EXPERIMENTAL WIRELESS
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Editorials.

D.C. Smoothing Devices for H.T. Supply.

IT is perhaps not generally realised that so-called direct or continuous current supply systems vary very much in their characteristics. Not only are some much steadier than others, but the irregularities in some systems are of a very different character to those in other systems. It is necessary to remember this, however, when comparing the efficiency of smoothing devices for eliminating the undesirable elements from an alleged D.C. supply before connecting it to the H.T. terminals of a receiving set. We were reminded of this when reading recently that an amateur found choking coils unnecessary, a condenser connected across the tappings from a non-inductive resistance being sufficient to give the desired effect. This might be so on one supply system but not on another. The trouble is not always due to the teeth and commutator segments of the dynamos.

It was noticed recently that the pointers of some of the switchboard instruments in an electrical engineering laboratory vibrated so much that accurate readings were impossible. On investigating the so-called D.C. voltage by means of a Duddell Oscillograph, it was found that there was a pronounced alternating component with a frequency of 150 cycles per second, due to the fact that the supply was from rotary converters in a substation supplied with alternating current of 25 cycles per second. The rotary converters were 6-phase machines, and the tappings from the armature winding to the slip-rings would pass the commutator bushes at the rate of 150 per second.

Other irregularities were present, but the cause of the instrument oscillation was this 150-frequency component.

It would add to the usefulness and interest of any information about tests of smoothing devices for D.C. supply systems, if writers always discovered and stated the type of station from which their supply is drawn. In some cases it might be advisable to insert a definite rejector circuit designed to remove any alternating component known to be present.

Wireless Section of the I.E.E.

AS many of our readers will know, a proposal has recently been made to inaugurate an Institute of Wireless Engineers. The object of the proposers was apparently to cater mainly for those professionally engaged in the technical side of the subject, but who do not possess the qualifications necessary for membership of the Institution of Electrical Engineers. There are already two organisations dealing with the technical side of radio work, viz., the Wireless Section of the Institution, and the Radio Society of Great Britain, the former a professional and the latter largely, but not exclusively, an amateur body.

Although doubt was felt by many as to the wisdom of starting a third organisation, the proposers drew attention to certain alleged disabilities under which radio engineers were placed as compared with other electrical engineers in applying for membership of the Institution; they also pointed out that the Wireless Section meets only in London. In the minds of most people the strongest argument in favour of the proposal is the success of the American Institute of Radio Engineers, an organisation entirely separate from the American Institute of Electrical Engineers.

The matter has been considered very carefully by the Committee of the Wireless Section of the Institution, which has made certain recommendations and these having now been approved by the Council are issued for publication (see page 115).

Calibration Department.

As announced in last month's issue, our Calibration Department is now re-opened as from 1st February, 1926. May we draw the attention of readers to the note which appeared on page 51 of the January issue regarding the care which should be exercised in the design of apparatus intended for calibration. General details regarding the calibration service, and coupon for the current month, will be found in the advertisement section of this issue.

Our Predecessor.

READERS of E.W. & W.E. will be interested to learn of the appointment of Mr. P. K. Turner to take charge of the Research Department of Messrs. Burndept Wireless, Ltd., which is a sequel to his retirement from the editorship of E.W. & W.E., in search of greater scope for practical development work. We hope that Mr. Turner's new activities will not prevent him from acting as a frequent contributor to this journal as we have reason to know that his contributions in the past have been keenly appreciated.

Our readers will, we feel sure, join with us in wishing Mr. Turner all success in his new sphere.

An Apology.

OUR attention has been drawn to the fact that Figs. 15 to 18 of Captain Miles' Lecture before the Radio Society of Great Briain, which were reproduced on page 29 of our last issue, were taken from a book, *Les Ondes Radio Electriques*, by M. N. Adam, the Technical Director of our esteemed contemporary *Radio-Eléctricité*. We regret that these figures were reproduced by the author of the paper without due acknowledgement.

Valve Nomenclature—A New Term Suggested.

By Professor G. W. O. Howe, D.Sc.

[R030

WHEN a steady continuous current flows through a coil of wire, the relation between the P.D. across its terminals and the current is a fixed ratio which we call the resistance of the coil. When an alternating current flows through the coil, the terminal P.D. may be regarded as made up of two components, one in phase with the current and the other 90 degrees out of phase with it. The former component should be equal to $I \times R$, and the power supplied equal to I^2R, using root-mean-square values of current and voltage; on account of non-uniform current distribution in the wire, and other causes, the value of R obtained from such considerations is generally larger than that determined with continuous current, and is referred to as the effective resistance or A.C. resistance. This must not be confused with the impedance; the effective resistance is that quantity which must be multiplied by I^2 to obtain the power supplied; the impedance is that quantity which must be multiplied by the current to obtain the terminal voltage. In a choking coil the impedance is usually many times as great as the effective resistance. The impedance is equal to the hypotenuse of a right angled triangle, one side of which is equal to the effective resistance, and the other side to the reactance. Now the reactance of any piece of apparatus depends on the frequency of the alternating current, the reactance of an inductive coil is proportional to the frequency, that of a condenser is inversely proportional to the frequency. Hence the impedance, which involves the reactance, depends on the frequency. By making a coil non-inductive or by making the frequency very low, it is possible to make the reactance so small that the impedance is practically independent of the frequency, but these are extreme limiting conditions and do not alter the fact that impedance is essentially dependent upon frequency.

In Fig. 1 the curve $ABCD$ represents a part of a characteristic curve of a piece of apparatus; each point on the curve gives the corresponding values of P.D. and current. Unless something is definitely stated to the contrary, it is always assumed that such curves refer to steady static conditions, that is to say, I_1 is the current which flows through the apparatus when the P.D. is maintained steadily at V_1. In some cases, such as with an electric arc, it may be necessary to maintain the conditions steady for a considerable time before taking the corresponding readings. When conditions are changing rapidly, it does not follow that the P.D. will be V_1 whenever the current has the value I_1.

