also

(b) it is important wherever possible to sell English-made equipment in dollar areas, so as to receive payment for them in dollars. This is of course easier to do if the English-made equipment employs U. S. standards of bases and ratings, because then the customers who are in the dollar area can replace their tubes with U. S. tubes bought at stores in the dollar area.

In addition to these definitely and immediately obvious commercial advantages there are other less tangible ones, having to do with the basic mass production advantage of having standard tube types throughout England and the Continent. It was fully as easy for the various British manufacturers to get together on U. S. standards as on anyone of their own. Actually, my own estimate of the situation is that it was much easier for them to get together on a U.S. standard than anyone of their own, because then none of them had to admit superiority on anyone of the others among themselves. I think, but am not quite sure, that Mr. Malleson said that after the war the British tried to standardize on a basis, ratings, etc., of their own, but were not successful. He confirmed my information, dating from my previous visit in the winter of 1944-1945 to the United Kingdom, that before the war there were many different bases in England. Thus there was a Philip's base (was this a side contact?), S. T. and C. base, a Mazda base, etc.

At this point he made an interesting comment which was that "Standard Telephone and Cables" (the I. T. and T. subsidiary) had been largely managed from the United States. This is an interesting slant coming from an Englishman.

I asked Mr. Malleson some questions regarding the relationship of various tube manufacturers in England to the British Valve Association (BVA). His reply was that in setting prices, he is not beholden to BVA. Thus he can make a low price as he sees fit. He described a case in which hosiery knitting machines were imported from the United States (I judge a good many of them came in) and the control on each one used two xenon thyratrons. He put his engineers at work immediately trying to build a xenon replacement tube to the same specifications, but the job was difficult and slow. A competitor in England jumped into the same situation, made mercury tube for replacement, and sold the mercury tube plus a modification kit to make them mercury tube serve satisfactorily. Then Mullard followed the competitor's example and made some mercury tubes too. The end result is that some of the replacements are xenon made by Mullard, and some are mercury made by either Mullard or the competitor.

In selling these tubes to the ultimate users, he establishes a price to them that is lower than the retail price, but not as low as the sale price to the equipment manufacturer. I believe in this respect he is following U.S. practice. The BVA practice would be to say the textile outfit is the user and should pay the list price, as sold in a retail store. In this respect Mullard does not follow the BVA practice in dealing with these industrial users.

Malleson criticizes the BVA, expressing the opinion that they should spend their time and efforts establishing standards, rather than spending their time and efforts establishing list prices, discounts, marketing procedures, etc.

Information given me by others, and useful in connection with these comments by Mallesons is that there is a much greater spread between tube retail prices to set manufacturers in England than in the United States. Presumably this grew up in the days when there was no standardization and therefore no competition for the replacement market. In those days, prior to World War II, because tubes were not standardized, the user of a particular kind of equipment was compelled to go to an outlet of the manufacturer of that equipment to buy replacement tubes.

Malleson stated that they have a patent pool arrangement with RCA and others in the United States, for non-exclusive licensing arrangements. They (Mullard) have frequent visits by engineers from Sylvania, and other U.S. companies. He says he is always very glad to have anyone in the United States adopt one of his tube designs. For example, on a certain precision voltage reference tube, RCA have taken on one of his designs. He gets no specific royalties on it, but is glad to have it happen.

I tried to find out why this pleases him, without too much success. One of his reasons is an increase in prestige, both for Mullard and for England. However, I am sure there is another more tangible reason. Probably partly it strengthens their position in the patent pool, and improves the contact with RCA generally.

He would like to see come into being an extensive between-competitors ("cross") sale of parts that go into the making of tubes in England, just as takes place in the United States. Thus, he would like to see the various manufacturers in England buy and sell among themselves bases, pins, bulbs, etc.

He had after the war a subminiature tube contract which was later canceled but had in the meanwhile enabled him to establish quite a valuable manufacturing facility. He told me that after the second world war Mullard begged, bought, borrowed, almost stole all kinds of modern tube manufacturing machinery (I wonder here whether he was thinking particularly of his own department).

Malleson himself was in the Navy during World War II, I believe a Commander in the Navy, as he is sometimes called Commander by his associates. He was engaged in counter-measure work in the Pacific area.

He told me that prior to World War II Mullard was definitely controlled by Dutch ownership. During the war this link was cut off completely, but the Mullard management and engineering group carried on very successfully during the war in spite of being cut off from the Dutch. He implied, it seemed to me, that much of the tube design engineering had been done in Holland before the war, the Mullard contribution being chiefly factory and production engineering at that time. But during the war the British group did the design work also.

Malleson stated to me that after the war the Eindhoven people wanted to get back on the old basis of control, but the British management group did not want to and have kept the operation financially separate. Thus they have a complete patent licensing agreement with Eindhoven, but no financial control arrangements that can put Eindhoven in a dominant position. Before the war Malleson used to go to Holland four or five times a year, now only about once a year.

Since returning to the United States, I have had some informal conversations with people here in regard to whether a similar situation exists relative to the Philips Laboratories in this country. Certainly the Philips Laboratory authorities have expressed to me the statement that Philips in the United States is completely American-owned, and that the primary contact with the Dutch is a patent licensing arrangement which makes it profitable for the United States group to promote the use in the United States of Dutch patents, and vice versa. Certainly the Philips organization in the U. S. is a successful concern as far as their research and development laboratories are concerned. However, there are indications that, perhaps for intangible reasons rather than tangible ones, in spirit at least if not because of financial arrangements, the Philips organization in this country is to a considerable extent dominated by the "Parent" organization in Eindhoven.

Apparently Mr. Malleson was trying to get across to me the point of view that this is not true of the British Mullard organization, that is, the British Mullard organization is genuinely dominated by British interests, and uses now the Dutch tie merely as a matter of scientific and technical convenience, and not because of any degree of financial control, or of intangible "parental" type of control from Holland. Note that I felt that he was very much interested in my having this point of view regarding the relationship between Dutch interest in Holland and the Mullard management.

I have asked myself this question: "Why did Mr. Malleson find it worthwhile to spend the evening with me telling me these things?" He seemed to enjoy the evening very much, and felt that it had been worthwhile to him. Why was this? Did it have to do with strengthening Mullard's position with the military, or in the United States?

I asked Mr. Malleson whether he expected that Mullard would make any financial profit in the manufacture of "trustworthy valves" or reliable valves. His reply was that experience had shown that in a new program like this one in the first year of manufacture they make little or no money, or lose a little. However, the second year they make a little money, and in the third year they are on a sound business basis. He evidently hopes that reliable valve manufacturing will ultimately work out to a point where all commercial valves are much more reliable than at present.

1.6 British Thomson-Houston Company, Research Laboratory, Rugby, England

Monday, 7 September 1953

1.6.1 Personnel contacted. My contacts were in the research laboratory; however, I found a very close relationship between research and engineering development. This relationship is much closer at Rugby than at the General Electric Company at Schenectady.

During the second day of my visit there was present at lunch a member of the Board of Directors who seemed to be much interested in the general policies of the relationship of research to engineering development and to the financing of research.

Various people contacted at Rugby were as follows:

Dr. Davis, director of research.

Dr. Wilkinson, deputy director of research, and directly responsible for arranging my visit.

A member of the Board of Directors of the British Thomson-Houston Company particularly interested in the electronics work.

Mr. H. de Knight, closely connected with the industrial electronics activities.

Mr. Rushworth, connected with the magnetron work as far as the experimental laboratory and magnetron development activities are concerned. I had met Mr. Rushworth in January 1945 at a similar visit to Rugby.

Dr. Dunsmuir, the man active in magnetron theory.

Mr. Scott, on TR switches.

1.6.2 Policies regarding research finances. At lunch the second day there was a lengthy discussion of the relationship of the financing of vacuum tube research to equipment engineering research and development. I found myself the center of the conversation, and found that I was in fact pleading a case of apparently great importance to Dr. Davis and Dr. Wilkinson, who are responsible for the direction of the laboratory. I found myself in the position of pleading this case into the ears of a member of the Board of Directors who had been brought there for that purpose.

I am going to state what the case was, the reason for stating it here being that the responsible directors of research at Rugby felt it was a very important point of view, and represented their own opinions. Therefore, though I shall state it in my language, I believe it is fair to say it represented their point of view also.

It appears inevitable that if adequate vacuum tube research is to be done for military and industrial objectives, the financial support of it must come largely from budgets for equipment development. The point is that in a great many cases a major new equipment, or a major improvement in existing equipment, is entirely dependent on the research and development leading to an improved or completely new vacuum tube. Yet the actual cost in dollars of the manufactured vacuum tubes used is a small proportion of the total cost of the equipment. Therefore, if the development and research costs for the bringing out of the new vacuum tube is carried as overhead on the vacuum tube itself, it makes the cost of the vacuum tube appear to be completely out of proportion to its actual manufacturing cost. Thus in general, in terms of a general commercial market, it would be impossible to market and sell this tube for other than the specific application, if it were loaded with this development cost.

Yet the reason for spreading the development cost is not for the general market, but for the specific market of making it possible to produce an equipment that is a major improvement over previous models, or perhaps an entirely new equipment. Thus the cost of research and development of the new vacuum tube should be considered as a part of the development of the entire equipment, and should come out of funds appropriated to equipment development. Thus in general the cost of vacuum tube research and development will represent a much larger share of the total engineering cost, than the cost of manufacture of the tube will be of the total equipment manufacturing cost.

Proper research financing must take this financial factor into account.

CHAPTER II

Reliable Tubes ("Trustworthy Valves")

2.0 Nature of Material

In this chapter there are reported, arranged according to place and date, various comments regarding engineering development of reliable vacuum tubes, of generally conventional design plan, as made to use in various research and engineering development laboratories in Sweden and in England during the summer of 1953.

2.1 Introduction

2.1.1 Objectives of this chapter. Because of the importance that has been attached in recent years in the United States to reliability in operation of electronic equipment generally, and particularly of tubes, in the correspondence preliminary to my trip overseas, I indicated one of my interests was in tube reliability. Actually I did not pay attention to reliable tube problems in Paris or Eindhoven, because the people that I dealt with were not involved in these problems. However, because of my requests of the kind mentioned, I did in England have an opportunity to discuss the question of what they call "trustworthy valves" in a number of establishments, and with both industrial and government people. Also, I had some opportunity to observe and comment on this matter in Stockholm.

Generally speaking, in England the trustworthy valve activity has been rather carefully directed, at least as far as the government sponsorship of the work is concerned, by the committee on valve development (CVD), which in England is backed up by government financial support. I am not implying that this is either good or bad, merely that it is an obvious fact of the situation there. Some of the people were very enthusiastic about the progress they had already made in improving reliability, with promise of more improvements to come.

2.2 A. B. Svenska Electronenror, Stockholm 20, Sweden

Tuesday, 18 August 1953

2.2.1 Personnel contacted. I met quite a number of people at this plant whose names I do not recall. However, the men responsible for guiding me around and for carrying out the conversation in English with me was:

Sigurd, Tomner. An engineer having apparently considerable responsibility, who had been at the Electron Tube Research Conference in Stanford in June 1953.

2.2.2 Reliability aspects of close-space triode and related work. I observed the manufacture in production, relatively small scale production, as this is a relatively small company, of close spaced triodes. In this work and wherever necessary in other kinds of manufacture, they carry on a great deal of what we call quality control on every tube. Thus they have shadowgraph instruments to check cathode and grid locations, and adjust capacitances to lie within specified tolerances by twisting side rods while the tube structure is in a capacitance bridge before assembly.

In their routine production operation one girl assembles each tube completely.

I asked whether they thought they could sell the tubes I saw them making, the 2051, 403, and 404 in the United States, either on a quality or price basis. The reply was they thought they could compete in the United States on both quality and price basis.

Their engineer in charge of tube productions said they could influence life to an important degree by changes in details of grid design. There was some language difficulty between us, but I gathered that he meant that misplacement or nonuniformity of grid wires could result in local cathode areas carrying more than their share of current density and therefore providing more than their share of transconductance. Such portions of cathode would fail early in life, representing a deterioration amounting to end of life.

The question arises, did he mean also that this local deterioration tended to poison the rest of the cathode?

He seemed very definite in his opinion that their experience indicated without question that this particular aspect of grid design and grid manufacture could have an important effect on tube life.

2.2 The Mullard Company, Mitcham, England

Thursday, 20 August, 1953

2.2.1 Personnel contacted. Men contacted were, primarily:

Mr. F. M. Walker, around 50 years of age, the number 2 man in the engineering staff at the plant where the tube reliability work is being done. The number 1 man was not in town on the day of my visit. However, Mr. Walker seemed fully competent to discuss the situation.

Dr. Roberts, a man in his early thirties, a Ph. D. in what we call engineering mechanics, whose job has been the scientific and analytical study of the problem of tube reliability. He seemed to me to be extremely competent, but I have come to question whether he has adequate authority, or rather whether his word is sufficiently well taken in the plant; however, Walker seemed to have a high regard for him.

2.2.2 General comments on the Mullard program on tube reliability. The Mullard Company seems to have very extensive plant facilities devoted exclusively to pilot production and small volume production of reliable tubes, (they call them trustworthy valves) and to be devoting a very considerable amount of engineering attention to this problem. They are getting substantial financial support from the CVD for this work, and the direction it takes appears to be strongly influenced by the C.VD. However, I gathered the feeling that they are also putting a fair amount of their own money into this activity, although I was not told this in so many words.

Later in my visits in England someone, I think Mr. Crampton at ASRE (Admiralty Signals Research Establishment) commented that Mullard had taken the lead in the reliable valve program. Perhaps one reason there is so much similarity in ideas as to the Mullard trustworthy valve work among various men in Standards Telephone and Cables, the General Electric Company Ltd., and Mullard is that the others followed along after Mullard. Another reason might be that the fact that all received some guidance and funds from the CVD might tend toward a similarity in approach, because there is obviously a very free exchange of information among them.

They first put primary emphasis on the study of what they called catastrophic failures, next on study of failures due to vibrations. This word catastrophic failure is used in this plant and in other places in England to describe failures, usually but not always occurring relatively early in life, which occur abruptly. Thus they call heater short circuits, glass fracture, and similar things catastrophic failures. Most if not all shelf failures are catastrophic failures.

The attention in my discussions with them was limited to miniature tubes.

They came early to the conclusion that the major causes of failures are traceable to wear of the mica. This wearing of the mica releases water into the tube as the mica flakes away, causing short circuits and changes in characteristics.

Thus in regard to causes of catastrophic failures (those not associated with gradual loss of tube function) they found that causes traceable to mica wear were perhaps five times as important as all other causes put together. I think this can be said to be the outstanding factor in our discussion, and one that they kept coming back to. There was discussion in this connection of the probability that mica wear and the release of moisture vapor into the tube resulted in a certain amount of cathode deterioration. Of course this would not cause catastrophic failure.

There was considerable discussion of causes of heater failures. A tabulated cause of heater failures is as follows:

(a) Failures due to poor welds of the heaters.

(b) Failures due to brittle heaters, that is the crystal grains being too large in the filaments of the heaters.

(c) Failures due to vibration fatigue.

2.2.3 Studies of glass failure. They have also studied failures due to fracture of the glass. One aspect of the properties of materials which they employ in their thinking is the curve in Fig. 2.2.3-1 which describes experimental results obtained by a man named Preston (I believe he is an American) and reported some time ago, on the time required for failure of a glass rod under stress. Notice that the time of failure is plotted logarithmically from 0.01 second to 1000 seconds after application of stress. Also, the data shows a range of times, no doubt because the material is not identical from sample to sample. However Preston gave them a basis for thinking in terms of a systematic dependence of time to failure on stress.

While Fig. 2.2.3-1a describes the typical time to failure for given stresses, Fig. 2.2.3-1b presents a distribution curve for the most dangerous stresses in electron tubes as

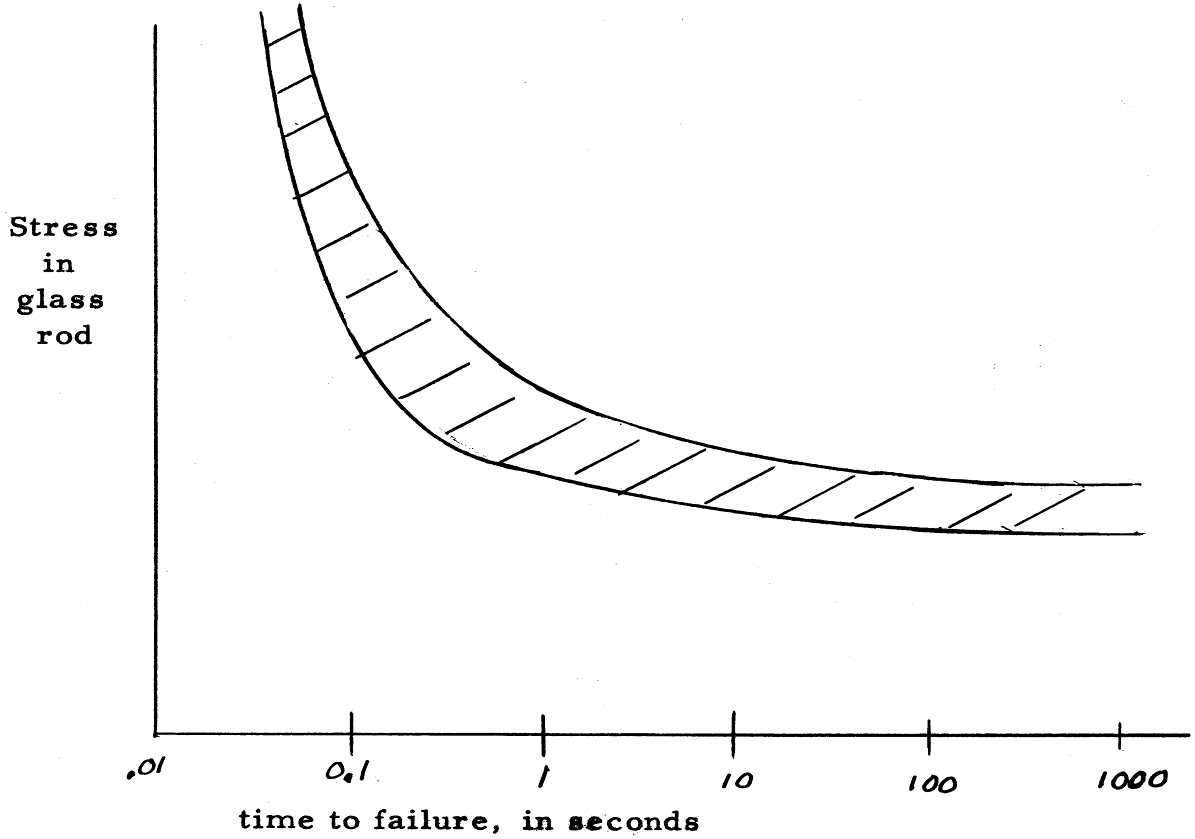
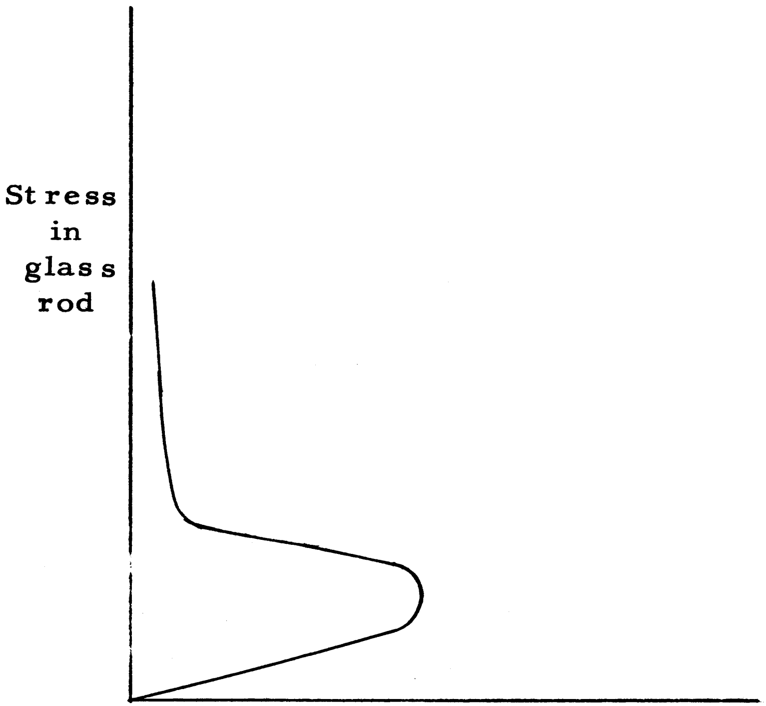


Fig. 2.2.3-1a. Dependence of time to failure on stress in glass



distribution of most dangerous glass stresses in electron tubes.

Fig. 2.2.3-1b.

currently built. The vertical scale is the same in the two figures. Thus it is obvious, as of course must be true for any successful engineering apparatus, that the most commonly occurring glass stresses are in the range in which time to failure is quite long.

However, the concept involved here implies that either due to the existence of an upward extending tail of the distribution curve in the short-time failure stress region, or an infrequent early failure for the commonly occuring stresses, that there should be some early catastrophic failures and some shelf life failures due to glass breakage. Fig. 2.2.2-2 is a curve described by the people at Mullard giving their expectations based on some actual factory, shelf, and field experience as to how to predict field failures. Perhaps more important than this, it shows the contrast between what they call their Phase 1 behavior and their Phase 2 behavior. While I am not completely clear on this matter, as I recall the Phase 1 curve describes the glass failure behavior in Phase 1 of their design attack on the problem, whereas the Phase 2 curve describes the behavior in the later attack. That is, the Phase 2 curve shows very distinct improvement, particularly as to expectation in the field. They were careful to point out to me their great interest in the observed appearance of the straight-line shape of the Phase 1 curve on which they had actual field experience, as well as shelf and factory experience.

They also emphasized that this straight-line relationship applies only if the stress is the same in the factory, on the shelf, and in the field. For example, presumably the measurement of time begins when the tube is put in the socket in the factory, for tests, etc., so that it is a valid comparison only if during shelf life the tube is also in the same kind of a socket as would be tested in the factory and during field use. It may appear that this kind of a comparison is of no great practical use, but it certainly is of interest in comparing different designs as to what to expect of them in the field.

I presume that the reason they do not have the third point, or did not draw one for me, on Phase 2 was that not enough time has elapsed to give this point. They are guessing at it by extrapolation on a straight-line basis.

One of the matters that they have given detailed attention to has been the seal where the leads go through from the outside to the inside of the tube. Fig. 2.2.3-3 illustrates certain aspects of this. At point P down in the lower left corner is shown a suggestion as to one place where stresses can occur as a result of the sealing on of the base of the tube to the envelope. There may or may not be a crevice at this point, and the stresses will be quite different, depending on whether there is a crevice or not.

They called my attention particularly to the way in which they use a Dumet seal between two nickel sections, the emphasis being on the detail geometry of the seal that they now employ. I presume, although I do not remember the specific comment, that they attribute some of the improvement from Phase 1 to Phase 2 to some of the details in the design of this seal. The really important thing, however, is that they have given very careful attention to a study of the relationship between details of the seal design and the end-of-life as a result of failure of the glass.

This whole story is fairly typical of the way in which Dr. Roberts and his associates, together with the factory people, have gone at the various problems of failure.

