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DEHS Award Winner Major Anthony Glover with judging team – Peter Hill, Peter Butcher, Dick Green & Tony Jones (*Photo: Dick Green*)

DEHS BEST PRESENTATION AWARD TO MESE STUDENT

As has happened since 2007, we were invited to judge "Best presentation" at the MESE 31 project presentation event on 27th July. This year, there were four judges: Peter Butcher, Dick Green, Peter Hill and Tony Jones. There were eleven students from Australia, India, Italy and the UK. As usual, the projects ranged across radar, communications and electronic warfare systems, flavoured in some cases by laser data links, unmanned air vehicles (UAVs) and innovative materials. Many of the devices and systems could provide solutions to current and future defence requirements. We assigned scores against "audience rapport", "audio visual use", "clarity of subject", "structure" and "end summary" criteria, in order to rank the presentations. We also considered how the speakers handled questions. This year, four presentations stood out and we had to reach consensus on a single winner very quickly. By some mysterious process, we agreed that the winner was Major Anthony Glover for his presentation on *The use of low cost software defined radio systems to detect transient 4G mobile phone uplink transmissions in order to identify and classify the user activity*.

Dick Green then handed over a cheque for £200 to Major Glover and informed him that, as part of the award, he would be given honorary membership of DEHS for one year. So, our long-established relationship with Shrivenham continues, but now entirely through the Cranfield University MESE course.

LIEUTENANT COMMANDER WILLIAM ('BILL') E. LEGG R.N.

It is with great sadness we report the death of 'Bill' Legg. Any member who met him will remember his enthusiasm for the preservation of the Royal Navy's contribution to radio communications and radar development. 'Bill' Legg was curator of the HMS Collingwood Museum during the period of 'Defence Cuts', which saw its very existence threatened. He managed to steer a course through all that entailed and we now have his lasting legacy in the current Museum.

Having recently visited the Museum, I can say that his hard work and dedication to recording the Royal Navy's developments are actively being carried on by Clive Kidd, 'Jacey' Wise and a team of volunteers.

'Bill' Legg joined the Royal Navy in 1951 as an artificer apprentice and after his five years training saw service in a number of ships and shore establishments. Commissioned in 1966, he was promoted to Lieutenant Commander in 1976. Retiring from active service in 1988, he served in HMS *Collingwood* as a retired officer until 1997.

We extend our condolences to his family and friends as we know he will be sadly missed.

Peter Butcher.

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THE CAVITY MAGNETRON:
THERE ARE KNOWN KNOWNS AND KNOWN UNKNOWNS - BUT ARE THERE ANY UNKNOWN UNKNOWNS?

Dr Mike Diprose

INTRODUCTION

In October of 2015 the author delivered the 11th South Yorkshire Network IET Annual Radio/Radar lecture at the South Yorkshire Aircraft Museum, Doncaster. It was entitled '*The cavity magnetron: known knowns, known unknowns, but are there any unknown unknowns?*' This follows one of the author's interests in the early history of the resonant cavity magnetron, stimulated by the 2010 CAVMAG Conference at Bournemouth, and by the correspondence appearing in *Transmission Lines* at regular intervals. The conventional story, that the resonant cavity magnetron was first invented by John Randall and Harry Boot (*above*) almost out of nowhere in early 1940, is challenged by evidence from Japan, Russia and elsewhere, but: '*Perceptions are truth – because people believe them'*, so said Epictetus a 2ndC AD philosopher. Remembering, however, our DEHS Chairman's stricture that '*the truth, the whole truth and nothing but the truth is acceptable'* (1), the historian looking back at the events of over 70 years ago should try to unravel the chronology and draw the veil from some of the myths and perceptions to draw as near as possible to the true state of affairs.

The title of the lecture was based on Donald Rumsfeld's (then Secretary of State for Defence in the US) famous answer to a question during a press briefing on February 12th, 2002 (2) - *'Reports that say that something hasn't happened are always interesting to me, because as we know, there are known knowns; there are things we know we know. We also know there are known unknowns; that is to say we know there are some things we do not know. But there are also unknown unknowns – the ones we don't know we don't know. And if one looks throughout the history of our country and other free countries, it is the latter category that tend to be the difficult ones.'*

At first his reply was not taken seriously in some quarters, but upon examination it is quite sensible, especially in the context of terrorism to which it referred. In applying this remark to his lecture, this author tried to look at the early history of the resonant cavity magnetron (RCM) under the three headings. In preparing the lecture, looking through the various references, the CAVMAG Proceedings and the TL correspondence, it became clear from the quantity of material there are many threads to the story. Eventually, however, to try to shed some light on the problem, **the author decided to go back to some of the original papers to see what Randall, Boot, Megaw and others had <u>actually</u> said (or not said) about their parts in the story.** In that lecture and in this paper, the author did not and does not offer any new material or information, but simply one person's view and opinions of the material he has read and sifted through.

This author has taken a view both that a certain amount of '*reading between the lines'* is required in reviewing the evidence, and that conclusions might be drawn from omissions as well as inclusions. **In order that the readers can judge for themselves if reasonable**

conclusions were drawn from the references, the author has extracted relevant sections from the papers and listed them as 'Quotes' numbered Q1 – Q58; these are set out at the end of this paper, which means the reader need not find the full papers for themselves, since doing that may be problematical for some readers. This author has primarily worked from these quotes.

Before starting the paper, the author would like to recount a story from Wilkins' biographical memoir of Randall (3), which illustrates how, by a slender thread, historical events can swing one way or another: how a seemingly trivial event can have momentous circumstances.

Randall wished to move from industry to academia, and in 1937 the award of a Royal Society *Warren Research Fellowship* (£700 p.a.) enabled him to do so. He decided to go to Birmingham to join the physics department under the enthusiastic leadership of Oliphant. This was agreed, and the award was announced. On the day of the announcement, Randall received a telegram from Tyndall at Bristol offering him a place there. Randall, however tempted, felt he could not change his mind after making the agreement with Oliphant, and so he went to Birmingham.

If the telegram had been sent a little earlier, then Randall may have gone to Bristol. If that had happened, would the RCM have been produced in 1940 just at the time it was desperately needed? Would another Randall have been in place to work with Boot to produce the same result?

It is highly likely a long period might then have passed before the device would have come into being: the Russians, Swiss and Germans were not interested in high power pulsed devices, and the Americans were nowhere near. The Japanese would have continued as they did, and perhaps we might have captured a device from them. All this is conjecture, however, since (luckily for the RCM) the telegram was sent too late, Randall went to Birmingham and the RCM arrived when it did.

THE KNOWN KNOWNS:

This section is not intended to be a comprehensive review of papers and patents from the 1930s, but rather to identify the major players in the RCM story and the influences they may, or may not, have had on the development of the RCM produced in the UK in 1940.

What is well known and undisputed is that Randall and Boot made a 6-cavity magnetron at Birmingham University, which produced on February 21st, 1940 - the first time it was switched on - about 400W c.w at ~10cm. This was rapidly developed by Megaw at GEC Wembley into the E1188 and then the E1189, of which number 12 was taken to the US by the Tizard Mission. This valve developed about 10kW peak power at 10cm and was a small, air cooled, compact device with valves no. 12 onwards having 8 cavities. It was ideal for pulsed airborne radar and many other uses, and the RCM made an enormous contribution to the allied victory in WW2. In addition, it has flourished in civilian, military, domestic and industrial applications, and is still being developed today.

Randall and Boot, however, were ***not*** the first to conceive of the multi-cavity magnetron (MCM), so under 'known knowns' there must be added:

- The Japanese scientist Dr. Kinjiro Okabe who had been studying magnetrons in the 1920s and had developed a split anode magnetron in 1927, with a working device generating at 5.6cm, whilst he was at Tohuko University. This work was continued by the Japanese Radio Company (JRC), starting in early 1932, although there was much opposition initially, as many within the company could not see a use for microwave devices and so thought the research was a waste of time and resources (4). Late in 1933, the Japanese Naval Technical Research Institute started a microwave programme due to the influence of Dr. Yoji Ito (34), who, whist studying the characteristics of the Kennelly - Heaviside Layer, had observed that reflections from ships and aircraft could be received. He was convinced that, for a detection device, the shorter the wavelength the better. He applied for a grant from the Science Research Committee of the Japanese Ministry of Education in March 1932, but it was refused (note that this pre-dated the Daventry experiment by some three years).

Convinced of his findings and the military potential of being able to detect aircraft and ships at long range, he was able to persuade the Japanese navy to fund the development of suitable short wavelength devices and equipment. The JRC, given its experience, was awarded a contract to develop compact, microwave sources. This resulted in an 8-cavity device in 1937 at 20cm. Further work with a variety of anode shapes and sizes, culminated in 1939 with the M3 valve; an 8-cavity anode (Mandarin type) RCM, water cooled, producing ~ 500W at 10 cm c.w. This was developed into the M312 device producing ~400W at 9.6 cm.

A Japanese military observation group visited Germany in May 1941 and were told about the British use of pulsed VHF radars. This approach does not seem to have occurred to the Japanese prior to their visit as Nakajima wrote 'Receiving this report, the Japanese navy was very much surprised, but soon started two projects on pulsed radars' (one VHF and one microwave). When their M312 magnetron device was pulsed, it produced 6 kW. The M312 was used in the Japanese Type 22 radar, principally by their navy. The energy was extracted from two adjacent cavities via wires acting capacitatively, compared with the European practice of extracting inductively via. a loop in one cavity. This work was done completely independently of others and was not known about until after the war, but **the Japanese did have a working RCM in 1937, pre-dating the Birmingham device by three years (4).** Towards the end of WW2, they were also working on a very high-power device in an attempt to produce a 'death ray' type device. Nakajima reports two prototypes were operating: one producing 20kW at 15cm and another producing 100kW at 20cm. Plans had also been made for a 500kW output at 10cm. All were rated on a continuous power output, not pulsed, so their expertise was clearly well advanced (4).

- **One would have thought that the US should have been ahead of the field, but that was not the case, although much work was going on**. On December 8th,1934, A. Samuel applied for a US patent for 2 and 4 cavity RCMs, although applications from both Hansell (2,217,745; filed 20/3/34) and Linder (2,157,179; filed 2/7/34) preceded Samuel's that year. Samuel worked for Bell Labs. (ITT). The patent was granted in 1936 (2,063,342). In a paper by Kilgore (5), it was written that RCA (GE) was applying for MCM patents from 1934 (e.g. 2,357,313; P.S. Carter, ITT). Further patent applications for microwave generators of one sort or another were filed in 1936: Hollmann (2,123,728; filed 27/11/36) and Rice (2,145,735; filed 29/11/36), and Samuel re-filed in 1937 (2,130, 510; filed 30/6/37). Helbig applied in 1938, following on from applications in Germany and the UK (2,305,781; filed 7/10/38). Wathen (7) reports that Linder, Rice, and Hansell, as well as Samuel were producing magnetron designs prior to 1940, but Wathen (8) also adds "*In general, however, while the results produced by early magnetron research were of considerable academic interest and served to generate a general picture of the mechanism of oscillation of the magnetron, none of the tubes devised was particularly suited to use in practical communication systems. Only with more mature understanding of circuits suitable for microwave-length use, could there follow a significant advance in the magnetron art.*" Waddell (6) refers to a paper by Kilgore (RCA Company) suggesting a patent dispute between RCA and Bell Labs. If that was the case, it may have hindered progress.

It seems, therefore, in the US, practical and usable magnetrons were in short supply, until the arrival of the Tizard Mission in October 1940 with the E1189, which was clearly a considerable surprise to the Americans and way in advance of anything they had. It certainly provided a strong stimulus for development and broke any patents log jam if there had been one. Such was the extent of the lead of the UK over the US that Tomlin (9**)** reports the US Senate called for a paper explaining why this was so. The result was a 1943 paper '*The Story of Radar'* which, apparently, neglected to mention the British effort or magnetrons.

- **Dr. Fritz Ludi, of Brown Boveri, in Switzerland,** started research in 1936 and by 1939 had developed a single cavity RCM, which he called a 'Turbator' (10, 11). This was able to generate 15W c.w at 15cm and a later version was capable of generating 10kW under pulse conditions. It used a large diameter cathode that was oxide coated (barium/strontium oxides) (12). It was not as powerful as other magnetrons that were developed, but it was tunable over a 15% range. Since there was little interest from the Swiss military at the time, it seemed to fade into obscurity until after WW2, when a version of the Turbator - the MD 10/2000 - went into production for microwave based communication links. A further version of the Turbator - the MF 150/2400 was developed and was used in the ground based, guidance transmitter for a surface-to-air missile system. Details were published in the Brown Boveri Review in 1949 (13) and two pictures appear in a publication (14). Development must have continued throughout the wartime, as it is reported they used strapping to overcome mode jumping in 1943. A Swiss patent (215,600; 1938) was applied for. Bowen was asked about this device after a lecture he gave in Sydney in 1946, and if the British or Germans had made use of it. He did not know, but added, "It is true that almost every country in the world described or patented magnetrons similar to the British RCM well before 1940 but it is equally true that none of them made it work effectively" (15). In addition, after presenting a paper to the Royal Society in 1946 Randall was asked if he knew about the Ludi patent during question time (16). He replied that the Brown Boveri device had certain similarities to the UK device, but it was never developed, without saying when he had learnt of it. Bowen's statement precedes that of Wathen (8) by some years but, although agreeing, almost certainly the two opinions were arrived at independently.

- **The Dutch were active in the field prior to WW2** but did not seem to produce cavity magnetrons. Posthumus developed theories of rotating field magnetron operation (which were used by Randall and Boot) but his magnetrons were split anode ones. Through Phillips he applied for patents in 1933. His experiments with 4 segment anodes yielded 30W at 40cm in 1934 and later 50W at 60cm with efficiencies greater than 50% (17).

- In a paper for the CAVMAG 2010 conference (18), Emilie Tesinska described in detail the work by **Czech physicist August Zacek** and fellow workers in Prague on magnetron oscillators. Although considerable contributions were made by those workers to the understanding of magnetron operation, it does appear the devices were low power, with external resonators and that no high-power magnetrons with internal resonant cavities were developed in Czechoslovakia until after the war.

- **Hans Erich Hollmann, (Telefunken, Germany),** applied in the US for a patent for a 4 cavity RCM in November 1935 via. G.E., which was granted in 1938 (2,123,728). Bushholz of Germany applied for patent no. 2,315,313 again via G.E. (all Telefunken patents were granted to G.E. from 1935 onwards, which caused the Germans concern over leaked secrets on MCMs (19a –*'Lorenz were actually in an awkward position as far as the official German radar programme was concerned, for they had more commitments in the international market than other firms. Their Blind Landing System, for example, was even then employed by the RAF under licence, and though they were looked upon as a security risk by the High Command, they forged ahead…..'* and 19b - *On the other hand, German radar secrets were being leaked to the US. For example, all of the Telefunken radar patents were also patent applied for and granted during the war in the US. As such, the German military did everything it could to keep the radar secrets from leaking out.)*

Arthur Bauer (DEHS) has unearthed three patents that had been lost for some time. Dr. Wilhelm Engbert and Hans Jacob Ritter von Baeyer applied on the 15th November 1938 for a patent for a high frequency oscillator (DE 763494), followed on the day after (16/11/38) by another application from Engbert alone (DE 938196). The latter was for an 'improved' Samuels valve and is said to have produced high powers for a long time (patent claim). It is suggested (20), that the patent sketches by Randall and Boot in their patent application were identical to Engbert's 1938 application. Both of these clearly show a 4-cavity resonator around a central interaction space, but Abb 6 of DE763494 seems to imply it is a thin disc within a tubular structure and with an external solenoid generating an axial magnetic field. Patent DE 748551 (filed 24/11/39) from Engbert again, is for a cathode temperature regulating device, so implying back bombardment was a problem in trying to attain higher powers from their devices. It is also interesting in that the document shows the processes an application goes through as there are corrected versions within it. The problem of self-heating of the cathode, as higher powers were sought by increasing the magnetic field strength, was well known in the 1930s and a problem for thin, tungsten cathodes. German work in the mid-1930s had led to the conclusion that large diameter cathodes were impractical in magnetrons. This was an erroneous result, but one which caused much delay in magnetron development (21) A. Helbig, working for Sanitas in Germany – a firm specialising in X-ray equipment - applied for US patent 2,305,781 in 1938 and had UK patent 509,102 granted in July 1939. Waddell (22) references a suggestion that Helbig's 1937 MCM gave 100W at 25cm with 8 cavities and by 1942 was giving 16kW at 20cm, again with 8 cavities – although Waddell questions the accuracy of the latter figure. It is not surprising that an X-ray equipment manufacturer should be interested in microwaves, since a method of producing them is to focus a beam of short wave energy at a target. There also exists a picture of an 8 cm, 4-cavity magnetron made by Lorenz in 1938 (23).