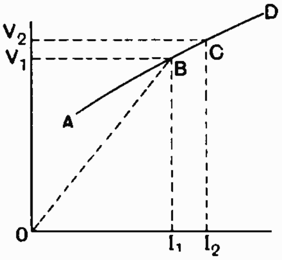

Fig. 1.

Now for many purposes we are not so much interested in the resistance V_1/I_1 at B or V_2/I_2 at C, as in the ratio of the increase of voltage to the increase of current, *i.e.*, in the slope of the characteristic curve at a given point or over a given range. Mathematically this is written dV/dI; it is the ratio of a small difference of voltage to the corresponding small difference of current. Now the question which arises, and which is the reason for writing this note, is: What shall we call this ratio? We cannot call it the resistance, since this term already has another definite meaning, and we cannot call it the impedance, since this term not only has another definite meaning but is associated with dependence upon frequency of alternating current. The ratio dV/dI has nothing to do with frequency, nor has it any essential connection with alternating current; it is merely the slope of a static curve which may entirely cease to have any reference to the conditions existing when rapid changes or oscillations are taking place. For these reasons it would appear that the most correct term to apply to the ratio is "differential resistance"; it is the quotient obtained by dividing a small difference between two voltages by the corresponding small difference between two currents, and the name suggested is almost self-explanatory.

If an oscillatory current is superposed upon a steady current, and it can be assumed that the static characteristic is applicable at the frequency involved—this assumes freedom from hysteresis or inertia of any kind, mechanical, electrical or thermal—then the differential resistance can be used to calculate the additional power due to the oscillatory current.

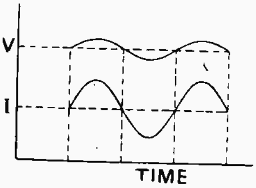

Fig. 2.

Let the steady voltage and current be V and I and let the amplitudes of the superposed alternating voltage and current be $\hat{v}$ and $\hat{\imath}$ so that at any moment

$$v = V + \hat{v} \sin \omega t.$$
and $$i = I + \hat{\imath} \sin \omega t.$$

We assume that $\hat{v}$ and $\hat{\imath}$ are so small that the characteristic can be assumed to be linear over the range of the oscillation; the ratio $\hat{v}/\hat{\imath} = r_d$ is what we have called the differential resistance. The power supplied is the average value of the product $v \times i$ taken over a complete cycle.

Now, $v \times i = VI + (V\hat{\imath} + I\hat{v}) \sin \omega t + \hat{v}\hat{\imath} \sin^2 \omega t$; the first term is constant, the average value of $\sin \omega t$ is zero and the average value of $\sin^2 \omega t$ is 0.5. Hence the power is $VI + \hat{\imath}\hat{v}/2$ or $VI + \hat{\imath}^2/2\ r_d$ or, since $\hat{\imath}/\sqrt{2}$ is the root-mean-square value of the alternating current, $I^2 \times R + i^2 \times r_d$, where i is the R.M.S. value.[*]

Hence the differential resistance is the effective resistance to superposed oscillations at the given point on the characteristic, on the assumption that the static characteristic is applicable at the alternating frequency. This assumption is usually permissible in thermionic valve problems, but is rarely ever approximately correct in the arc. Confining ourselves to the valve and assuming the applicability of the static characteristic to high-frequency oscillations, it must be remembered that the current given by the characteristic is the purely thermionic current. The total alternating current entering an electrode at a high frequency will consist of two components, one that which we have just considered in phase with and proportional to the alternating voltage, and inversely proportional to the differential resistance, the other proportional to the capacity between the electrodes and 90 degrees ahead of the voltage. The first is independent of the frequency, the second is proportional to the frequency. The differential resistance and the capacity reactance are in parallel and may be combined to give the impedance, but this will depend on the frequency. There is no justification whatever for referring to the differential resistance as the impedance of the valve or other apparatus; such a misuse of the term can only lead to confusion.

In the same way, a conductance which is not the value of I/V but of dI/dV, should be referred to as a differential conductance. It is the reciprocal of a differential resistance. There may be many cases in which the word differential may be safely omitted without any danger of misunderstanding; the mutual conductance of a valve, for example, would always be taken as the ratio dI_a/dV_g, unless something very definite were stated to the contrary.

Our main object in writing this article, however, is to protest against the application of the term "valve impedance" to a ratio obtained under static conditions, having no connection with frequency and constituting merely one component of the real impedance.

An Experimenter's Wireless Laboratory.

By Leonard A. Sayce, M.Sc., Ph.D., A.I.C., and
James Taylor, M.Sc., Ph.D., A.Inst.P.

[R201

Introduction.

SOONER or later in the experience of most experimenters there comes a time when real progress is rendered very difficult by the lack of facilities for making electrical measurements. Much may be done, of course, by the use of "rule of thumb" methods provided that no radical departure is made from standard practice, but it is in breaking new ground and attempting new departures that one is apt to sigh for the well-equipped laboratory with its array of costly instruments. There is no reason at all, however, why every amateur should not possess his own laboratory and be able to make all the more usual electrical measurements with reasonable accuracy. In the present series of articles, therefore, we shall show how this can be done with the least possible expenditure and in order that the methods of measurement may be fully understood by the less advanced worker, a sufficient explanation of the fundamental principles underlying them will be given.