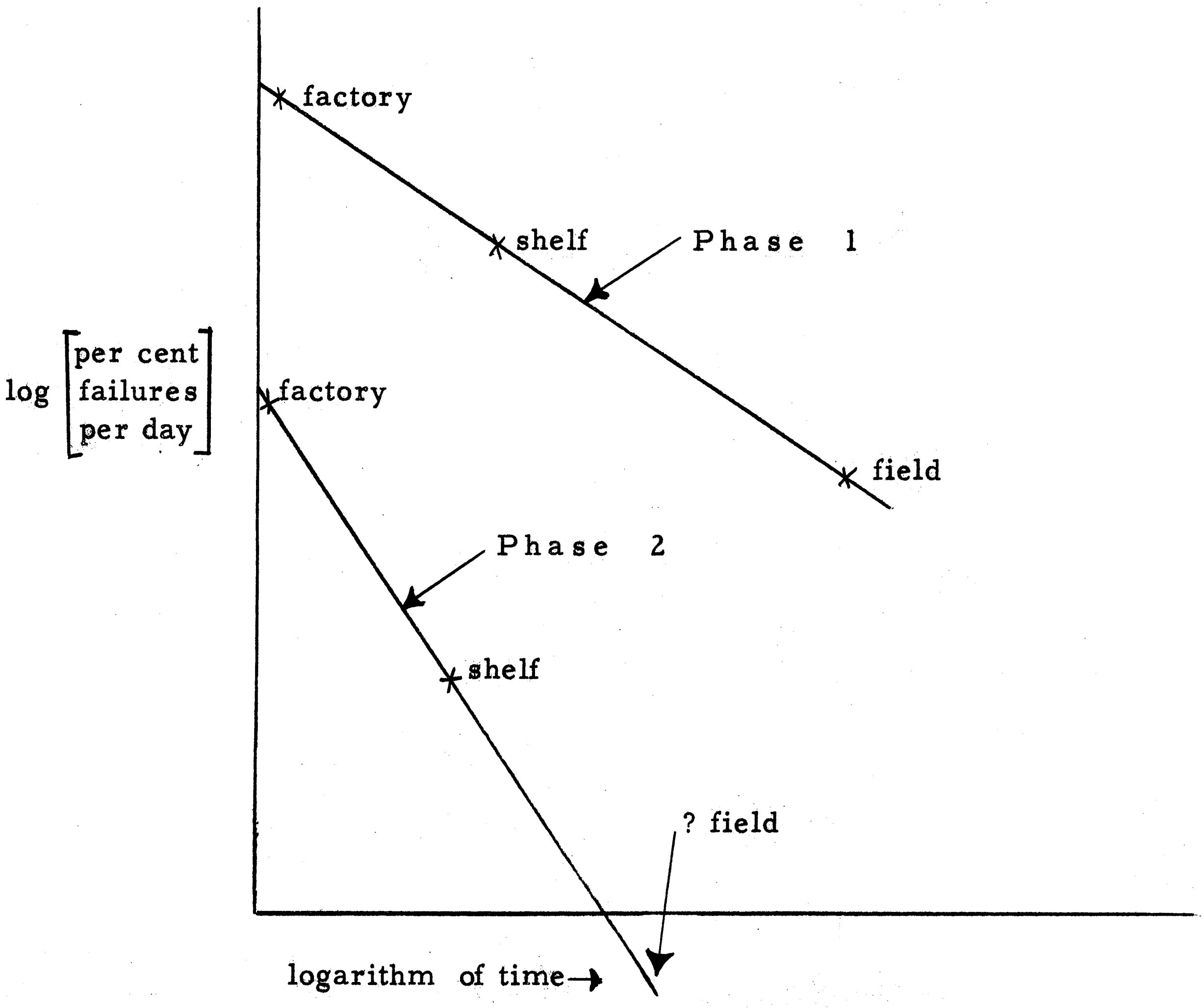


Fig. 2.2.3-2 Straight-line dependence of glass failure rate, providing stresses are kept the same (i.e. in the socket throughout, or out of it throughout).

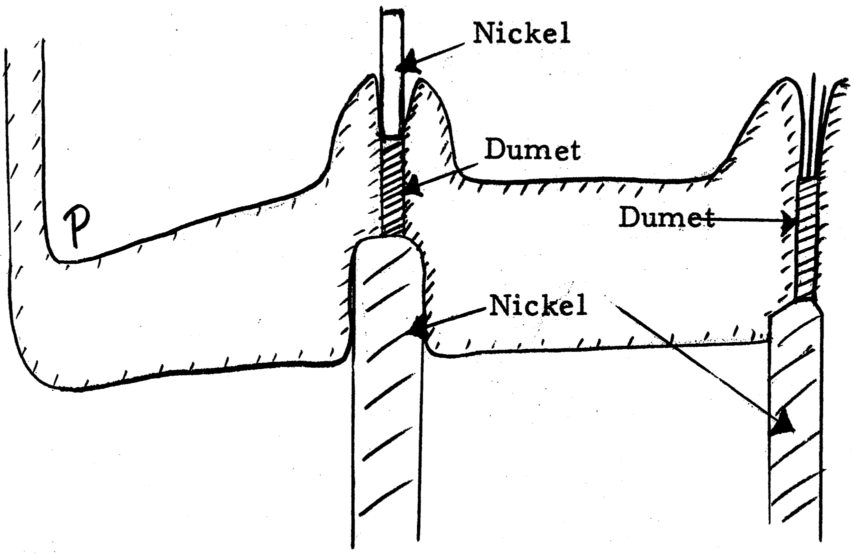


Fig. 2.2.3-3 Detail of Dumet seal.

2.2.4 Failures traceable to mica. The question of failures traceable to mic was considered very important at all the laboratories in which I discussed "trustworthy valves". I am a little confused as to what was said where, although I tried to make reasonably careful sequential notes. A somewhat more detailed set of comments on this question appears in the later discussion of the trustworthy valve program at the Brimar Valve Works of Standard Telephone and Cables.

However, I think everyone, including the Mullard people at Mitchum, agreed that in one way or another, failure of the mica support of the electrode structure was responsible for many failures. Inasmuch as this story was pretty much the same at Standard Telephone and Cables, and at the Mullard plant at Mitchum, the discussion of this aspect of the matter is largely postponed to the comments made at the Brimar Valve Works.

There was however, some discussion at Mitchum of the point of view that gases first released by the wearing away of mica could poison the cathode. This led to a little discussion of the matter of how cathodes are poisoned. The point of view advanced here was that the poisoning of a cathode is sort of a two-way operation, that is, there can be during useful life not only poisoning of the cathode, but also recovery of the cathode. Thus in general, during the life of a tube, it is to be expected that there are at all times some influences tending to cause poisoning, and some at work tending to cause recovery, which might be called reactivation.

The actual net result depends on which of these two processes occurs the most rapidly.

Thus if the rate of poisoning is greater than the rate of recovery the cathode will gradually deteriorate. However, if the rate of recovery is for any given period greater than the rate of poisoning, the tube will tend to come back to a stable state, which represents the maximum obtainable in the general environment and design and processing habits of the particular tube.

2.3 Standard Telephones and Cables, Brimar Valve Works, Footscray, England.

Monday, 31 August 1953

2.3.1 Personnel contacted. The important subject matter at Footscray had to do with the reliable tube activity. They are very proud of this work. [2]Most of my contact was with the chief engineer, Mr. E. G. Rowe, but I had lunch with Mr. Spagnaletti, the works manager. However, there was casual conversation with various other men whose names I did not record. Thus the significant names are:

Mr. E. G. Rowe, Chief Engineer of the Brimar Valve Works, who has close contact with U. S. practice, and has attended meetings of JETEC in the U. S.

Mr. Spagnaletti, Manager of the Brimar Valve Works, a relatively young man having a great deal of drive and competence.

2.3.2 Results of their reliable valve program. They have what they call a Phase I reliability program; this program and its results are unclassified. They feel that they have for practical purposes eliminated accidental failure and mica poisoning, and have left only the cathode problem. Mr. Rowe commented that engineers must have been pretty dense not to have eliminated the accidental failures long ago. He told me that on certain "reliable" tubes they have achieved a record of 500 hours of life test without failure of any tubes.

Mr. Rowe is extremely proud of their reliable valve accomplishments. I made a couple of inquiries elsewhere as to whether other people felt his pride was justified. The comment in reply was to the general effect that although perhaps Mr. Rowe makes somewhat stronger claims than the actual accomplishments warrant, he does have a great deal to be proud of.

He made the comment that their manufacturing problem is made difficult by the fact that their manufacturing volume is moderate yet the number of types made quite large. Thus RCA in the U.S. makes 135,000,000 tubes a year, comprising a relatively small number of types, whereas S. T. and C. produces 6,000,000 a year involving a relatively large number of types.

Mr. Rowe commented that he attributes a considerable part of their success to his policy of having mathematicians and scientifically-minded engineers working in the same room with production engineers. He believes that the mutual interchange of ideas that is thus made easy is helpful to both.

2.3.3 Testing and quality control of reliable valves. Mr. Rowe described to me how they test production-run reliable tubes on a control test rack. During a production run they will, out of each 7 days or 5000 tubes, whichever is the shorter time interval, take 48 sample tubes for the control rack test. These 48 tubes will be vibrations tested at 170 cycles per second at between 4 and 5 g's. The filaments are turned on and off at intervals of a few minutes for quite a few hours in each of three orientations. Then complete electrical tests are made. If more than 2 of the 48 fail, they take another 48 from the same batch. If more than two of the second 48 fail, the whole 5000 or 7-day lot is rejected.

This frequency scanning shake test machine, used in research and development work, was at the Radio Show in London on the day of my visit. I saw a picture of it; it looked just like the one I had seen at Mullard's a few days earlier. They had also at Footscray, for development studies, a 170-cycle, 4 g shake test machine like the quality control one mentioned above, except that they could apply voltages and measure currents while the tube was being shaken.

At one point in the reliable tube production line they have a device which shakes the tubes at 50 cycles and displays the resulting microphonics on an oscilloscope. More than a certain reading causes a rejection. I believe Mr. Rowe said that all reliable tubes have to pass this test.

Mr. Rowe stated that he thinks that a well-made tube needs only a few hours of test, maybe 3 hours. He thinks the 48-hour run-in test (required by JAN-1 specifications) is absurd. He seems to think this is merely a way of discovering what tubes should never have been finished anyway--they should have been caught in quality control tests.

On reliable tubes they give all heaters an over-voltage test.

2.3.4 Analysis and test of structural and vibration properties. Mr. Rowe made it very clear early in the discussion that they use in their engineering staff engineers and mathematicians who calculate from the laws of mechanics and a knowledge of structural properties of materials what the resonance properties of various elements in a tube should be; then they measure these resonances, and obtain a very good check. The resonances they calculate and measure include the resonances of grid side rods and of laterals for each grid. Fig. 2.3.4-1 illustrates the type of graph they obtain, indicating relative amplitudes of various resonance vibrations, for a given type of excitation. He mentioned that they about a year before my visit sent to TRE, with a request for release for publication, a manuscript describing this work, but had as yet had no reply. He showed me the manuscript he said he had sent them; it looked interesting.

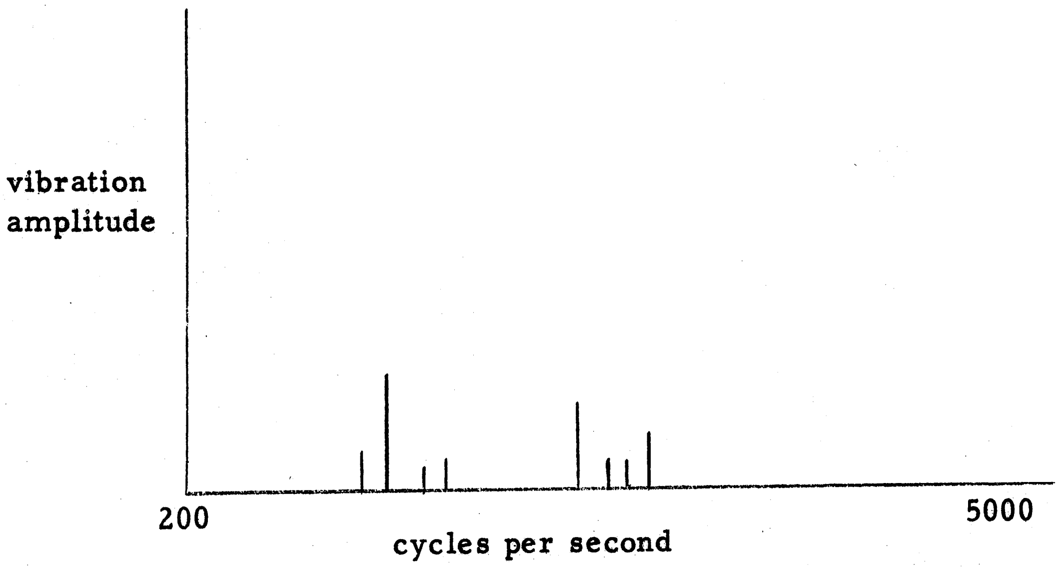


Fig. 2.3.4-1 Spectrum of resonances in vacuum tubes.

2.3.5 Glass. They have their own glass evaluation laboratory; the instrumentations include thermal expansion testing equipment. They believe there should be used for the tube envelope a glass with a slightly greater expansion coefficient than the button glass has, so that the envelope will grip the button. They have for the Brimar plant two sources of glass, the British Thomson-Houston Co. and the General Electric Co., Ltd.

2.3.6 Nickel. Mr. Rowe told me that they have never had as much silicon in their nickel as is present in most of the nickel used in the U.S. They have other impurities instead. They believe that it is because of this difference that they have never had quite the degree of sleeping sickness trouble that U. S. manufacturers have had. They are making interface studies, but feel that the interface resistance layer is more important in the U.S. than in the United Kingdom because of the difference in the nickel used.

2.3.7 Loose mica. They believe that looseness of electrodes in the micas has been an important cause of trouble. They stated "mica dust poisons the cathode." I commented that perhaps it was gas and moisture entrapped in the mica and released as the mica wears away that does the poisoning. The only reply was a nod of the head; what this meant I do not know.

As to the cure of looseness of the mica, they place emphasis on clamping tabs, rather than attempting to depend on tightness of fit. Cathodes are fastened (at one end only, the other end left free for expansion) in the way shown in Fig. 2.3.7-1. Note that in this figure there is a ring around one face of the mica, and two clamping tabs rather than a ring at the other face.

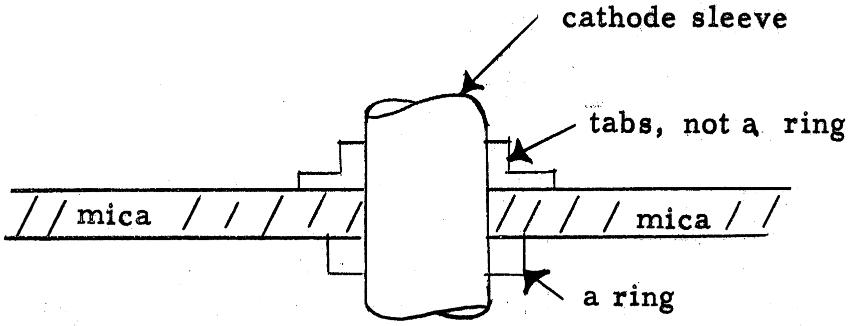


Fig. 2.3.7-1 Rings and tabs fastening cathode to mica

2.3.8 Heater connection welds. They talked about a sort of nickel bus bar arrangement for supplying the electrical connections to the heater. The idea is that a tungsten-to-nickel weld is made where there is firmness and accessibility, so that the weld can be surely a good one. That perhaps requires a nickel-to-nickel weld elsewhere, but that is not so critical. This was done undoubtedly to minimize breakage of this weld in vibrational service. This nickel bar arrangement is illustrated by the sketch in Fig. 2.3.8-1.

2.3.9 Lint. They are very conscious of the problem of the effects of lint in vacuum tubes. To avoid lint they have the girls assembling the tubes work in an air-conditioned room, and have them look at the operations they are doing with their hands through a pane of glass. In the early models of these devices, in the frames to hold glass, the back, top, and plate was of lucite. This lucite became electrified and attracted lint, so they changed to metal.

The next step, now being taken, is to have a jet flow of air outward from the work space into the room space. The velocity of this flow has to be about right; if the flow is too fast it causes turbulence which is worse than no air flow. If the velocity of air flow is too slow it is not effective.

2.3.10 Heater filaments. They use on reliable tubes helix heaters, although on some other tubes they use folded filaments. On reliable tubes they give all heaters an over-voltage test.

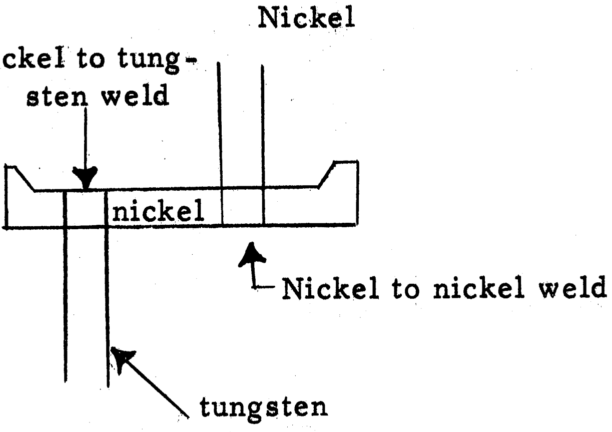


Fig. 2.3.8-1 Addition of a nickel bar to make tungsten to nickel weld at a more accessible location

2.3.11 Methods of assembling reliable valves. They showed me three different ways of assembling tubes, as follows:

(a) A little jig is used into which the parts are inserted, then micas are slipped over this jig. This procedure is the same as is common in the United States. It is used for standard commercial tubes.

(b) There is used a jig comprising the same thing as used in the (a) procedure, plus some metal leaves that pivot on a post and are cut out to accept only good parts (cathodes, etc.).

(c) There is used a jig with wooden-handled levers that forces the rods, grids, etc., into correct alignment, and exercises a clamping action. This was designed as a part of the reliable tube program. It has been found that girls assemble tubes in this jig just as rapidly as in an older (a) method, and with greater dependability. In due course they expect to make all tubes using this (c) method.

This last described device is an excellent example of a mechanical aid to skill that does not run into a lot of money, does not require a large production volume to justify getting or making it, yet appreciably facilitates production as well as improving dependability.

It looks to me as though it should be possible to devise an automatic feed to this machine thus doing away with the assembly girl almost altogether.

On the stem-making machine they had a girl putting on the glass cylinders. However, she was in reality monitoring the operation of the machine, not really doing much herself.

2.4 Standard Telephone and Cables, Inc., Ilminister, England

Tuesday, 1 September, 1953

2.4.1 Personnel contacted. Not very much work is done on tube reliability at Ilminister, however, a few comments of interest were made. The people at Ilminister commented that they feel Mr. Rowe at the STC Brymer Valve works is doing an excellent job, but is perhaps a little over-enthusiastic as to his accomplishments. The people involved in the tube reliability at Ilminister are:

Mr. Ullrich, acting director.

Mr. Foulkes, responsible for power tubes, gas tubes, and reliability.

2.4.2 Reliability of tubes. The Ilminister objective in tube reliability is to develop tubes having long life, that is, many years of life, under telephone service conditions which do not usually involve vibration.

Yet they do subject their tubes to vibration tests. In their development work they use shock test, applied maybe five times a second, rather than a vibration test. Being a shock test (moderately large g's, perhaps as much as 30) this test excites all resonances in the tube from 5 cycles per second to 1200 cycles per second. It has been their experience that any tube that stands up for half an hour in this test will pass the standard frequency test. However, they treat their shock test as a destructive test; thus they test only samples, and discard the samples after the test.

Their cathode leads are ribbon-like in sections, each welded to a cathode of rectangular shape; then on the way out the ribbon is lashed through two hooks in the cathode. Two such cathode leads may be employed.

The attachment of the heater lead to the tungsten filament of the heaters is made as in Fig. 2.4.2-1. This has no weld; the tungsten is slipped into a hollow nickel tube, then the tube is pressed flat, and with a ridge in it, as shown in the figure.

The heater is a single helix formed into an M shape, as shown in Fig. 2.4.2-2.

They showed me a tube in which the leads from the grid to the button posts were not spot welded, but made using a sort of silver soldered joint, using a pin-point arc-weld type of heating. It is evident that they go to great pains to eliminate entirely the possibility of failure from the breakage of arc welds.

They have the pentode on a life test rack that has been there for eight years. They were once asked to develop a tube for a DC amplifier that would be stable to the extent that slow changes (drift) would not be greater than corresponding to a few millivolts of grid voltage. They did many things, finally found roughly that a change of 10 millivolts of heater voltage would produce a change equivalent to one millivolt of grid voltage. This meant that their bogey required that the filament temperature be held constant to within 1°C, which is not practical, even if the ambient temperature is held that close. It was difficult to get batteries stable enough for filament supply, to study the problem.

2.5 The General Electric Company, Ltd., Research Laboratory.

Wembly and India Pavilion, near London. Wednesday, 3 September 1953

2.5.1 Personnel contacted. I do not precisely recall whether the work on reliable tubes was being done at Wembly or at India Pavilion, nor do I remember clearly whether or not it strictly

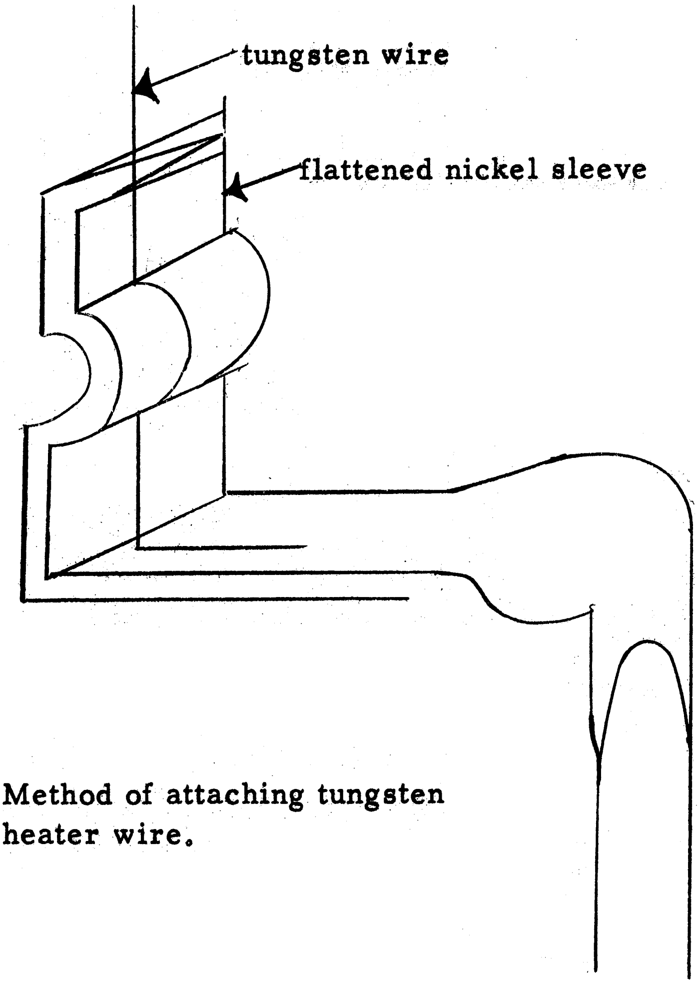


Fig. 2.4.2-1 Method of attaching tungsten heater wire.

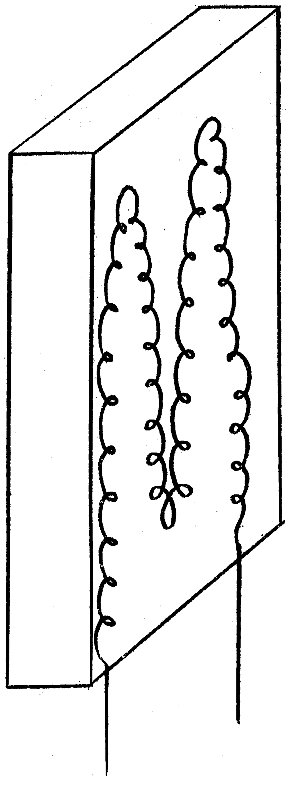


Fig. 2.4.2-2 Heater filament consisting of a single helix formed into an M shape.

or only partially is a research laboratory activity. However, I will give the following names here, some of these being men whose relationship to this particular problem I am not too sure of.

Dr. Ryder, Chief Scientist. (physics training)

Mr. Bell, in charge of all communication type research, including and chiefly valves. He was definitely involved in the reliable tube test program.

Mr. Jenkins, the man directly concerned with and responsible for the reliable tube program.

2.5.2 Trouble due to electron bombardment of glass. In their studies of tube reliability they have found cases where they believe there was cathode trouble caused ultimately by electrons bombarding the glass. They try wherever possible to avoid having electrons bombard the glass, in any tube design.

They have had cases in which tubes in which electrons bombarded the glass performed very well, until tubes appeared in which a new batch of glass had been used. The tubes with the new batch of glass exhibited cathode poisoning. They attribute this as being probably due to chlorine being driven out of the glass by the electrons. They believe that this batch of glass had by mistake a little salt (NaCl) in it. Chlorine not only uses up free barium, they believe it also produces in the cathode a chemical compound which fuses at cathode temperature.

At any rate, the poisoned cathodes showed evidence on examination of being fused into a surface harder than normal. It was not possible to scrape the oxide off with a knife blade as is true with a good cathode.

2.5.3 Poisoning of cathodes by anodes. I asked as to their opinion of the possibility of cathodes being poisoned by anodes. They believe that sometimes cathodes are poisoned as a result of action at the anodes. They attribute this chiefly to oxygen, if it occurs during useful operation of the tube.

They have had cases of sulphur poisoning; in those cases tubes were baked in an oven heated by gas, the gas fumes containing sulphur. The sulphur penetrates along grain boundaries to the anode surface, then comes out of the anode surface and poisons the cathode. When this happens, it occurs during processing rather than later.

2.5.4 Poisoning of cathode by mica dust. They believe that mica dust has been an important cause of tube cathode failure. They believe that mica dust settles on the cathode, that the heat then liberates gas (particularly water vapor) from the mica dust, with cathode poisoning resulting. To prevent the presence of mica dust they do three things,

(a) they make the micas fit tighter,

(b) they use double micas to reduce the bearing surface,

(c) they use clamping tabs wherever they can.

2.5.5 Absence of glass failures. They reported having had no particular glass failure trouble. All tubes are dropped in boiling water as a test. They believe that any glass failures are the result of residual stresses.