In spite of all this work, the Germans were as surprised as the Americans when they obtained their first examples of a British RCM, but not nearly so pleased. *('I knew we had fallen behind in these developments, but I had thought until now that we were, at least, in the same race!*' – Goering: (24))

- **In 1935, N.F. Alekseev and D. E. Malairov were given the task of investigating RCMs by Professor Bonch-Bruevich at the SRI-9 Research Institute, Leningrad.** By 1937 they had working devices (4-cavity, ~9cm, 300W c.w, ~ 20% efficiency) and went on to report 2, 4 and 8 cavity devices using tungsten cathode and anode blocks at 1, 2.5, 5 and 7.5cm wavelengths. This work was published in Russia in 1940 and was known to the Germans who would have had access to Russian publications, as hostilities between them had not started then. In 1944, a translation appeared in the March 1944 edition of the Proceedings of the IRE of their 1940 article (25).

The paper does describe results and some broad descriptions of the apparatus, but lacks much detail. There is little description of the measurement techniques, loading circuits or how the energy was extracted (a point Randall himself had commented on (26). There are references, but they are not referred to in the text and only one (their ref 3) is dated prior to the work (1936). The 6 others are 1938 (five) and one from 1939. It does say the work was done in 1936/37 and most of it was with demountable magnetrons with continuously running vacuum pumps. It gives a result of 8W c.w output at 7.7cm wavelength with a single cavity; 7W out at 9.9cm for a dual cavity anode block and 170, 300 and 116 W at 9.0, 9.0 and 9.1 cm respectively for a 4-cavity anode block. Efficiencies for the above examples were given as 3%, 9%, 18%, 20% and 22.5% respectively. Some devices were made as sealed units, giving a typical output of 120W at 9.1 cm and 22.5% efficiency. They were aware of cathode back heating as they mentioned it in 'Remarks' in the results table. Their descriptions of what, exactly, is meant by a single cavity, however, leaves much to be desired as the diagrams are very unhelpful.

Figure 3 in the Russian paper purports to describe a single cavity anode, but the diagram shows two cavities, one smaller than the other, coupled together with a slot, but also having a slot on the opposite side of the smaller cavity as well. The overall diagram of their one cavity anode block (Figure 4 in their paper) shows these two cavities situated adjacent to the edge of the block and coupled to the outside of the anode! Figure 2 (their paper) is said to be an arrangement for an anode block with 8 single cavities, but it shows 8 of the double cells around a central cavity. Does this mean the smaller cavity contained the cathode so the 8-cavity device had 8 cathodes, but if so, why are they connected by slots to the central space? Their Figures 4 and 5 make sense if the central space has the cathode wire, with the 2 or 4 cavities coupled to it. I suspect the confusion is deliberate obfuscation by the Ministry of Disinformation, which undoubtedly flourished in the Soviet Union.

Borisova, in her CAVMAG 2010 paper (27) fills in some of the gaps. She states that the first magnetron produced by Alekseev and Malairov (August 1936) used a 4-cavity anode block of tantalum with a tungsten wire cathode which produced about 10W at 9cm. In September 1936, they began to use water cooled copper anodes and by March/April 1937 4-cavity magnetrons were producing about 300W at 9 cm with 20% efficiencies. At the end of 1937 they made some sealed devices with tungsten cathodes and a 4-cavity copper anode block, which gave outputs of around 120W c.w with efficiencies of 22.5 %. During 1938 they went on to develop 4-cavity devices at 1, 2.5. 5.0 and 7.5 cm wavelengths. A paper by Fritz (28) given at a German magnetron conference in 1944 has a picture of a Russian anode block (Abb 7). In spite of the confusions in their paper, Alekseev and Malairov undoubtedly had RCM devices working prior to 1939.

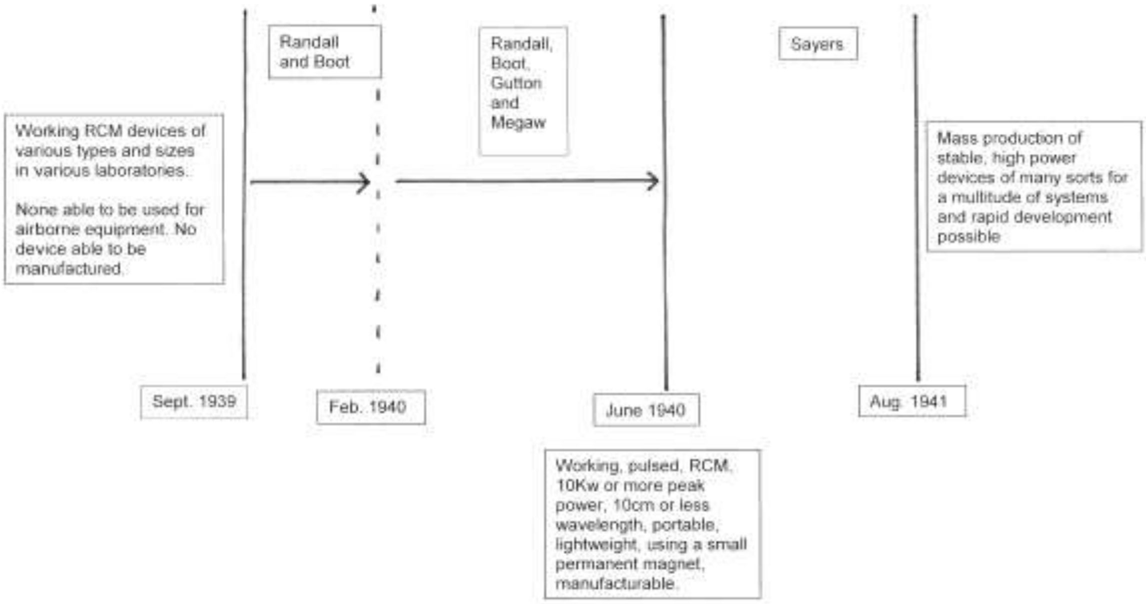
It is quite clear that many of these patents show resonant cavities around a central cathode and there are claims that there were working devices, producing centimetric power, both c.w and pulsed. The Japanese and the Russians had produced working cavity magnetrons similar to those of Randall and Boot, prior to their device, but it is highly unlikely that Randall and Boot or anyone else in the UK knew of them in 1939/1940. So, whatever they did or did not know, Randall and Boot were not the first to propose the idea.

One can see how this interest in magnetrons evolved throughout the 1930s, given the rapidly expanding need for communications, both civil and military, at that time, and the increasing interest in radar by the military, coupled with the flow of scientific publications on the UHF region and commercial competition, it is not surprising that so much was going on and RCM devices were being patented or developed in so many countries. Burman said in his paper to CAVMAG 2010 (23), “Mark Oliphant’s group at Birmingham University was set up in the latter part of 1939 with the main remit of producing a high-power generator of centimetre waves which it was known would be required for the creation of effective airborne AI and ASV radar. It is surprising that in view of the amount of RCM information in the public domain that no initial effort was put into evaluating magnetron performance as a means of achieving this objective”.

I think it can safely be said that after CAVMAG 2010, the correspondence in TL, with what has emerged over the years in other places and the passage of time, the major players in the cavity magnetron story have been identified. It is unlikely that news of a cavity magnetron having been produced elsewhere in the world in the 1920s and 1930s will emerge, unless something comes from Italy and the Marconi Company. In his summary of magnetron history, Wathen (29) states that magnetron research was carried out in United States, Great Britain, Germany, Czechoslovakia, Japan, France and Russia, which indicates that the principal countries have been identified although Wathen (in 1953) makes no mention of Switzerland and the Turbator.

Given the review of known knowns above, the author would like to draw three distinct boundary lines of development in the RCM story (Figure 1).

Figure 1: a timeline diagram of developments



The first line can be drawn in September 1939. The author believes that, in spite of the mass of material, papers and patents being produced, nowhere in the world was there a working, pulsed, RCM device capable of giving 10 kW or more peak power at wavelengths of 10cm or less, which was portable due to its compact size, which used a small permanent magnet and which was 'manufacturable'.

The second line can be drawn in June 1940, when the UK was the only country in the world to have such a device as described above, with four men principally responsible for producing the radical development – Boot, Gutton, Megaw and Randall – although many others had an input, one way or another.

The third line can be drawn in August 1941 when Dr. J Sayers introduced strapping to stabilise the modes of oscillation and thus enabled very high powers and efficiencies to be obtained e.g. an E1189 type device produced a peak power of 150 kW at 65% efficiency. Consequently, strapped RCM devices were manufactured in tens of thousands during WW2 and in countless numbers thereafter.

Thus, the list of known knowns can be said to be complete, but the interactions that may or may not have taken place between the various groups or who knew what and when and did one group or another know of the work of other groups is a different matter indeed and it is in this area that the controversies arise. Just how much credit should Randall and Boot receive for their input? Exactly what Randall and Boot knew of other devices has generated, shall we say, a vibrant set of exchanges in TL. Broadly speaking there are those correspondents who believe it is quite possible that they generated their design spontaneously and did so. Others believe this is not the case and that, somehow, they knew of the other work. The arguments are fully aired in TL and they will not be rehearsed here, but the author suspects that a compromise course of '*un peu de tout'* rather than one or the other is the most likely scenario.

THE KNOWN UNKNOWNS.

From his reading so far, the author has identified at least 4 known unknowns. He suggests they are:

1) Did Randall and Boot know about other cavity magnetron devices prior to designing their own?

2) What interactions occurred between the various inventors, researchers, companies and groups, if any?

3) US companies seemed to own most of Europe's telecoms industry in one way or another, with information as well as money flowing from Europe to the US. What effect did this have on the RCM story and scientific, commercial and political interactions and events?

4) Given 3), what happened in the US between 1934 (Samuel) and the arrival of E1189 no. 12 in 1940? Why were they so far behind?

The author feels that the third will prove to be the most significant historically, and the fourth will be instructive in shedding light upon the way in which device development proceeds, when the emphasis changes from commercial concerns to military imperatives. Some time, the full story will emerge of US commercial involvement in WW2. The author would be grateful to hear of any references on the subject as this work may already have been done. He does remember reading a book about ITT, but that was as an undergraduate and was a (very) long time ago. For the purposes of this paper, the author will confine himself to examine the first two.

Although the credit for the RCM is usually attributed to Randall and Boot at Birmingham in February 1940, it has been demonstrated above that working RCM devices were produced in Japan and Russia prior to that and the Turbator in Switzerland. Perhaps the first question to be asked, then, is how likely was it that the British knew of this work?

- The Japanese developments were being paid for by their navy and were for their navy and military, so it is unlikely that any details were published openly.
- The Russians were slow to publish their work, especially as scientists had the KGB looking over their shoulders, with many going to the Gulags. Dmitry Rozhansky, for example, was one: 'In 1930, as frequently happened then, he spent several months in prison being interrogated in relation to the "Sabotage Case of Electric Industry". His interrogators, as he told later to his friends and family, tried to extract from him a confession that "he was inventing a device able to distantly read the thoughts of Comrade Stalin" He was released without explanations and returned to his job' (30). Alekseev and Malairov published in a Russian journal in 1940 (31**)** and although known to the Germans, it was not published in the West until 1944 (25). Wilkins (32) checked dates and the editor of the Russian journal did not receive the manuscript until the 28th April 1940 – two months after the first successful run of the Birmingham device. Boot wrote in a letter that 'we did not know of the Russian work until the English translation became available', (32).

- Bowen has already been quoted at the beginning of this paper (15) saying that he did not know if the British or Germans had made use of the Swiss Turbator and in **Quote Q13** (16), Randall also stated that he did not know of the Russian work until late in the war and did not think the Swiss device was ever developed.

The author believes, given the statements and circumstances, that none of the Japanese, Russian or Swiss magnetrons played any part in the genesis of the Birmingham device. That is not the same as saying, however, that Randall and Boot did not receive help and guidance. They must have been subject to many influences; the place and atmosphere where they worked; the people they worked with and met and the advice they were given; their reading of publications, patents, books etc.; as well as their own abilities and the circumstances. Many of the influences would have involved what not to do, rather than the opposite. The patents and publications prior to the first timeline (Fig 1) were for devices that could not be developed into high power, pulsed portable magnetrons eventually capable of MW outputs. Would anyone seriously suggest that A.L. Samuels' patented device (US 2,063,342) would be able to produce MW pulses? What not to do, however, can be extremely instructive to researchers of the calibre of Randall and Boot. All will have played a part to one extent or another. In trying to evaluate them the author has turned to their own publications and some commentaries to seek clues as to how the resonant cavity magnetron design evolved.

Randall paid tribute to the dynamic atmosphere in Oliphant's department.

> ***Quote Q1.*** *There was no lengthy research program leading to our discovery of the cavity magnetron at the end of 1939, but there existed a very special set of circumstances in which we were, in some ways, fortunate to be involved. We were fortunate in that we were in Oliphant's laboratory at the University of Birmingham.*
>
> ***Q22*** *An important factor should be recorded at this point. The whole lab was buzzing with talk of resonators and particularly of the Hansen papers. This naturally had a great influence on Boot and me as well as other members of the team; and we have always fully acknowledged that the atmosphere in which we worked influenced the direction of our thoughts.*
>
> **Q53**. *Randall 'fully acknowledged that the atmosphere in which we worked influenced the direction of our thoughts'*.

Although they were working in a vibrant environment, their colleagues were focusing on the problems of the klystron and its resonators, so Randall and Boot were not part of the mainstream. Whether this was planned or not is unknown. Oliphant may have kept them apart deliberately to go down the magnetron path or it may just have been the circumstance of their late return from Ventnor. That had a benefit however, because since they were sidelined to some extent, they were able to develop their thinking without undue interference.

> ***Q2.*** *In fact, there was really no interest in microwaves until the summer of 1939.*

This sets the scene leading up to September 1939, in Oliphant's laboratories prior to the extensive visits to radar stations – Ventnor in the case of Randall and Boot. Upon returning, however, the attitude seems to have shifted considerably:

***Q30** But now we were all interested in the possibility of producing microwaves. We had seen how very large powers at 10-m wavelength could be produced by pulsed operation of relatively small tubes, and the prospect of doing this at microwave frequencies for air-borne and ship-borne use was most exciting, but apparently impossible. So we all returned to Birmingham to try.*

Randall and Boot were enthused by their study of the 10m systems and had been shown that pulsed operation of small valves could produce high peak powers but at the low average powers with which the valves could cope. They now wanted to do this at 10cm rather than 10m. The start, however, was not auspicious.