To bring the above object within the reach of everyone the purchase of ready-made apparatus must be reduced to an absolute minimum. Now, most electrical instruments depend ultimately upon the measurement of current and there is not much that cannot be done if we can do this with accuracy. And so we shall see that a really efficient electrical laboratory can be equipped by the purchase of only one expensive instrument—a calibrated microammeter. Other accessory apparatus will, of course, be required, but these can, in most cases, be home-made, and where calibration is required, the Calibration Department of this journal is available at a very small cost. It may give the reader confidence to know that in preparing the instruments to be described here for photographing and testing, the authors have carefully followed their own instructions and have resorted to more refined standard instruments only as a final test of the home-made arrangements.

District Current Measurements.

Theory.

If a potential difference V is applied to the ends, A B, of a resistance (see Fig. 1), it is found that the current through the resistance can be expressed by the relation

$$I = \frac{V}{R} \qquad \cdots \qquad \cdots \qquad (1)$$

where I is the current, in amperes,

V is the voltage across AB in volts and
R is the resistance in ohms.

This is the *most fundamental* equation in direct current work and is known as Ohm's Law.

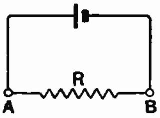

Fig. 1.

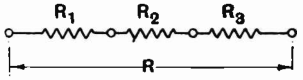

Fig. 2.

We see that the equation contains three unknown quantities ; it is therefore sufficient to know any two of them in order to determine the third. That is to say, current may be measured in terms of voltage and resistance, voltage in terms of current and resistance and resistance in terms of current and voltage. We may express this as follows :—

$$I = \frac{V}{R} \qquad \cdots \qquad \cdots \qquad (1a)$$

Current in any circuit or part of any circuit =

$$\frac{\text{Total voltage across the resistance}}{\text{Resistance}},$$

$$V = IR \qquad \cdots \qquad \cdots \qquad (1b)$$

Voltage drop along the resistance = Current × Resistance,

$$R = \frac{V}{I} \qquad \cdots \qquad \cdots \qquad (1c)$$

Resistance of circuit or part of circuit =

$$\frac{\text{Total voltage across the part of the circuit considered}}{\text{Current passing through the circuit}}.$$

Resistances.

Resistances in Series.

If two, or more, resistances are connected in series (Fig. 2), the total resistance, R, is equal to the sum of the separate resistances. This may be expressed as an equation :—

$$R = R_1 + R_2 + R_3 + \quad .. \quad (2)$$

Resistances in Parallel.

Let us consider two resistances, R_1 and R_2 ohms (Fig. 3), in parallel and connected to a battery so that a current, I, flows in the main circuit. This current divides at A into two portions: I_1 in the resistance R_1 and I_2 in the resistance R_2, so that $I=I_1+I_2$.

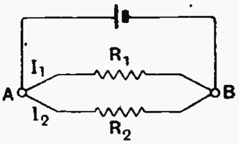

Fig. 3.

Let the potential drop from A to B be V volts, then the drop both in R_1 and in R_2 must be V volts. If R is the combined resistance of R_1 and R_2 in parallel we have :—

$$I = \frac{V}{R}$$

$$I_1 = \frac{V}{R_1}$$

$$I_2 = \frac{V}{R_2}$$

but

$$I = I_1 + I_2$$

$$\therefore \quad \frac{V}{R} = \frac{V}{R_1} + \frac{V}{R_2},$$

and dividing through by V, we have

$$\frac{1}{R} = \frac{1}{R_1} + \frac{1}{R_2} \quad .. \quad .. \quad (3)$$

or, putting the equation into words, the reciprocal of the two resistances in parallel is equal to the sum of the reciprocals of the two resistances. If, for example, both R_1 and R_2 were 1 ohm, then $1/R = 2$ and $R=\frac{1}{2}$. Thus we see that the combined resistance must always be less than either of the resistances.

Finally, by a slight extension of the above equations, we find that :—

$$I_1 = \frac{R_2}{R_1 + R_2} \times I \quad .. \quad (4)$$

$$I_2 = \frac{R_1}{R_1 + R_2} \times I \quad .. \quad (5)$$

$$\therefore \quad \frac{I_1}{I_2} = \frac{R_2}{R_1} \quad .. \quad .. \quad (6)$$

or, the currents through the resistances are in the inverse proportion to their resistances.

Apparatus.

For the present purpose of measuring the voltage, current and resistance of D.C. circuits, the following apparatus is required :—

1. A microammeter having a range of 0-120μA and of known resistance. As this is to be the fundamental instrument in all the above measurements and in many others to be described later, it is essential that it should be of reliable performance, readily portable, "dead-beat," and of reasonably short time-period. So far as we know, the only instrument that fulfils these conditions adequately is the "Unipivot" galvanometer of the Cambridge Scientific Instrument Company (45, Grosvenor Gardens, S.W.1). It is made in a variety of patterns, but the "Type I," shown in Fig. 4, calibrated 0-120μA and approximately of 50 ohms resistance, is the most suitable and costs £6 10s. This is the only really expensive item in our laboratory equipment, and although it is almost indispensable, a Weston Relay, converted as previously described in these columns (Vol. I., page 721, Sept., 1924) may be used with fair success. If this course is adopted the instrument should be sent for calibration to the Calibration Department of this journal.

2. Resistances of 1 000 ohms, 100 ohms, 10 ohms and 1 ohm respectively. These can be purchased, accurate to one part in a thousand, for about seven shillings each, and a very convenient type, shown in Fig. 4, is made by Messrs. F. E. Becker & Co. (17, Hatton Wall, E.C.1).