2.5.6 Flying lead miniature tubes. I was informed that the British services are going in quite extensively for miniature tubes using flying leads, in which the leads come out in the form of tails without sockets. All connections are then made by soldering. Of course this is an extension into miniature tube practice of existing practice in sub-miniature tubes. This is commented on later in connection with observations at TRE.

2.6 Telecommunications Research Establishment (there is a newer name, but this one is best known). Great Malvern, England.

Wednesday, 9 September 1953

2.6.1 Personnel contacted. I discussed quite a number of things at Malvern with various people, but the only one with whom I discussed the reliable valve problem was Hopkinson, thus we will list only him as follows:

Hopkinson, a man handling a great deal of the standards work for more or less conventional tubes, as used not only at TRE, but by the services generally. He is very well informed in rather detailed ways as to what is used where, what standards exist, and what the reliable valve program in England has been aiming at for a number of years. He has an elaborate file of reports on this subject. He is not himself an experimenter or a research man, rather a man whose job it is to keep track of work of this kind done by others, including the service laboratories and contractors to the government.

2.6.2 TRE's position in the British reliable valve and standards program. Hopkinson discussed in considerable detail TRE's activities relative to the standards as well as to reliability problems. He showed me the British CVD (Committee on Valve Development) report K-114 dealing with these matters. He also showed me a document 1001, part of a series 1002, 1003 and so forth. These are a set of standards of nomenclature, tests, etc., prepared by and for the British Armed Services jointly. Hopkinson is preparing for issuance in perhaps six months an appendix to this series, perhaps to the 1001 document, dealing with reliability tests.

According to Hopkinson 2.5 g is the usual tube vibration test, as to acceleration, used. The underlying basis for this test is that the tubes should be able to stand the same g's as the equipment. However, the fans and sometimes other things may produce more damaging vibration locally, than the airplane produces. This must be taken into consideration in establishment of the vibration test for the tubes.

2.6.3 Comments by Hopkinson on S.T.C. and Mullard personnel in reliable valve work. Hopkinson discussed in some detail the activities of Mr. E. G. Rowe, whom I had visited earlier at the Footscray plant, that is the Brimar Valve Works of Standard Telephone and Cables, where Rowe is chief engineer.

Hopkinson commented that back several years Mr. Rowe supported immediately and enthusiastically the idea of going to miniature seven-pin and nine-pin bases. This early support of this standard helped the British greatly and added to Mr. Rowe's strength. Mr. Rowe used to be associated with Marconi Osram Valve Company (MOV), where he had some trouble knowing who his bosses were. This is because MOV serves jointly the Marconi Corporation and the General Electric Company Ltd., (Osram); also EMI (Electrical and Musical Instruments) is an important stockholder. Hopkinson endorsed my feeling that Rowe likes and is well supported by his management in Standard Telephone and Cables.

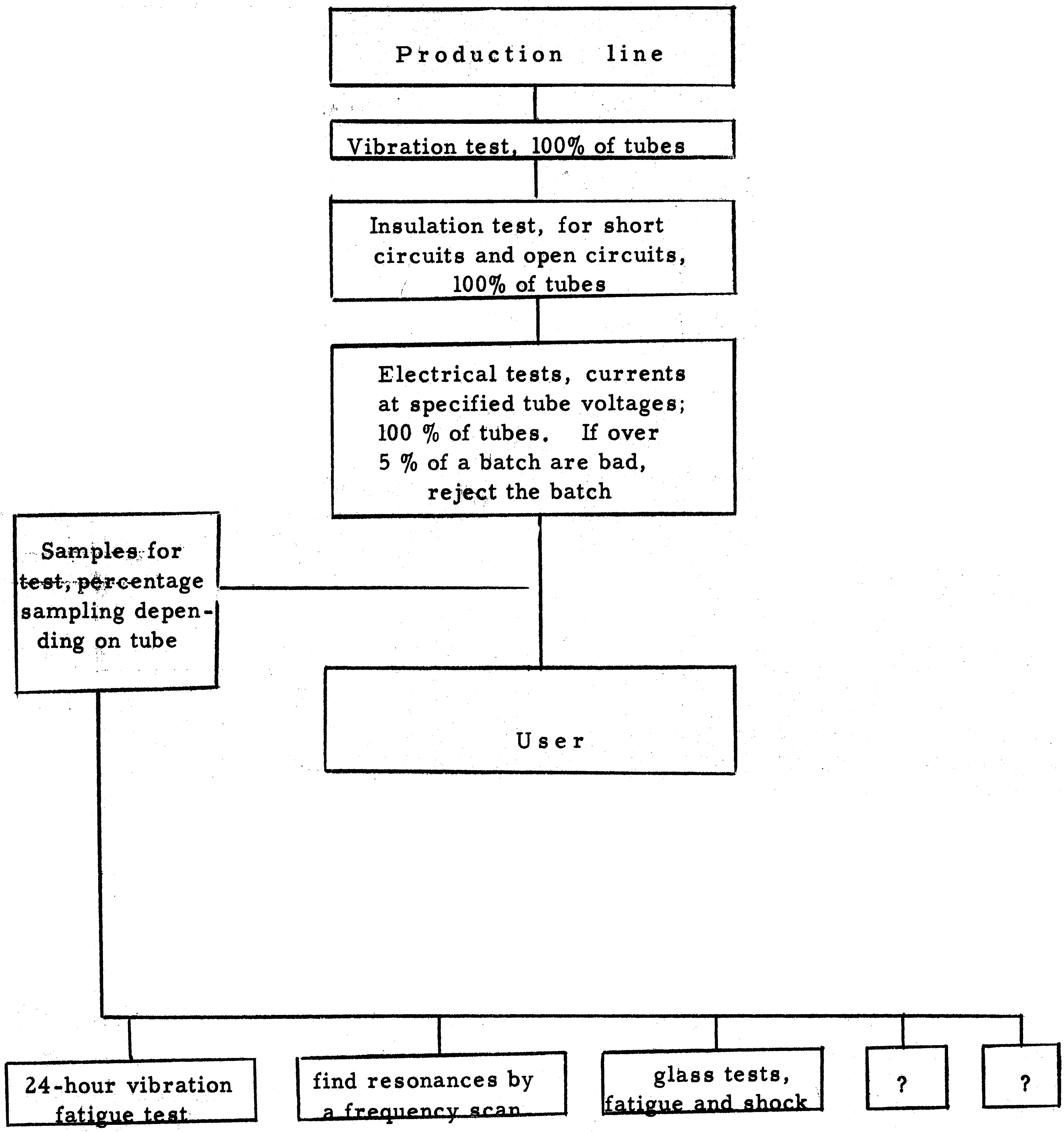


Fig. 2.6.1-1 Testing procedure for reliable valves (from conversations with Hopkinson, TRE).

Fig. 2.6.1-1 is Hopkinson's plan, in use for British Armed Services, for testing reliable valves. He hopes that eventually all valves will be tested this way. British TV set users are getting fussy about tube quality.

Mr. Hopkinson said that Dr. Roberts at Mullard is a very valuable man, and has made excellent contributions to the Mullard reliable valve work, but is not allowed to go into the factory except when asked, and is generally not given much freedom to do things and make changes. In contrast, the team of scientific and practical engineers that Rowe employs works very well as a group operation.

2.6.4 Strong endorsement of flying lead miniature tubes. Hopkinson described the merits of flying lead miniature tubes, as built without bases, but with tails, in the miniature size. Of course the use of such tubes is very common in sub-miniature sizes, but he would like to see it used extensively in the miniature size.

He mentioned a particular case in which a high gain amplifier, providing a gain of perhaps 10^5 in several stages, was built using 3 valves and with ordinary bases. This did not have very satisfactory stability from day to day, or hour to hour, requiring frequent calibration in its use in instrumentation. A new amplifier was built using the same circuit design, but employing valves without bases and with leads soldered in. This did provide adequate stability for laboratory use. The improvement over the device using tubes with bases was very striking. Question: in regard to this experimental fact, reported by Hopkinson, of the major improvement in stability of this amplifier in going from tubes with bases to flying lead tubes, why does this increased stability appear? One reason Hopkinson gives is that in the usual construction there exists a contact resistance in the base, contacts are variable and the changes in these contacts give changes in the gain. An additional reason I think may be significant, is that the material of the base offers a surface leakage path which is affected by moisture, whereas that along the glass between leads in a tube is not.

Another reason is that, for endorsing flying lead tubes, quite aside from the amplifier stability matter, when a tube does not have a base, the tube can be supported physically by a metal envelope, which is by spring tension kept close to the tube envelope and fastened to the chassis so as to give good heat conduction. Thus the temperatures of tubes for a given dissipation can easily be reduced. Some tests have shown this to be as much as a 70° change. In some chassis, this method of heat removal may perhaps make it unnecessary to use a blower fan on the chassis. This would be very desirable, because in many cases the blower fan is the most important cause of vibration.

The metal straps, for holding tubes and providing heat transfer from them, when mounted not in bases, are made of beryllium copper, could be of phosphor bronze. They must be of good spring material, and provide good heat transfer.

CHAPTER III

Miscellaneous Tubes and Electronic Research

3.0 Selection of Material

This chapter will contain, arranged according to dates and places, comments made during the trip of various kinds dealing with tubes or applications of tubes, or electronic principles generally, that do not fall either under the heading of microwave tubes, or under the heading of reliable tubes ("trustworthy valves").

3.1 Laboratoire Central de Telecommunications (LCT)

(The Research Laboratory of the French I. T. and T. subsidiary). 46 Avenue de Breteuil, Paris.

Tuesday, 21 July 1953

3.1.1 Personnel contacted. In the discussions at LCT of miscellaneous tubes and applications the men involved were:

Georges Chevigny, assistant director of LCT.

Gerard Lehmann, member of research staff of LCT, and chief engineer of "Society des Servomecanismes Electroniques", Ter Rue Chanez, Paris XVI. Dr. Lehmann, although having an extensive background in microwave tubes and circuits, is now not in vacuum tube work, but is rather active in more general electronic matters. Thus he has an interest in solid state problems; however, his primary present interest appears to be servomechanisms and magnetic amplifiers. He is chief engineer of a small company organized for the purpose of capitalizing on some I.T.andT. servomechanism patents.

Mr. Beckman, in charge of electron tube work at LCT.

3.1.2 Scope of LCT activities. The fields covered at LCT were reported to me as including:

(a) Late research and development on small ultra high frequency and microwave space charge control tubes, that is, triodes and tetrodes.

(b) Development and manufacture of large power tubes, typically tubes on a power level such as 50 kilowatts, 100 kilowatts, and 200 kilowatts for broadcast use in all radio bands.

(c) Research and development on gas tubes, including thyratrons, ignitrons, and voltage regulator tubes.

(d) Research and development on magnetic amplifier and servomechanisms.

(e) Research and development on some conductors and ferrites.

LCT activities include manufacturing, in fact, all of their research and development activities are closely linked with the manufacturing program. They work only on devices that are reasonably close to the manufacturing stage, particularly in regard to the vacuum tube program.

They are very well equipped to build and test very large radio type power tubes, and are proud of their competence for building such tubes.

3.1.3 Servomechanisms and magnetic amplifiers. In the afternoon Lehmann and I discussed various matters other than vacuum tubes. He is very much interested in all prospective uses of magnetic methods, as for example, the use of ferrites at high frequencies, and of magnetic amplifiers using iron cores at low frequencies.

He maintains very close connections with Professor Charles Guilland, Laboratoire de Bellevue, Centre National de la Recherches Scientifique, Paris, who is head of the magnetic laboratory at this center. This magnetic laboratory has as one of its principle activities an attempt to understand the fundamental properties of ferrites.

Lehmann feels (and I am inclined to agree with him) that many if not most of the functions performed by thyratrons can be done better by magnetic amplifiers. However, he does not at the moment see that the magnetic amplifiers have progressed to the point where they can replace ignitrons in the resistance welding field. In discussing the work on magnetic amplifiers done at the Naval Research Laboratory Company and the Westinghouse Electric Corporation in the United States, Lehmann emphasized his feeling that many of the ideas used at both places were extensions of ideas presented in two books by Uno Lamm, of the ASEA (Swedish). One of these books was published about 1945, the other about 1950.

One of Lehmann's chief interests, occupying perhaps half of his time, is his work as Chief Engineer of "Societe des Servomechanismes Electroniques", organized to capitalize on some patents held by I.T. and T.

Lehmann emphasizes the desirability, in work with servomechanisms, of using transient response (time domain) and frequency domain types of analysis in combination, rather than using the frequency domain analysis exclusively. He believes that the tendency in the United States has been to emphasize the frequency domain analysis almost to the exclusion of the time domain.

In relation to magnetic amplifiers, Lehmann feels that U.S. engineers are overemphasizing the use of the transient response study of magnetic amplifiers, and that they should use also the frequency domain approach.

For example, U.S. engineers appear to be working very hard to obtain magnetic amplifiers with very short time constants, whereas there are applications in which an infinite time constant is useful. Thus there can be considerable utility for a device in which gain = $1/\omega$, which corresponds to an infinite time constant.

A comment might be made here, that he indicates that U.S. engineers working on servomechanisms tend to use the frequency domain analysis to the exclusion of the time domain analysis, whereas U.S. engineers working with magnetic amplifiers tend in the opposite direction. Inasmuch as magnetic amplifiers are frequently used as elements in servomechanisms this is a peculiar situation, if true.

In addition to his recent paper in Electrical Communications, March, 1953, entitled, "Rotary Amplifiers and Servomechanisms", Lehmann will publish soon (in the spring of 1954) a paper on magnetic amplifiers showing how he uses in combination the frequency response and transient response analytic approaches. This will be published first in L'Onde Electricit, subsequently in Electrical Communications.

He showed me an extensive and impressive looseleaf compendium of electronic knowledge in French, called "Techniques L'Ingenieur Electronique"; C.Monteil is general editor of the entire series including all kinds of engineering. The authors of the electronic portions are Pierre Besson and Gerard Lehmann.

3.1.4 Power tubes, including gas tubes. In the general tube development section they are making tubes patterned after the Eimac 4-125A (or was it just the 4-125?) That is, they have the same size, same pin spacing, etc. as the Eimac tube. However, the pins were sealed right into the glass, not being part of a separate base. This is very quickly to become a production activity, not merely a pilot activity. This practice of sealing the pins into the glass is related to the British practice of encouraging the use of flying lead tubes in miniature sizes.

LCT were making many of a tube very similar to the U.S. type 810, but larger and having somewhat greater plate dissipation. They were also making tubes very similar to and essentially interchangeable with the U.S. type 889 tube.

They were making tubes similar to the General Electric (U.S.) 5544 and 5545 inert gas thyratrons. They said that the General Electric Company in their data sheets specified 800 hours life under maximum duty ratings, 2000 hours under less severe duty, whereas the comparable mercury vapor tubes have at least 10,000 hours rating. (This is familiar information.) The LCT comment was critical of U. S. practice in building and using the 5544 and 5545. I believe LCT make these tubes as replacement tubes for American equipment, without being enthusiastic about them. This means to me that they do not understand the importance of the freedom from environmental temperature effects that is possessed by the 5544, and not shared by the comparable mercury tubes.

A significant fact evident in the statements just made is the very strong tendency noted wherever I went in Europe to use United States type numbers. It is apparent that this tendency is not limited to microwave tubes, but exists also relative to tubes having general utility in radio and electronic control equipment, and even into tubes at fairly high power levels. In the high power tubes being built at LCT at the present time I could see very substantial similarities to the design pattern of the F.T. and R. 125 and 124 tubes of ten years ago.

3.2 Brown Boveri Manufacturing Company, Baden, Switzerland

Tuesday, 4 August, 1953

3.2.1 Personnel contacted. In regard to the electronic equipment of a miscellaneous and non-microwave nature at Brown Boveri, responsible personnel contacted were as follows:

Dr. Theodore Boveri, a director of the company, and in charge of engineering and manufacturing. He is a son of the man who founded the company.

Mr. Kesselring, a relatively young man in charge of the electron tube work, possibly also of some of the electronic equipment work.

3.2.2 Industrial electron tubes. There was rather extensive manufacture of electron tubes of the types used in industrial electronic applications, that is, relatively large power tubes and rectifier tubes. Many of these tubes were built with the same base design (i.e., to fit in the same socket) and essentially the same electrical characteristics and general rating properties as many standard RETMA tubes in the U. S. Thus they were making 869B and 857B large power rectifier tubes, and 872A medium power rectifier tubes, these being gas tubes, also 866 rectifier tubes and 810 power amplifier tubes.

In addition, they were building tubes that are interchangeable as to base and envelope properties with the 872A, 869B, and 857B gas rectifier tubes, but also have grids in them, making them essentially thyratrons.

This is another illustration of the fact that throughout Europe there has been extensive adoption of the American type numbers and base and important characteristic specifications. Note that in this case this is done for the commercial industrial electronic equipment market rather than primarily for the military market.

3.3 Philips Gloelampfabriken, National Research Laboratory, Eindhoven, Holland

Tuesday, 11 August, 1953

3.3.1 Personnel contacted. The arrangements for contacts at the National Research Laboratory of the Philips Company at Eindhoven were made through Dr. Duffendack of the Philips Laboratories at Irvington-on-Hudson. I requested particularly opportunities to see Dr. J.H.A. Kleynen, Dr. Jonker, Dr. Penning, and Dr. Bruining. I was able to see all of these except Dr. Penning, who was not available because of an extended illness. Dr. Kleynen has to a considerable extent taken over the responsibilities formerly held by Dr. Penning. Dr. Kleynen acted as my host and arranged for my visits to all of the various sections of the laboratory, and entertained me at lunch.

The men contacted on specific types of miscellaneous tube activities were as follows:

Dr. Kleynen, in charge of microwave tube research and development, who attended the Stanford Electron Tube Conference in June, 1953.

Dr. Jonker, a veteran research worker at Eindhoven, recently having done some excellent work on secondary emission.

Dr. Bruining, one of the better known researchers at Eindhoven who has recently been working on cathode ray tubes of various kinds, including television and storage tubes.

Dr. G. Ahsmann, a young man working on the AC impedance of cold cathode discharges.

Mr. Groendyk, working on a secondary emission scale-of-ten counter tube.

3.3.2 Photographs of fields and trajectories in space-charge control tubes. They have a very interesting scheme for obtaining photographic pictures of electric fields and of electron trajectories in space-charge control tubes. The electric field potentials are obtained by stretching a medical rubber sheet 1 millimeter thick, about 3ft. by 1 1/2 ft., over forms shaped to simulate electrodes. The stretch involves about a two-inch or so difference in elevation at the extremes. Then they photograph the water level meniscus to determine equipotential. After determining the potentials, they put enough glycerine on the rubber to make it a little sticky, then roll small balls on the surface. These balls will follow the flux lines. Without the glycerine they follow electron trajectories; both kinds of pictures are taken. They photograph the specular reflections from the balls, to obtain the record. By using an AC fluorescent source, the pictures of the balls are made to come at intervals, showing electron velocities as well as trajectories. He gave me a number of photographs obtained this way.

This is not a particularly new technique with them, but it is nevertheless a very interesting technique, and one that I had not heard described before. The various examples of use of the technique that he showed me include trajectories of rejected electrons in cases of a negative plate or negative suppressor grid, beam formation by grid arrangements, and the entrance of electrons into a positive grid after making several loops around the grid before entrance. One of the most interesting records is that of a beam power tube design, showing the beam focusing action of the second grid. I have these pictures available. The detail and contrast is not very strong, so they do not stand photostatic reproduction. I can have photographic copies of them made if anyone wishes. Several have been published in recent Philips research reports.

3.3.3 AC impedance of cold cathode discharges, a telephone switching problem. Considerable work was being done by G. Ahsmann, a relatively young man, on the a-c impedance of cold cathode discharges of frequencies up to 2000 cycles per second. This has to do with the possibility of using such discharges as switching elements in telephone circuits.

A previous Philips writer named Geel, working many years ago, made an investigation in which he found that the a-c equivalent circuit for some gas discharges requires the use of a negative inductance.

The general idea is that for a simple series circuit such as Fig. 3.3.3-1, the usual stability criterion, that

$$\begin{aligned} R_s + R > 0 &\quad \text{provides a stable circuit} \\ R_s + R < 0 &\quad \text{provides an unstable circuit} \end{aligned} \qquad (1)$$

is replaced by the criterion

$$\begin{aligned} \frac{R_s+R}{L} > 0 &\quad \text{provides a stable circuit} \\ \frac{R_s+R}{L} < 0 &\quad \text{provides an unstable circuit} \end{aligned} \qquad (2)$$

Then if L is negative, the condition becomes $R_s + R < 0$ for stability, $R_s + R > 0$ for instability.

For some discharges Geel found it necessary to use the equivalent circuit in Fig. 3.3.3-2a for the discharge. In this figure C_2 is the capacitance due to the electrodes, and is for most interesting frequencies small enough so that its effects are not important. Fig. 3.3.3-2b is another equivalent circuit that has been used for certain discharges. Ahsmann's experiments were carried out mostly in helium; argon and neon behave about like helium, but krypton and xenon behave very differently. He said early in the discussion that he worked at pressures around 9 or 10 mm of mercury, but some of his comments as to dimensions in his discharge do not seem to me to fit with those pressure values. His plate current was about 8 milliamperes. He said that most of his work had been with discharges in which the distance between electrodes was small enough so that there was no positive column; that is, the anode was just beyond the negative glow.

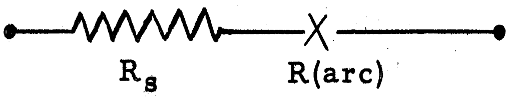
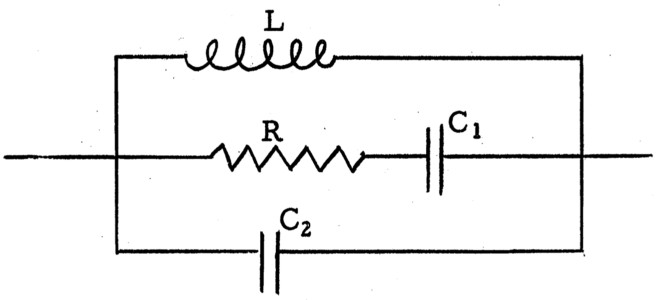
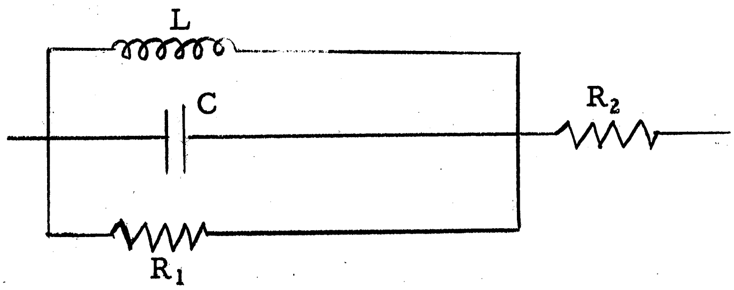


Fig. 3.3.3-1 An arc of a-c resistance R in series with the resistance R_s



(a) Two capacitances, a resistance and an inductance



(b) Two resistances, a capacitance and an inductance

Fig. 3.3.3-2 Equivalent circuits that have been used to describe the a-c impedance properties of cold cathode discharges

His experimentation consisted of measurements of the small signal a-c impedance as a function of frequency. He represented the data as in fig. 3.3.3-3. This particular curve is what he obtained when he measured the impedance with conditions having an anode fall space, then changed so there was none, and made a vectorial subtraction to get the impedance due to anode fall of potential only. Fig. 3.3.3-4 illustrates this on a different diagram. At B in fig. 3.3.3-4 there was an anode fall space; at A there was none.

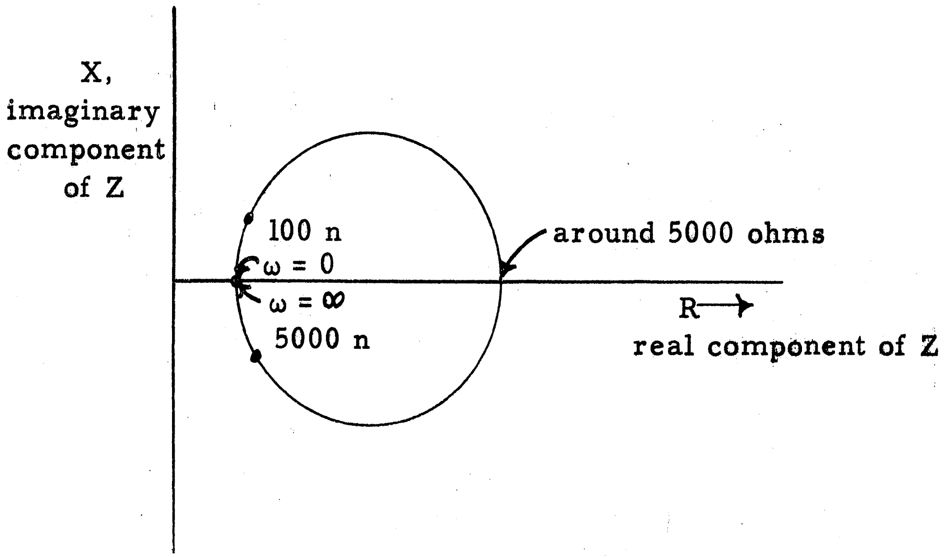


Fig. 3.3.3-3 Frequency domain representation of the a-c impedance of the anode fall space of a cold cathode discharge.