***Q5**. We had been allocated the less spectacular task of making miniature Barkhausen-Kurz tubes as possible receivers and were also trying to excite cavities by gas discharge tubes. We were unsuccessful and disenchanted with this task.*

***Q15**. Initially, under Oliphant, a considerable attempt was made by Sayers and others to improve the klystron as a centimetre-wave power oscillator, and in fact a good deal was achieved in this connection. At the same time, an attempt was made by the authors to use the Barkhausen-Kurz valve as a detector: before long, however, interest moved towards the central problem of the production of high power at or below 10 centimetre wavelengths.*

***Q22**. When Boot and I eventually returned to Birmingham the more interesting tasks had been assigned and we were left with the task of discovering whether a Barkhausen-Kurtz tube could be used as a detector. After several weeks experiments in the corner of a large teaching lab we decided that this was not possible. Boot and I worked very closely together both then and throughout my subsequent stay in Birmingham which terminated in the autumn of 1943. Consequently, during this early comparatively leisurely period in 1939 we were isolated from the main stream of activity in the lab and had plenty of opportunity to discuss the problem of short wave transmitters and particularly whether the klystron as it then was could produce the power required to give a satisfactory early-warning system. We concluded, as history shows, that it would not be sufficiently powerful.*

So, with the ability to plough their own furrow they started work on thinking about how a magnetron could be developed, which overcome the deficiencies that they had identified with the klystron.

***Q4**. It seems to have been taken for granted that the wavelength to aim for was 10 cm or less, which seems rather ambitious in retrospect.*

***Q7**. In November 1939, at the risk of incurring some unpopularity from our fellow workers, we concentrated our thoughts on how we could combine the advantages of the klystron with what we believed to be the more favorable geometry of a magnetron.*

***Q11**. One of the chief limitations of the klystron as a power oscillator lies in the difficulty of producing high current electron beams of suitable cross-section. This* *limitation led Boot and myself to think of a possible alternative, and we realised that a magnetron would be free from this defect. Having the example of the klystron before us, it appeared that existing magnetrons made no proper use of internal resonators. The resonators of the klystron were made of copper to give low losses; the nature and shape of the resonators were such that high frequency stability was provided for; moreover, their high heat conductivity gives means of ensuring large heat dissipation when high power is used.*

***Q15**. It appeared that the major difficulty in the way of attempts to improve the klystron by a large factor was that of getting sufficient power into the electron beam, necessarily of small cross-sectional area: it was realized by the authors that a magnetron would be free from this defect. One of the outstanding advantages of the klystron was the use of internal resonators such as had been described by Hanson[2] and Rayleigh[3]. Resonators of this type, turned from solid copper, give low h.f. losses, high frequency stability, and are capable of large heat dissipation.*

From the preceding quotations, it can be seen that Randall and Boot were aware of the inherent problems with the klystron and also aware of the potential of a well-engineered magnetron design to overcome them. They knew the benefits of copper resonators, as being stable and with good heat dissipation for high power operation. They also knew that the single frequency operation of resonators was no drawback for pulsed radars and that a wavelength of 10cm or less was required. The resonators used in klystrons were not suitable for magnetron operation, so they began to look at alternative resonator designs – resulting in the ring of cylindrical resonators symmetrically spaced around the cathode.

So, starting with what they knew:

***Q6.** The only other known sources of microwaves were sparks and split anode magnetrons. The latter were glass envelope tubes usually with only two segments giving vanishingly small powers below 10 cm, and their efficiency was very low. We found them interesting initially because it was obvious that their geometry should allow large input powers. No focused beam was needed as in the klystron. We also knew that these early magnetrons had been made to work in France with large diameter oxide cathodes so we felt sure that large peak anode powers could be used if a properly engineered design could be devised.*

***Q8**. We had to confine the radio frequency fields in resonators, as did the klystron, in order to get high circuit efficiency, and it must be made of solid copper to dissipate the power we felt could be put into a magnetron-like geometry. It was realised that many different shaped cavities could be considered as resonators (Hansen and Richtmeyer [their reference 2]).*

***Q16**. The Hanson papers were by this time available in the laboratory, but it was clear that the designs (hollow spheres, cubes and "doughnut" shaped cavities) could not be associated with the cylindrical symmetry of the magnetron.*

***Q11.** No existing type of resonator was of a shape suited to our purpose.*

__Q15.__ The problem was, therefore, to design a magnetron, capable of giving a large anode current, and which also incorporated a resonator system of suitable properties. The main investigation therefore initially resolved itself into (a) the design of a suitable type of resonator, the determination of its size, and the method of grouping a number of resonators in a device of cylindrical symmetry. (b) A method of construction which would ensure high electrical and thermal conductivity. (c) The design of an h.f. output-circuit in relation to the chosen resonator system.

Given this background and the requirements for their design which result from the above, they then started to develop their own ideas. Randall and Boot were academics in an academic environment and would naturally have turned to publications in the first instance and there is evidence to show they did so.

__Q21__. Browsing in this shop one day, I found a copy of Jones's translation of Hertz's "Electric waves" which I acquired with some interest in view of our coming radar activities. At that time no thoughts of magnetrons of any kind had occurred to me, but the use by Hertz of small rings of wire each with a short gap as detectors of his electric waves stuck in my mind. Boot was in no way concerned with this.

__Q16__. The Hanson papers were by this time available in the laboratory,…………

__Q6.__ Fortunately we did not have the time to survey all the published papers on magnetrons or we would have become completely confused by the multiplicity of theories of operation. The most acceptable paper to us was that of Posthumus [l] which dealt with the principle of rotating electric fields and traveling waves in magnetrons and also considered greater numbers of segments than two.

__Q6__. We also knew that these early magnetrons had been made to work in France with large diameter oxide cathodes…….

__Q18__. …….was also realised from the French publications available that the use of an oxide-coated cathode was desirable.

__Q8.__ The resonator diameter was 12 mm because H. M. Macdonald [their reference 3] in 1902 in his book, __Electric Waves,__ had calculated that the resonant wavelength of Hertz's wire loop resonator was 7.94 times its diameter, and we were aiming to produce 10-cm radiation.

With this background, and using the advice that they had received they went ahead with their own designs.

__Q22.__The only device we were able to think of which could possibly combine the desired attributes of resonators and high-power output was a magnetron. We were well aware of modifications that had taken place in magnetron construction during the pre-War years particularly at the GEC, Wembley; General Electric in Schenectady and also in Germany. As a former colleague of E.C.S. Megaw at the Research Laboratories of the GEC, Wembley, I was aware of his activities in this field.

However, none of these magnetron devices was satisfactory from our point of view, since they did not embody truly enclosed resonators and consequently showed *marked frequency-pulling on load. It was at this stage that I thought of the cylindrical extension of a Hertzian dipole and Boot and I jointly tested the system of 6 such resonators clustered round and facing into the anode-cathode space. We presumed that there would be strong electromagnetic coupling between the different resonators and therefore inserted a coupling loop so that power could be withdrawn from one resonator only.*

Q11. *No existing type of resonator was of a shape suited to our purpose. In November, 1939, we hit upon the idea of using cylindrical resonators slotted so that the slots were parallel to the cathode axis, and opened into the anode-cathode space. This type of resonator, illustrated in section in Fig. 1a, is really a cylindrical extension of the original Hertzian dipole. An alternative resonator consisted of a short-circuited quarter wavelength of parallel wire transmission-line (see Fig. 1b). Each of these designs, as Fig. 1 shows, was of such a form and shape that a number of them could be arranged round a central cathode.*

Q16. *It was decided therefore to use either a three-dimensional extension of the well-known Hertzian wire loop or a corresponding extension of a short-circuited quarter-wave line.*

Q8. *Hertz in 1889 had also used a resonator in the form of a wire bent into a circle leaving a small gap as a detector in his early experiments, but this was not a cavity. A cylindrical extension of Hertz's wire loop became a cylinder with a slot along a generator and it occurred to us that a number of these would fit around the slotted anode of the magnetron we were trying to invent. Also, it would be very simple to make in the laboratory workshop. Only drilling, turning, and slotting would be needed. It also occurred to us that a series of 1/4 wave deep radial slots would also serve as resonators as they were 3-dimensional versions of a lecher line. The first anode block was made there in December 1939 and is shown in Fig. 1.*

Q23*. We had the idea of the cavity magnetron in November 1939 and we showed the first copper block to Bragg and Appleton when they paid a visit to the lab during that month.*

Thus, in a very short space of time, it all came together in the initial design, which was manufactured in the university workshops in November/December 1939 (there is a slight ambiguity about dates between **Q8** and **Q23** above) and the following two quotes summarise the situation after that device was tested on February 21st 1940:

Q17*. One of the outstanding features of the design at this stage was the use of an enclosed resonator-anode system, and in this feature, it differed radically from all earlier magnetrons known to the authors.*

Q19*. It is perhaps useful to summarise the main conclusions which had been established by the first experiments outlined above*
(i) A magnetron with a completely enclosed resonator system had been successfully operated at a wavelength of 10cm, at an efficiency (10 – 15%) comparable with that of other known high-frequency (generators?) such as the klystron.

(ii) The use of a number of closely coupled resonators in the same oscillator apparently offered no difficulty.
(iii) The use of a combined copper anode block and resonator system ensured adequate heat dissipation, low electrical losses, and the potentialities of simple manufacture.
(iv) The use of a single, simple, output circuit to draw h.f. power from the system was established and it was clear that such an output circuit would enable power to be fed in a comparatively easy manner to aerial systems and wave guides.

By the 21st February, 1940, the basic work had been done and their magnetron device worked first time, producing about 400W at 9.2cm wavelength. They had produced a working device with the salient benefits listed in **Q19** above. There was still much to do, however, before the second timeline was reached and a practical device, suitable for airborne use was produced. Randall and Boot were well aware of this and also about the development work that still needed to be done:

***Q20**. The following steps in the development of the valve were obvious but necessary consequences of the early experiments.*
(a) The completion of a sealed-off version of the valve.
(b) The testing of other designs – e.g. different numbers and sizes of resonators.
(c) The introduction of large cathodes to provide the high anode current necessary for high-power operation.
(d) The introduction of a modulated power supply.
(e) The adequate measurement of r.f. power.

At this point, once a working device was demonstrated, they report they turned to the Admiralty and Megaw at G.E.C., Wembley. The latter was able to turn the very large laboratory system into a manufacturable device and also incorporated the French concept of large diameter oxide coated cathodes.

***Q9**. This unexpectedly spectacular beginning in copper and sealing wax was obviously going to need additional effort. The results were communicated to Sir Charles Wright, Director of Scientific Research, Admiralty, and other government scientists. The research laboratories of the General Electric Company, Wembley, England, had close links with the Admiralty at that time in that they were engaged in work to produce 50-cm power for shipborne radar, and there worked E. C. S. Megaw who had for some time been concerned with conventional magnetrons. It was arranged for him to visit Birmingham in the hope that help could be provided soon with the unfamiliar technological problems of constructing a sealed off cavity magnetron of copper and glass and brazed joints; for we wished to make a tube in which the cathode was oxide coated and supported on two side arms with Housekeeper seals similar to the output arm used on the first continuously pumped tube. This design has scarcely changed to date as can be seen from Fig. 4, and it was made possible at that time by the technique introduced by the GEC of making vacuum tight seals by compression bonding of copper by gold wire rings.*

The first two sealed off magnetrons were made at GEC, Wembley, one an exact replica of the tungsten filament design described above and the other having a 4-mm diameter oxide cathode, indirectly heated. The anode was shortened to 2-cm active *length to fit into an existing permanent magnet, and air cooling fins were added. Moreover, S. M. Duke of their technical staff was seconded to Birmingham and supplies of indirectly heated cathodes were arranged from other industrial tube research laboratories so that work on pulsed magnetron operation could get under way. Powers exceeding 10-kW peak were obtained at 10-cm wavelength.* ***(1976)***

Q20*. In the achievement of (a) considerable help was obtained, directly and indirectly, by collaboration with the Research Laboratories of the G.E.C., Wembley with whom information was exchanged in April 1940. In this way, a great deal was learned about the technique of sealing glass to copper, and of joining copper to copper by means of a gold wire under pressure between the two surfaces at temperatures below 500^{o}C. Indirectly great help was received from S. M. Duke, a former member of the Wembley staff, who joined the Birmingham group in June, 1940.*

An important feature in the development of the cavity magnetron was the introduction, in May – June 1940, of large oxide-coated cathodes. The French workers Gutton and Berline were already using such cathodes in the more conventional split-anode magnetrons just prior to the war. This information was already available to G.E.C., Wembley, and at about the end of 1939 to the Birmingham workers. Shortly after the first experiments described in this paper both the G.E.C. Laboratories and the authors produced samples embodying this feature; this enabled peak-power outputs of 10 – 15 kW to be obtained at efficiencies of 10 – 20% within a few months of the initial experiments. ***(1946)***

Q18*. Furthermore, although a tungsten cathode 0.75 mm in diameter was used (as being more suitable for c.w operation), it was also realized from the French publications available that the use of an oxide-coated cathode was desirable. The subsequent introduction of such cathodes by both the Research Laboratories of the General Electric Company, and the Birmingham group is referred to below.* ***(1946))***

Thus, by May/June 1940, the second timeline had been reached and there existed in the UK alone, a device which could produce 10kW pulses at wavelengths of 10cm or less and which was compact and reliable and which could (and very soon was) incorporated into radar systems.

Rapidly, Randall, Boot and others were experimenting with more cavities and different, shorter wavelengths and by the autumn of 1940, several different magnetron types had been built and tested and one device had been sent to the USA, where it astounded the scientific establishment.

Q12*. The obvious steps are frequently the correct ones, and these followed each other, both in the University of Birmingham, and in the Research Laboratories of the General Electric Company, during the next few months. Fig. 3 shows a view of the very first cavity magnetron in which wax joints and other crudities of initial trials are evident. From this to a sealed-off magnetron, with robust copper glass seals, in a form suitable for service use, was a step in which our industrial friends at Wembley and our colleague, Mr. S. M. Duke, played a great part. The introduction of oxide-coated cathodes was a step which enabled still higher powers to be obtained. Before*

> *long, pulse-operation was achieved, and peak powers of 10-15 kilowatts at 10 cms. were obtained at efficiencies of 10-20 per cent. Before the autumn of 1940 experimental tests had been completed which showed that the cavity-resonator principle could be applied over a wide range of design; power was produced at 5 and 3 cm. wavelengths; using 30 resonators of the slotted type, shown in Fig. 1b (p. 305), oscillations of low power were demonstrated at 1.9 cms (**1946)***

> ***Q25**. By contrast with the pioneering work with sealing wax, industrial development of a rugged portable device, with high-current, oxide-coated cathode, was extremely rapid, notably with the aid of E.C.S. Megaw and S.M. Duke of G.E.C., Wembley. Impressive 10 kW pulses at 10 cm wavelength were soon achieved. A radar operating at 10 cm was in operation by May. Following the famous scientific mission to President Roosevelt in the USA, the 10cm magnetron was demonstrated at Bell Laboratories in October 1940. By November,1940, Bell Laboratories had supplied working copies of the British magnetron to the radar research teams at MIT. **(1986)***

This account of progress with the RCM has been entirely from the point of view of Randall and Boot from publications by them or about them. Only rarely do they state they received help, although one can infer that happened from the texts. They mention Oliphant in connection with his laboratory (**Q1**) and G. E. C. Wembley, Megaw, S. M. Duke and Sir Charles Wright (Admiralty) in their 1976 and 1986 papers (**Q9, Q22, Q25**) and G.E.C. Wembley, Duke, Gutton and Berline (but not Megaw) in the 1946 papers (**Q12, Q20**). From the author's reading of the papers, these mentions were for contacts made after the first run of the Birmingham magnetron. The author cannot find a reference to meetings that took place before or during the design process in the papers, but stands open to correction. There is evidence, however, that Randall and Boot did have meetings or sought advice in the early days.