It is necessary, further, to have a resistance of 10 000 ohms and this may conveniently be made variable in ten equal steps in the

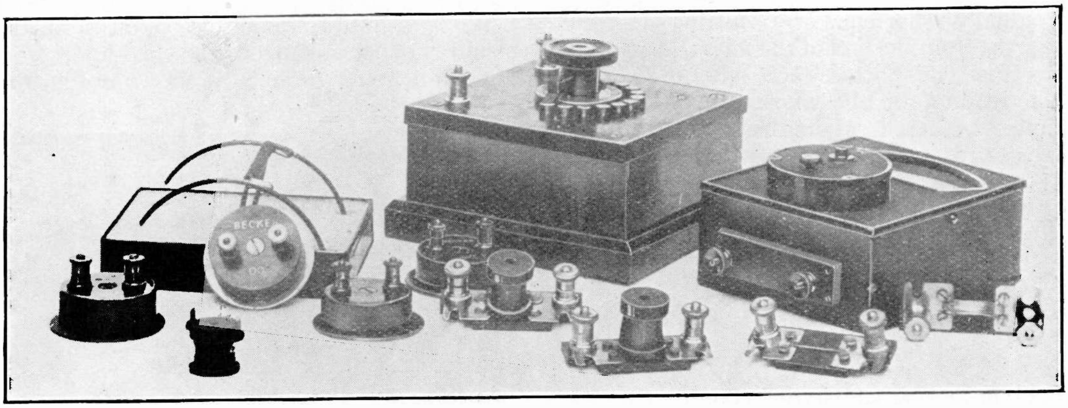

Fig. 4. *"Unipivot" galvanometer, resistance box, shunts and change-over switch.*

manner shown in Fig. 4. A laminated switch-arm is mounted at the centre of an ebonite panel measuring 5 ins. by 5 ins. by $\frac{3}{8}$ in. Beneath the panel and grouped in a circle $3\frac{1}{2}$ ins. in diameter are ten ebonite bobbins of the dimensions shown in Fig. 5, each bearing, at its wider end, two 6 B.A. by $\frac{1}{8}$ in. screws holding little soldering tags as terminals. [Of course, it is not essential to use ebonite for these bobbins or to make them one's self. Some very suitable bobbins, made of varnished hard wood and fitted with connecting wires, instead of soldering-tags, can be obtained from Mr. Ernest Turner, Chiltern Works, High Wycombe, Bucks.] Each bobbin is wound non-inductively with No. 40 S.W.G. double silk covered "Eureka" (or "Constantan") wire to a resistance of 1 000 ohms. In order to do this, fasten one end of the wire, reel out 40 feet of it, and double it back upon itself for a further 40 feet, solder the ends of the wire to the two soldering tags respectively, and wind the doubled wire upon the bobbin, finishing at the looped end.

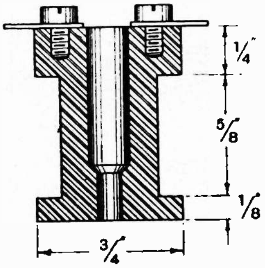

Fig. 5.

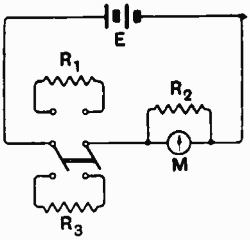

Fig. 6.

Having wound the ten bobbins in this way, soak them all in molten paraffin wax and proceed to adjust each of them accurately to 1 000 ohms. For this purpose, the circuit shown in Fig. 6 is used. E is a 4-volt accumulator, M the microammeter, R_1 and R_2 the standard resistances of 1 000 ohms and 1 ohm respectively (do *not* use them in the reverse order), R_3 the bobbin being tested and a double-throw double-pole change-over switch puts in circuit R_1 or R_3. This switch will often be required and Fig. 7 explains its construction. The base is a quarter-plate negative box filled with paraffin wax in which there are six holes filled with mercury. The two sides of the "rocker" are made of No. 12 S.W.G. copper wire joined together, but insulated one from the other by a piece of ebonite, or glass, tube fastened on with sealing-wax.

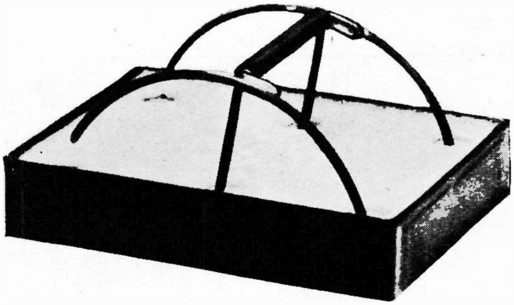

Fig. 7. *Change-over switch.*

Each resistance coil is put into the position R_3—the soldering-tags being conveniently gripped by "tie-clip" terminals—and is gradually shortened by cutting half-inch lengths from the end of the loop and soldering up the ends after each alteration, until the reading of the microammeter remains quite unaltered, when the switch is rocked backwards and forwards. Finally, the bobbins are fixed to the ebonite panel by 6 B.A. bolts, screwed into " blind " holes in the ebonite, and are wired up according to the diagram of Fig. 8. A view of the back of the completed instrument is shown in Fig. 9.

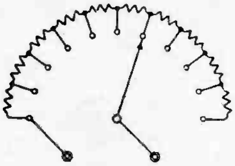

Fig. 8.

3. A set of four accurately adjusted resistances, usually called " shunts," which when connected across the terminals of the microammeter will reduce its sensitivity by a definite amount and cause its scale to read :—

1.—0 to 1.2 milliamperes.
2.—0 to 12 milliamperes.
3.—0 to 120 milliamperes.
4.—0 to 1.2 amperes.