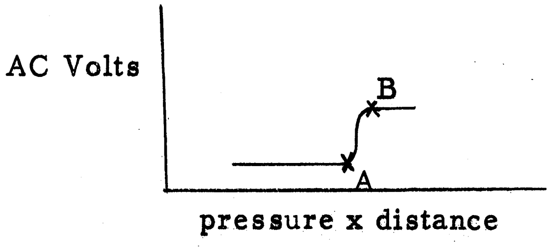


Fig. 3.3.3-4 Anode fall space eliminated by a change in pressure.

Measurements, made by Ahsmann and I believe by others similarly, for the total discharge have shown various peculiar behaviors as in Figs. 3.3.3-5 and 3.3.3-6.

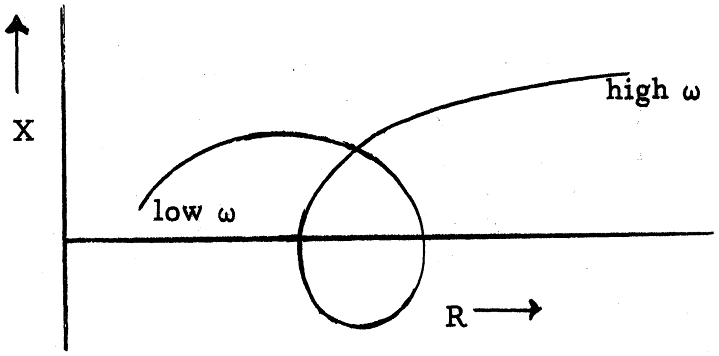
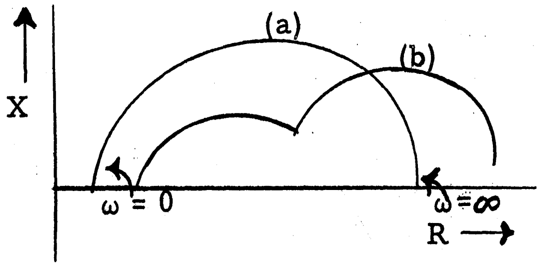


Fig. 3.3.3-5 Illustrative diagram of a-c impedance for a complete discharge.



(a), (b) are two different discharges.

Fig. 3.3.3-6 Diagrams similar to Fig. 3.3.3-5.

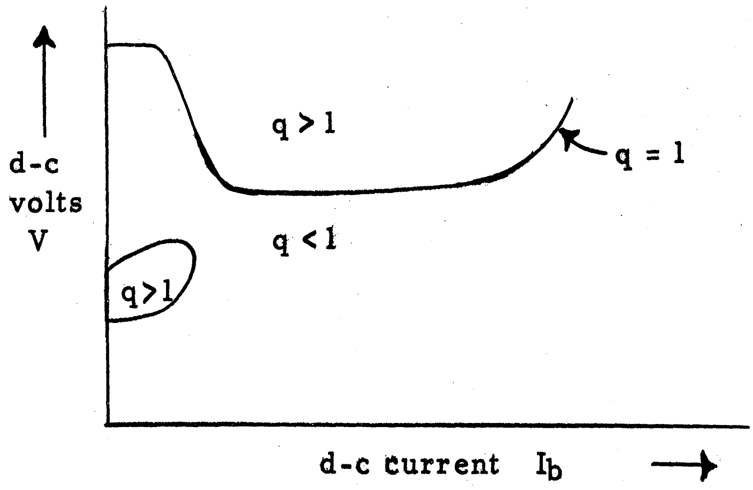


Fig. 3.3.3-7 Volt-ampere curve for a helium discharge.

In the usual breakdown condition equation

$$J_b = \frac{J_{ph} \exp(\alpha s)}{1 - \gamma_i (\exp \alpha s - 1)} \tag{3}$$

the Philips people have used the symbolism

$$q = \gamma_i (\exp \alpha s - 1) \tag{4}$$

thus

$$J_b = \frac{J_{ph} \exp(\alpha s)}{1 - q}$$

Symbols J_b, J_{ph}, α, s, and γ_i have meanings as in the Chapter XVII treatment of voltage breakdown in "Fundamentals of Engineering Electronics", 2nd edition. They then indicate regions in a volt-ampere diagram for which $q < 1$ and $q > 1$; see fig. 3.3.3-7. This curve for helium has two branches in the low current range.

If the discharge is momentarily at any point for which $q > 1$, the current will increase. If it is a point where $q < 1$, the current will decrease. Extension of this concept leads to the concept for small a-c signals, of a negative inductance property for certain portions of the small loop down and to the left of the figure.

At this point I made my own comments to the effect that the existence of a negative small signal inductance means in effect that an increase in the current results in a decrease in the energy of various forms in the arc. This may represent partly kinetic energy of the particles, but is probably chiefly energy of ionization of the ions present.

Ahsmann has no satisfactory theory to account for his very nearly circular figures describing properties of the anode fall space. His observed numbers are completely in disagreement with any proposed theory. When his circle diagram is translated into inductance and capacitance magnitudes, the equivalent circuit inductance turns out in one case (8 milliampere helium) to be around 3 henrys, and in another case 0.1 henry. The capacitance turns out to be perhaps 0.05 microfarads, which is larger by a factor of 10,000 than the complete space charge capacitance for this geometry. He thinks that perhaps the effect is due to shortening and lengthening of the fall space distance.

My own feeling is that the significant fact in his observation may be the indication of a resonance at some frequency between 500 and 1000 cycles per second. This is of the order of magnitude of the frequency, or perhaps below it, at which an ordinary atmospheric pressure arc ceases to follow the static characteristic. This suggests to me that a change in the diameter of the gas discharge active region may be involved, and that a study of ion movements in response to frequency somewhat paralleling Llewellyn's ultra frequency theories for electrons might apply. Thus it looks to me like an ion transit time effect.

Just as "inductance" here represents merely the fact that as the current changes there must be added energy to make the change, so there may be some similar electron or ion dynamic parallel as to capacitance.

3.3.4 A beam deflection counter and display tube. In the general area of Jonker's work I was shown a small beam deflection tube developed for use as a counter and display tube. The development work on this tube was being done by a man named Groendyk, who described it to me. He is associated with Jonker.

Fig. 3.3.4-1 illustrates the geometry of the tube, together with some indication of the circuitry. This tube, called the E1T, has static characteristics as shown in fig. 3.3.4-2. An illustrative load line is shown in this figure. It is evident that by suitable pulsing it is possible to shift from any one of the points marked with a cross on the static characteristic to the adjacent similar point. Fig. 3.3.4-3 shows the shape of pulse which is required to do this.

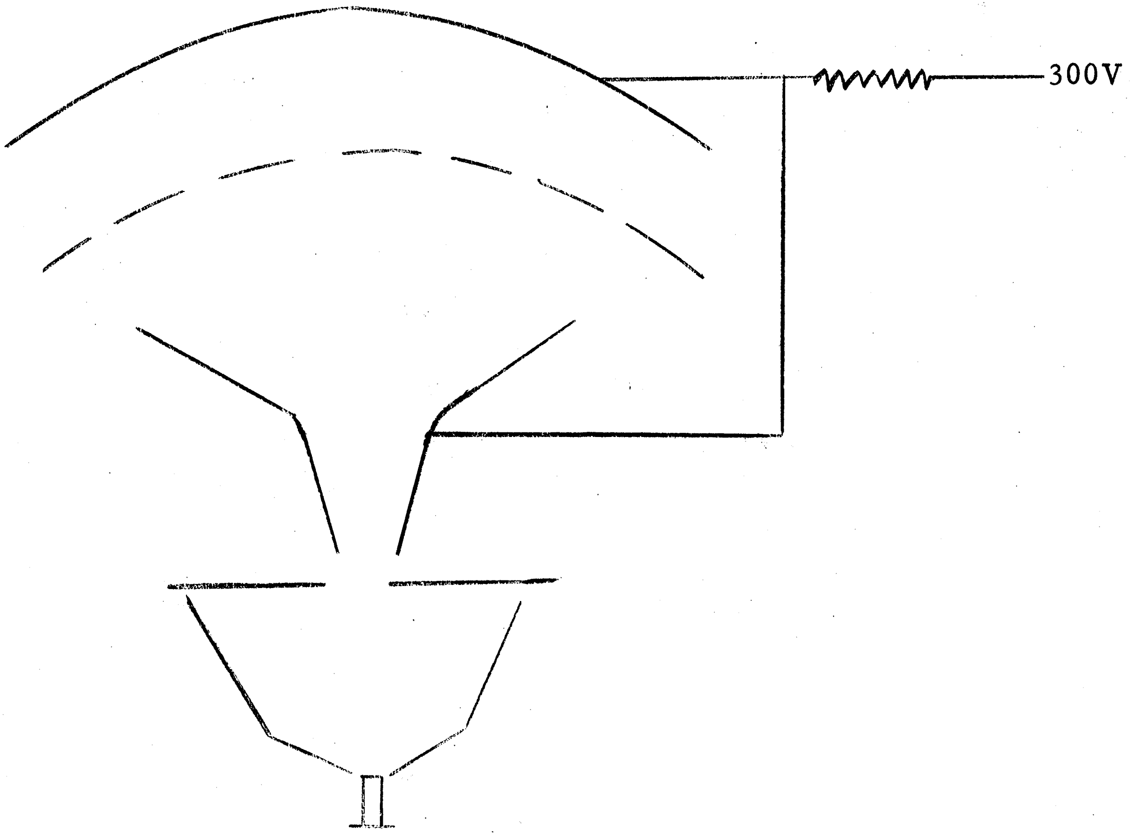


Fig. 3.3.4-1 Schematic diagram of the E1T beam deflection counter tube.

Groendyk demonstrated a counting circuit assembly using these tubes. He also connected the tube to a radiation counter and demonstrated its use in that service. In its present form it counts up to a rate of about 35,000 per second. They believe that the principle is extendable up to 1,000,000 operations per second. Notice that this is a high-vacuum device.

The whole design was based on a need for a counting tube that will operate from a 300 volt d-c line. It should be inexpensive to build; I was told that the Philips organization is manufacturing and selling these tubes, as the type E1T.

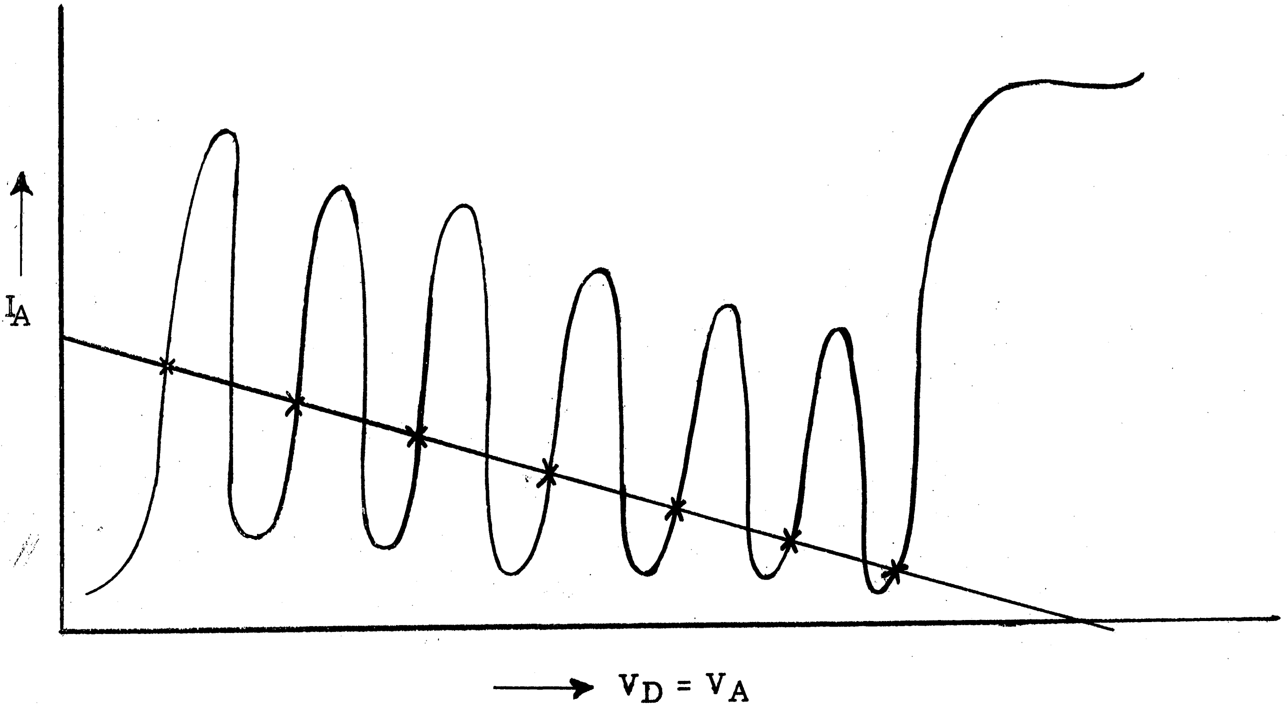


Fig. 3.3.4-2 Static characteristic of E1T beam deflection tube.

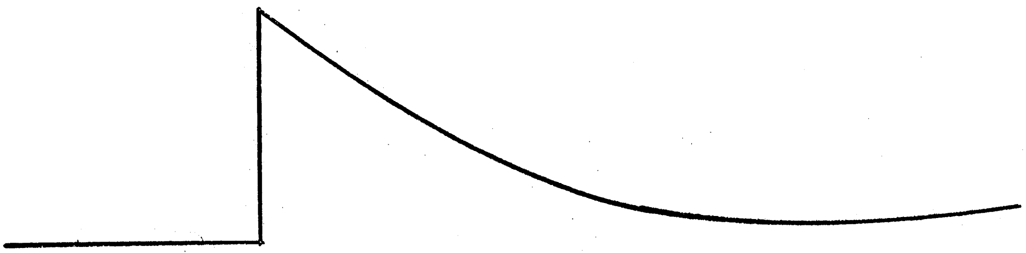


Fig. 3.3.4-3 Shape of pulse required to cause stepping of the E1T tube.

They have tried to use this principle in a telephone circuit switching tube, and have built a switching tube using the same principle employing 6 output channels. The general idea would be to use 6 output resistors; the output voltage would occur across the appropriate resistor. Laboratory models have worked satisfactorily, but so far there is no prospective market promising enough to warrant manufacturing switching tube devices for telephone service.

3.3.5 Discussion of secondary emission with Dr. Jonker. I had considerable discussion with Dr. Jonker about the work on secondary emission. He has published a number of very interesting papers recently, and I felt it desirable to have him express some of his methods of experimental approach, and his theoretical points of view.

He showed me the apparatus that he and his assistants have been using for their secondary emission studies. It is a very large, typically physically electronics type of research apparatus, obviously very expensive and capable of being used to obtain a considerable amount of information. In this respect I was reminded of the apparatus that LeRoy Apker used for a number of years at the General Electric Research Laboratory in Schenectady for determining various electrical properties of surfaces.

Using this very complex apparatus Dr. Jonker and his associates have been for several years studying the angular distribution of secondary electrons as a function of the angle of incidence of the primaries, of the energy of the secondaries, and of course the nature of the surface. Much of his work has been published (he has sent me a complete file of reprints) but some of the more recent work had not reached the publication stage at the time of my visit.

It is useful to discuss his work in relation to the familiar distribution diagram for the energies of secondary electrons, which appears in fig. 3.3.5-1.

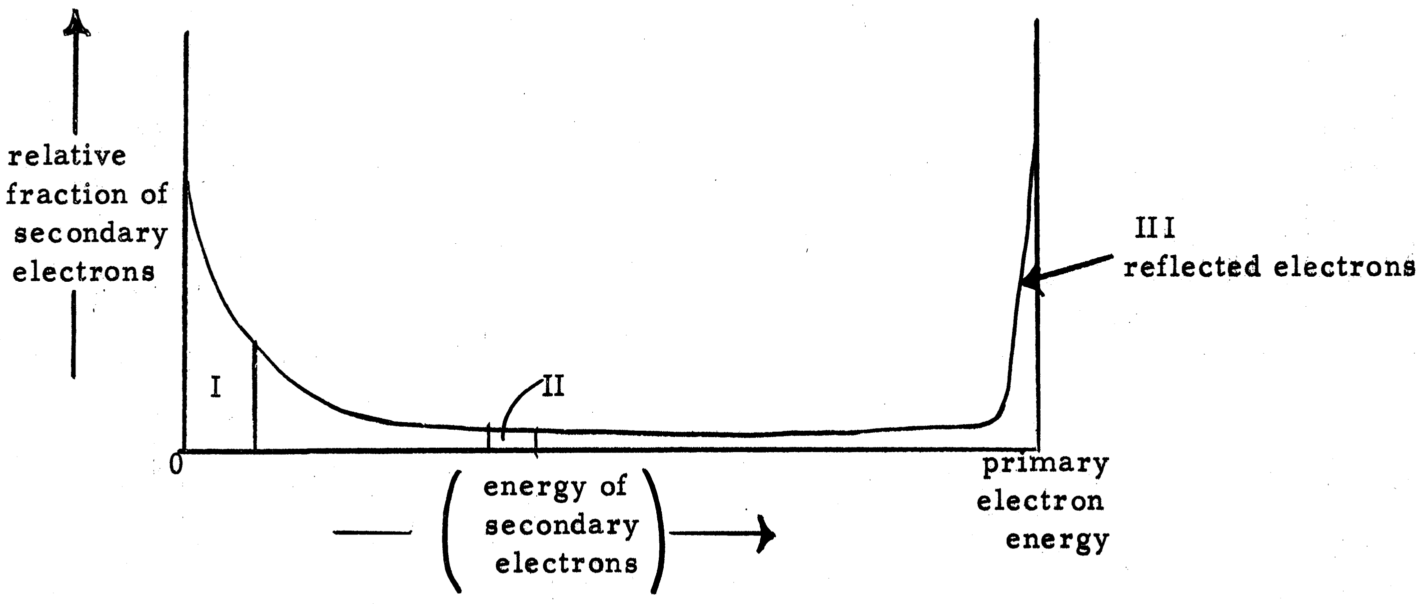


Fig. 3.3.5-1 Typical distribution curve for energies of secondary electrons.

Referring to this figure, Dr. Jonker and his associates made careful studies in three regions. These are marked in the figure as I, II, and III. The I region includes the low energy group of electrons, II an intermediate energy group, and III consisted of reflected electrons. His work shows that for pure metal surfaces, on which most of his work was done, the low and intermediate electrons (groups I and II) have an angular energy distribution that is not affected by changes in the direction of arrival of the primaries, whereas the angular distribution of the reflected electrons is quite markedly affected. For the reflected secondaries, the greatest current density is directly back in the direction from which the beam came.

I asked him about the possibility of increasing the stability of commercial secondary emitting electrodes (dynodes in photo tubes). His comment was that his experimentation has all been with pure metals, which have adequate stability. That this is true is obvious from the regularity and repeatability of his experimental data.

He commented, however, that it seems reasonable to suppose that the deterioration of secondary emitting surfaces used in dynodes may result from some sort of electrolysis. The fact that deterioration increases with increasing current density suggests the electrolysis hypothesis, because electrolytic action has such a dependence on current.

More recently he has been studying secondary emission from the surface of soot. The results are as follows:

(a) The I and II secondaries tend to have a maximum current density back in the direction from which the primaries come.

(b) The total emission is much less than with carbon surfaces. His explanation for this reduction is that it is merely an effect of the geometry, due to roughness.

To illustrate the second point, it is evident with reference to Fig. 3.3.5-2, for primaries that land in crevices, the electrons in directions other than that from which the primaries come strike the surface and re-enter. This effect can reduce the total secondary emission by a factor of as much as 4.

This point of view gives a measure of how much secondary emission could perhaps be reduced by geometrical design of the anode (for example by using deep anode pockets). Soot is probably about the extreme in fineness of structure of crevices, therefore of effectiveness in reducing secondary emission. However, for soot the total secondary emission is only down by a factor of 4 or 5 from its value with smooth carbon, untreated.

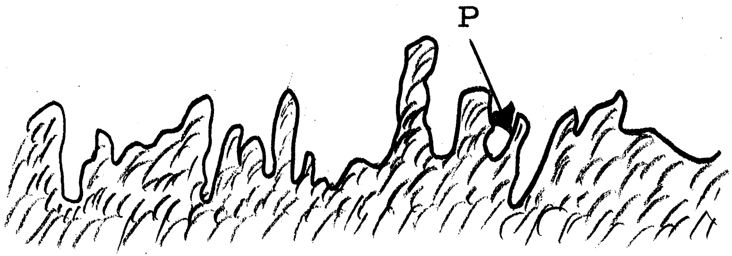


Fig. 3.3.5-2 A surface of soot; for primary electrons impinging in a crevice, as at P, most of the secondaries will not emerge.

Jonker found, as others have, that as the primary energy is reduced the proportion of electrons having energies near the primary (reflected electrons) increases. The point here is that the number of secondaries in reflected categories stays the same, whereas the number of low energy electrons declines.

Jonker discussed somewhat informally his theory of secondary emission, which is quite completely described in his articles. He pointed out to me that the essential point of the theory is the "Whittington Effect". This name is given to the fact that the rate of electron release by an electron passing through a metal becomes greater as the electron velocity declines. Therefore most of the low energy secondaries start near the end of the journey of the primary electrons. He feels that all of the observed phenomena check very well with this point of view.

Pure metals have ratios of secondary emission current to primary current, at the maximum point on the secondary emission curve, up to perhaps 1.8, whereas this ratio for special surfaces may reach 8 or 10.

I asked him about the making of measurements, similar to those he has made on pure metals, on secondaries from insulators (glass, etc.). He said he was going to try to make such measurements, but that he knows it will be much more difficult than the work on metals has been.

He works with primary beam currents around 1 microampere. The current is kept at this small value to eliminate space charge effects. To get the angular resolution for such currents in the beam measure currents down to 10^{-12} or 10^{-13} ampere, because at each angular segment he measures only a small portion of the secondary current.

Overall, Jonker feels quite confident of the essentials of his theory of secondary emission.

Translation of the theory into utilitarian terms indicates that the most important property of a secondary emitting substance is its absorption of energy from the electrons to which the primary electrons have given energy. He finds theoretically a shape of curve relating to dimensionless quantities which agrees very well with experimentally observed curves of the ratio of secondary to primary current. However, there are at least two parameters of the material which must be properly chosen to cause agreement.

He has made some measurements on secondary emission from single crystals, in an attempt to repeat and extend the findings of Davisson and Germer many years ago. There are two distinct aspects of such a measurement, as follows:

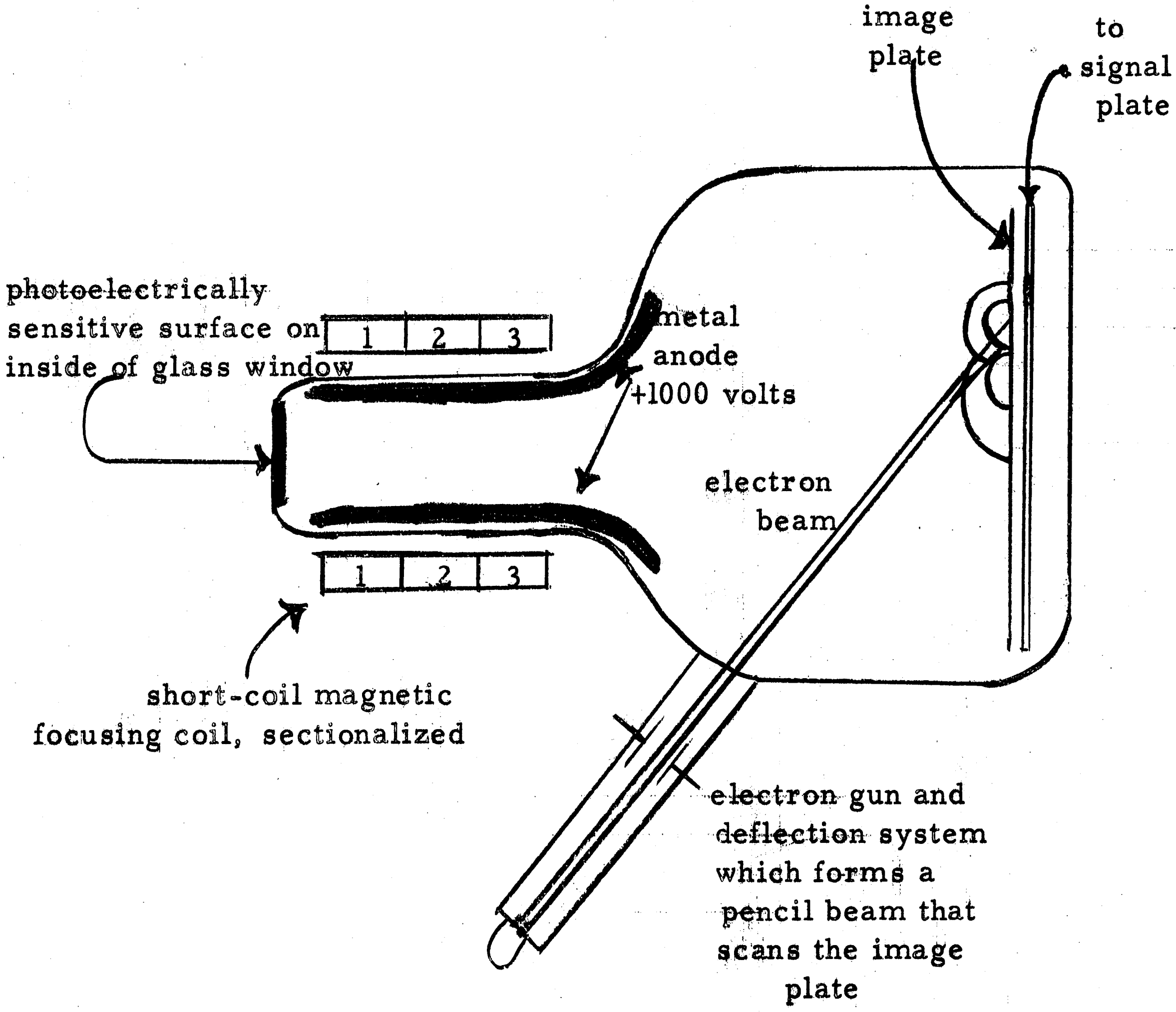
(a) The angular distributions of secondaries is presumably different, particularly for the reflected (that is, high energy) electrons from a single crystal target as compared with the usual metal surface.