In **TL Dec. 2006**, Donald Tomlin wrote: '*Randall or Boot told him that they were briefed by Sir Charles Wright in charge of research and development of radar at the Admiralty, who suggested they look at alternatives to the rhumbatron. They then studied from first principles acoustic generation in organ pipes and cavities, the Hertzian equations, the experiments of Hertz and standard and non-standard methods of radio signal generation e.g. ring style oscillators*'.

Q54. E. D. R. Shearman and D. V. Land have recently written of Randall and Boot *'An element in their thinking may have been the seminal suggestion to Oliphant on a visit to the Admiralty Station at Haslemere that the combination of the split-anode magnetron with resonators after the pattern of the Klystron... might be a way forward to shorter wavelength operation.'*

Q55. Also in a letter about strapping, Sayers wrote, *'Oliphant... asked Randall to see whether copper cavity resonators could replace the little loops of wire with nickel plates attached which formed the resonators in the magnetron valves then available.'* Sayers also stated that he gave Randall advice on the design of magnetron resonators. This does not, of course, invalidate Randall's memory of Hertzian loops - an inventor is seldom fully aware of all the factors contributing to an invention. Concerning Oliphant, Boot and Randall wrote *'The authors owe a great deal to him for his encouragement and inspiration throughout a difficult* *period.*' Oliphant confirms his involvement in initiating the cavity magnetron though the form of the resonant cavity was Randall's: *'His elongation of the Hertzian detector was brilliant'*.

One of the first influences upon Randall was the discovery of the book by Hertz on 'Electric Waves', which was to have a significant effect on the outcome of the war, although Randall did not realise it or imagine it at the time (**Q21, Q8**) *'…. the University College bookshop in Aberystwyth and went under the name of Galloway. Browsing in this shop one day, I found a copy of Jones's translation of Hertz's "Electric waves" which I acquired with some interest in view of our coming radar activities'.*

It was a casual purchase with a most unexpected outcome. Randall apparently bought it because he did not have much knowledge of high frequency oscillators which Wathen said was an advantage:

> **(*Q43)*** *'Apparently, neither of these physicists had any great previous knowledge or experience in the magnetron art; thus, they were unhampered by prejudices based on the preceding experience of others or on the confused statements in the rather large amount of literature already in existence on magnetrons'.*

This was the situation in September 1939, but in just a few months they had designed the basic RCM, the basis of which hardly changed for years, although the packaging did and many improvements were added. After returning from their acclimatization period at Ventnor in September 1939 they were working on the Barkhausen-Kurz project, which they abandoned, but they must have been thinking about magnetron design in this period. They said that

> (***Q7***) *'In November 1939, at the risk of incurring some unpopularity from our fellow workers, we concentrated our thoughts on how we could combine the advantages of the klystron with what we believed to be the more favorable geometry of a magnetron'.*

The author believes some reading between the lines is required. From **Q7** Randall and Boot said "we concentrated our thoughts", which means they must have been thinking about the problems prior to November in order to be able to concentrate those thoughts. They were clearly visiting people, since Tomlin mentions they went to be briefed by Sir Charles Wright at the Admiralty who suggested alternatives to the rhumbatron. Wilkins **(Q54 and Q55)** recounts that it was suggested to Oliphant, during a visit of his to the Admiralty at Haslemere, to combine the split anode magnetrons with klystron resonators and Sayers recounts Oliphant suggested to Randall that he investigate whether copper resonators could replace the split anodes in magnetrons and also that he (Sayers) gave advice to Randall on magnetron resonators. Wilkins also writes **(Q55)** *Oliphant confirms his involvement in initiating the cavity magnetron though the form of the resonant cavity was Randall's: 'His elongation of the Hertzian detector was brilliant'.* This latter sentence suggests that Randall may have been influenced by Oliphant and that Oliphant was fully supportive of the efforts by Randall and Boot, although he gave full credit to Randall for the elongation of the Hertzian detector.

Extracts **Q54** and **Q55**, with that of Tomlin (10) show that Randall and Boot were not working in isolation but were being advised on possible ways forward. There are other indicators that they were well briefed.

Extracts **Q16, Q53**, and **Q22** tell of the general influence of the working environment but the latter part of **Q22** is significant in the author's opinion: '*We were **well aware** of modifications that had taken place in magnetron construction during the pre-War years particularly at the GEC Wembley; General Electric in Schenectady and also in Germany. As a former colleague of E.C.S. Megaw at the Research Laboratories of the GEC, Wembley, I was aware of his activities in this field.'*

The words 'well aware' tell that Randall and Boot must have been very well briefed indeed. Since they had no detailed knowledge of magnetrons in September 1939 ***(Q43**, 'Apparently, neither of these physicists had any great previous knowledge or experience in the magnetron art*….), to be 'well aware' of developments in pre-war years in the UK, US and Germany (interesting in itself) a month or two later means they must have been brought up to speed by those well experienced in the field. Since the numbers of patents and papers about magnetrons had proliferated during the 1930s, to have made an independent study of them all without guidance would have taken many months. Randall and Boot acknowledge the situation themselves ***(Q6)** 'Fortunately we did not have the time to survey all the published papers on magnetrons or we would have become completely confused by the multiplicity of theories of operation'.* Wathen confirms this ***(Q43)** '...they were unhampered by prejudices based on the preceding experience of others or on the confused statements in the rather large amount of literature already in existence on magnetrons'.*

Randall and Boot also *said (**Q6)** 'The most acceptable paper to us was that of Posthumus [l] which dealt with the principle of rotating electric fields and traveling waves in magnetrons and also considered greater numbers of segments than two'.* Unless they were told it was significant, given they did not have the time to read all the literature, they could not have made that statement. The author believes that the person who was most able to help Randall and Boot and set them well on their way on the right path was E.C.S. Megaw of the G.E.C. Company, Wembley. He was an acknowledged magnetron expert with many years' experience, publications and contacts. Indeed, he had published with Posthumus a statement agreeing on Posthumous's theory of rotating-fields after an academic disagreement in the literature **(Q57** *'The dispute ended later in the year with a joint contribution to Nature by Posthumus and Megaw. They expressed agreement that the term dynatron as applied to high-frequency magnetrons was inappropriate in view of the accurate predictions afforded by the rotating-field theory'***).** Apart from his expertise he had excellent contacts with the French and it was to him they brought the magnetrons with oxide coated cathodes in 1940. (Extracts **Q20**, **Q6**, and **Q18** tell of the French input via. the large oxide coated cathode and how that was known to GEC at Wembley then to the Birmingham workers at the end of 1939 **(Q20)**.)

But, what of Megaw? His paper of 1946 (33), sets out his view of the development of the RCM. Although due credit is given to Randall and Boot, the author feels that (reading between the lines again) much is being said about the input of others to the device in a restrained and subtle manner.

In **Q27** Megaw summarises the events as a series of steps within a systematic design procedure based on pre-war work. *'The result of these steps was an immediate increase in pulse power and life by a factor of at least 10, with a similar reduction in magnet weight.'*

Megaw defines the steps as the multi resonator system of Randall and Boot, the large oxide cathode of Gutton and the combination of them into a manufacturable device with a small permanent magnet which was most suitable for rapid introduction into service use. He appears to give equal weight to all three. He then adds '*The systematic development of design procedures, based on pre-war work, played a major part in the 100-fold increase in pulse output power at 10cm, which was achieved between June and December 1940'.*

In quotes **Q28** and **Q29** Megaw points out his own experience and the proliferation of papers on the subject between 1933 and 1939 – many of which, he states, were unhelpful. He confirms the importance of the theoretical work of the Japanese in the late 1920s, that of Posthumus in 1934-1935 and mentions a contribution from himself and two colleagues, Herriger and Hulster, which helped to clarify the operation of magnetron devices. He points out in **Q30** both the erroneous conclusions of some German contributors that large diameter spiral tungsten cathodes lead to a drop in efficiency and that early work on oxide coated cathodes resulted in short lifetimes due to inadequate cooling, although GEC had apparently overcome some of the problems in their E880/NT75 magnetron valve. In quotation **Q31** Megaw discusses the problems of multi-anode magnetrons and the early belief that having more than 4 segments produced no benefits. In addition, many segments also made starting oscillations difficult. Posthumus used eccentric cathodes to overcome the latter problem but although higher frequencies could be obtained with more segments, efficiencies were low and wavelength range was limited. In the commercial market, it was considered that high frequency generators should have at least two octave ranges and efficiencies of around 50%. There was no sense that a fixed frequency, low efficiency device was marketable, as the perceived need was for communications devices which produced signals which could be varied in frequency and modulated.

Megaw and colleagues investigated modulating magnetrons by space charge grids and investigated resonator frequency stabilization and found that it was a very effective technique at 50 cm (**Q32**). In collaboration with a colleague from the H.M. Signal School – Mr. J. F. Coates – a very successful demonstration was made of pulsing a c.w magnetron: '*On the application side typical examples are our detailed study of modulation by space-charge grids and the development of resonator frequency stabilization as a highly effective practical technique for wavelengths of the order of 50 cm; this last had in fact a direct bearing on some of the circuit problems of centimetric magnetrons. And finally, as an early indication of the shape of things to come, a test carried out with Mr. J. F. Coates of H.M. Signal School in November, 1938, may be mentioned. A pulse output of 1.5 kW at 37 cm was obtained from an E821 magnetron, designed for 150 W c.w. output at 1 metre; it was concluded that no fundamental problem was involved in short pulse operation of magnetrons.*

The quotation **Q34** summarises Megaw's view of the situation current in 1938 in which he states much of the essential background work had been achieved, although there were still different opinions on cathode size. He believed then that internal oscillatory circuits were necessary and although some designs had appeared in patents and literature they were either not pursued commercially or were ineffective. He mentions work by Slutzkin in 1934 which laid the basis of many of the designs and mention is made of two devices for 3 cm and 5cm which were developed in 1937 and 1940 respectively (presumably by GEC, as no other originator is named). The latter used a metal envelope as a waveguide output for the energy

(**Q35**). A footnote (**Q38)** notes the Samuel patent 2,063,342 lacked a means of extracting the energy – a problem with other designs as well.

Megaw does not mention either the Japanese or Russian cavity magnetrons in his paper. He should have known about the latter, since the original paper of 1940 had been re-published in the US in 1944, and may well have known about the former when he wrote his account for the IEE convention in 1946. Why they are not mentioned is a matter of conjecture alone. Whether it was because he was writing about the situation prior to 1940 and he had not heard about the devices then so they do not appear in his list of prior art, or for other reasons, is unknown.

So, with much work done, internationally, both useful and not, a working understanding of what was happening inside a magnetron, oxide cathodes on their way, pulsed operation shown to be possible and the need for internal oscillatory elements established, the ground was fertile for the next step (**Q33**). Quotation **Q37** from his paper adds to this list the stimulating effect of the development of klystron cavity resonators and their use of inductive loop coupling and then gives Randall and Boot the credit for making their contribution. (**Q37)**. *The stimulating effect of the development of cavity resonator techniques in the klystron, and not least the loop-and-line technique for coupling to the load, must also be noted. It was under this stimulus that Randall and Boot developed the multiple circuit copper-block structure in a practically fruitful form which provided the basic solution of the centimetre-wave circuit problem adopted for all the subsequent developments in this country and the United States.*

Megaw makes the point that it was Randall and Boot who generated the idea of the RCM as it was developed and never tries to claim that it was his (or anyone else's) idea. (**Q33***) It is generally known that early in 1940 an experimental high-power magnetron using cylindrical cavity resonators as circuit elements was independently produced by J. T. Randall and H. A. H. Boot* at Birmingham University, and that the rapid development of magnetrons of this kind as powerful pulse generators opened up the field of centimetric radar so far as transmitter requirements were concerned.* In his paper, he continues the early history with an account of how the months of April to July progressed after Megaw says **(Q39)** '*In March 1940 an urgent need arose for a pulse transmitter on about 10 cm for A.I. radar. ………..….A few weeks later contact was made with the work of Randall and Boot at Birmingham University'.* Megaw goes on **(Q40, Q41)** to relate how their own AI radar design (perhaps that one mentioned in **Q35)** using a four-segment magnetron was put aside in favour of the Birmingham device and how that was developed by GEC Wembley into a practical, usable, radar friendly valve.

UNKNOWN UNKNOWNS

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CONCLUSIONS

From the preceding discussion, the author believes the following happened:

In the autumn of 1939, with the war starting and the pressure for defence equipment building up, with such a strong need for a centimetric generator by yesterday, everyone was working flat out for that goal, without thinking of long term consequences, commercial or credit wise. There was an urgent job to be done and I think all groups piled in during late 1939 and 1940 to get on with it. Randall and Boot were not magnetron experts in the summer of 1939, but they certainly were by February 1940. I believe they were well briefed by Megaw at GEC and perhaps also by Admiralty research staff before they started on their design, which is how they knew the problems of other designs, which papers to focus on and in which direction to focus their thinking.

They designed, built and tested their cavity magnetron and it worked. With Megaw's expertise it was rapidly improved in terms of reduced physical size, mechanical structure and its cathode type, and the skills were learned and practised at Birmingham so they could, and did, produce their own devices. As we know, progress was decisive and rapid and late in 1940 the Tizard Mission took the knowledge to the USA with E1189 no. 12.

When Randall and Boot returned from Ventnor, deliberately or otherwise, they were not allocated to work in the mainstream klystron project. This gave them time to reflect on the generation of high power microwaves, the limitations of klystrons and the possible benefits of magnetrons. The author believes they had one or more briefings from Megaw, who was well able to point them in the right direction. As a leading magnetron expert, he was in a position to feed Randall and Boot with plenty of information: the current state of the art, the problems with existing patents and devices, the confusion of much of the theoretical literature, which papers to concentrate on and the up to date information on cathode design, and which paths to avoid; they also had advice from the Admiralty, Oliphant and Sayers. Just how many of these meetings they had the author does not know, but there were enough to make them 'well aware' of past work and to give them ideas for future development.

Armed with this and the buzz of Oliphant's lab with its focus on resonant structures, not to mention the book on Hertzian oscillations, and with the weaknesses of previous magnetron designs pointed out to them, and seeing the problems with the klystron as a high-power oscillator, coupled with the fact that for military operations a single frequency was sufficient as the devices were pulsed and no modulation was required, then fixed tuned resonators would be fine. The klystron work going on around them would also make them 'well aware' of internal cavity resonators and inductive loop pick-up.

Both Randall and Boot were exceptional and made good use of the advice, their own reading and their ability to get things done.

Randall and Boot studied the problem and with the scene well set, they came up with the concept of an anode block, containing a central cathode in an anode/cathode interaction space, with resonant cavities; symmetrically spaced around that interaction space, end spaces between the edge of the anode block and end caps to ensure good coupling between cavities and power was to be extracted by an inductive loop in one of the cavities.

This enabled Randall and Boot to complete the first, lab-based, system, which performed very well on the 21st February 1940, and this marked the end of the first phase and was probably the extent of Randall and Boot's significant innovative input. It worked well in their lab, but it was an elephantine structure. Following the success of the run, others followed until it was certain the operation of the new magnetron was reliable and then Megaw was brought in for the second phase, which was the physical improvement of the device, pulsed operation, the introduction of oxide coated cathodes and design for manufacture. The author suggests that Randall and Boot had little innovative input into the second stage and that their ground-breaking work was essentially finished by the February the 21st demonstration, but they had made the novel, innovative steps which paved the way for generations of magnetron devices and at exactly the right time for the war effort.

Megaw rapidly improved design and construction and made it into a compact, manufacturable, usable device for avionics.