Each shunt is mounted upon a little ebonite panel arranged to clip upon the microammeter terminals, as shown in Fig. 10,

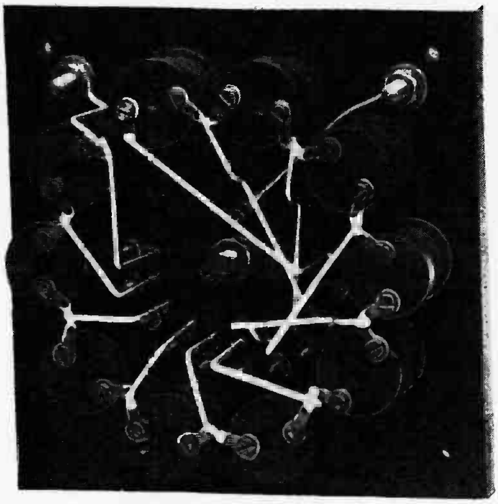

Fig. 9. *Wiring and arrangement of the resistance box.*

and is provided with two terminals for connecting with the required circuit. Shunts Nos. 1 and 2 are wound upon ebonite bobbins,

Fig. 10. *Galvanometer and shunts.*

like those described above, whilst Nos. 3 and 4 are short lengths of rigid wire and require no special support.

Of course, the values of the shunts could be calculated by using equation 6 (above), but they certainly would not be equal to any of the resistances that we have in hand at present. By the following method, however, they can be made with very considerable accuracy.

A resistance coil, approximately equal to the resistance of the microammeter, is first required. This is made of No. 40 S.W.G. "Eureka" wire, wound as before, upon an ebonite bobbin. About four feet of the wire is required. The microammeter is connected in series with a "spent" dry cell and its reading adjusted near to its maximum by means of the 10 000 ohm resistance in series with it. The resistance coil is then placed in parallel with the meter and is altered until the reading of the latter is exactly one-half of its former value. The resistance of the coil is then equal to that of the microammeter.

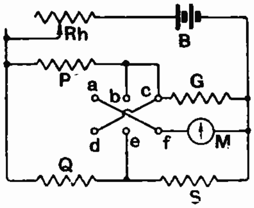

Fig. 11.

Each of the shunts is adjusted by employing the circuit shown in Fig. 11. *P* and *Q* are standard resistances; *G* is the resistance equal in value to that of the microammeter, *M*; *S* is the shunt that is being adjusted whilst R_h is a rheostat to limit the current supplied by the battery, *B*; *abcdef* is the mercury-cup switch already described.

No. 1. Shunt (0 to 1.2mA).

P=1 000 ohms.

Q=100 ohms.

B=2-volt accumulator.

R_h=the variable 10 000 ohm resistance set at 2 000 ohms.

S=six inches of No. 40 D.S.C. "Eureka" wire.

It will be found that the microammeter shows a reading lower when the switch is in the left-hand position (*a* to *b*, *d* to *e*) than when it is in the right-hand position (*b* to *c*, *e* to *f*). Gradually reduce the length of *S*, rocking the switch backwards and forwards at each alteration, until the reading is the same in both positions. The final adjustments should be made with the shunt wire soldered to its final fittings. (*N.B.*—The solder must be allowed to become quite cold before testing, otherwise the Seebeck effect introduces serious errors.) The resistance of the shunt is now precisely one-ninth of that of the microammeter, so that when it is slipped in place across the terminals of the latter and a current is made to pass through the shunted instrument, one-tenth of the total current traverses the meter whilst the shunt carries the remainder. Thus the range of the instrument is now 0-1.2mA.

No. 2 Shunt (range 0-12mA).

P=1 000 ohms.

Q=10 ohms.

B=6-volt accumulator.

R_h=the variable 10 000 ohm resistance set at 1 000 ohms.

S=a cable made of ten five-inch strands of No. 40 S.W.G. Eureka wire. This plan is used to save getting an odd length of rather coarser wire and is quite satisfactory.

The procedure is exactly the same as in making No. 1 Shunt.

No. 3 Shunt (range 0-120mA).

P=1 000 ohms.

Q=1 ohm.

B=6-volt accumulator.

R_h=100 ohms.

S=three-quarters of an inch of No. 22 S.W.G. bare Eureka wire, as used for filament rheostats.

The resistance of the shunt is varied, not by altering its length but by scraping it with the blade of a penknife.

No. 4 Shunt (range 0-1.2A).

P=10 000 ohms.

Q=1 ohm.

B=6-volt accumulator.

R_h=10 ohms.

S=7½ inches of No. 22 S.W.G. bare Eureka wire "zig-zagged" ten times—so as to be, in effect, a strip ¾-inch long—and soldered into position.

In this case it is essential that the leads of the shunt and the microammeter should be as short and thick as possible. The resistance of this shunt should be adjusted, as before, by scraping it.

When the shunt has been adjusted in the manner already described, it is still far from correct, for, however short and thick the leads may be, their resistance and the contact-resistance of their connections are comparable with that of the shunt itself; *i.e.*, in the order of $\frac{1}{200}$ ohm. Nevertheless, it is evident that the *combined resistance* of the shunt, contacts and connections is of the required value. We require therefore a means of making the shunt equal to this combined resistance. To effect this, remove all the apparatus except that shown in Fig. 12*a* and bridge the mercury cups *e* and *f* by a very stout copper wire. The micro-ammeter, shunt and connections have not been disturbed so that, as they stand, the instrument will still read the required range: 0-1.2A. Connect the 10-ohm resistance and a six-volt accumulator to *e*, as shown in Fig. 12*b*, and notice the reading of the meter, d_1. Then connect the shunt to the terminals of the meter by means of its hooks and again connect it in series with the 10-ohm resistance and the 6-volt accumulator, Fig. 12*c*. The reading of the meter, d_2, is now lower than d_1, because the resistance of the leads and their contacts is now absent. Finally, scrape, or file, the shunt wires until $d_2 = d_1$ and the shunt is completed.

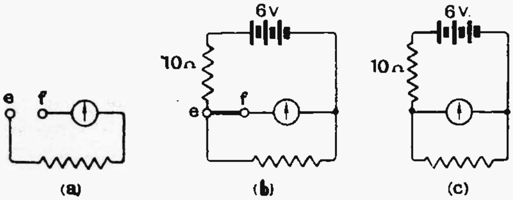

Fig. 12.