(b) There should be some sort of line spectrum of the energy response. That is, when the primary electrons have energies at or near discrete values, the secondary emission should be much larger than for primary energies at or near other values.

In one set of observations Jonkers experimented on a single crystal and obtained the (b) effect but has not been able to repeat it subsequently.

3.3.6 Television image iconoscope. A very important part of the Philips Research and Development activity has to do with television. I discussed television system operation with one of their research men for a while, then with that as a background was very glad to have comments on television tubes by Dr. Bruining. He is one of their most well-known and best tube research and development men. He is now working on television pick-up tubes.

They are not using or planning to use image or thicon tubes because they consider such tubes to be complicated, difficult to manufacture, and expensive. They have, however, developed and shown to me what they call an image iconoscope, which uses a combination of electrostatic and magnetic focusing. Their image iconoscope tube is about as illustrated in fig. 3.3.6-1. In this figure the image plate is a semi-insulating, perhaps mosaic type of plate. When the scanning beam rests momentarily at any one point on the image plate, that point emits secondary and acquires a high-energy equilibrium condition. Secondaries from this spot spread out and return to other points on the image plate. The way in which the secondary spray around is important in the details of tube design.



There is about 1000 volts potential difference between the photocathode and the image plate, also about 1000 volts between the gun cathode and the image plate

Fig. 3.3.6-1 The Philips image iconoscope (1953).

Bruining drew a sketch as in Fig. 3.3.6-2 to describe the behavior for any one spot on the image plate. Each spike in this figure occurs when the scanning beam hits the particular spot for which the curve is drawn. Restoration to the normal condition occurs during the decay between spikes. The signal is contained in the change in the height of the spike.

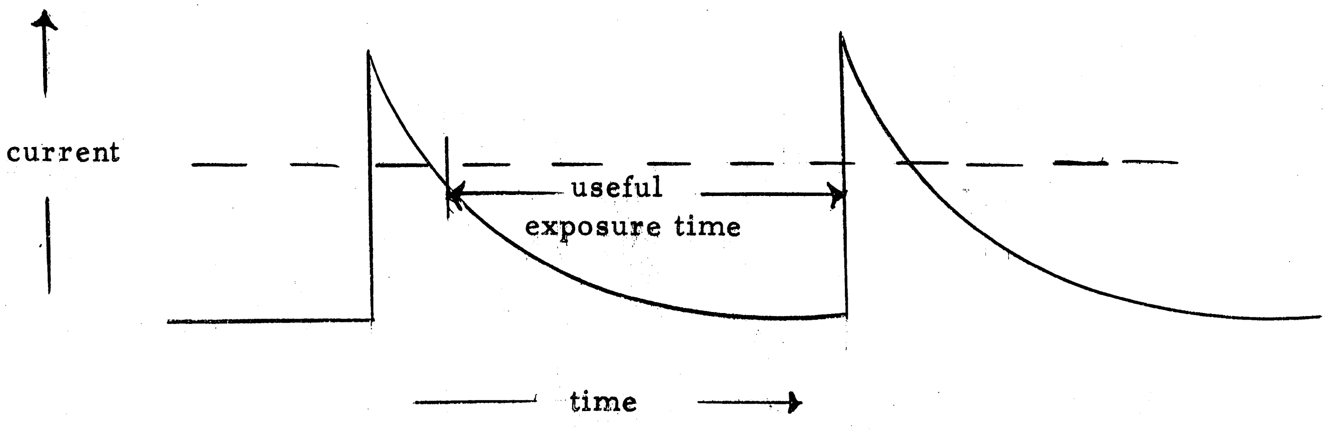


Fig. 3.3.6-2 Response of a spot on Philips image iconoscope to scanning beam. Current spikes occur when the scanning beam hits this spot; restoration occurs in the decay.

This device works rather well in strong light (I believe they mentioned 10-foot candles); in weak light there is not adequately uniform response throughout the field, partly because of edge effects involved in spraying out the secondary electrons. Another point here is that light has a useful effect on this device only during the interval between the spikes, which means not all of the time. The amount of useful exposure time depends of course on the decay rate of the spikes.

They use a short coil magnetic focusing field on the photoelectrons. They use +1000 volts on the anode metallic film inside the tube, producing a diverging field for the photoelectrons; the magnetic coil provides converging of the electrons. A thesis by Francken (or a similar name) recently issued analyzes the electron optics completely. They promised to send me a copy of this thesis. They are quite proud of this electron optical system. The focusing coil is sectionalized with current separately controlled for the various sections.

Overall, it was represented that this image iconoscope has the following features:

(a) High-energy equilibrium stabilization. That is, a spot on the image plate stabilizes while the beam is present at the high energy secondary emission equilibrium point.

(b) A magnetic coil is used to give photoelectric focusing. They use a sectionalized coil, in which current in each of the three sections is separately adjusted by experiment to give best results.

(c) A high light level is required to give adequate signal-to-noise response. I gathered that the important noise is a systematic noise related to edge effects of the sprayed out secondaries.

(d) It is simple to build.

They are much interested in the application of this tube and other of their television work to industrial "closed-circuit" television.

3.3.7 Videcons for television camera use. I gathered that one of the important reasons they are not working on an image orthocon type of device is that they feel that the videcon, using photoconductive surfaces, will eventually be better than photoelectric devices.

They showed me a videcon in the research stage. It works, but as yet they are having trouble due to response time (rather recovery time) of each local region being too long. In other words, they feel that the important research problem is one having to do with semiconductive properties rather than with electron optics. Their videcon was very small in diameter, not over 1" anywhere. In external appearance it was practically a straight one-inch diameter cylinder.

A question occurred to me, perhaps because I am not skilled in this particular art, as to whether the semiconductive problem might be less serious if they used more area. The science of electron optics is certainly such as to permit using more area.

3.3.8 Color television activities (relatively minor). As to color television, they indicated to me that they are not now working on a three-color tube. However, they are working on a flying-spot pick-up for color film. They showed me a rather nice demonstration involving the use of three tubes, one having a green phosphor, one a red phosphor, and one a blue phosphor. Half-silvered mirrors were used to combine the three images. They also showed me a three-color camera using dichromatic lenses and three pick-up tubes.

One of the men there had seen within the past year a demonstration of color television using the RCA three-color tubes; he was greatly impressed by the difficulty of maintaining it in adjustment. It is perhaps worthwhile to mention here that as an independent but curious observer, not involved in or really particularly personally interested in the color television art and science, I have received this comment from several completely distinct and individual sources during the past two years, in some cases a rather vigorous comment from men who have actually had the job of trying to make some of these things work.

3.4 Associated Electrical Industries (AEI), Aldermaston, England

Friday, 21 August 1953

3.4.1 Personnel contacted. The Associated Electrical Industries is a research establishment jointly supported by a group of electrical manufacturers in England. I believe the supporters include the British Thompson-Houston Company and I am sure it includes Metropolitan-Vickers. They do a variety of work of a nature that can be useful to all the companies and is for the most part basic research rather than of an applicational nature. They are giving considerable attention to information theory, but I did not discuss this aspect of their work with them to any great extent, except to note that it is considerably influenced by labor.

Personnel contacted were as follows:

Dr. T. E. Allibone, director of the laboratories, and a man who has considerable knowledge of the United States.

Dr. Gabor, who is of course very well known and serves them occasionally as a consultant. He happened to be in Aldermaston the day I was, and we discussed a number of things at lunch with him.

Dr. D. R. Petrie

Dr. M. Hain, electron physics.

Dr. D. R. Chick.

3.4.2 The electron microscope. One of the important activities at Aldermaston has to do with achieving improvements in electron microscope resolution. Their aim is to bring the resolution down to 4 microns, as compared with 50 microns in existing devices. To achieve the 4 micron resolution will require voltage and current stabilization to about 2 parts per million. It also requires elimination of astigmatism and of spherical aberration.

Astigmatism results from imperfect symmetry in the lens system, and can be corrected by an asymmetrical system of controlled strength and orientation. In order to accomplish this, the astigmatism is observed in the beam, then removed by a change of current strength. In the present equipment this correction lasts for too short a time. They believe the transient nature of the astigmatism correction can be overcome by further work.

To accomplish adequate current stabilization for the magnetic focusing system requires that three distinct component units must be made stable within two parts per million. These are:

(a) A d-c voltage source. For this they employ a mercury cell, only relatively recently known, held at a controlled temperature, constant to 0.1°C. The mercury cell provides a stable voltage at approximately 10 volts.

(b) A resistor through which the reference current flows; they use precision resistors in a constant temperature bath.

(c) An amplifier; they are using a chopped-signal amplifier, employing negative feedback stabilization in the frequency range from 5 to 10,000 cps, primarily to eliminate the 50 cycle hum coming from the a-c power system supplying the laboratory.

By the use of these three (a), (b), and (c) items they achieve current stabilization for the magnetic focusing devices to two parts per million as desired.

Stabilization of the high beam voltage is more difficult to obtain primarily because of the difficulty in determining when adequate stability is achieved. It is impractical to use precision high resistors as voltage dividers. They use what amounts to a mass spectrometer for determining when the voltage is stable. Thus they apply the voltage to be studied on an electron gun, then pass the electrons from the gun into a magnetic field, and require that they pursue a half-circle path into a slit. The magnetic field must be stable to two parts per million, as must the physical length of the metal piece governing the diameter of the circle, in order to make this determination satisfactory. To keep the magnetic field constant, they use the stabilized current described above. To keep the distance constant they maintain the temperature constant. Up to now, even when using these precautions, there remains slow drift of voltage as yet unexplained.

Three schemes are proposed for eliminating spherical aberration, as follows:

(a) The scheme illustrated in fig. 3.4.2-1 compensating for spherical aberration. Note that this scheme has as its basic component a lens, Fig. 3.4.2-1A, which provides negative spherical aberration in one plane. Two of these lenses are used as in Fig. 3.4.2-1B, together with appropriate cylindrical lenses. Dr. Hain believes this scheme to be the most promising of the three that have been proposed.

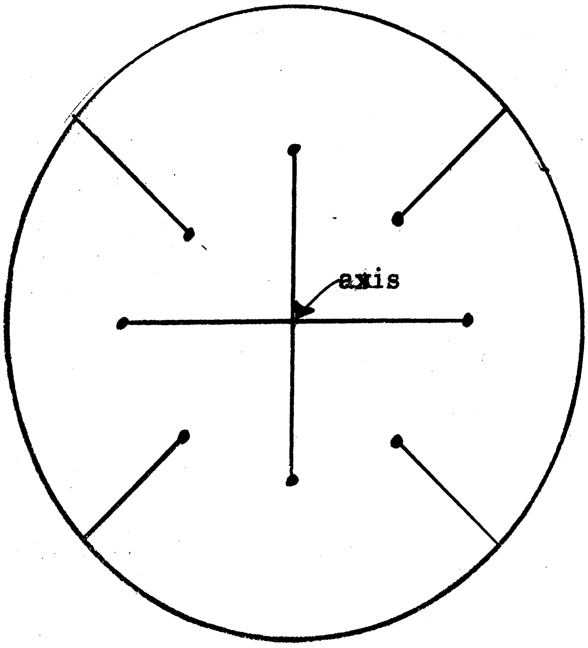


Fig. 3.4.2-1A A lens to provide negative spherical aberrations in one plane. In this device V varies as $r^4 \cos 2\phi$, E varies as $r^2 \cos\phi$, thus providing negative spherical aberration in one plane.

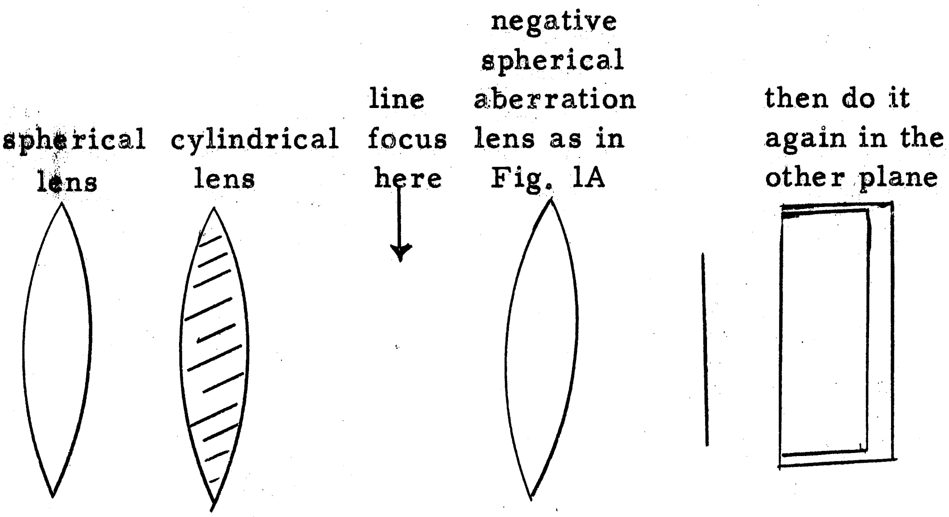


Fig. 3.4.2-1B Method of using the Fig. 3.4.2-1A lens providing negative spherical aberration.

(b) Another possibility that has been proposed is to put an electrode along the axis, and use only skew electrons for the image, or perhaps to put an electrode on the axis that electrons can penetrate.

(c) The use of space charge properly controlled, a scheme suggested by Dr. Gabor. This does not seem very practical.

In the stabilization circuitry, they have to be very careful to avoid oscillations in the amplifiers. They stated in this connection a safe criterion for non-oscillation, as follows:

The longest time constant in the loop must be n times longer than the next longest time constant in the loop, where n is the desired amplification. This is a completely safe condition. They use here gains up to 50,000. They also have a Metropolitan-Vickers commercially manufactured electron microscope, also a very old experimental one.

3.4.3 A network computer Their network computer design is similar to that employed by Whinnery, in that it consists of an array of network points interconnected by means of resistors or for that matter, any kind of uniform impedances. Such an array satisfies the Laplace equation and permits establishment of boundary conditions arbitrarily. Of course the precision of the solution is dependent upon the fineness of the granular structure imposed by the use of discrete points.

They have added the feature of injecting currents into the resistors to make the device solve Poisson's equation. In this way they can in fact solve any equation having the Laplace operator on one side, by a process of iteration. To solve Poisson's equation, they guess at the space charge distribution, solve Poisson's equation for this space charge, and where the electrons would go in the resulting field. Either the electrons going as indicated produce the postulated space charge or not. The correct solution is one in which the field causes the electrons to pursue such paths as to produce the space charge assumed in the first place.

They find it possible to have variations in the resistors statistically averaged out. Thus they use 0.1% resistors, but are able to get overall precisions of 1 part in 10,000 (or was it one part in 100,000?).

They can also use this network computer for solving for the microwave field distribution in a cavity. In this case the differential equation again has a function of some sort on the right, requiring setting of currents and iteration. They have a telephone switch scanning system which displays the error on a long persistence oscilloscope. They adjust the currents until the error is gone.

They have another similar network computer board for solving the fourth-order equation of a stressed beam. In principle this employs two meshes coupled to one another, one made up of high resistances, the other of low resistances. Two boundary conditions can be established along the edges; the other boundary conditions are to be found.

3.4.4 Van de Graff particle accelerators. They have and are continually working on two Van de Graff machines, one a large one in which they are now getting between 3 and 3.5 mev, current 100 microamperes. Their immediate ambition is to get up to 5 mev.

The small Van de Graff machine, which operates at about 0.5 mev or less, was built as a scale model prototype of the big one, in order to give them a maximum of experience before going to the big one. They have, however, found it possible to do very interesting research work employing the low-voltage unit, so that it is kept continuously in use.

The present limitation to around 3 mev on the large machine is due to voltage breakdown in the interior. They believe they can overcome this, and eventually get up not only to the 5 mev design figure, but perhaps even to 6 mev.

They use instrumentation which provides a current balance, accounting for all of the current carried by the belt (of the order of 200 microamperes). This highest voltage operation they are able to achieve does not occur at the best vacuum. The best performance as to supporting a voltage is at an intermediate vacuum, which as I remember is around 10^{-4} millimeters of mercury. They attribute the difficulty to the effects of X-rays.

During normal operation of the apparatus there exists of course a very intense flux of X-rays throughout the interior of the control chamber and the apparatus contained therein. The control apparatus is of course mounted at the top of the column in a chamber within a heavily shielded enclosure, for protection of the external surroundings against X-rays. The positive ion source is in the control chamber at the top, the ions moving downward from the control chamber under the influence of the high-voltage field. The man in charge of the device believes the undesired voltage breakdown involves in some way electrons traversing the accelerating region from bottom to top, causing X-rays, some of which radiate downward and produce more electrons. They have made tests which indicate that enough such electrons originate in the gas of the accelerating region (that is, not at the bottom plate) to sustain the desired condition. Thus the experimental behavior is such as to demonstrate that the breakdown is not a result of events occurring at the bottom plate.

The research work with the low-voltage Van de Graff machine is in the nature of observing interesting line spectra (energy spectra) discoverably with energies of the order achievable in the small machine.

3.5 Physics Laboratory, University College, University of London, Gower Street, London

Friday, 28 August, 1953

3.5.1 Personnel contacted. The visit to the Physics Laboratory at University College grew out of an Upper Atmosphere Rocket Research meeting held at Oxford, August 24th to 26th. That conference is not reported on herein, as it did not deal with vacuum tubes or related subjects. However, because of the ionosphere studies, there did appear an involvement in gaseous conduction problems. As a result of this it seemed desirable to visit the laboratories at University College. As the techniques used there are of interest in gaseous conduction problems, both of the vacuum tube type and in the ionosphere, they are reported on here. The man in charge is Dr. H.S.W. Massey, who had been responsible during the past eighteen months for arranging the meeting at Oxford. Personnel contacted included:

Dr. H.S.W. Massey, professor of physics and in charge of the laboratories.

Dr. Boyd, who assisted Dr. Massey in organizing the conference, and showed us through the laboratories at Gower Street.

Dr. Hasted, experimenting on metastable collisions.

3.5.2 Measurements of collision probabilities. At the Physics Laboratory at Gower Street several men are studying collision probabilities (that is, collision cross sections). They showed me experimental arrangements for producing beams of positive and negative ions, and of metastable atoms, as well as electron beams having various properties.

Included in this effort have been some attempts to measure the Townsend beta coefficient, that is, the production of new ions per unit distance of an ion advance, but with no real success. It has been found very difficult to measure this quantity because it is so extremely small that it is masked by other effects. Thus the staff have come to feel they are justified in ignoring it.

In one apparatus they are measuring collision probabilities in atomic hydrogen at low pressures. The reason for doing this is that this particular collision probability is calculable on the basis of quantum mechanics, and they want to compare their results with predictions of the theory. To do this they must know how much atomic hydrogen there is in their mixture of molecular and atomic hydrogen. This has been determined by drawing a sample of the mixture, subjecting the sample to conditions in which the atomic hydrogen is allowed to recombine into molecular hydrogen, and measuring the heat produced in a microcalorimeter.

Dr. Boyd gets negative ions for experimental study by providing a lateral aperture to a low energy discharge, and sorting the negative ions out from the positive ions by the use of electric and magnetic fields. They find that negative ion attachment energies for atomic gases run to perhaps 2 or 3 electron volts. The similar energy of attachment for molecular negative ions (molecular oxygen, nitrogen, or hydrogen) is of the order of one volt. He seemed unfamiliar with the idea proposed by Dr. Bennett at the Bureau of Standards some time ago that it was nearly impossible for negative ions to exist in close juxtaposition with a hot cathode.

Boyd has worked extensively with probes in gas discharges. Illustrative of his work was a hydrogen discharge tube into which a probe was inserted for the purpose of measuring the energy distribution of the electrons. In striated discharges he observes an electron energy distribution curve having a double hump. There is first a low energy hump, exhibiting an approximately Maxwellian distribution, then a high energy hump which exhibits almost a line spectrum distribution, in the neighborhood of 10 volts. The 10 volts of energy is believed to be acquired in falling through the step between striations. As he moves the probe along the discharge the 10 volt hump disappears, then reappears, repeating this behavior with the spacial periodicity of the striations. Actually, in the apparatus, he does not move the probe through the striations but rather moves the striations past the probe by slight changes in pressure. He mentioned making studies by means of a probe discharge in equilibrium with the discharge, for example, employing sodium vapor in a heated tungsten enclosure.

Dr. Hasted discussed recombination, and collisions with metastable atoms. He feels he has confirmed the familiar point of view that recombination is relatively probable if the energy exchange is small (this is a familiar concept). He has had considerable experience in analysis and experimentation with systems in which metastable atoms produce ions of other gases. He gets beams of metastable atoms by lateral escape from discharges, but of course these metastable atoms have only thermal energies.

Their measurements of collision cross section sometimes involve ion beam currents as low as 10^{-12} amperes. This leads to collision product currents of only 10^{-14} or 10^{-15} ampere.

They measure these currents by chopping and then using an AC amplifier, or with an electrometer circuit. They say they cannot get dependable high resistors, such as we are able to obtain in the U.S., for use in their input circuits.

We discussed briefly the solution of the differential equation for ion density, employing the Laplace operator as in the ambipolar diffusion equations, but for a situation in which no ionization is taking place. This is the condition during the decay of plasma, as in a thyratron during control recovery (deionization). He was not familiar with the use of this transient form of the differential equation but the approach seemed reasonable to him.

In his atomic hydrogen collision probability measurements and ion beam measurements he used the magnetic mass spectrograph principle very freely. He also sometimes used equipment similar in principle to the Bennett mass spectrometer. He believes they were using this for some time before Bennett began employing it.

3.6 Standard Telephones and Cables, Research Laboratory, Enfield, England

Monday, 31 August, 1953

3.6.1 Personnel contacted. The important material to be covered here, as to activities at Enfield, have to do with techniques; the only person contacted relative to techniques at Enfield was:

Mr. A.H.W. Beck, in charge of that portion of the research activity for Standard Telephones that is located at Enfield, and the author of a very excellent book on velocity modulation tubes.

3.6.2 Vacuum pumping and leak detector technique. Mr. Beck's studies of the production of high density beams has attracted attention to high degrees of vacuum. The reason for this is that in high density beams even with very little gas present, enough ionization takes place to enable one to see visibly the outline of the electron beam from the light produced. This light of course comes from excitation at collision with gas particles. He is able to make such observations in some cases with a vacuum as good as 10^{-7} mm of mercury. It is his belief that the vacuum in a well-exhausted vacuum tube is about 10^{-8} mm of mercury. He thinks that in many cases the vacuum is better than is believed commonly, because of the inability of vacuum gauges to measure the existing vacuum.

In his laboratory operations, when working on hard tubes, that is, tubes operating at a high vacuum, he uses a mercury diffusion pump and liquid oxygen. It was interesting to me to find that in England, universally, liquid oxygen rather than liquid nitrogen is used for cooling purposes, whereas in the United States liquid nitrogen is commonly used.

In operations on gas tubes he uses oil diffusion pumps, rather than mercury diffusion pumps.