Although I believe Randall and Boot had been well briefed, I do not believe anyone gave them a design or drawings, for example, to build. If such a design existed it would have surfaced before they started. The author concludes Randall and Boot did design the resonant cavity arrangement themselves, but did have extensive help and guidance in the beginning. After their initial successes, Megaw and G.E.C. made it into a practical, usable device. Practically, the Birmingham magnetron was useless, but it paved the way.

Examining the UK patent 588185A shows how the various influences were combined.

The first part is a provisional description submitted August 22nd 1940, by three applicants – Randall, Boot and Charles Wright of the Admiralty (**Q58**). It describes a 6-cavity device, with a tungsten filament cathode (treated to increase emission or indirectly heated), water cooled or air cooled, using a powerful electromagnet or permanent magnet for c.w or interrupted cw. It is characterised by a plurality of cylindrical resonators arranged symmetrically about the central, circular anode/cathode space with small gaps joining that space to the resonators. There are end spaces and output is via. a single copper loop into one of the resonators. It is stated that high output powers are possible because high emission currents can be generated as no beam focusing is required as in oscillators of the klystron type. No claims were made at this stage.

The second part is the complete specification, submitted August the 15th 1941 by the same three applicants. It is far more detailed, giving a general description, a theoretical basis with some design equations and material and manufacturing details. The figures are explained and a series of claims made. In addition to the usual oscillator function, claims are made for the inclusion of control grids.

The main claim is for the plurality of cylindrical resonators around a central circular space containing an oxide coated, indirectly heated cathode with end spaces enclosed by end caps. The end caps are secured by gold or tinned silver washers, pressed and then heated so that the washers alloy to the end caps and main body. The interaction spaces are completely enclosed in a conducting body. Output is via. an inductive loop. The figures show an 8-cavity device.

So, what is being patented in 1941 bears little resemblance to the original device Randall and Boot made for the February 1940 trial. It has an 8-cavity anode, sealed by a GEC gold washer technique, with a cathode shown by the French to be far better than the tungsten wire previously used. The device drawn in the patent diagrams appears to be that of the GEC engineered product not the laboratory experimental device.

How this came about and why no mention is made of GEC in the patent, in spite of the fact the device package was substantially GEC, must qualify as a known unknown. Clearly, there had been much behind the scenes activity.

Once the device had gone to America and it proved to be such a success, then from early 1941 onwards (and especially after timeline 3 and strapping) it began to dawn on people just what had happened, and what had been invented and the academic and commercial possibilities that were emerging. The author believes that some positioning began to occur as to who did what, as the various parties tried to ensure their contributions were recognized one way or another. This positioning even occurred between Randall and Boot as (**Q2**) Randall most definitely states Boot played no part in the development of the Hertzian dipole elongation idea. In the 1946 papers (**Q12, Q20**) G.E.C. Wembley, Gutton and Berline and Duke are recognized, although no mention is made of Megaw's name. Many years later in 1976 and 1986 publications (**Q9, Q22, Q25**), describing the wartime years, Megaw is named along with others. The author believes this probably reflects the positioning going on just after the war between individuals, companies and government departments, but once the dust had settled 30 – 40 years later, Megaw was included.

As stated at the beginning of the paper, the author has not presented any new material, nor any new, significant insights. There were, clearly, RCMs in existence prior to 1940 in Japan, Switzerland, Germany and Russia, but it was the British in 1940 and 1941 who made it into a significant device that made such a substantial contribution to the war effort. Randall and Boot were at the forefront of its development, as were Megaw, Gutton and Sayers.

The author has tried to approach the task and draw conclusions from the written evidence, from reading between the lines and from giving significance to certain words or phrases. Readers can decide whether or not this is valid and the author welcomes comments. In particular, if any of the sections in the 'known knowns' can be expanded upon that would be very helpful. The section on the Turbator could well do with clarification, for example, and any details of the device would be welcomed as it seems one of the least known. Sanitas and Helbig also looks very interesting as they were approaching the magnetron from a very different perspective at first.

One can speculate on what might have happened if Randall had gone to Bristol or if Megaw been killed in an air raid or the French prevented from bringing their devices to England or Sayers taken seriously ill? However, the 'what ifs' did not happen and the RCM did. And there the story will stand unless an unknown unknown suddenly changes state to become a known unknown.

QUOTATIONS

The author and DEHS gratefully acknowledge the permission from the IEEE to use the quotations Q1 – Q10.

H.A.H. Boot and J.T. Randall, "Historical notes on the cavity magnetron," IEEE Transactions on Electron Devices, vol. 23, no. 7, pp 724 – 729, Jul 1976. © 1976 IEEE doi: 10.1109/T-ED.1976.18476

Q1. There was no lengthy research program leading to our discovery of the cavity magnetron at the end of 1939, but there existed a very special set of circumstances in which we were, in some ways, fortunate to be involved. We were fortunate in that we were in Oliphant's laboratory at the University of Birmingham. (p. 274)

Q2. In fact, there was really no interest in microwaves until the summer of 1939. (p. 274)

Q3. But now we were all interested in the possibility of producing microwaves. We had seen how very large powers at 10-m wavelength could be produced by pulsed operation of relatively small tubes, and the prospect of doing this at microwave frequencies for air-borne and ship-borne use was most exciting, but apparently impossible. So we all returned to Birmingham to try. (p.274)

Q4. It seems to have been taken for granted that the wavelength to aim for was 10 cm or less, which seems rather ambitious in retrospect. In a very short time Sayers, who had joined the team from Cambridge, had constructed a continuously pumped c.w klystron operating at about 10kV anode voltage. (p. 274)

Q5. We had been allocated the less spectacular task of making miniature Barkhausen-Kurz tubes as possible receivers and were also trying to excite cavities by gas discharge tubes. We were unsuccessful and disenchanted with this task. (p. 274)

Q6. The only other known sources of microwaves were sparks and split anode magnetrons. The latter were glass envelope tubes usually with only two segments giving vanishingly small powers below 10 cm, and their efficiency was very low. We found them interesting initially because it was obvious that their geometry should allow large input powers. No focused beam was needed as in the klystron. We also knew that these early magnetrons had been made to work in France with large diameter oxide cathodes so we felt sure that large peak anode powers could be used if a properly engineered design could be devised. Fortunately, we did not have the time to survey all the published papers on magnetrons or we would have become completely confused by the multiplicity of theories of operation. The most acceptable paper to us was that of Posthumus [l] which dealt with the principle of rotating electric fields and traveling waves in magnetrons and also considered greater numbers of segments than two. (p. 274)

Q7. In November 1939, at the risk of incurring some unpopularity from our fellow workers, we concentrated our thoughts on how we could combine the advantages of the klystron with what we believed to be the more favorable geometry of a magnetron. (p. 274)

Q8. We had to confine the radio frequency fields in resonators, as did the klystron, in order to get high circuit efficiency, and it must be made of solid copper to dissipate the power we felt could be put into a magnetron-like geometry. It was realized that many different shaped cavities could be considered as resonators (Hansen and Richtmeyer [2]). Hertz in 1889 had also used a resonator in the form of a wire bent into a circle leaving a small gap as a detector in his early experiments, but this was not a cavity. A cylindrical extension of Hertz's wire loop became a cylinder with a slot along a generator and it occurred to us that a number of these would fit around the slotted anode of the magnetron we were trying to invent. Also it would be very simple to make in the laboratory workshop. Only drilling, turning, and slotting would be needed. It also occurred to us that a series of 1/4 wave deep radial slots would also serve as resonators as they were 3-dimensional versions of a lecher line. The first anode block was made there in December 1939 and is shown in Fig. 1. The resonator diameter was 12 mm because H. M. Macdonald [3] in 1902 in his book, ***Electric Waves,*** had calculated that the resonant wavelength of Hertz's wire loop resonator was 7.94 times its diameter, and we were aiming to produce 10-cm radiation. It was thought unwise to use more than six resonators initially particularly as a 12-mm diameter anode seemed to suggest reasonable values of anode voltage and magnetic field. The slots were 1 mm wide and 1 mm deep, and this gave sufficient copper between the resonators to provide adequate anode cooling by means of an external water jacket. An oxide cathode was thought too complex for a start so the thickest tungsten wire for which there was a heater transformer was used, namely 0.75 (sic) diameter. It was known that magnetron cathodes should have "end hats" to prevent axial escape of the electrons, and the end spaces of the anode block were as much to accommodate these as to provide coupling between the resonators. (p. 274/275)

Q9. This unexpectedly spectacular beginning in copper and sealing wax was obviously going to need additional effort. The results were communicated to Sir Charles Wright, Director of Scientific Research, Admiralty, and other government scientists. The research laboratories of the General Electric Company, Wembley, England, had close links with the Admiralty at that time in that they were engaged in work to produce 50-cm power for shipborne radar, ***and there worked E. C. S. Megaw who had for some time been concerned with conventional magnetrons. It was arranged for him to visit Birmingham in the hope that help could be provided soon with the unfamiliar technological problems of constructing a sealed off cavity magnetron of copper and glass and brazed joints; for we wished to make a tube in which the cathode was oxide coated and supported on two side arms with Housekeeper seals similar to the output arm used on the first continuously pumped tube.*** This design has scarcely changed to date as can be seen from Fig. 4, and it was made possible at that time by the technique introduced by the GEC of making vacuum tight seals by compression bonding of copper by gold wire rings.

The first two sealed off magnetrons were made at GEC, Wembley, one an exact replica of the tungsten filament design described above and the other having a 4-mm diameter oxide cathode, indirectly heated. The anode was shortened to 2-cm active length to fit into an existing permanent magnet, and air cooling fins were added. ***Moreover, S. M. Duke of their technical staff was seconded to Birmingham*** and supplies of indirectly heated cathodes were arranged from other industrial tube research laboratories so that work on pulsed magnetron operation could get under way. Powers exceeding 10-kW peak were obtained at 10-cm wavelength. (p. 275/276)

Q10. The foresight in high places in England which had released the secrets of radar to university scientists still persisted. Sir Henry Tizard, scientific advisor to the British Government in two world wars, had been pressing for some time for a similar disclosure of scientific and technical information to the United States government so that their enormous potential for development and manufacture could be put to effective use. Eventually, he was allowed to proceed as far as Canada where he saw the Canadian Prime Minister, Mackenzie King, who was visiting President Roosevelt the next day and who promised to discuss with the President the possibility of receiving a scientific mission from England. On the 21 August 1940, the President agreed to meet Tizard at the White House on 26 August. Tizard also received permission from his College in London to leave England on 21 August 1940! (p276)

J.T. Randall: Radar and the Magnetron. Journal of the Royal Society of Arts, vol. 94 (4715), April 12th 1946, pp. 302 - 323
Sir Robert Watson-Watt, C.B, F.R.S., in the Chair.

Q11. One of the chief limitations of the klystron as a power oscillator lies in the difficulty of producing high current electron beams of suitable cross-section. This limitation led Boot and myself to think of a possible alternative, and we realised that a magnetron would be free from this defect. Having the example of the klystron before us, it appeared that existing magnetrons made no proper use of internal resonators. The resonators of the klystron were made of copper to give low losses; the nature and shape of the resonators were such that high frequency stability was provided for; moreover, their high heat conductivity gives means of ensuring large heat dissipation when high power is used.

No existing type of resonator was of a shape suited to our purpose. In November, 1939, we hit upon the idea of using cylindrical resonators slotted so that the slots were parallel to the cathode axis, and opened into the anode-cathode space. This type of resonator, illustrated in section in Fig. 1a, is really a cylindrical extension of the original Hertzian dipole. An alternative resonator consisted of a short-circuited quarter wavelength of parallel wire transmission-line (see Fig. 1b). Each of these designs, as Fig. 1 shows, was of such a form and shape that a number of them could be arranged round a central cathode. (p.307)

Q12. The obvious steps are frequently the correct ones, and these followed each other, both in the University of Birmingham and in the Research Laboratories of the General Electric Company, during the next few months. Fig. 3 shows a view of the very first cavity magnetron in which wax joints and other crudities of initial trials are evident. From this to a sealed-off magnetron, with robust copper glass seals, in a form suitable for service use, was a step in which our industrial friends at Wembley and our colleague, Mr. S. M. Duke, played a great part. The introduction of oxide-coated cathodes was a step which enabled still higher powers to be obtained. Before long, pulse-operation was achieved, and peak powers of 10-15 kilowatts at 10 cms. were obtained at efficiencies of 10-20 per cent. Before the autumn of 1940 experimental tests had been completed which showed that the cavity-resonator principle could be applied over a wide range of design; power was produced at 5 and 3 cm. wavelengths; using 30 resonators of the slotted type, shown in Fig. 1b (p. 305), oscillations of low power were demonstrated at 1.9 cms. (p. 308)

Q13. Mr. D. M. Tombs: With regard to the history of the development of cellular magnetrons, I understand that in Switzerland in 1941 F. Ludi produced them before it was popularly known that they were being produced here.
Professor Randall: Certain ideas were published by Brown-Bouverie which had some similarity to those developed in this country; the ideas were as far as can be ascertained never developed to the stage of producing a power oscillator of the type developed in Britain.
Mr. P. Parker: I believe that a paper was published by the Institution of Radio Engineers about 1940 on the subject of the magnetron and that it was written by two Russians.
Professor Randall: The I.R.E. paper appeared much later than the speaker mentions, so far as I am aware, and was in essence a reproduction of the Russian work. The Russians, like the Swiss, had only worked with very small amounts of power and did not in any way realize to the full the possibilities of the idea. The British work was carried out entirely independently; we had no knowledge of it until quite late in the war. (p. 313)

Q14. Professor Randall has referred to some of the facets of a co-operation which, I believe, was unsurpassed and unequalled in any part of the war effort. It was a co-operation in which the natural philosopher and the engineer in the university worked with the physicists and the mathematicians and the workers of all kinds in the industry, with the men in the Government establishments and with the uniformed Forces, from top to bottom rank, making an extraordinarily reassuring and happy story of full interplay between all the contributory factors necessary to the winning of the war. (p. 314)

The author and DEHS gratefully acknowledge the permission from the IET to use the quotations Q15 – Q41.

H.A.H. Boot and J.T. Randall: The Cavity Magnetron. "Proceedings at the Radiolocation Convention," Journal of the Institution of Electrical Engineers, pp. 928 – 938, vol. 93, part 111 A, 1946. © 1946 IEE

Q15. Initially, under Oliphant, a considerable attempt was made by Sayers and others to improve the klystron as a centimetre-wave power oscillator, and in fact a good deal was achieved in this connection. At the same time, an attempt was made by the authors to use the Barkhausen-Kurz valve as a detector: before long, however, interest moved towards the central problem of the production of high power at or below 10 centimetre wavelengths. It appeared that the major difficulty in the way of attempts to improve the klystron by a large factor was that of getting sufficient power into the electron beam, necessarily of small cross-sectional area: it was realized by the authors that a magnetron would be free from this defect. One of the outstanding advantages of the klystron was the use of internal resonators such as had been described by Hanson[2] and Rayleigh[3]. Resonators of this type, turned from solid copper, give low h.f. losses, high frequency stability, and are capable of large heat dissipation.
The problem was, therefore, to design a magnetron, capable of giving a large anode current, and which also incorporated a resonator system of suitable properties.
The main investigation therefore initially resolved itself into (a) the design of a suitable type of resonator, the determination of its size, and the method of grouping a number of resonators in a device of cylindrical symmetry. (b) A method of construction which would

ensure high electrical and thermal conductivity. (c) The design of an h.f. output-circuit in relation to the chosen resonator system. (p. 928).