Measurement of Current.

It will be seen that we are now provided virtually with five ammeters and are in a position to measure direct currents as widely different as those passing through a valve filament and through the telephones of a crystal set respectively.

Readers may have every confidence in the accuracy of the shunted instrument if the above instructions are followed carefully. As evidence of this, the shunts shown in Fig. 10 were made exactly as described and, in each case, the error of the shunted instrument was well under 1 per cent., the mean error of all the shunts being 0.43 per cent.

Measurement of Voltage.

When an ammeter is measuring the current through a circuit it is desirable that the resistance of the instrument should be as low as possible, for otherwise there is an unnecessary waste of power. The desirable features of a voltmeter are, however, exactly the reverse. A voltmeter is used to indicate the difference in electrical pressure, or "potential," between two points in a circuit and if the voltmeter is of low resistance it may cause a sufficient alteration in the circuit to lower appreciably the actual voltage which it is required to measure. The ideal voltmeter is one which has a very high resistance and which consequently requires a very small current to operate it.

It follows immediately from Ohm's Law that the current through a resistance is proportional to the voltage across the ends of the resistance (equation 1*a*). Thus we have merely to connect our microammeter, either shunted or alone, in series with a high resistance, *R*, Fig. 13, in order to use it as an efficient voltmeter whose range can be adapted to suit any requirements. For instance: If a resistance of 10 000 ohms is placed in series with the meter and the latter is shunted for 0-1.2mA then the combination becomes a voltmeter reading 0—12 volts. Further examples are given in the following table :—

	Series resistance (ohms).	Range of meter. (mA)	Range of combination (volts).
1	1 000	0-120	0-0.12
2	10 000	0-120	0-1.2
3	2 000	0-1.2	0-2.4
4	5 000	0-1.2	0-6
5	10 000	0-1.2	0-12
6	10 000	0-12	0-120

In all cases the voltage as stated should be multiplied by the factor $\frac{R+G}{R}$ where

R = Series resistance and

G = Combined resistance of meter and shunt, if any.

In the first case given in the table this correction is quite considerable, in the second case it is much less so, whilst in all the others it is negligible.

The variable 10 000 ohm resistance is very suitable for use as the series resistance and by its means the range of the instrument can readily be altered to meet various requirements. If, however, an entirely self-contained unit is preferred, a resistance coil and a shunt, if required, can be mounted, like the ampere shunts of Fig. 10, upon a little ebonite panel arranged to clip upon the micro-ammeter terminals. The resistance coil can be made by reference to the 10 000 ohm resistance, using the circuit shown in Fig. 6 for the purpose, and, since compactness is now required, it may be wound with No. 47 S.W.G. D.S.C. Eureka wire, the resistance of which is approximately 235 ohms per yard.

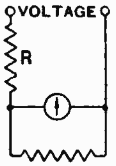

Fig. 13.

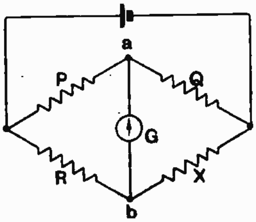

Fig. 14.

Measurement of Resistance.

Now that we are in a position to measure both voltage and current we can always calculate the third term of Ohm's Law and so obtain the resistance of any circuit (see equation 1*c*). It is usually inconvenient to measure a given resistance by the direct application of Ohm's Law unless it is so high that the resistance of everything else in the circuit can be ignored. Most methods of measurement depend rather upon the *comparison* of resistances than their direct measurement; in fact, it may be taken as a rule that an electrical quantity can be measured more accurately by comparison with standards than in any other way. In the present case we shall describe only one method for the comparison of resistances, one that is particularly useful for resistances of low value. A brief reference must be made, however, to what, in a laboratory with the necessary equipment, is certainly the most accurate and general method of all, the Wheatstone bridge.

If the circuit of Fig. 14 be assembled, comprising four resistances, P, Q, R and X, together with a cell, and a sensitive galvanometer, G, it will almost always be found that the galvanometer indicates the presence of a current through it. It is possible, however, to vary one, or more, of the resistances until the galvanometer gives a "null" reading (*i.e.*, shows no current). When this is so, then "*a*" and "*b*" must be at the same potential and it can be shown that under these circumstances

$$\frac{P}{Q}=\frac{R}{X} \text{ or alternatively } \frac{P}{R}=\frac{Q}{X}$$

Thus, if we know the values of any three of the resistances, we can immediately calculate the value of the fourth.

Further—and herein lies the special virtue of the method—if, for instance, $P:Q::1\,000:1$ then $R:X::1\,000:1$. That is to say, if X were the unknown resistance, it would be measured in terms of a resistance R a thousand times as great and the accuracy of the method would be proportionately increased. In the "Post Office Box"—the most convenient form of the bridge—P and Q are always made in some decimal proportion so that when the bridge is balanced—when no current is flowing through the galvanometer, G—then the value of X is obtained simply by multiplying or dividing that of R by some multiple of ten.

The above principle can be applied in a great number of ways, not only to the measurement of resistance but to the measurement of capacity and inductance. Its practical application to the measurement of resistance will not be described here, however, because we cannot obtain the full advantages of the Wheatstone bridge unless the resistance coils are more accurate than those we are in a position to make. Further, to profit by the method, we should require a galvanometer considerably more sensitive than our micro-ammeter and, although such an instrument could be made without much difficulty, it would take longer to use and be much less convenient than the following methods.

1. *Low Resistance.*

This method is suitable for the measurement of resistances up to about 50 ohms, and depends upon the fact stated in equation 1*b*, that the voltage applied between the ends of a conductor is equal to the product of its resistance and the current. Thus if two resistance coils are joined in series and the same current is passed through both of them, the voltage across each coil is proportional to its resistance.