He employs a leak detector of this own development, as published in the technical press. In his leak detector, using butane, he employs a magnetic ionization gauge, reading to 10^{-8} mm of mercury. In normal use the ion gauge employs perhaps 5000 v and several hundred gauss, whereas in leak detector use the voltage and magnetic field are less, being down to one-half or one-third the values as used in the gauge. He finds that butane is better than hydrogen for leak detector use.

He has given consideration to a scheme for controlling the amount of gas in a system in order to employ the presence of ions usefully in connection with high density electron beam focusing. One possibility is the use of a thoria getter, impregnated with hydrogen, to maintain a constant pressure of molecular hydrogen within the tube.

3.7 Standard Telephone and Cables, Ilminister, England

Tuesday, 1 September 1953

3.7.1 Personnel contacted. There is discussed here a gas counting tube. Other comments on the Ilminister work appear in Chapters 2 and 4. As far as the presently discussed item is concerned, the personnel are:

Mr. Ullrich, acting director

Mr. Foulkes

Mr. Creppes,

3.7.2 Gas counting or stepping tube. Their gas counter tube employs about 7% argon, 1% hydrogen, the rest neon. This given a mixture at about the bottom of the Penning neon-argon curve.

Their most important trouble with this tube has been the appearance of occasional minus 1 counts, that is, with three tubes in parallel, one tube will count 1 more than the other two, in perhaps 300 hours. We discussed the possible reasons for this in considerable detail. Fig. 3.7.2-1 is a sketch of electrode shapes. At each impulse the discharge advances one section.

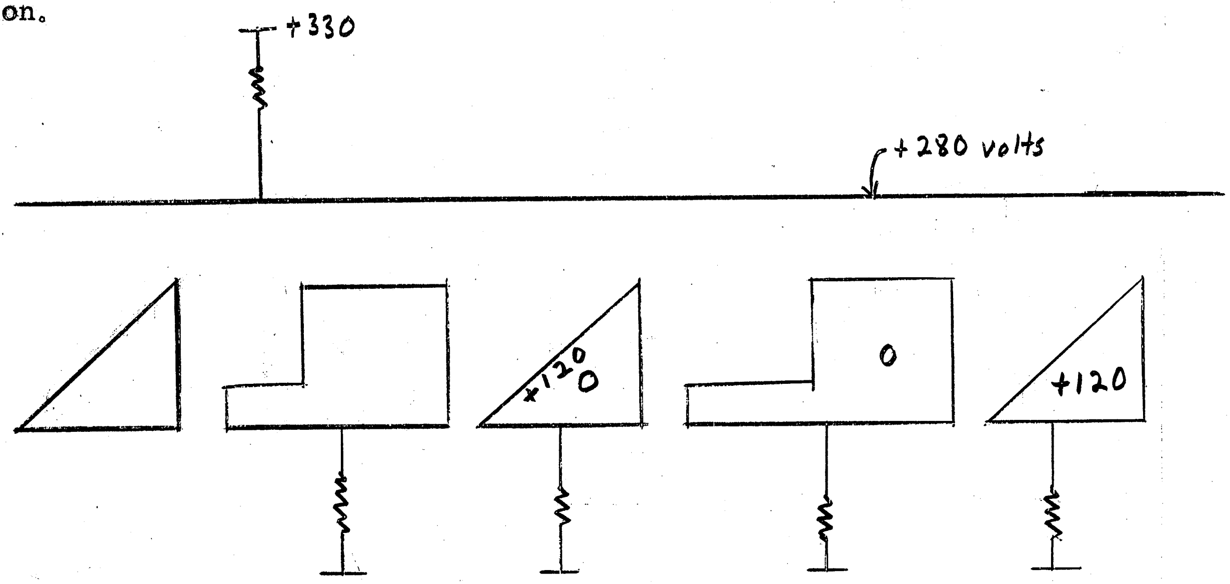


Fig. 3.7.2-1 Electrodes in the S.T. and C. gas counting tube.

In some of the earlier devices they had trouble during initial operations. At first the low voltage would be 150 volts, but with continuous operation it would rise gradually to 180 volts after many hours, perhaps hundreds of hours. They finally cured this by operating for quite a long time (perhaps two hours) with an AC current flowing continuously between two of the electrodes. The voltage gradually rose until it stabilized at the ultimate 180 volt value. They attribute this to atomic hydrogen, presumably produced by the discharge, going to the electrode and affecting the glow discharge potential. The discharge is a normal glow discharge, not an abnormal glow discharge. At 180 volts the voltage is characteristic of the gas (mixture neon, argon, and hydrogen) and a modified surface.

The question was discussed, was this penetration of hydrogen into the surface, or was it a chemical composition on the surface? They did not really think it was a hydride.

They have had trouble with the purity of the argon and the neon, being more or less at the mercy of their one supplier, the British Oxygen Company, who say that the argon and neon are 99.9% "spectrally pure" (this does not mean spectroscopically pure). The neon is made for neon sign lights, and they are not in a position to put specifications on its purity.

3.8 Mullard Corporation, Discussion with Mr. Malleson at dinner in London

Thursday, 3 September 1953

3.8.1 Personnel contacted. Mr. Malleson, manager of the division of the Mullard Corporation that deals with government, commercial, and industrial tubes as distinct from tubes in the entertainment industry.

3.8.2 Comments on the use of Germanium Diode. Mr. Malleson described an experience involving the use of germanium diodes. He mentioned an electronic industrial equipment manufactured by people in his department, and showed to him in a form which used many thermionic diodes. He said: "Why not use germanium diodes"? The answer was they had tried it, but the apparatus ran so hot that the germanium diodes gave a lot of trouble.

3.9 British Thomson-Houston Company, Research Laboratory, Rugby, England

Monday and Tuesday, 7 and 8 September 1953

3.9.1 Personnel contacted. At the British Thomson-Houston Company I visited the research laboratory. However, there was a close relationship between the work of what they call their research laboratory and development engineering. There was a much closer tie between the research personnel as such, and the people actually developing new vacuum tubes, than exists for example between the General Electric research laboratory at the Knolls in Schenectady and the vacuum tube engineering department in the Schenectady works. The people dealt with as to miscellaneous types of tubes were as follows:

Dr. Davis, director of research.

Dr. Wilkinson, deputy director of research, who was responsible for the details of my trip through the laboratory.

Mr. H. deB. Knight, in charge of the work on converters for industrial service. I had an introduction to Mr. Knight from Mr. H. C. Steiner of the General Electric Company, presently chairman of the Electronics Committee of the AIEE.

Mr. Whiteley, dealing with applications of industrial tubes and magnetic amplifiers.

Mr. Melville, working on circuit applications of magnetic amplifiers.

3.9.2 Electronic power converter tubes. We discussed their work on large electronic power converters of the steel tank type, which have in some cases of course several anodes in the same tank, ratings now from several hundred to several thousand kilowatts. As is done everywhere, they now employ grids in front of the anodes, both to suppress backfire, and to provide some measure of grid control. However, we devoted primarily attention to the effect in suppressing backfire.

It is a familiar fact in electronic power converter work that the use of grids to suppress backfire causes there to be an increased voltage drop in the tube, which causes a larger dissipation. Their experience is that as they make the grids more powerful, that is, deeper in the direction of flow of the electron plasma current, they gain an improvement of backfire suppression at first very much more rapidly than they loose in dissipation. This means that a reasonably deep grid profits them more in the direction of decreasing the gradient at the anode surface than it costs them in the way of increasing the voltage drop through the grid.

All of their steel tank rectifiers now are sealed off devices, none of them continuously pumped. They have standardized on 6 anodes, with one grid per anode. The performance is better than with continuously pumped devices. The metal exposed does the necessary clean-up job for getting rid of residual gases.

Their practice is standard and similar to that in the United States as to use of ignitor anodes in ignitrons. In some applications the statistical nature of the firing of the ignitrons offer difficulties. This is particularly true in an X-ray application, where the apparatus must start off always on the first voltage application, after several hours or days of idleness. That is, they cannot afford to have the apparatus hesitate and misfire a few times after it has been standing idle for some time.

3.9.3 Magnetic amplifiers vs. thyratrons or ignitrons. They have in a number of applications removed the thyratrons of the ignitron firing circuits and used instead magnetic amplifier techniques. They showed me a magnetic amplifier chassis, airborne, operating on 1500 cycles, for two voltage-regulator and one speed-regulator application. In this case the magnetic amplifier is 25% heavier, and costs more than the corresponding device incorporating vacuum tubes. Below 100 kilowatts, they tend to use tubes rather than magnetic amplifiers, on 50 cycle applications.

The application people at the British Thomson-Houston company, when asked about Ramey's paper on magnetic amplifiers, stated that they do not like Ramey's "trick" circuit, because they have found no application for it. (Of course the chief merit of Ramey's paper was his method of description of magnetic amplifiers, not his particular circuit). One of the difficulties in using magnetic amplifiers for small applications is the necessity for using very small wires in the coil. Even when low impedances are used, it is still necessary to use small wires (3.2 mills in diameter in some cases).

Regarding thyratrons, at the British Thomson-Houston Company, they have found that on straight DC discharges they do not get rapid cleanup. Thus the presumption is that cleanup does not occur during the period when pulse current is passing at low plate voltages, but occurs at other points during the pulse.

3.9.4 Radar modulators. They have often used at Rugby for radar modulators spark gap modulators at pressures from 1/2 atmosphere to two atmospheres. Also they have used protective gaps rated at 5000 volts and at 16,000 volts, to provide protection in case the magnetrons do not function. At one time they tried a circuit employing a rectifier in series with a modulator, and found it not necessary.

Melville gave me a reprint of his paper containing circuits for pulsing radar by means of ignitrons, also by magnetic amplifiers.

3.9.5 Vacuum technique matters. There has been a change sometime fairly recently from the use of housekeeper seals, as was practiced in England for a long time, to a rather wide present use of fernico seals. In the use of fernico there is a problem of copper plating. Some find that the glass doesn't stick if the plating is thick enough to do any good.

In building steel tank rectifiers, in order to keep the vacuum tight, they are careful to have grain orientation at right angles to the direction of air penetration. Also they never use along the same seam, an inside weld and outside weld, as this gives a chance for air to be trapped between the two welds.

CHAPTER IV

Microwave Tubes

4.0 Nature of Material in this Chapter.

In this chapter there will be gathered together the reporting on comments on microwave tubes that can be considered unclassified, made at various places on the Continent and in England. The material is arranged according to sequence of dates and with the place name.

4.1 Laboratoire Central de Telecommunications (LCT)

46 Avenue de Breteuil, Paris Tuesday, 21 July 1953

4.1.1 Personnel contacted. This organization is the research laboratory of the French subsidiary of the International Telephone and Telegraph Company. Personnel contacted at LCT were:

Georges Chevigny, assistant director of the research laboratories at Paris,

Gerard Lehmann, member of the research staff of LCT, and chief engineer of Societe des Servomecanismes Electroniques, Ter Rue Chanez, Paris XVI.

E.M. Deloraine, Technical director for the International Telephone and Telegraph Corp.

Mr. Beckman, responsible for electron tube work under Chevigny's direction.

For general comments regarding electron tube research and development by I.T.andT. in Europe generally, see Chapter I.

4.1.2 Organizational comments. Mr. Chevigny and Dr. Lehmann and Mr. Delaraine were in a sense old friends of mine, as they had been located at 67 Broad Street, New York City, during World War II, in the research laboratories of the Federal Telecommunications Corp., the U.S. subsidiary of I.T. and T. During World War II I was on the staff of the Radio Research Laboratories at Harvard University;; Division 15 of the NDRC, of which the Radio Research Laboratory was a part, had contracts with Federal for vacuum tube research and developments, the work being carried out by Chevigny and Lehmann. In the course of this work Chevigny had been responsible for the design and building of the vacuum tubes, whereas Lehmann had been responsible for the theoretical work and for the measurements on the tubes after construction. These World War II tubes had been ultra-high-frequency triodes and tetrodes for operation at several hundred watts. Their effort had been to try to make, at a power level of a few hundred watts and at voltages of perhaps 2000 or 3000 volts, tetrodes operating comparably as to efficiency, etc., to the resnotrons used in the Tuba equipment sent to England for British use.

Thus I had learned during the war that Chevigny is a man of very considerable competence as a director of vacuum tube research and development, and that Lehmann is a very good theoretical man, particularly successful in designing and having built instrumentation equipment, and in interpreting the results of measurements made therewith. Their work during World War II had been in ultra-high-frequency tubes at frequencies below 1000 megacycles, using enclosed circuits and cavities.

Mr. Deloraine had been the director of the research activity in New York City.

The LCT laboratories are clean, orderly in arrangement, air conditioned where necessary, and generally modern in a modern building. They did not however have UHF or microwave test equipment on anything like the scale that exists at the Compagnie Generale de Telegraphie San Fils; they do not need for the work at LCT the extensive microwave equipment that Warnecke has. They have reasonably adequate test equipment for the tubes they are developing.

The LCT laboratories are well equipped with modern glass lathes, their microwave tube assembly is done in a room that is well planned and staffed with skilled people. They use jigs and machine tool attachments extensively and successfully, but on uhf tubes at least do not employ expensive mass production machinery.

4.1.3 UHF tubes to be interchangeable with U.S. types 2C39, 2C43, 4X150A. The LCT activities in Paris appear to include manufacturing, in fact their entire tube research and development activity is very closely linked with manufacturing programs. They carry out research and development work only on tubes that are expected soon to be in the manufacturing stage.

Chevigny called particular attention to the following tubes:

(1) Tetrode: LCT no. 3.861B, similar to and socketwise interchangeable with the Eimac 4X150A. They expect to have tubes of this type available from their manufacturing units by the end of 1953.

(2) Triode: LCT No. 3.852A, similar to and interchangeable socketwise with the U.S. 2C39A; they have plenty of tubes of this type in stock now.

(3) Triode: LCT No. 3.881A, similar to and interchangeable socketwise with U.S. 2C43. It was expected in July that manufactured tubes would be available by the end of 1953.

One of the economic factors controlling the LCT activities at present is that the NATO organization calls for interchangeability of tubes between military equipment made by the various NATO countries. Thus mention was made at LCT of the fact that the three tubes LCT 3.881B, similar to Eimac 4X150A, LCT 3.852A, interchangeable with US 2C39A, and LCT 3.881A, interchangeable with 2C43, must meet JAN specifications. How literally this is to be taken I do not know. On the whole, I think the specifications they are putting on these tubes are in some respects more severe than the JAN specifications on the comparable US tubes.

Within the I.T. and T. organization, and for that matter in France as a national unit, the LCT group is the appropriate one to take care of this program, because of the extensive experience Chevigny and Lehmann had with this general type of tube in the United States during World War II, and to some extent before World War II.

It is possible in Paris for Chevigny to do something he wanted to do in the United States (1943-45) but never could, that is, to have the tube manufacturing operations carried out very closely paralleling his laboratory development operations. His development laboratory for the three tubes mentioned above is in effect a miniature edition of what the manufacturing set up will be. The reason they can do this in Paris and could not in the United States is that the manufacturing operations of these tubes are not so highly mechanized as would be true in the U.S.

Chevigny and Beckman are very proud of their work on the 3.861B (4X150A counterpart) and feel that they will have a tube which will be generally superior to the 4X150A, and particularly more uniform in characteristics from tube to tube. My judgment is that they may very well be right, in regard to such things as grid emission (of course they use gold plated grids) and various other limiting properties of the tubes.

I asked Chevigny if he expected to sell these 3.861B tubes in the United States. His answer was that this was not the objective of their development. Their objective is to provide a source of such tubes that does not require payment in dollars. However, they will of course be very glad to sell any of these tubes to the United States if we want them. He anticipated that the price in the United States for the 3.861B would be a little higher than the price of the 4X150A, for they do not plan to try to be competitive in price within the United States.

In my judgment, it may very well turn out that for certain experimental purposes the 3.861B may do things that the 4X150A will not; this may be of value in the U.S. Also, it may be that competition with the 4X150A, in performance, if not in price, will have a useful effect on Eimac, providing the 3.186B does turn out in fact to be superior in interesting respects to the 4X150A. Thus I believe it would be desirable for the Signal Corps to obtain some 3.186B tubes and make an evaluation of them relative to the Eimac 4X150A.

4.1.4 UHF Triode for use in a coaxial line grounded cathode amplifier. Beckman showed me a UHF tube built in a way to permit use in a coaxial line as a grounded cathode amplifier. Thus the grid was connected to the inner cylinder of the input coaxial line, and the cathode to the outer cylinder. This means that the connection to the grid of the tube had to come through the connection to the cathode. The connection from the outer cylinder to the cathode was a metal disk; there were two small holes in the disk diametrically opposite through which two wires came making the UHF connection to the grid. The grounded cathode amplifier just described did not register with me very clearly, and I am not completely sure that I have described the detail connections entirely correctly. However, there is no question but what they had a device which they believed could be used successfully at uhf microwave frequencies as a grounded cathode amplifier inserted into a coaxial line. This had a carrier wave telephone objective. The frequencies involved may have been uhf frequencies of the order of 1000 megacycles rather than being in the order of 3000 megacycles.

4.1.5 Close-spaced triodes. They showed me a technique they are using for obtaining a grid cathode spacing of 0.13 millimeter (this is about 5 mils). The problem here is one of obtaining a controlled small spacing, yet having a glass vacuum seal across UHF or power entrance at the input. In general their technique was to make the grid seal first (a kovar to glass to kovar seal) then, in a machine operation involving grinding, obtain a precise spacing. Thus the irregularities of the glass seal are taken care of by introducing precision in the spacing after the seal is made. Then the middle parts that have been ground to precision are soldered (I believe they use gold to solder) in an induction furnace. I was told they have been successful in making this gold solder joint very close to the glass seal. All this implies of course that the precision is maintained, or at least a precisely predictable positional relationship obtained, during the soldering operation.

They have ambitions toward developing this process so as to give them precise enough control to make a tube having a spacing of the order of one mil. That is, they wish to make a tube that is competitive as to closeness of spacing with the Western Electric 416A tube, but using techniques very different from those used in the 416A.

4.2 Compagnie Generale de Telegraphie Sans Fils (CSF), Research Laboratory

23 Rue de Maroc, Paris. Wednesday, 22 July, 1953

4.2.1 Personnel contacted. The following individuals, together with some others whose names are not recalled, were interviewed at CSF:

Dr. R. Warnecke, director of the microwave research laboratory.

Dr. Doehler, who has their primary theoretical strength in the magnetic and carcinatron area.

Dr. Guenard, who has considerable microwave circuit theory strength, and carries much of the managerial load for Warnecke, but is not theoretically up to Doehler. He attended the Electron Tube Research Conference at Stanford in June, 1953.

Peter Braillard, secretary to the president of the company. Braillard, who is a young man, has no great technical strength, but is an extremely able "leg man" for the president of the company. He was with us at lunch and made it his business to see that I was looked after.

Dr. Bernier, in charge of their millimeter wave work. Warnecke described Bernier as being a "student of his". This has to do with Warnecke's activities as a member of the faculty of the University of Paris.

Mr. Goldberger, an American (U.S. citizen) engineer who has spent a year with Warnecke in Paris, during the fall of 1953 returned to New York, and is associated with CSF's commercial representative, the American Broadcasting Company.

4.2.2 General comments on the CSF microwave research activities. The CSF research activities in microwaves is very extensive; there are about 165 people on Warnecke's staff, most of them on microwaves. They are working on traveling wave amplifiers, on reflex iclylstron oscillators, on klystron amplifiers, on millimeter wave oscillators and amplifiers, and on magnetron amplifiers, and on carcinotrons.

Warnecke is very imaginative, and a man of apparently unlimited energy. He appears to have built the microwave laboratory up from nothing, and is very much its prime mover and effective leader. He has working for him men who are both theoretically and experimentally competent, and seems to have excellent financial support from the CSF organization. At this point it is probably in order to mention comments made elsewhere in Europe regarding the CSF organization, (or more particularly Warnecke's activities), to the effect that the CSF group have not been successful in producing useful marketable tubes.

Guenard, working directly under Warnecke, is responsible for work on the carcinotrons, reflex iclystrons, and travelling-wave amplifiers, but is not responsible for the millimeter wave work. Guenard is not nearly as well informed as to the theory of electron behavior in microwave fields as is Doehler who works for him. Guenard is reasonably well informed in regard to microwave circuitry. He is probably stronger than Doehler on experimental and pilot design work.

Guenard was at the conference on electron tube research held at Stanford University in June 1953. It was my recommendation to Warnecke, and I would like to see this endorsed by Raytheon and the Signal Corps, that in subsequent years Doehler rather than Guenard came to the Electron Tube Research Conferences. It is my feeling that Doehler has a much better understanding of the theory of the carcinotron and the magnetic amplifier, and particularly of the difficulties involved in both, than either Warnecke or Guenard.

A few years ago (around 1948-50) Doehler published a paper, or rather a series of two or three papers, giving some theoretical interpretations of magnetron behavior. It happens that I had been looking at the papers early in June 1953. I mentioned this to Doehler, saying that I thought he had given a very nice array of experimental information, and that it presented interesting factual material, but that I did not agree with his theories there presented. His reply was "Neither do I agree with them now." He would be a very useful man to have come to the United States for an exchange of views.

4.2.3 Reflex klystrons, particularly type RX-63. Guenard was quite proud of a reflex klystron, the RX-63, which they have built for commercial use at 8 centimeters. He stated that it frequency modulates linearly plus or minus 6 megacycles by voltage variation of the repeller without an appreciable change in amplitude. The output level is constant at about 8 milliwatts over this frequency range.

This reflex klystron can certainly be thought of as manufacturable. The question arises, and was not answered to me, because perhaps I did not ask it, are they actually selling any of these, or is it merely something that they have carried through the development stage and have not marketed?

4.2.4 Travelling wave amplifiers. Guenard is very proud of a completely packaged S band travelling wave amplifier that they have in their display cabinet. The solenoid coil for providing the focusing field is included as a part of the package inside the all-metal casing. One end of the casing comes off, permitting removal of the tube. It is a conventional travelling wave amplifier. The thing they are especially proud of is that it is a completely packaged unit.

This unit was of interest to me because it represents something that CSF could have for sale. I am referring to the criticism made of CSF that they do good research and development work, but don't manufacture anything in the way of microwave equipment for sale. It is true that Guenard did not talk about how many of these packaged units have been made and sold, but was proud of its existence.

4.2.5 Millimeter wave research. The millimeter wave research at CSF, in charge of Bernier, is carried on in the range between 4 millimeters and 10 millimeters. The emphasis is on devices of the travelling wave type, particularly of the backward wave oscillator type. The group are very familiar with work of this nature reported by the Bell Telephone Laboratories. They showed me a structure built to operate as the Bell Laboratories devices operate. The circuit grid is so fine that in order to see what is required it must be viewed through a microscope. The appearance is as indicated on fig. 4.2.5-1.

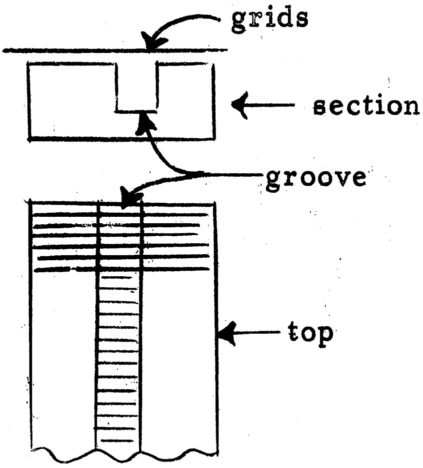


Fig. 4.2.5-1 Grids across the top.

They report considerable success in producing oscillations in the 8 millimeter range. They are very well equipped as to instrumentation in this frequency range. They have 8 millimeter wave guide, frequency meters, oscilloscopes, presentations of tube performance, etc.

However, they are, like everybody else, working at very low power levels, and when I asked the question "How does one get high continuous power levels at these frequencies?" they had no answer, at least none they were willing to give me.

4.3 Brown Boveri Manufacturing Company, Baden, Switzerland

Tuesday, 4 August, 1953

4.3.1 Personnel contacted. The following men took part in showing me around at the Brown Boveri plant:

Dr. Theodore Boveri, a director of the company, and in charge of engineering and manufacturing. He is a son of the man who founded the company.

Mr. Kesselring, a relatively young man in charge of the electron tube work, possibly also of some of the electronic equipment work.

Their "reception engineer", a man whose name I do not remember, about 55 to 60 years of age with a long background of engineering experience, who has the primary responsibility of seeing that technically minded visitors like myself see what they want to of the plant and the engineering staff personnel.

Two assistants to Dr. Ludi, who is in charge of the microwave tube work and is the inventor of the "turbator", an interdigital microwave magnetron. Dr. Ludi was out of the city on the day of my visit.