Q16. The Hanson papers were by this time available in the laboratory, but it was clear that the designs (hollow spheres, cubes and "doughnut" shaped cavities) could not be associated with the cylindrical symmetry of the magnetron. It was decided therefore to use either a three-dimensional extension of the well-known Hertzian wire loop or a corresponding extension of a short-circuited quarter-wave line. (p. 928)

Q17. One of the outstanding features of the design at this stage was the use of an enclosed resonator-anode system, and in this feature, it differed radically from all earlier magnetrons known to the authors. (p. 929)

Q18. Furthermore, although a tungsten cathode 0.75 mm in diameter was used (as being more suitable for c.w operation), it was also realized from the French publications available that the use of an oxide-coated cathode was desirable. The subsequent introduction of such cathodes by both the Research Laboratories of the General Electric Company, and the Birmingham group is referred to below. (p. 929)

Q19. It is perhaps useful to summarise the main conclusions which had been established by the first experiments outlined above

(i) A magnetron with a completely enclosed resonator system had been successfully operated at a wavelength of 10cm at an efficiency (10 – 15%) comparable with that of other known high-frequency such as the klystron.

(ii) The use of a number of closely coupled resonators in the same oscillator apparently offered no difficulty.

(iii) The use of a combined copper anode block and resonator system ensured adequate heat dissipation, low electrical losses, and the potentialities of simple manufacture.

(iv) The use of a single, simple, output circuit to draw h.f. power from the system was established and it was clear that such an output circuit would enable power to be fed in a comparatively easy manner to aerial systems and wave guides. (p. 929)

Q20. The following steps in the development of the valve were obvious but necessary consequences of the early experiments.

(a) The completion of a sealed-off version of the valve.

(b) The testing of other designs – e.g. different numbers and sizes of resonators.

(c) The introduction of large cathodes to provide the high anode current necessary for high-power operation.

(d) The introduction of a modulated power supply.

(e) The adequate measurement of r.f. power.

In the achievement of (a) considerable help was obtained, directly and indirectly, by collaboration with the Research Laboratories of the G.E.C., Wembley with whom information was exchanged in April 1940. In this way, a great deal was learned about the technique of sealing glass to copper, and of joining copper to copper by means of a gold wire under pressure between the two surfaces at temperatures below 500°C. Indirectly great help was received from S. M. Duke, a former member of the Wembley staff, who joined the Birmingham group in June, 1940.

An important feature in the development of the cavity magnetron was the introduction in May – June, 1940, of large oxide-coated cathodes. The French workers Gutton and Berline were already using such cathodes in the more conventional split-anode magnetrons just prior to the war. This information was already available to G. E. C., Wembley, and at about the end of 1939 to the Birmingham workers. Shortly after the first experiments described in this paper both the G. E. C. Laboratories and the authors produced samples embodying this feature; this enabled peak-power outputs of 10 – 15 kW to be obtained at efficiencies of 10 – 20% within a few months of the initial experiments. (p. 929/930)

M.J. Lazarus: Electromagnetic radiation: megahertz to gigahertz. A tribute to Heinrich Rudolf Hertz and John Turton Randall. I.E.E. Proceedings, vol. 133, Pt A, (2) March 1986 pp. 109 – 118. (History of Technology) © 1986 IEE

Extracts Q21 – Q24 are taken from a letter sent to Lazarus (09/04/1984) by Randall and which were quoted in full in Lazarus's paper.

Q21. In 1939, we had a flat in Aberystwyth which we used for holidays and which was a prospective place to which my wife and small son might go as and when war began. In July 1939, we were spending a short holiday there and already knew from the activities of John Cockcroft and Watson-Watt that groups of physicists from various universities, including Birmingham, were to pay visits to the long wavelength radar stations established on our south and east coasts by Watson-Watt and his colleagues. These visits were to last several weeks, the object being to acquaint us with the operation of radar at a wavelength of approximately 11 m. The "obscure" bookshop you mention was not in the least obscure and was in effect the University College bookshop in Aberystwyth and went under the name of Galloway. Browsing in this shop one day, I found a copy of Jones's translation of Hertz's "Electric waves" which I acquired with some interest in view of our coming radar activities. At that time, no thoughts of magnetrons of any kind had occurred to me, but the use by Hertz of small rings of wire each with a short gap as detectors of his electric waves stuck in my mind. Boot was in no way concerned with this. (p 115)

Q22. When Boot and I eventually returned to Birmingham the more interesting tasks had been assigned and we were left with the task of discovering whether a Barkhausen-Kurtz tube could be used as a detector. After several weeks experiments in the corner of a large teaching lab we decided that this was not possible. Boot and I worked very closely together both then and throughout my subsequent stay in Birmingham which terminated in the autumn of 1943. Consequently, during this early comparatively leisurely period in 1939 we were isolated from the main stream of activity in the lab and had plenty of opportunity to discuss the problem of short wave transmitters and particularly whether the klystron as it then was could produce the power required to give a satisfactory early-warning system. We concluded, as history shows, that it would not be sufficiently powerful.

An important factor should be recorded at this point. The whole lab was buzzing with talk of resonators and particularly of the Hansen papers. This naturally had a great influence on Boot and me as well as other members of the team; and we have always fully acknowledged that the atmosphere in which we worked influenced the direction of our thoughts. The only device we were able to think of which could possibly combine the desired attributes of resonators and high-power output was a magnetron. We were well aware of modifications that had taken place in magnetron construction during the pre-War years particularly at the GEC Wembley; General Electric in Schenectady and also in Germany. As a former

colleague of E.C.S. Megaw at the Research Laboratories of the GEC, Wembley, I was aware of his activities in this field.
However, none of these magnetron devices was satisfactory from our point of view, since they did not embody truly enclosed resonators and consequently showed marked frequency-pulling on load. It was at this stage that I thought of the cylindrical extension of a Hertzian dipole and Boot and I jointly tested the system of 6 such resonators clustered round and facing into the anode-cathode space. We presumed that there would be strong electromagnetic coupling between the different resonators and therefore inserted a coupling loop so that power could be withdrawn from one resonator only. (p. 115)

Q23. We had the idea of the cavity magnetron in November 1939 and we showed the first copper block to Bragg and Appleton when they paid a visit to the lab during that month. (p. 115/116)

In a further letter to Lazarus, (12/04/1984) Randall added some more information:
Q24. There is an old book by H.M. Macdonald, Professor of Mathematics at Aberdeen University in the earlier part of this century. I think the title is "Electric waves". In this book, he calculates the wavelength of radiation to be expected from a Hertzian dipole of diameter *d* and finds the value, if my memory is correct, of 7.98 *d.* As you know, the observed wavelength of our first block turned out to be 9.8 cm. This was measured a few days after the first operation by a simple Lecher wire system employing a small neon lamp.'

The 'old book by H.M. Macdonald' was in fact the published edition (Cambridge University Press, 1902) of an Adam's Prize Essay written by the Cambridge mathematician H.M. Macdonald F.R.S., with the expressed purpose of analysing Hertz's experiments. For a Hertzian loop dipole resonator of diameter *d* Macdonald calculated a wavelength $X = 7.95\ d$ which indeed agreed closely with experiment for d = 12 mm in the first cavity magnetron. (p. 116)

Q25. By contrast with the pioneering work with sealing wax, industrial development of a rugged portable device, with high-current, oxide-coated cathode, was extremely rapid, notably with the aid of E.C.S. Megaw and S.M. Duke of G.E.C., Wembley. Impressive 10 kW pulses at 10 cm wavelength were soon achieved. A radar operating at 10 cm was in operation by May. Following the famous scientific mission to President Roosevelt in the USA, the 10-cm magnetron was demonstrated at Bell Laboratories in October 1940. By November, 1940, Bell Laboratories had supplied working copies of the British magnetron to the radar research teams at MIT. (p. 116)

Q 26. Harry (Henry) Boot loved to play jokes on people. One of his pranks was to spread a story that the first cavity magnetron block was the rotating part of a six shooter! His sense of fun resembled the impish wit of Maxwell and Heaviside. (p.118)

E. C. S. Megaw: The High-Power Pulsed Magnetron, a Review of Early Developments. "Proceedings at the Radiolocation Convention," Journal of the Institution of Electrical Engineers, pp. 977 – 984, vol. 93 (5), part 111 A, 1946. © 1946 IEE

Q27. The multi-resonator system developed by Randall and Boot at Birmingham University and the large oxide cathode developed by Gutton in Paris for a different type of magnetron were combined in a construction, designed for use with a small permanent magnet, which

met the requirements for airborne service and was suitable for quantity production. The result of these steps was an immediate increase in pulse power and life by a factor of at least 10, with a similar reduction in magnet weight.
The systematic development of design procedures, based on pre-war work, played a major part in the 100-fold increase in pulse output power at 10cm., which was achieved between June and December 1940, (p. 977)

Q28. When the present author presented the results of the first study of the subject in this country before The Institution in 1933 the literature comprised a dozen papers; by 1939 it had multiplied more than tenfold (cf. Harvey, "High Frequency Thermionic Tubes," Chapman and Hall, 1943). Unfortunately, the published art did not gain as much in clarity as it did in volume and the real difficulties were added to by the tendency to postulate new "types of oscillation" to explain fresh facets of the subject as they were revealed by successive investigations. While there is much that is still not understood, even in the simplest of magnetrons, it is probably true that there is no need to invoke any processes of oscillation maintenance different from those recognizable in the Japanese work of the late 1920's in order to account for all the practically significant results between then and the present day. (p. 977)

Q29. Four-segment magnetrons were described in the early Japanese work, and a 12-segment system was tried, unsuccessfully, in 1932 by the author—with the idea that, if Hull's second solution giving circular orbits for the steady state were correct, very high frequencies might be obtained by reducing the inter-segment distance. Posthumus' successful development of the 4-segment magnetron in 1934-35 and the "rotating field" theory by which he explained its advantage over a similar 2-segment system was, however, an outstanding contribution and one which anticipated some of the results of recent theoretical work. There was, however, a good deal, especially in the behaviour of 2-segment magnetrons, which this theory did not account for and alternative explanations of the selective negative resistance effect were sought. It was only when the ideas of precessional resonance between the electron orbits and the standing wave of potential round the anode segments were developed by the author and by Herriger and Hulster to the point of yielding the same type of relationship between operating conditions and dimensions as that given by the rotating field theory that it was realized that the spiral electron paths of the latter were simply the mean of the looped paths to be expected in reality. (p. 977)

Q30. Almost all the magnetrons of the pre-war period used tungsten filament cathodes, which were quite adequate for the early experimental c.w. requirements; in the days when variable high voltage d.c. supplies were inconvenient and expensive it was even considered an advantage to be able to limit the input by reducing emission. German data on the effect of large-diameter spiral tungsten filaments indicated a drop in efficiency with increasing cathode diameter, especially in 4-segment valves, and this misleading result was widely believed. (p. 978)
Trials of oxide and thoriated-tungsten cathodes in the early 1930's gave bad results on life in the heavily-loaded, radiation cooled structures which had been established for pure tungsten filaments; the problem was solved in 1937 for thoriated tungsten, and this technique was used in the E88O (NT75) to meet the requirements of one of the earliest Service applications of magnetrons. (p. 978)

Q31, This restriction of established practice to small cathodes was also related to the general conclusion that the use of more than 4 segments was of little practical value; together they formed a kind of vicious circle which prevented the combination of many segments with large cathodes, now so obviously desirable for the shortest wavelengths, from following as a natural consequence from the earlier work. The fundamental point was simply that with a small cathode there is a large reduction in the oscillating tangential field near the cathode in a 4-segment as compared with a 2-segment system with the result that in the former, except at small values of *H/ffc,* the optimum load impedance is high and the starting of oscillations difficult. Although Posthumus' fortunate discovery that eccentricity of the filament in the anode (or less satisfactorily, of the valve in the magnet) obviated this difficulty at the price of an increase in minimum wavelength, 4-segment valves remained more difficult to make with uniform characteristics than 2-segment ones. The few studies that were made with more than four segments, and particularly with six, indicated an increase in these difficulties. There is little doubt that such valves could have been made with central filaments to cover a relatively small wavelength range with rather low efficiency; but they would not have appeared attractive at a time when useful ranges of one or two octaves and efficiencies of the order of 50% were regarded as normal requirements, with the "electronic" oscillator in the background as a wide-range low power source for the shorter wavelengths. (p. 979)

Q32. On the application side, typical examples are our detailed study of modulation by space-charge grids and the development of resonator frequency stabilization as a highly effective practical technique for wavelengths of the order of 50 cm; this last had in fact a direct bearing on some of the circuit problems of centimetric magnetrons. And finally, as an early indication of the shape of things to come, a test carried out with Mr. J. F. Coales of H.M. Signal School in November, 1938, may be mentioned. A pulse output of 1.5 kW at 37 cm was obtained from an E821 magnetron, designed for 150 W c.w. output at 1 metre; it was concluded that no fundamental problem was involved in short pulse operation of magnetrons. (p. 979)

Q33. It is generally known that early in 1940 an experimental high-power magnetron using cylindrical cavity resonators as circuit elements was independently produced by J. T. Randall and H. A. H. Boot* at Birmingham University, and that the rapid development of magnetrons of this kind as powerful pulse generators opened up the field of centimetric radar so far as transmitter requirements were concerned. In this Section, the main steps in this development and the considerations which prompted them are traced. (p. 979)

Q34. It will be seen that by 1938, although mistaken ideas were still current about the effect of cathode size and therefore about power-handling capacity, much of the essential background for the development of high-power pulsed magnetrons was already established. It was primarily the circuit problems for centimetre waves which remained to be solved, and it soon became clear that in solving them the normal ideas of tunable circuits external to the valve must be abandoned if high powers were to be obtained. Internal oscillatory circuits, more or less integral with the anode segments, appeared quite early in the practical art but were rarely favoured in industrial development on account of their inflexibility. In addition to the structures with axially resonant segments, developed in multiple form by the French, the combination of the anode segments with one or more oscillatory elements, which could be regarded either as sections of low-impedance line or as open-ended cavities, can be traced from the work of Slutzkin in 1934. Many interesting variants of these, workable and otherwise, are to be found in the patent literature. (p. 979/980)

Q35. A low-power single circuit design of this type developed in 1937 for about 3 cm is illustrated in Fig. 3; and a high-power 5 cm design (Fig. 4) illustrates the combination of a water-cooled resonant segment system with a metal envelope serving as output wave guide. (p. 980)

Q36. Fig. 4. Water-cooled resonant-segment magnetron design for high power c.w. operation at 5 cm wavelength (March 1940). (p. 980)

Q37. The stimulating effect of the development of cavity resonator techniques in the klystron, and not least the loop-and-line technique for coupling to the load, must also be noted. It was under this stimulus that Randall and Boot developed the multiple circuit copper-block structure in a practically fruitful form which provided the basic solution of the centimetre-wave circuit problem adopted for all the subsequent developments in this country and the United States. (p. 980)

Q38. * The earliest proposal for an anode-resonator system of the hole-and-slot type appears to be that in U.S. Patent No. 2063342 of 8th December, 1936 (A. L. Samuel). This, like subsequent similar proposals and laboratory designs, lacked a satisfactory method of coupling the resonators to the load. (footnote; p. 980)