The circuit employed is shown in Fig. 15, where B is a two-volt accumulator cell, X the unknown resistance and R one of the standard resistances, either 10 ohms or 1 ohm, whichever is nearer in value to X. R_h is a rheostat but it is required only if X is under 5 ohms.

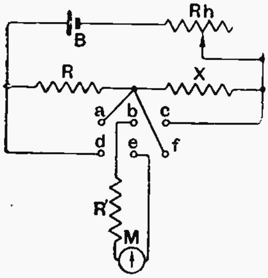

Fig. 15.

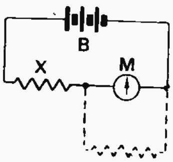

Fig. 16.

The microammeter, M, is arranged as a voltmeter, *i.e.*, it is connected in series with the 10 000 resistance, R^1. *abcdef* is the mercury-cup switch shown in Fig. 7.

The method of measurement is very simple :—

Let d_R=reading of the meter when the switch is in the left-hand position, a to b and d to e connected ; and d_X=reading of the meter when the switch is in the right-hand position, b to c and e to f connected.

Then $\dfrac{X}{R} = \dfrac{d_X}{d_R}$, *i.e.*, $X = \dfrac{R \times d_X}{d_R}$

2. *High Resistance.*

The following method is suitable for measuring resistances from 50 ohms upwards. Although there are certainly more accurate methods, yet it requires no additional apparatus and its precision is comparable with that of the other measurements described.

Connect a six-volt accumulator, B, Fig. 16, in series with the unknown resistance, X, and the microammeter, M, if necessary, choosing a shunt for the latter such that the current gives at least a half-scale deflection. Let this current be I amps. Then, measure the exact voltage of the accumulator by the method already given and the required resistance $X = V/I$

In measuring resistances higher than 100 000 ohms an ordinary dry-cell high-tension battery should be substituted for the accumulator, B. For example, an H.T. battery was connected in series with the microammeter and a grid-leak and a current of 70μA was indicated. The instrument was then used as a voltmeter and the battery was found to have an E.M.F. of 97 volts. The resistance of the grid-leak was thus

$$\frac{97}{70 \times 10^6} \text{ ohms} = 1.38_5 \times 10^6 \text{ ohms} = 1.38_5 \text{ megohms.}$$

In the above method no account is taken of the resistance either of the accumulator or the meter but, in dealing with resistances of over 50 ohms, the error is very small and it becomes progressively smaller as the resistance becomes higher.

APPENDIX.

As the method here described for the making of shunts is original, an explanation of its theory may not be out of place :—

If I_1=current flowing when switch is in left-hand position, a to b, d to e connected (Fig. 11).
I_2=current flowing when switch is in right-hand position, b to c, e to f connected.
S_1=combined resistance of shunt S, and microammeter M,

then

$$\frac{I_1}{I_2} = \frac{Q+S_1}{P+G} \text{ (see equation 6, above) } \quad .. \qquad (7)$$

$$S_1 = \frac{G \times S}{G+S} \text{ (from equation 3)}$$

When the readings are the same for both positions of the switch :—

$$I_2 = \frac{G+S}{S} \cdot I_1$$

Equation (7) becomes

$$\frac{I_1}{I_1 \cdot \dfrac{G+S}{S}} = \frac{\dfrac{G+S}{Q+G+S}}{P+G} = \frac{QS+QG+GS}{(P+G)\,(G+S)}$$

$$\therefore \quad \frac{S}{G+S} = \frac{QS+QG+GS}{(G+S)\,(P+G)}$$

$$\therefore \quad SP+GS=QS=QG+GS$$

$$\therefore \quad S(P-Q)=QG$$

$$\therefore \quad S = \frac{Q}{P-Q} \cdot G$$

i.e.,

If (1) $P = 1000$ and $Q = 100$ then $S = \frac{1}{9} G$
$P = 1000$ $Q = 10$ $S = \frac{1}{99} G$
$P = 1000$ $Q = 1$ $S = \frac{1}{999} G$
$P = 10\,000$ $Q = 1$ $S = \frac{1}{9999} G$

Some Remarkable Effects of Large High-Frequency Electric Currents.

By *G. H. Farnes, B.Sc.*

[R100·009·1

MOST amateur constructors are occasionally brought in touch with some apparently unaccountable phenomena in the working of their wireless apparatus. Some of these troubles may be due to faulty insulation or electrical connections, but others, especially if the lay-out of the components of the set has not been carefully planned, will be due to capacity and inductance between the actual wiring in the set. In receiving sets the trouble is often difficult to locate as the currents in the high frequency circuits are only a few thousandths of an ampere. When, however, one is dealing with large high frequency radio currents of several hundred amperes, such as those at our high-powered commercial transmitting stations, the effect of "stray" capacities and inductances is sometimes brought into alarming prominence. If there are any closed metallic circuits near the coils carrying the high frequency currents they will have circulating currents induced in them, especially if the capacity and inductance of any loop happens to have a natural period of oscillation which is the same as, or a multiple of, the main aerial oscillation. Such closed circuits are formed by iron frameworks of the building, iron conduits for electric light conductors, iron framework of switchgear and by the numerous leads connecting the apparatus. If these circuits have any appreciable resistance great losses occur which result in heating and often in the burning out of conductors. A few strange effects which have come within the writer's personal experience show how careful one must be when dealing with large high frequency currents, not only by having perfect direct insulation but also as regards neighbouring conductors and partial conductors which are not connected to the current-carrying conductors in any way.

In the Inductance Room of a High-powered Transmitting Station.

At a large transmitting station, during alterations to the room housing the aerial inductance, it was necessary to erect a temporary wooden wall. One day this became damp due to rain and hence was made a partial conductor. The eddy currents induced in it caused it to heat up and eventually burst into flames.