4.3.2. The turbator (interdigital magnetron oscillator). The turbator, invented by Dr. Ludi in 1938, is in its present manufactured design a 12-anode interdigital magnetron, made of pressed metal parts as shown in Fig. 4.3.2-1, the supporting members being ring-shaped. The whole

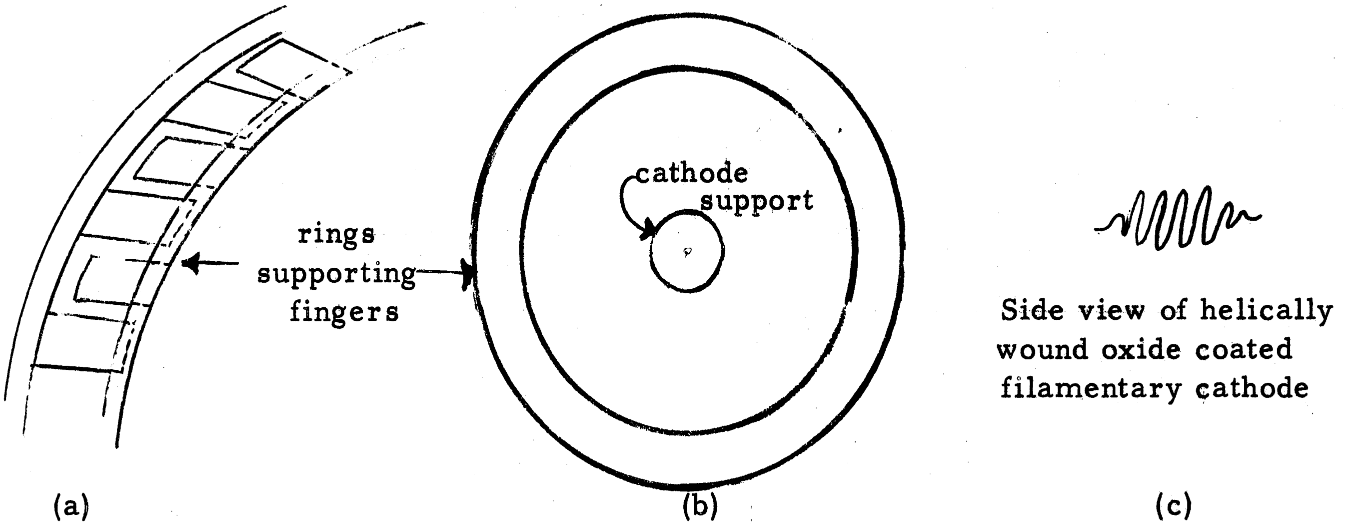


Fig. 4.3.2-1 The turbator.

assembly appears, looking along the axis, as in fig. 4.3.2-1b. Fig. 4.3.2-1a is an attempt at a perspective sketch, showing how the fingers are attached to the rings that support them. The cathode is an oxide coated filament in helical form, a bit rough in design. Fig. 4.3.2-1c is a sketch of a side view of the filament.

The output was quoted as being 10 watts. I was given commercial leaflets describing the turbator which I have kept in my files.

The frequency range is stated on the leaflets given me as 1750 to 2100 megacycles, whereas casual comments by less informed people indicated frequencies as being from 2400 to 2700 mcs. It is possible that the higher frequency is correct for the more recent commercial designs.

As indicated in fig. 4.3.2-1 the parts are all of pressed metal. They are enclosed in a glass envelope, the whole construction being very inexpensive. The device is tunable by adjustments on the Lecher wire type of circuit connected to two straight leads brought out to the envelope. Attachments of these lead wires on the inside are to properly selected fingers. The selection of the fingers of course determines the nature of the loading on the device.

Turbators are being manufactured in appreciable volume (a few hundred at a time) for use in a multichannel pulse code communication system, employing perhaps 20 channels, employed for tying together the Swiss military radar network. Dr. Boveri said that there was competition for development of this system by the Swiss subsidiary of I.T. and T. Obviously I.T. and T. had much greater knowledge of microwave systems at their disposal than does Brown Boveri, because I.T. and T. can draw on the resources of their British and French companies. The Swiss military authorities wanted to give the order to a completely Swiss company. The I.T. and T. subsidiary made a claim to being a Swiss company, and the apparatus would have been built in Switzerland. However, the engineering development would largely have been done outside of Switzerland, not adding to the knowledge of Swiss engineers. Brown Boveri obtained the contract on the basis that they would do the development work in Baden, and would thus retain the knowhow in the country.

The turbator was mentioned to me quite casually very early in discussions with Kesselring, the English-speaking and quite responsible young electronics engineer who showed me around the electronics area. He said there were two kinds of turbators, the CW 10 watt variety (actually high duty cycle pulsed), tunable, and another, not used in a tunable way, employed in radio therapy. On the way through the factory I was shown the therapy model but not the other. However, at the end of the tour through the tube factory I asked to see the 10-watt kind, and was immediately and very willingly taken back up two flights of stairs to the office of Dr. Ludi, the inventor of the turbator. Dr. Ludi was not in that day, and none of his 3 or 4 assistants spoke English. However, I was shown the parts and the internal electrode assembly of the 10-watt CW turbator, and was given reprints and catalog literature (printed in German).

Other comments regarding Brown Boveri activities will be found in Chapters 1 and 3. In Chapter 1 Dr. Boveri's comments regarding the Russians are reported, and in Chapter 3 their industrial electron tube activities are described.

4.4 Philips Glelampfabrikenen, National Research Laboratory, Eindhoven, Holland

Tuesday, 11 August 1953

4.4.1 Personnel contacted. Arrangements to see the activities at the Philips Laboratories were set up with the assistance of Dr. O.S. Duffendack, director of the Philips Laboratories at Irvington-on-Hudson. In our visits to the various microwave units in the Philips plant I met quite a number of individuals, but by and large I did not retain their names. Therefore the only name I have for the microwave work is that of Dr. Kleynen who has charge of it, and was my host and guide. Thus though other names are listed in Chapter 3, there is only one name listed here, as follows:

Dr. J.H.A. Kleynen, in charge of microwave tube research and development, who attended the Stanford Electron Tube Research Conference in June 1953, and visited in Ann Arbor shortly thereafter.

4.4.2 Close-spaced microwave triodes. In the microwave development laboratory they were bringing out a close-spaced triode built roughly in accordance with Fig. 4.4.2-1. In relation to this figure the following dimensions apply:

grid wire diameter, 2.5 microns, that is about 0.1 mil.
spacing between grid wires, 10 microns, that is, about 0.4 mil.
spacing from cathode to grid, 10 microns, about 0.4 mil.
the active structure diameter, 1 millimeter.

Thus this is a triode in which the area of exposure of cathode to grid is that of a circular disk one millimeter in diameter, having a cathode-to-grid spacing of 10 microns or about 0.4 mil. Interesting features of this are the use of wires as spacers, and the use of an L cathode carefully surfaced to provide a smooth cathode.

I was not advised what the results of tests on the first tubes were, but they seemed quite optimistic regarding the success of this development. It is obviously an attempt to compete with the Western Electric 416A triode, or perhaps obtain something superior to the 416A.

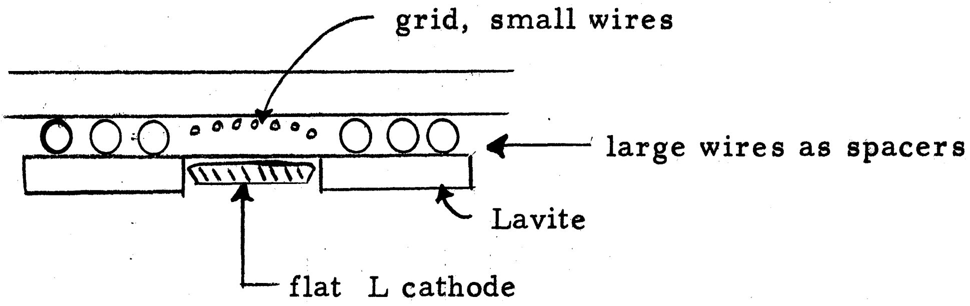


Fig. 4.4.2-1 General design plan of the Philips close-spaced planar triode.

Dr. Kleynen commented that they are now making to NATO specifications tubes of the 2C40 (or was it 2C34?) type but that they are going to change the base to permit the capacitance to be lower. At least this is a tube of the general lighthouse variety in which spacing is moderately close, but a limiting applicational feature is the capacitance. This is typical of the European tendency to work basically toward RETMA specification as to bases and general electrical characteristics, but to attempt with some success to make improvements as to other properties such as capacitances, reproducibility, resistance to vibration, insulation resistance, etc.

Of course one of the important difficulties with close-spaced triodes as to ultimate power level has to do with grid current limitations. With this in mind, I asked Kleynen why, in his opinion, gold on the grids helps to reduce secondary emission, in triodes. His reply was that he presumes it is because gold, being chemically inert, prevents the formation of emitting compounds on the surface of the grid wires. See also later comments regarding the effects of L cathodes.

4.4.3 Type L cathodes in magnetrons, and space-charge control tubes. Dr. Kleynen stated that at Philips in Eindhoven they are using L cathodes in microwave magnetrons very successfully. It is indicated that Duffendack is promoting in the U.S., through the Philips Laboratory at Irvington-on-Hudson, the use of impregnated type L cathodes. Eindhoven is at the present time making cavity type L cathodes.

I raised the question as to whether in space charge control tubes (triodes, tetrodes, etc.,) barium escapes from L cathodes and gets into the grid, causing grid emission. Kleynen's comment in reply was that he thinks that oxide cathodes also release barium which gets onto the grids, and that the reason L cathodes may sometimes appear to result in more grid emission than for similar oxide cathodes, is that the L cathodes are operated at a higher temperature, therefore the grids become hotter.

L cathodes operate in magnetrons at an actual temperature of about 1100°C (1373°K) as compared to somewhat lower temperatures (perhaps 1000°C) in other applications. He stated that in magnetrons using L cathodes he can get about 20 amperes per square centimeter of emission, in lighthouse and similar tubes (space-charge control triode) he can get around 2 amperes per square centimeter.

He showed me some magnetron anodes designed to employ a hobbed anode, 12 millimeters in diameter, using an L cathode, also a hobbed anode 8 millimeters in diameter. They make their own hobs. Kleynen said that he brought back from New York some "vitallium" metal for making hot hobbing dies. He obtained these from a concern at 224 W. 39th Street (or was it 224 E. 39th St. ?) New York City. They are making to NATO specifications some 2C40 tubes (I believe this number is correct). I made some inquiries of Dr. Kleynen relative to the somewhat negative reception that was for some time at least given to L cathodes in the United States He was quite frank to admit the existence of this negative reception, and believed that it was due to a misunderstanding. He is a very strong proponent of L cathodes, but does not hesitate to discuss their weaknesses, nor to discuss mistakes that have been made in getting them more widely used. He stated a belief that the L cathodes originally sent to the United States were rather poor ones, and gave L cathodes a bad name in the U.S. He feels it has been very difficult to overcome this initial bad name even though the L cathodes now available are much better.

He showed me an X band magnetron, of the strapped multicavity type, using an L cathode, that had been run (pulsed) for 2000 hours. In the course of this test they used up 5 hydrogen thyratrons modulator tubes. The magnetron then failed because of strap deformation, the cathode being still in perfectly good condition. They are producing regularly in the factory 4J50 magnetrons using L cathodes.

He stated that L cathodes show no harm from operating temperature-limited, but that exposure to the atmosphere makes reprocessing necessary.

In their building of cavity type L cathodes, they have tried many different types of material for packing into the cavity. At first they used barium and strontium carbonites, but the evolution of gas was so rapid and the hazard (of a mechanical force nature) to the cathode from this gas during activation was so severe that it required several hours to process the cathodes safely. Too rapid cathode processing resulted in too rapid evolution of gas, with resulting damage to the cathode. So they stopped using carbonates in the cavity; they have used in the past an oxide, now they use some mixture that he did not seem disposed to tell me about in detail. I believe, from subsequent information obtained elsewhere, that it is an aluminum compound. Undoubtedly the information regarding this detailed composition is available in the United States.

He showed me a 1.2 megawatt pulsed magnetron oscillator that has been operated successfully on a 2000 to 1 duty factor, but gets too hot at a 1000 to 1 duty factor, because of an average power limitation on the anode.

They find that it is more expensive for them to make magnetrons using an oxide cathode with sintered nickel than for them to make the L cathode. This is reported to be partly because of the problem of getting materials. (This did not completely make sense to me, but it seemed to be a firm comment on his part, and I have no means of knowing independently what their materials problem is.)

4.4.4 Magnetron oscillator with a toroidal circuit. Kleynen showed a toroidal magnetron, in his museum of early experimental devices. It worked, but has never been developed further than the original working model. It is simply a hollow tube wound as a helix, then closed into a toroid, with a single wire attached to all loops at the outermost points. It is then put into a glass envelope with a cathode in the center of the toroid. The power is coupled out by a little loop adjacent to one of the turns. It oscillated but had many modes. It was used witha magnetic field parallel to the axis of the toroid. It might be interesting to know whether the frequency was a continuous or a step function of the voltage.

4.4.5 Klystrons. They are making many klystrons, including units operating at 10 watts, five percent efficiency at 12 millimeters; 100 watts, 14% efficiency at X band; 100 watts, 20% efficiency at S band, 1000 watts at still lower frequencies. This is an illustration of the rather common use of high power klystrons in Europe. Very high power klystrons operating near 1000 megacycles are currently being made. Of course this is also true in the U.S., at the present time; however, high power klystron development in Europe preceded that in the United States by several years. For some years immediately after the war Europeans gave a great deal more attention to klystrons (except reflex klystrons) than was given in the United States. Kleynen is quite proud of their line of klystrons.

4.4.6 Microwave-frequency breakdown in waveguides. One of the younger men of considerable ability was carrying out a series of fundamental experiments on breakdown phenomena in gases at microwave frequencies in wave guides, not in cavity structures.

The power for these experiments is provided by a two-gap glystron delivering 10,000 megacycles, producing power up to 100 watts CW. This is one of the klystrons built at Eindhoven. The power is delivered by the klystron into a standard X band wave guide. After passing through a few centimeters of wave guide the power passes through a water-load type of wave guide attenuator equipped with thermometers to measure the power taken by the attenuator, built about as sketched in fig. 4.4.6-1. The resistance thermometers consist of wire wound resistors, the wires being wound on cylindrical insulated supports immersed in the water. Kleynen told me that the time constant of response of these resistance thermometers is about 1/10 of a second. This sounded a little short to me; I presume his statement was correct. The standing wave ratio of this device is very good, when used as a water load.

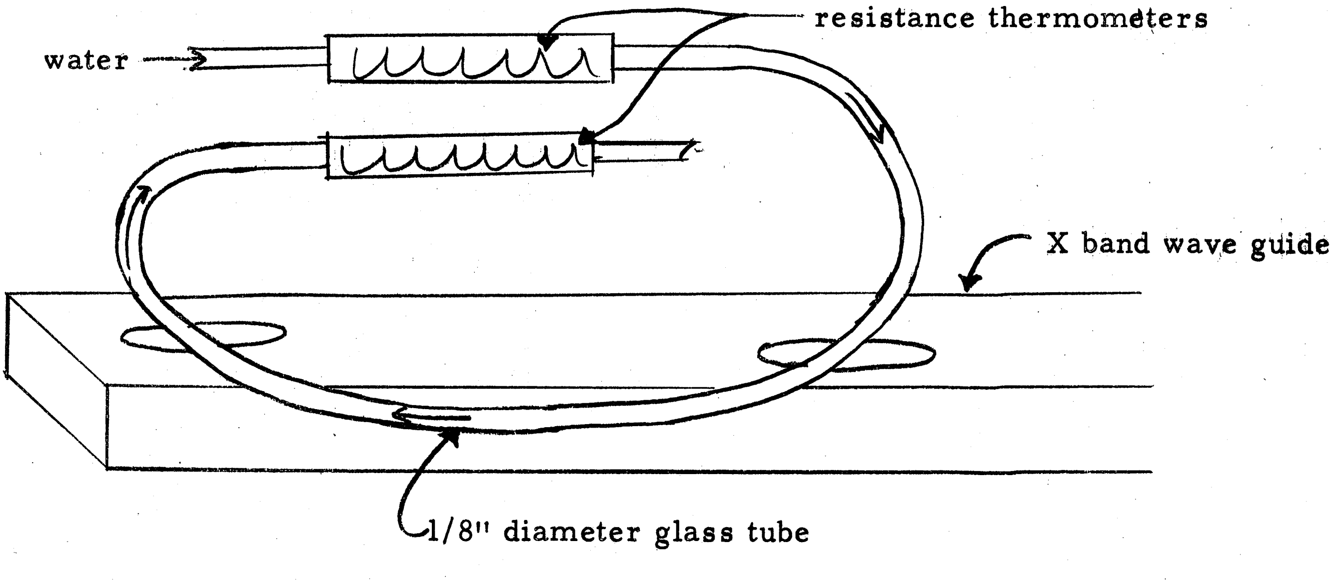


Fig. 4.4.6-1 Water-flow X band attenuator. Depth of insertion of glass tube into guide is varied to control attenuator.

Two of these devices are used in the voltage breakdown experiment, one as an attenuator, one as a water load. The amount of attenuation is controlled by varying the depth into the guide to which the glass tube is lowered (it would seem to me that if the power is to be controlled this way the position of the tube laterally rather than its depth should be adjustable, but either is of course possible.) The greatest power practically usable in the experiment is about 40 watts into the water load· to obtain this power, the klystron delivers 100 watts, the attenuator pad then taking out 60 watts. In determining each point on the curve he increased the power level gradually; breakdown occurred abruptly. The tests were all under CW conditions, not pulsed tests. His test results have been indicated in Fig. 4.4.6-2.

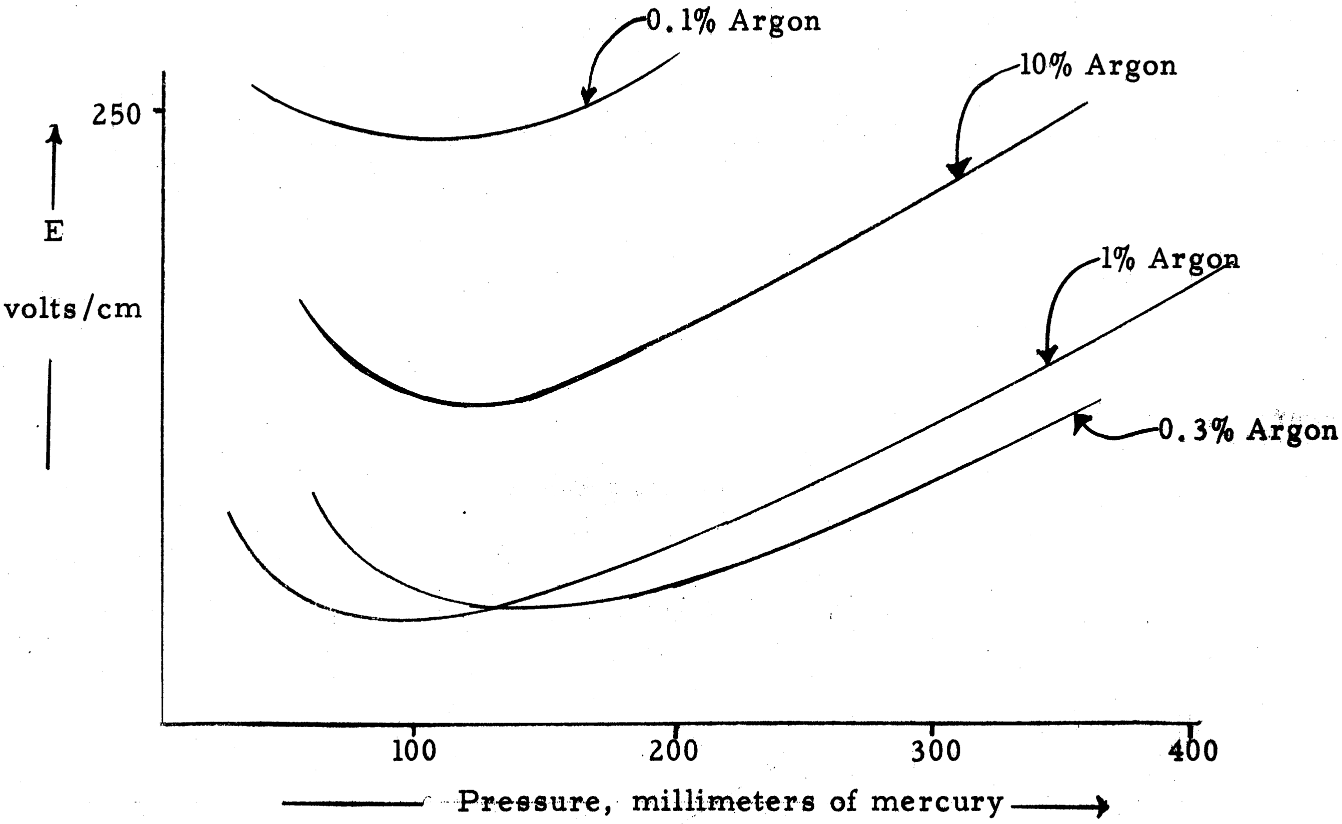


Fig. 4.4.6-2 Approximate forms of curves of field strength to cause microwave frequency breakdown in a straight section of wave guide at various pressures. Frequency 10,000 mc, standard X band waveguide used, maximum power usable for the test 40 watts CW. Gas employed was neon, with indicated percentages of argon added.

The gas used in these tests was neon with definite percentages of argon added, varying from 0.1% to 10%. Note that

(a) Every curve has a minimum, that is, a total pressure (helium and argon combined) at which breakdown occurs at the least field strength. Thus each curve has a shape generally similar to that of the Paschen's law curve for d-c breakdown.

(b) The minimum becomes less sharp as the argon content becomes less.

(c) At any given pressure there is a particular argon content which gives the least breakdown field.

(d) This argon content for least breakdown voltage at a given total pressure is in the neighborhood of .3%, but varies somewhat with total pressure (that is, the curves shown in the figure cross one another).

The pressures used ranged from about 10 millimeters of mercury to between 300 and 400 millimeters of mercury. The minima occur between 50 millimeters and 100 millimeters of mercury. The upper limit to each curve was governed by the availability of power, up to 40 watts.

At the moment breakdown occurred, it caused reflection from the point of breakdown; the discharge then moved from the point of initiation toward the power source but did not pass the attenuator. The breakdown took place in a section of plane waveguide about 8 centimeters to 10 centimeters long; thus there was no cavity involved.

4.4.7 Explanation of the shape and relative positions of the microwave breakdown curves. The experimenter at Eindhoven seemed to have no very clear explanation of these curves. There will be given in this section an explanation which I outlined to him, and which it is believed quite completely accounts for all the major features of the curves. The explanation employs the fig. 4.4.7-1 time base sawtooth concept, somewhat similar to the distance base sawtooth concept used on pages 438-442 of the second edition of my book "Fundamentals of Engineering Electronics".

Fig. 4.4.7-1 is a time base sawtooth diagram for very pure neon, that is, extremely little argon (less than 0.01%). In a microwave field, the electrons can gain energy from the field cumulatively only as a result of collisions. Thus in a microwave field if an electron starts from rest and does not experience collisions, it merely oscillates back and forth at some steady amplitude governed by the strength of the field, and its own mass. However, at a collision, it is possible for an electron to initiate a new pattern of oscillation from an initial condition possessing some energy, which is added to that obtained by the UHF field. Cumulative action of this kind can cause some of the electrons to experience a growth of energy as along the sloping front of a sawtooth in Fig. 4.4.7-1. For some fraction of the electrons this cumulative process will continue until the excitation energy is reached and exceeded. After the excitation energy is exceeded there exists an appreciable chance that excitation will occur. If excitation occurs before ionizing energy is reached, this sawtooth does not result in ionization. If however the rate of collision is moderate enough, or the rate of growth fast enough, so that a few electrons proceed on through, on a probability basis, to an energy in excess of ionizing energy V_I there can be ionization. This behavior implies a dependence of ionization rate on pressure, for given field strength. This dependence is of the same general nature as the dependence of Townsend's first ionization coefficient (Townsend's alpha) on electric field strength. Thus, by analogy,

(a) For very low pressures, at a given microwave field strength, the time rate of ion productions will be small because there will be very few collisions.