Q39. In March 1940, an urgent need arose for a pulse transmitter on about 10 cm for A.I. radar. A 4-segment glass magnetron with thoriated tungsten filament was designed, to give about 0.5 kW peak directly into a wave guide with 10 kV 0-25 A input and 3,500 oersteds field. This was preferred, for the light mean loading involved, to a multi-segment design based on existing data for the supposedly different "tangential resonance" oscillations mainly on grounds of expected efficiency.
A few weeks later contact was made with the work of Randall and Boot at Birmingham University. At that time their 6-segment copper-block valve, operating on the pump, had produced about 150 watts c.w. output at 9-9cm with 7kV 0-15 A input. The field of 1300-1400 oersteds was produced by a large electromagnet with about 5 in air-gap. A 0-75-mm tungsten filament was used in a 12-mm anode, 40 mm long. Insufficient data were available to decide the type of oscillation involved but it was noted that the anode voltage agreed quite well with that calculated from the "tangential resonance" formula. In discussing the design, the author suggested that it could be improved and simplified, and the magnet weight greatly reduced, by using closed metal ends for the block, in place of the original glass-to-metal seals, and side-arm seals for the cathode. A sealed-off design on these lines (Fig. 5), with some further minor improvements, was produced in collaboration with Birmingham University; this was designed to fit the 2.75 in air-gap of a standard 50-lb electromagnet. Its performance, limited by the emission and life of the tungsten filament, was similar to that of the original model and reached outputs of the order of 0.5 kW. In this design the gold seal technique, developed by D. A. Boyland several years earlier, and suggested for use in the magnetron by R. le Rossignol, was introduced as a clean and simple method of attaching the copper end-discs to the block after mounting the cathode. (p. 980)

Q40. At the time of the first discussion at Birmingham the chief interest in the copper-block structure, so far as the commitments of the G.E.C. Laboratories were concerned, was as a basis for high power c.w. designs for communication on rather shorter wavelengths. But with increasing pressure on the need for 10-cm A.I. it was considered whether a design using this technique could provide a lighter and more powerful pulse source than the dull-emitter resonant-segment magnetron which was already in development, with good prospects of producing as much peak power as the Birmingham valve. Both of these, as they stood, involved electromagnets which were inconveniently large for airborne use, one on account of the large gap and the other on account of the high field-strength requirement. (p. 980)

Q41. A design was worked out on this basis using a block with cross-sectional dimensions nearly the same as those of the Birmingham valve. But the anode length/diameter ratio was chosen to give a good compromise between power and magnet weight, the end spaces were kept small so that an existing 6-lb permanent magnet with 1.5 in gap could be used, and a large diameter thoriated-tungsten spiral cathode was introduced.
It was estimated that this design should give at least 1 kW peak output at about 5 kV.
At this point the samples of Gutton's 16-cm resonant segment valve, M. I6 (Fig. 6), which had been promised in June 1939 were received. In the meantime, it had been greatly improved by the introduction of a large oxide cathode. In spite of the author's recommendation of the use of thoriated tungsten, following his own successful experience in pre-war magnetrons, the oxide cathode had been preferred on account of extensive French experience with it in ordinary transmitting valves. These 16-cm magnetrons, which had already given pulse powers of the order of 1 kW, were brought to Wembley by Dr. M. Ponte of the Compagnie Générale de Telegraphie Sans Fil and were disclosed to us with the authority of the French Government. This was the starting point of the use of the oxide cathode in practically all our subsequent pulsed transmitting valves and as such was a significant contribution to British radar. The date was the 8th of May, 1940. (p. 981)

R. L. Wathen: Genesis of a Generator – The Early History of the Magnetron. Journal of The Franklin Institute, vol. 255 (4), April 1953, pp. 271 – 287.

Q42. Magnetron studies both of theoretical and experimental nature began about 20 years before World War II. When the possibilities of the magnetron were first appreciated, workers in the United States, Great Britain, Germany, Czechoslovakia, Japan, France, and Russia began intensively to conduct research to increase understanding of the magnetron art (p.271).

Q43. Apparently, neither of these physicists had any great previous knowledge or experience in the magnetron art; thus, they were unhampered by prejudices based on the preceding experience of others or on the confused statements in the rather large amount of literature already in existence on magnetrons. (p. 275)

Q44. One must acknowledge that in other laboratories, microwave magnetrons had been under construction making use of anode structures having somewhat similar physical appearance to the British anode, but these devices always lacked some of the important features of the British magnetron and were inefficient and incapable of high power output. None of them ever produced more than a fraction of the energy generated by the original Birmingham magnetron. For instance, the US patent No. 2,063,342 (filed December 8th

1934) to A.L. Samuel is sometimes referred to as the original proposal for the multi-cavity resonator anode; however, the majority of Samuel's important claims were disclaimed and now appear in the Hansell patent 2,217,745 (filed March 20th 1934) and in the Linder patent 2,157,179 (filed July 2nd 1934). In both the Samuel and Hansell devices, the resonant circuit consists of grooved anode inductive elements and the capacitance between the cathode and the anode. Also N. T. Alekseev and D. E. Malairov (29) tell of Russian work in 1936 on a low performance magnetron with an anode asserted to be related to the British design. (p 276)

The completely new magnetron structure was designed by Boot and Randall (28) in November 1939, including the true cavity resonator type of anode system, the number of cavity resonators and their dimensions, and the coaxial line output device, the latter following the design of the coupling loop output circuit previously used in the klystron. (p 277)

Q45. At Birmingham, experiments continued finally to prove that the cavity resonator idea as applied to magnetrons was of a general nature; successful construction and test were made of an 8-cavity, 5-centimetre wavelength anode, a 14-cavity, 5-centimetre tube, a 6-cavity, 3-centimetre tube, and a 30-cavity, 2-centimetre tube, the latter having been tested by the end of September, 1940. (p 279).

Q46. In 1939, E.G. Linder (8) reported a contribution to the magnetron art which showed an important trend toward the designs used in more modern devices This was a split anode, tank-circuit magnetron in which the split cylindrical anode was made approximately one-quarter wave in length, the two anode segments being connected at one end to act as a tank circuit. An output of 20 watts at 8 centimeters and an efficiency of 22 per cent was achieved. C.W. Hansell (9), C.W. Rice (10), A.L. Samuel (11) and many others were active in the magnetron field prior to 1940. In general, however, while the results produced by early magnetron research were of considerable academic interest and served to generate a general picture of the mechanism of oscillation of the magnetron, none of the tubes devised was particularly suited to use in practical communication systems. Only with more mature understanding of circuits suitable for microwave-length use, could there follow a significant advance in the magnetron art. (p 273)

M.H.F. Wilkins: John Turton Randall. 23rd March 1905 – 16th June 1984. Biographical Memoirs of Fellows of the Royal Society, vol. 33, December 1987, pp. 492 – 535.

Q47. JOHN RANDALL was an unusual scientist who made outstanding contributions in three very different areas of science. First, he made his mark in solid-state physics. Next, for radar he invented (with H. A. H. Boot) the cavity magnetron, which was probably the most decisive contribution of science to the winning of World War II. Lastly, and most significantly, he entered biology and built up a biophysics laboratory that was a world leader in pioneering the new area of molecular biology and contributed to both the discovery of the DNA double helix and of the sliding filament mechanism of muscle contraction. Randall's success derived from his exceptional energy, foresight and sound judgement. Although an original and even somewhat maverick scientist, he had a very shrewd understanding of how established society worked and, as a result, he achieved great success as a scientific entrepreneur, fund-raiser and administrator. But he was never content with such success and his greatest enthusiasm was always for personal engagement in laboratory work. It is sometimes claimed that creativity springs from contradiction; the aspect of contrast in Randall's personality and in his work in some ways supports that idea. (p493)

Q48. possibly her example contributed to John's unusual ability for single-minded, self-contained determined effort. In any case the Lancashire background probably contributed to John's down-to-earth pragmatism, (p 494)

Q49. He appreciated the pragmatic atmosphere at Wembley. 'Competition was rife and there were those (not Campbell) who were not unwilling to take advantage of any minor success.' Certainly, Randall learnt to take the initiative himself and that was a lifelong characteristic. At Wembley, he gave the impression of being self-contained and S. M. Duke told me with some awe and respect that Randall was regarded at the G.E.C. as a 'dark horse'. (p 498)

Q50. But the Royal Society had recently set up Warren Research Fellowships, the rules of which made a passing reference to industry. Randall was awarded such a Fellowship at £700 per annum. Randall wrote that Bragg and Fowler were involved: 'I owe it entirely to them.' The question was then which university would be the best to go to? On the recommendation of Fowler, he decided to go to Birmingham where Oliphant, having left his position with Rutherford was energetically transforming the physics department. On the day the Fellowship award was announced, Tyndall telegraphed Randall offering him a place at Bristol where Mott, whom Randall knew well, was very active on solid-state theory; but Randall did not feel he could change his decision. (pp 499/500)

Q51. In the late 1930s there was widespread interest in microwaves throughout the world and similar work was done in many centres on multisegment magnetrons in which the anode consisted of separate parts arranged peripherally in a ring. Megaw, at G.E.C., Wembley, was making such magnetrons for radar, there was Gutton and Berline in France, Groszkowski and Ryzko in Poland, and workers at the Japan Radio Company, Japan. In view of the fact that there was a great deal of work being carried out by experienced magnetron specialists, it is much to the credit of Randall and Boot that it was they, inexperienced newcomers to the field, who made the revolutionary step forward. They did this stimulated by the very pressing need in wartime Britain for improved radar. (p 508)

Q52. And Randall (Lazarus 1986) wrote in 1984 '...none of these...was satisfactory from our point of view, since they did not embody truly enclosed resonators and consequently showed marked frequency pulling on load'. Apart from the nature of the cavities, Randall and Boot's success depended on their use of a loop in one of the cavities to extract power from the whole system. (Such a scheme was not specified by Samuel or by the Russians.) Another advantage of the Randall and Boot design was that the copper anode formed the vacuum vessel so that there were no losses in a glass envelope that was often used to enclose split-anode magnetrons. The remarkable success of the Birmingham design arose from a happy combination of several design features. (p 508)

Q53. Randall 'fully acknowledged that the atmosphere in which we worked influenced the direction of our thoughts'. (p 508).

Q54. E. D. R. Shearman and D. V. Land have recently written of Randall and Boot 'An element in their thinking may have been the seminal suggestion to Oliphant on a visit to the Admiralty Station at Haslemere that the combination of the split-anode magnetron with resonators after the pattern of the Klystron... might be a way forward to shorter wavelength operation.' (p 509/510)

Q55. Also in a letter about strapping, Sayers wrote, 'Oliphant... asked Randall to see whether copper cavity resonators could replace the little loops of wire with nickel plates attached which formed the resonators in the magnetron valves then available.' Sayers also stated that he gave Randall advice on the design of magnetron resonators. This does not, of course, invalidate Randall's memory of Hertzian loops-an inventor is seldom fully aware of all the factors contributing to an invention. Concerning Oliphant, Boot and Randall wrote 'The authors owe a great deal to him for his encouragement and inspiration throughout a difficult period.' Oliphant confirms his involvement in initiating the cavity magnetron though the form of the resonant cavity was Randall's: 'His elongation of the Hertzian detector was brilliant'. (p 510)

Q56. Official exchange of information with G.E.C. was not until April 1940 but presumably Randall and Megaw were in contact unofficially. Neither Boot nor Randall mentioned Samuel. (p 508)

J.E. Brittain: The Magnetron and the Beginnings of the Microwave Age. Physics Today, vol. 38(7), 1985, pp. 60 -67 (p66)

Q57. Research on multiple-segment anodes in magnetrons culminated in the development of a fruitful theoretical concept known as the rotating-field theory, which was disclosed in 1935. This theory proved a vital key in the later work by Boot and Randall on the cavity magnetron. The new theory was proposed by K. Posthumus, a researcher with Philips Company at Eindhoven in the Netherlands. In 1934, Posthumus published a brief paper in *Nature* in which he reported the discovery of a new type of oscillation in a magnetron operated with a magnetic field intensity well above the cut-off value. His analysis indicated that the frequency was doubled if four anode segments were used instead of the usual two, and he predicted that more than four would generate even higher frequencies. E. C. S. Megaw had been experimenting with multiple-segment anodes at the British General Electric Company at Wembley and wrote to *Nature* with a critique of the Posthumus interpretation. Megaw was well informed on the subject of magnetrons and magnetron theories. He had recently published a paper summarizing both magnetron theory and magnetron research since Hull's first papers. Megaw also had announced his discovery of secondary cathode emission due to electron bombardment in a split anode magnetron, in a paper published in *Nature* in 1933. In his critique of the Posthumus paper, Megaw stated that he believed the alleged new type of oscillations actually were identical to the "dynatron type" that he had discussed in his IEE paper the previous year. He commented that he had tested a "squirrel cage" multiple-segment magnetron with negative results. Postumus responded in an item published in *Nature* that he doubted the validity of the dynatron theory as applied to the magnetron. He reported that he had produced oscillations with an eight-segment anode at a frequency four times that of a two-segment design. He also revealed that he had developed a new theory of "rotating field" oscillations in magnetrons. Posthumus outlined his rotating field theory in a paper published in the *Wireless Engineer* in 1935. Adapting a technique commonly used in the analysis of induction motors by electrical engineers, he had resolved the electric field due to oscillations in a magnetron into two components rotating in opposite directions. He explained that electrons in the magnetrons tended to travel tangentially in synchronism with a component of the rotary field." The dispute ended later in the year with a joint contribution to *Nature* by Posthumus and Megaw. They expressed agreement that the term dynatron as applied to high-frequency magnetrons was inappropriate in view of the accurate predictions afforded by the rotating-field theory. The magnetron research at the Philips Laboratory that spawned the rotating-field theory was related to the development of a microwave communication system. (p 66)

Q58. The patent header reads: John Turton Randall, D.Sc., and Henry Albert Howard Boot, B.Sc., both of the Physics Dept., The University, Edgbaston, Birmingham, 15, and Charles Seymour Wright, C.B., O.B.E., M.C., M.A., Director of Scientific Research, Admiralty, London, S.W.1, all of British nationality, do hereby declare the nature of this invention to be as follows: - (*etc*).

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EDITORIAL

***Peter Butcher*.**

Firstly, I am sure we would all wish to welcome our chairman, Phil Judkins back from his own and his wife Pam's illness. Throughout this spring and summer of their extremely worrying times, Phil has continued to publish *eDEN* and to lead us and we can only say THANK YOU and to wish Pam a full recovery.

As you are aware the Autumn Symposium will be the last event that DEHS will hold, as the burden of arrangements has finally proved too much for your existing Committee. This is, I feel, not unexpected, given the advancing years of our membership, with increasing difficulties and, indeed, incentive to travel long distances. I understand the problems and can offer no reasonable solution.

Time for a change!

As suggested in my last Editorial, we need articles to publish as our ongoing record of Defence Electronics History. We need to do this before our generation of innovators and developers is lost forever. I have recorded my own modest contribution and would urge other members to record theirs. Future Historians will need to access first-hand accounts of developments from a variety of accounts and different viewpoints if they are fully to understand how a development occurred.

As an example of the above statement, I am indebted to Dr Mike Diprose for permission to publish his detailed 'post mortem' analysis of probably one of the most important developments of the century, that of the Multi-Cavity Magnetron. As you read this detailed account, you become aware of the complexity of personalities, aims and influences behind such an invention, not the least of these being political expediency! Even the main protagonists found it hard to agree on some details, when asked to speak about the work at a later date!

Owing to the importance of this article we are taking the unprecedented step of devoting an entire issue to it, in order that readers have all the relevant list of 'Quotations' available as they read through, without having to wait for the next issue of 'Transmission Lines'.

Please help a future 'Mike Diprose' to investigate some project you worked on.

***Peter*.**

DEHS COMMITTEE MEETING

Peter Butcher

1. **Apologies for absence**: None

2. Minutes of January meeting have been published in TL by Peter Butcher.

3. **Finance**: At 1 September, total funds £10,201.34 made up of £6.010.30 current a/c, £4,191.04 savings a/c. Again, a very successful year, with many thanks to Dick!

4. **Membership:** Latest figures from Dick show 170, of whom 28 have not yet renewed as at 1 September; all 28 are being written to or telephoned personally, and their eDENs/TLs are stopped.

5. **Website.**

Liz reported that the new website was now up and running although not yet open to the outside world. Feedback from Committee Members was requested on content, design and header images.