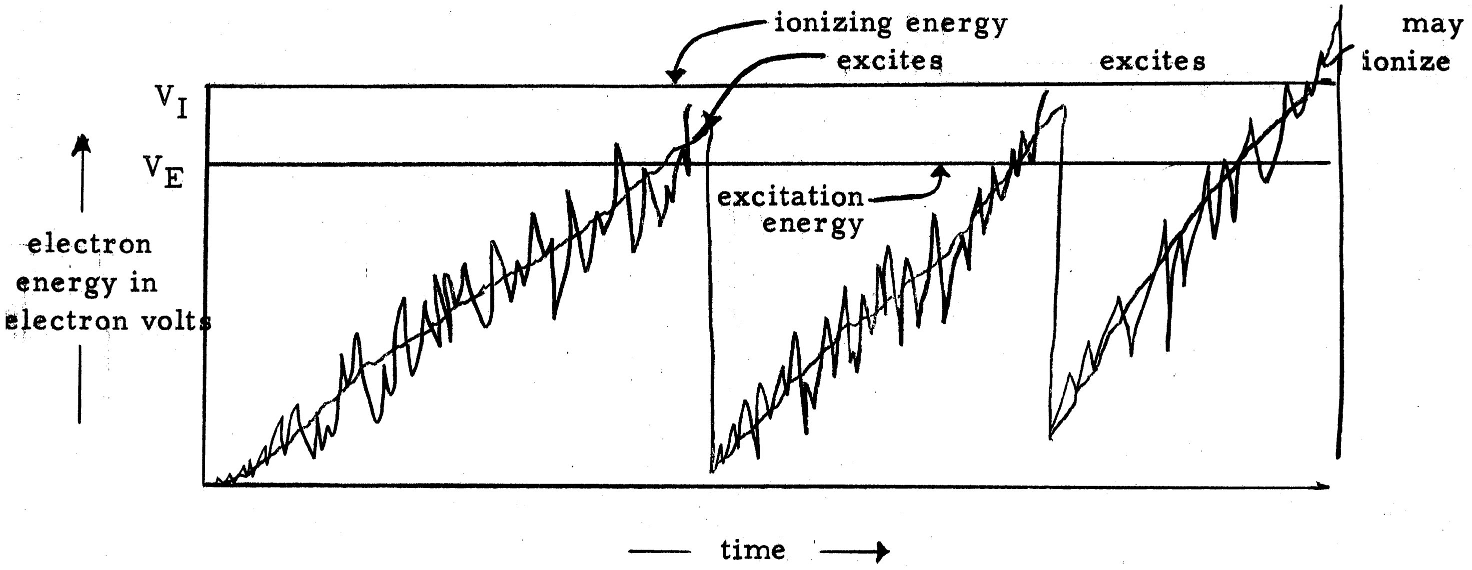


Fig. 4.4.7-1 The time base sawtooth diagram for production of excitation and ionization by microwave power in a single gas.

(b) For high pressures, the time rate of ion productions will be small because the loss of energy will take place at exciting rather than ionizing collisions.

(c) For intermediate pressure there will be a maximum in the curve of time rate of ion productions vs. pressure.

Fig. 4.4.7-2 is a diagram similar to Fig. 4.4.7-1, but drawn to account for the behavior in a mixture of both neon and argon. The ionization energy for an argon atom is less than the excitation energy for a metastable neon atom. Therefore, with argon present, each metastable neon atom will soon collide with an argon atom, and cause ionization of the argon atom, thus producing a new electron. However, as soon as an electron's energy gets above the excitation potential for argon, excitation of argon can occur, with the result that the electron loses its energy and cannot excite neon to a metastable state. As the argon content increases above small values, the probability of this happening becomes greater, so that more field strength is required to produce breakdown. This explains why there is at any given total pressure a fractional argon content that results in a less breakdown field than for any other argon content. At a higher argon content, too few electrons reach the energy needed to excite neon, whereas at a lower argon content, there are too few argon atoms present to accept energy readily from the metastable neon atoms.

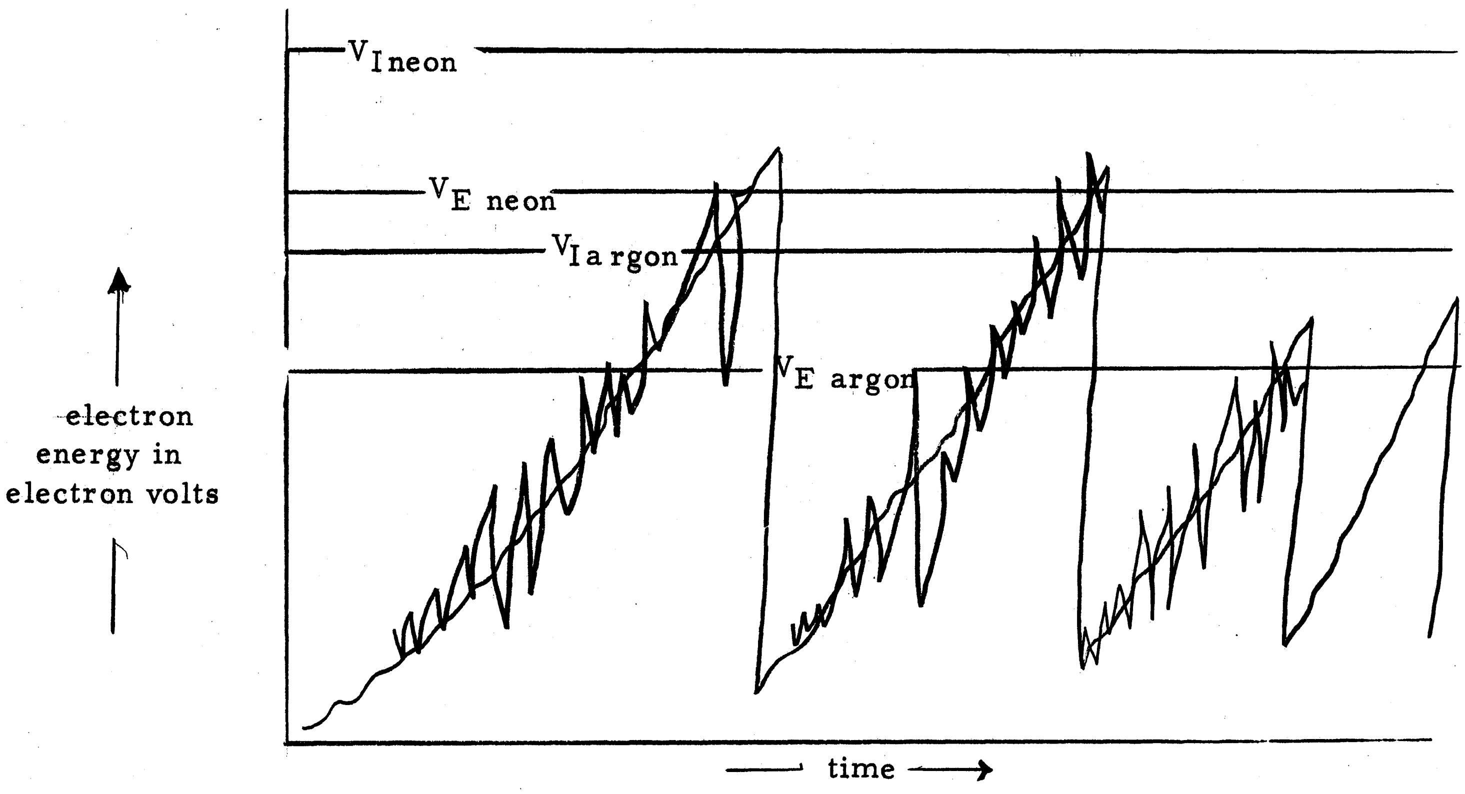


Fig. 4.4.7-2 A diagram similar to Fig. 4.4.7-1, but for a mixture of neon and argon.

4.5 Professor Alfven's Laboratory at the Royal Institute of Technology, Stockholm, Sweden

Monday, 17 August 1953

4.5.1 Personnel contacted. I was introduced to Dr. Alfven by Professor Gunnar Hok, one of my associates at the University of Michigan who was educated at the Royal Institute of Technology and was visiting in Stockholm on vacation. He accompanied me on the visit to Dr. Alfven's laboratories, and also to the A.B. Svenska Elektroner plant. I was introduced to a number of Professor Alfven's assistants, but I do not remember their names nor did I make a careful record of them. They were mostly younger men, either young members of the instructional staff or men working for a doctor's degree or both. There was a close tie between Dr. Alfven and the A.B. Svenska Elektroner. The only name mentioned here will be

Professor Alfven, an authority on astrophysics having a particular interest in the behavior of solar prominences, and generally the effect of electric and magnetic fields on the movements of excited gases in various astronomical bodies.

4.5.2 Relation of Dr. Alfven's scientific work to his electron tube inventions. Dr. Alfven's primary interest is in matters having to do with the heavenly bodies, particularly such things as the behavior of highly ionized gases, as for example in the movements of the sun and solar prominences.

He has made many studies, including some scale model studies, as to how particles may be expected to behave under various extreme conditions. He has applied these studies to engineering needs and it is out of this that has come his invention of this strophotron and of other electron tubes. The strophotron is described later under the A. B. Svenska Elektroner visit. He has also done some other work of some interest outside of the microwave field, including the invention of a counting tube.

4.6 A. B. Svenska Elektroner, Stockholm 20, Sweden

Tuesday, 18 August 1953

4.6.1 Personnel contacted. I met quite a number of people at this plant whose names I do not recall. However, the men responsible for guiding me around and for carrying out the conversation in English with me was as follows:

Signard Tomner, an engineer having apparently considerable responsibility who had been at the Electron Tube Research Conference at Stanford in June 1953.

4.6.2 General comments as to activities. Discussions at this plant were largely confined to their work on microwave tubes, or at least tubes for operation at ultra-high frequencies, or close to ultra-high frequencies. Thus their close-spaced triodes, which they referred to as comparable to our 2C51, 403A, and 404 (these being Western Electric numbers) were for ultra-high frequency or microwave relay link or concentric cable transmission of telephone signals. The whole emphasis appeared to be on communication arts, particularly those related to telephone, relay communication, and similar commercial communication as distinct from radio.

They have a very close relationship with Dr. Alfven at the Royal Institute of Technology in Sweden, which I also visited. They are building a reflex klystron somewhat similar to 1768. They were also designing for manufacture a voltage tunable tube (the strophotron), based on Alfven's work. The description of this tube will be given here rather than in the discussion of the visit to Alfven.

4.6.3 Close-spaced microwave triodes. In their factory they were building tubes which they called the 2C51, the 403A, and the 404, all being triodes.

In the tube they called the 2C51, supposed to be our RETNA number, the general design of the structure was about as shown in fig. 4.6.3-1. Here they were using for the grids tungsten wire of 0.7 mil diameter, wound 250 to the inch. The cathode-to-grid spacing was reported to be 4.5 mil plus or minus 20%. Thus they used mica spacers and conventional tube construction. These tubes were being made in a routine production operation. One girl assembled each tube completely, rather than having the work split up among different girls. The grid wires were gold plated. In another location in the plant, a sort of development laboratory, they were building, I believe experimentally, a tube somewhat similar to this, but using 0.3 mil tungsten wire for the grids, wound 350 to the inch. They showed me this grid through a microscope.

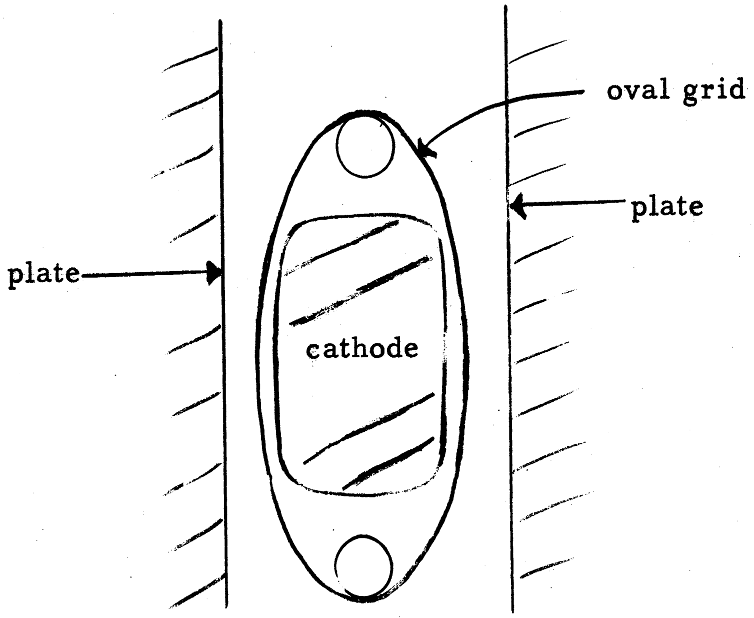


Fig. 4.6.3-1 The A.B. Svenska close-spaced triode called the 2C51.

In their work on close-spaced triodes, and in some of their work on general high frequency tetrodes and pentrodes, they do a great deal of what we call "quality control" on every tube. Thus they have a shadowgraph instrument to check the cathode and grid location on every tube. Also they adjust capacitances to meet tolerances by twisting side rods a little while the tube is in a capacitance bridge.

I asked whether they could sell these tubes, the 2C51, the 403 and the 404 in the United States either on a quality or price basis. The reply was that they thought they could compete in the United States on both the quality and price basis. This entire effort has a very strong element of emphasizing tube life, <u>dependable</u> tube life.

Their engineer in charge of tube production said that they had found they could influence tube life to an important degree by changes in grid design. There was some language difficulty in the exchange of information with him, but I gathered that he meant that misplacement or nonuniformity of grid wires would result in local cathode areas providing more than their share of transconductance. This portion of the cathode would fail early, representing a deterioration extensive enough to cause end of life. I wondered afterwards whether he implied that this deterioration of one portion of the cathode tended to poison the rest of the cathode.

4.6.4 <u>Reflex klystrons.</u> They are building a low-power reflex klystron for the Eriksson Telephone Company in Sweden, to operate at 4000 megacycles in a microwave link. It is to serve as a pulse transmitter, also as a CW local oscillator. Eriksson is asking for 10,000 hours average life.

This little reflex klystron has external cavities. It has the added feature of a space-charge control grid at the cathode. The section is something as shown in fig. 4.6.4-1.

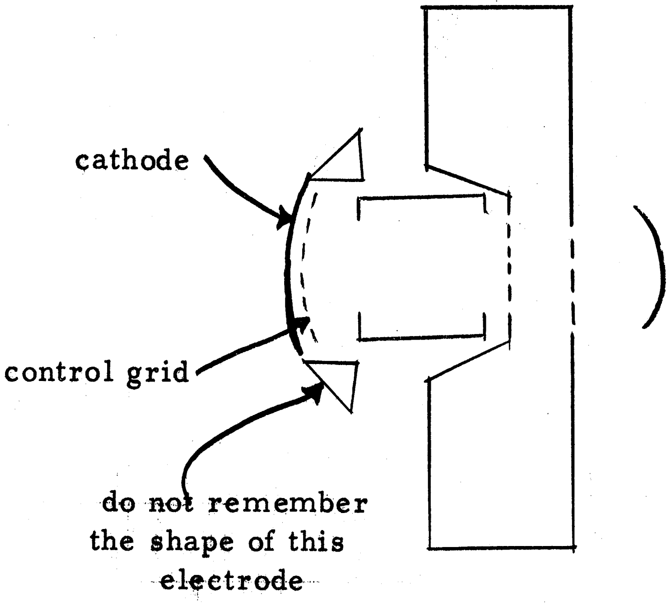


Fig. 4.6.4-1 A.B. Svenska Elektroner reflex klystron I believe they gave this the number 1768.

The device is designed for 1-watt output pulses, perhaps 50 milliwatts CW. It employs 75 milliamperes emission per square centimeter of cathode.

They have built in the past some thermally tunable klystrons having similar electrode structures. They put five of the thermally tuned ones on life test on normal output. One had its filament destroyed by mistake in testing, and one developed a short circuit (I suppose in the heater) almost immediately. The other three have been operating continuously for 8900 hours, that is something over one year. They believe that similar life can be obtained from the kinds of tubes that have external cavities now being built.

They use copper to glass housekeeper seals on the disk seals for the disks comprising sides of the cavity.

4.6.5 The strophotron, Alfven's Barkhausen-Kurtz oscillator with magnetically controlled trajectories. At this factory they work rather closely with Dr. Alfven, a member of the faculty of the Royal Institute of Technology in Stockholm. The strophotron is a tube designed by Alfven, or rather a manufacturable and tunable modification of a tube designed by Alfven. It is essentially a Barkhausen-Kurtz oscillator, with the beam trajectory magnetically governed. Fig. 4.6.5-1 is a diagram of the original tube as described to us by Alfven at the Royal Institute of Technology. In general the effect of the magnetic field is to make the electrons move along the tube in a direction perpendicular to the paper, as in Fig. 4.6.5-2. In so doing they oscillate back and forth in Barkhausen-Kirtz type of motion whose frequency is governed by the voltages on the electrodes. The trajectory eventually leads to a collector. Thus in a sense the central electrode at plus 1000 volts plays the part of a grid in a Barkhausen-Kurtz oscillator as far as establishing the field is concerned, but does not collect the electrons.

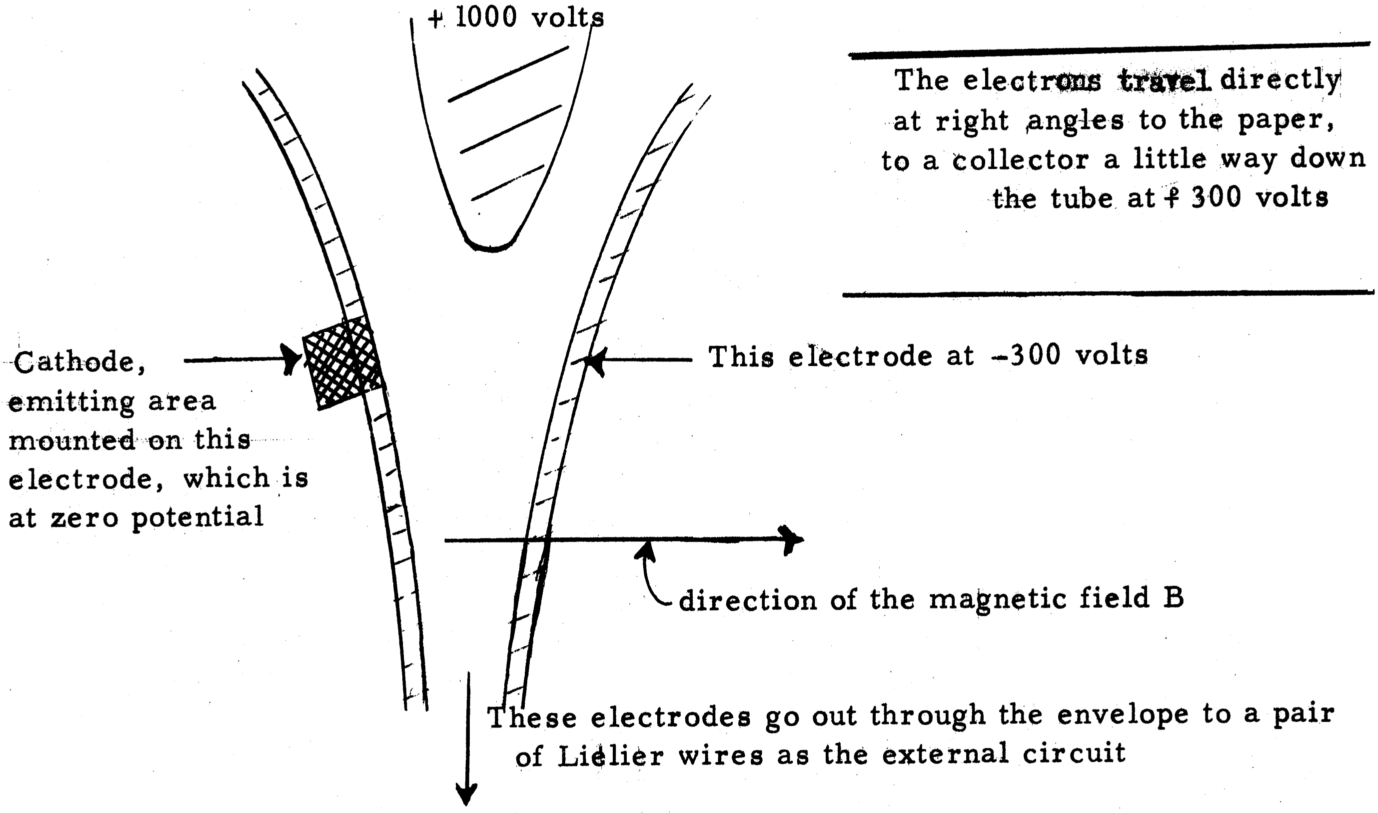


Fig. 4.6.5-1 Alfven's initial design for the strophotron.

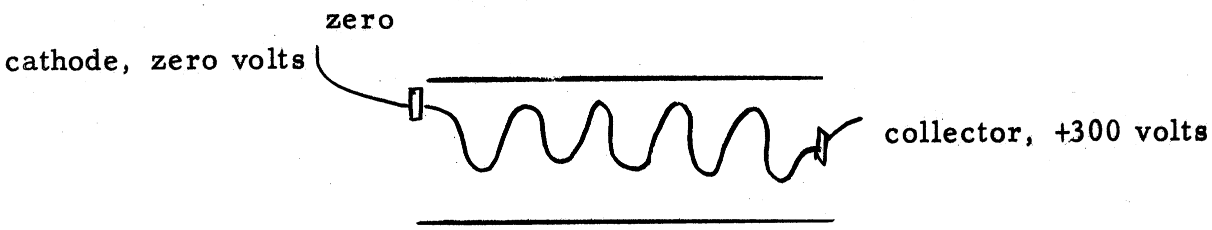


Fig. 4.6.5-2 Electron paths in Alfven's strophotron; this is looking down from the top of Fig. 4.6.5-1.

The frequency range of this tube can be perhaps 2 to 1 in frequendy, the frequency varying as the square root of the voltage varies. The Eriksson Company wants these for use to produce frequency modulation. The important matter for them is linearity of the dependence of frequency on the voltage, rather than a wide frequency range, in tubes to be manufactured.

Fig. 4.6.5-3 shows the nature of the design being prepared for manufacture by the A.B. Svenska Elektroner. This employs a coaxial cavity tuned by a plunger, operating in a 3/4 mode.

Alfven claimed 30% efficiency, at an output of 2 or 3 watts, whereas Svenska men who are developing the tube intending to produce it, talk about an efficiency of 20%. Efficiency is not really important at these power levels.

The tube being designed for manufacture by A.B. Svenska is being planned for a tuning range from 800 to 1300 megacycles, the voltage being 700 volts at the low frequency end. This is the voltage on the tuning electrode which is the top electrode.

Note that the electrode whose voltage controls the frequency does not draw current. This is the top electrode of Fig. 4.6.5-1 or the inner coaxial conductor in Fig. 4.6.5-3.

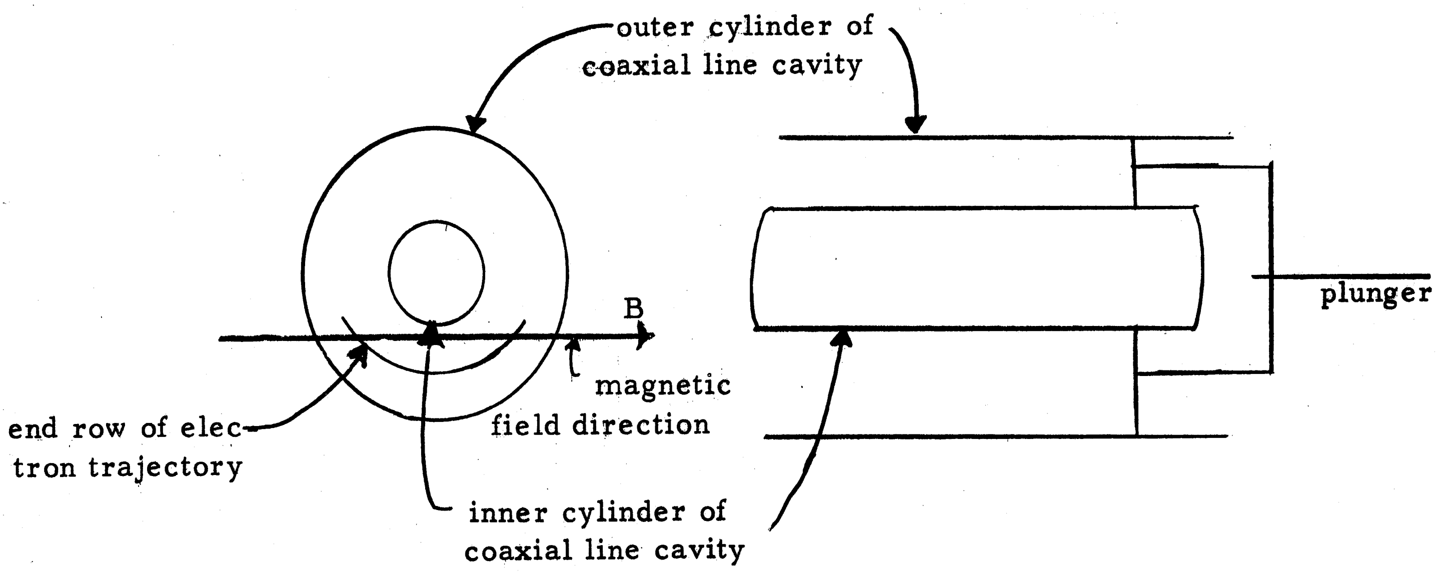


Fig. 4.6.5-3 Section of the form of the strophotron being designed for production by A.B. Svenska Elektroner.