6. Reviews:

a**. eDEN 55 – 62**. All fine and plenty of member input.

b.**TL March/June 2017** All excellent and well received! T.L. March 2017 contained two articles on Barkhausen Kurz oscillators, translated and re-published from Funkgeschichte, the official journal of the GFGF, by kind permission of the Editor, Peter von Bechen. As a reciprocal arrangement, Peter von Bechen had translated and published Peter Butcher's article on the Orling-Armstrong Detector, from the same March issue of T.L.

c. **12 May Cryptos London Conference** All the input received suggested this went very well. Many thanks to Arnold Rosen and Dick Green.

7. Plans 2017/8:

a. **13 October AS 2017: Savage 'Little' Wars**. Simon advised 24 booked to date.

i. **Venue. STEAM**; Tony (to whom our most grateful thanks for his work in liaison with STEAM) advised that during September he will be progressively collecting data for preparation of maps and route-guides and parking advice for AS 17 attendees. This, together with Tony's advice about the manoeuvring of large/heavy exhibits into the hall from the vehicle park, will then be distributed with the October 1st eDEN and/or earlier to all attendees if their email addresses are known. Tony will also try to prepare A4/A3 sized printed notices for the two entrances to the hall. Towards the end of September Tony will firm-up and order the following: Catering; Screens; Projectors; Chairs for Attendees and Tables for the lighter exhibits, books, etc. For technology, Tony will bring his laptop (which takes HDMI but not VGA), as also will Phil (has HDMI/VGA adaptor); a 3rd would be welcome as a reserve. Power-Extension Cables and HDMI cables will also be brought but should not be necessary. Keith will require a table for publications.

ii. Speakers – Liz Bruton/ Graeme Gooday: Brian Austin: Mike Dudgeon; Phil Judkins/Arthur Bauer. All contacted, timetabled and organised by Mike.

iii. Exhibition. This being the last AS, everyone is being asked in eDEN and TL to bring an exhibit. Committee members put forward items from their own collections. Tony noted that it would help if all exhibitors created their own labels, notices, inscriptions, etc for their items and be available to answer questions. The big worry is SECURITY and prevention of damage to items. Phil will incorporate this into the notices in September 15th TL and October 1st eDEN.

iv. AGM: Keith, Simon, Liz and Mike are candidates for re-election. Future AGMs (as there will be no Symposia) to be held on Summer Visits and/or by internet.

b. eDEN 63 [1 October] and onwards. Keith wondered if it might be possible to reproduce these in condensed volumes for more general circulation, and would be happy to produce the final publications. Copyright will be discussed with Phil.

c. TL September 2017 and onwards Peter and Phil have decided to make the September and December TLs single-paper issues (September – Mike on the RCM; Dec, Peter on his career), March 2018 will consist of two papers from Walter Blanchard. We all thank Peter for all the work he has done over the years with producing TL.

d. Leeds Interwar Project. In brief, Kapil Subramanian suffered major health issues and returned home. Prof Gooday was able to secure time from another quality researcher, Dr Michael Kay, but this is limited as Michael has another, full-time, job. This research will now be carried forward between Prof Gooday, Phil, Michael Kay and Liz. Keith, who has provided very comprehensive and helpful briefing papers, commented that it was pleasing to see that the project is now being actively pursued and will give further help; he also saw a need for a further phase which will cover areas not possible to pursue at this time.

e. June/July 2018 Summer Visit: BVWS, Dulwich. Keith will be our contact with Dulwich. The museum is undertaking extensive building work and thus the proposed visit in 2017 was postponed until 2018.

8. Any other business. None.

Peter Butcher. 3 September 2017.

DEHS
Defence Electronics History Society

AGENDA FOR DEHS 2017 ANNUAL GENERAL MEETING

To be held at Steam, Museum of The Great Western Railway, Fire Fly Avenue, Swindon, on Friday 13 October 2017

1. **Membership report.** The current membership will be reported at the meeting.

2. **Financial report.** The Statement of Account for 1 January to 31 December 2016 has been circulated in both *eDEN* and *Transmission Lines* and is reproduced below.

3. **Election of Committee Members retiring by rotation.** To consider election of the following Committee Members retiring by rotation and offering themselves for re-election:
   (a) Keith Thrower.
   (b) Simon Blumlein.
   (c) Dr Mike Diprose.

4. **Any other business**

Dr Phil Judkins, Chairman, DEHS, 1 September 2017.

DEFENCE ELECTRONICS HISTORY SOCIETY
Statement of Accounts 1 Jan - 31 Dec 2015

INCOME	2015 £	(2014) £	EXPENDITURE	2015 £	(2014) £
Members' subscriptions	2956.05	(2446.39)	Transmission Lines: production & distribution [Note 1]	3834.50	(2297.43)
Members' donations	365.42	(532.82)	Admin: post & website	206.79	(166.77)
Bank Interest	1.67	(26.61)	Awards & sponsorship [Note 2]	2150.00	(600.94)
Publications, reprints & Proceedings	1442.77	(1180.42)	Publications, reprints & Proceedings	602.15	(1644.42)
Events: bookings	1416.00	(991.00)	Events: catering, exhibition mat'ls & speaker expenses	922.54	(894.92)
Misc: sponsorship & eqt sales [Note 3]	883.44	(1000.00)			
TOTAL INCOME	7065.35	(6177.00)	**TOTAL EXPENDITURE**	7715.98	5604.48

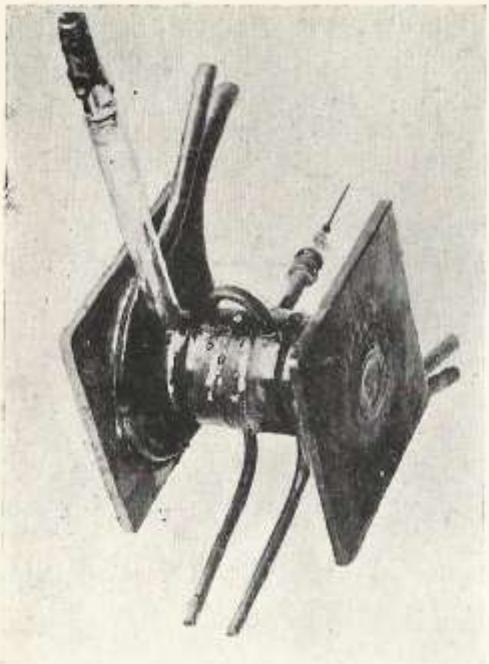
	Current	Saver	Total	2015	(2014)		2015	
Opening Bank Balances at 01 Jan 2015 (£)	2874.46	4187.33	7061.79			Total Income (£)	7065.35	
Closing Bank Balances at 31 Dec 2015 (£)	2222.16	4189.00	6411.16	-650.63	(572.76)	Total Expenditure (£)	7715.98	-650.63

Notes:
1. This includes Dec 2014 TL cost (£460.80), thus the TL cost for the four 2015 editions is £3373.70 (vs £2758.23 for the four 2014 editions).
2. Awards & sponsorship comprise £1500 to Leeds University for Interwar Project; £200 MESE Award; £200 MESE project sponsorship and £250 Restoration Award to the Electronics Restoration Trust (ERT).
3. We record our gratitude to:
   - the Communications & Electronics Museum Trust (CEMT) for their £500 sponsorship of 2015 and 2016 Burns Lectures;
   - Dr Mike Diprose for donating £383.44 from sales of electrical equipment.

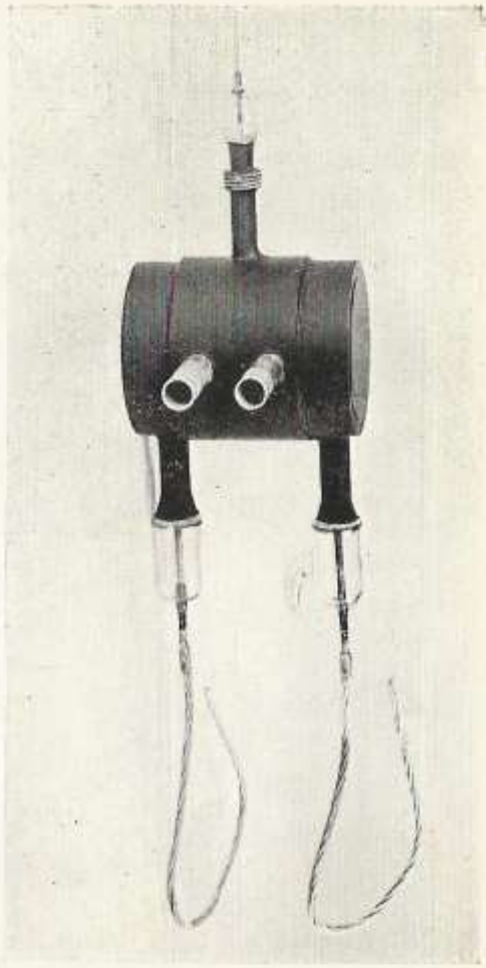
Dick Green
DEHS Treasurer
18 Jan 2016

The pictures below are from ‘*Science at War*’ by J.G. Crowther and R Whiddington, C.B.E., F.R.S., published in 1947 by HMSO under Crown Copyright which is believed to have expired.

A



B



C

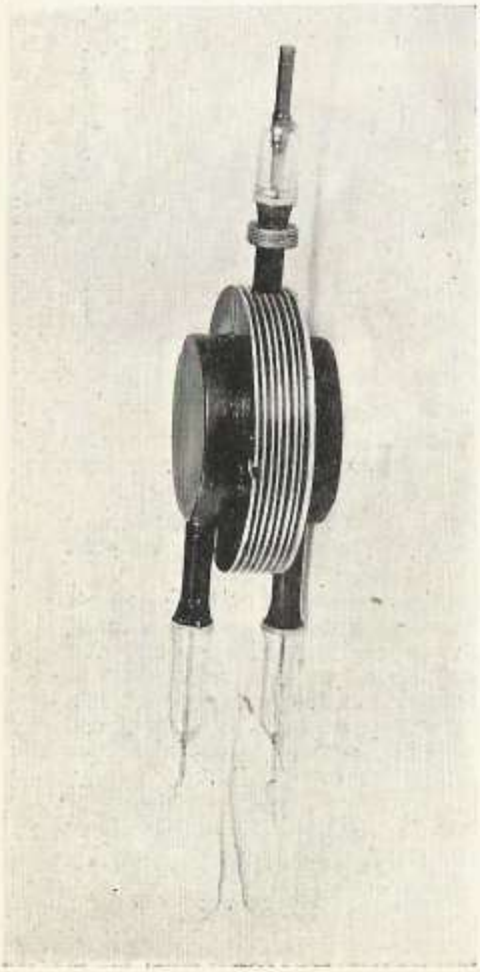


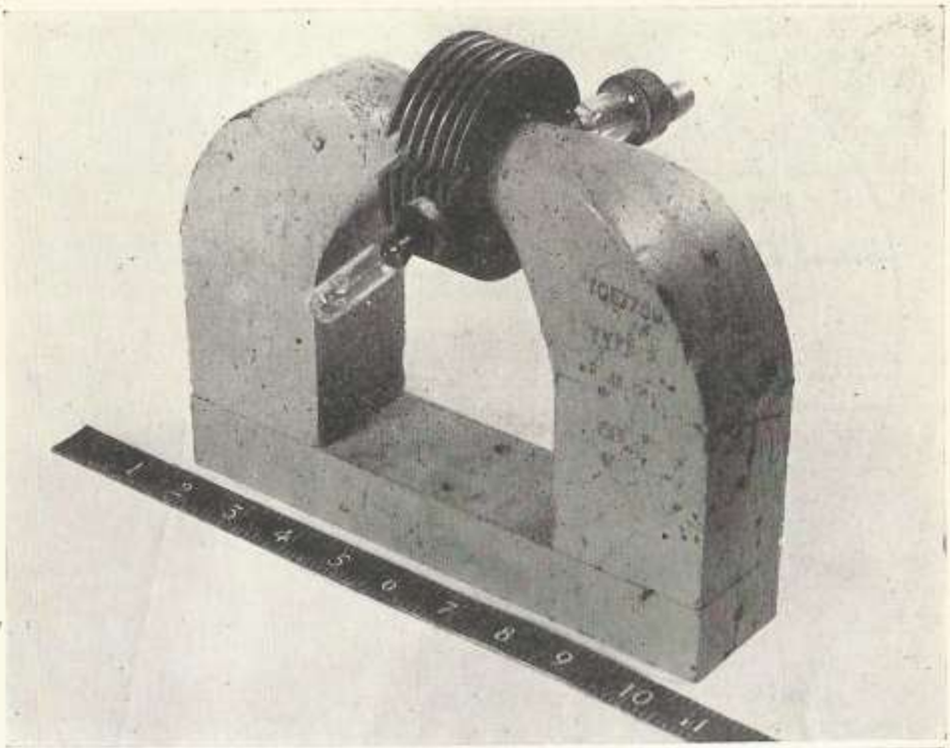
PLATE IX

The Evolution of the Cavity Magnetron

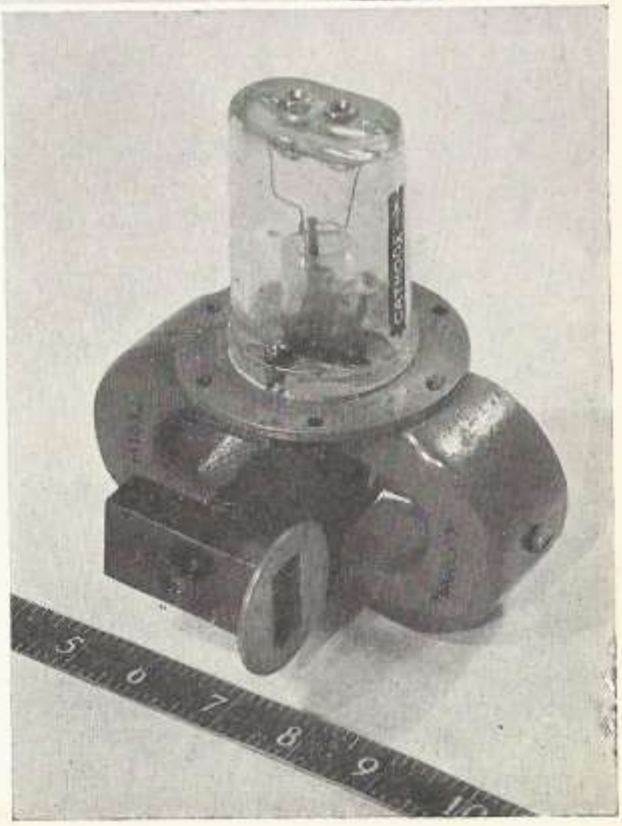
A. After much effort at Birmingham University Physics Laboratory, Dr. Randall and Mr. Boot had made the first successful cavity magnetron by April, 1940, and sent it to the General Electric Company, Wembley, who very quickly adapted it to—

B. a neat industrial design. This was June, 1940. Note in both these cases the neat glass and metal seals and the water-cooling arrangements.

C. By the end of June the General Electric Company had produced an air-cooled design which used exactly the same cavity design but a greatly improved cathode. The pulse output was ten kilowatts as against the original half-kilowatt and the magnet, thanks to the co-operation of the magnet firms Messrs. Darwins, Ltd., and Messrs. Jessop & Sons, Ltd., now weighed only 6 lb.



D



E

D. The magnetron complete with magnet. These all operate at ten centimetres. Shorter waves than ten centimetres were on the way, and by 1942 a magnetron delivery power at three centimetres was available, mainly a joint effort by the Birmingham team and the British Thomson Houston Company at Rugby.

E. A late model ; it is a "package" with magnetron and magnet complete with rectangular wave guide out of which the power pours. A continuation pipe of any length required can be bolted on to the flange shown at the bottom of the plate.