In the same inductance room by nature of an experiment two lengths of damp wood were laid across each other forming two sides of a triangle. For some time nothing happened but when a third piece was laid acrosss to complete the triangle, and hence the circuit, the wood quickly heated up and caught fire. On another occasion it was noticed that the iron lock on the door was unbearably hot due to hysteresis and eddy current losses. If a watch is worn near these coils it is quickly disorganised. By walking round the room and holding the point of a pencil near various metallic objects, such as an instrument case or a telephone, a stream of sparks can often be drawn.

There will occasionally be a potential between two people in this room due to their different electrical capacity and on one occasion when the writer shook hands with a friend in this strange inductance room there was a stream of sparks from one hand to the other.

These strange effects are not confined to the inductance room but are also to be noticed under the aerial system which, by the way, is half a mile long and about six hundred feet wide. The stays of the aerial masts were insulated by large porcelain strain insulators and trouble was experienced due to these "flashing over." When they were taken down and examined it was found that they had been practically molten inside. There is a barbed wire fence round the aerial site and the portion of this which is under the aerial lead-up becomes so hot by the induced current that it cannot be handled. Sparks can be drawn from this also by holding a piece of metal near to it.

On one occasion an iron stay wire was attached to a wooden pole carrying the earth wires and the mutual inductance between the earth wires and the stay wire was sufficient to cause the induced high frequency current to heat the stay wire to a white heat.

On another occasion the effect of a slight stray mutual inductance and capacity had a more serious effect. A steel wire halyard was left attached to the top of a 460-foot wooden lattice mast, the lower end of the halyard being connected to a winch and thus practically earthed. When the station was working a potential was induced between the halyard and the wooden top of the mast which can be regarded as insulated by virtue of the wooden structure. This caused sparking and the first thing noticed, after several days' running, was that the top of the mast was burning. It was in fact a fire 460 feet in the air. Imagine the difficulty in obtaining firemen to tackle this job. The cost of each mast was approximately £5,000, not to mention the large financial loss due to closing down the station. Finally two firemen climbed the mast by means of the diagonal bracing members. It took them nearly an hour, but when they did reach the top they soon cut away the burning members by means of axes and prevented the fire from spreading.

Mathematical Conception.

Let us see now from a simple mathematical point of view the importance of small inductances and capacities when dealing with high frequency currents.

Now the voltage induced in an inductance is given by

$E = 2\pi f\,LI$, where E is in volts
f=frequency of current
L=self or mutual inductance in henries
I=current in amperes.

Thus we see that the induced voltage (and hence current) is directly proportional to the current in the nearby circuit and also directly proportional to the frequency of the current. Now with normal alternating currents for power or lighting the frequency is usually of 25 or 50 cycles per second but with radio currents frequencies of 50 000 cycles or 1 000 times as great are quite low. Thus with radio currents the voltages induced will be 1 000 or more times that which would result if the circuits were carrying ordinary alternating currents.

A similar formula applies for the currents induced due to capacity, *i.e.*,

$I = 2\pi f\,CE$, where I is the current flowing "across" the capacity and
C=capacity in farads.

Hence the "stray" current is again proportional to the frequency which is very large. Another important point is borne out by these equations. The higher the frequency (*i.e.*, the shorter the wave-length) the greater are the stray currents caused. Hence in designing short wave receivers where the frequency is extremely high one has to be even more careful of any unintentional inductive or capacity couplings between leads or components.

Some Facts and Notions about Short Waves.

A Lecture delivered by Capt. DUNCAN SINCLAIR, before the Radio Society of Great Britain, on 16th December, 1925.

[R110

LAST month Captain Miles, of the Admiralty, read a paper before the Radio Society of Great Britain entitled a "Review of Short Wave Development." At the time the Society honoured me by requesting this paper, I chose practically an identical line of thought, but on reading Captain Miles' paper, it at once became apparent that my remarks would need modification if I were to present you with fresh subjects.

I have therefore reviewed my paper to make it follow more on the lines of a sequel to this former paper, and have made it more theoretical and suppositious. At the same time my remarks have become briefer, which may be an advantage as giving more time for discussion.

I am going to define a short wave as one of the order of less than 100 metres length. Perhaps, before so many short-wave experts, some of my points may prove to be ancient history, but they may still, for all that, be worthy of consideration.

It rather seems that the majority of experimenters approach short waves with feelings of considerable trepidation. They have heard that the frequencies are enormous, hand capacity effects are simply terrific, and so on ; and before they ever come to handle the work themselves they have quite made up their minds that they are attempting the impossible. They expect little or no results, and consequently are not disappointed by failure.

Due consideration of the practical side of the problem does not show it to be so very difficult, however. We still are really dealing in terms of wireless as we understand it for the higher wave-lengths, with comparatively negligible variations ; and we still employ identical components, again with very minor differences. What it really means is that we must expect to exercise rather more care in the construction and operation of the apparatus—not a difficult matter for those who take real interest in their work.

It seems obvious that, if more care is to be taken, the simplest designs only should be tried in the first place. We turn again to the simple straight circuits. And, after all, simplicity and efficiency are so often synonymous.

For the benefit of those who are not great technical experts, I am purposely limiting my language so that it should be comprehensible to the least trained mind.

I propose to deal with certain facts and notions, the latter somewhat my own conception, of phenomena beyond the actual apparatus which occur in this class of radio research. They may be *entirely erroneous*—or, on the other hand, may contain the germs of matters of vital importance. I can merely put them before you for what they may be worth, in the hope that they will help in leading to correct and definite conclusions.

In dealing with wave-lengths which get shorter and shorter as research extends, it would rather appear that a close analogy is to be found in the science of "Heat." Temperature can be reduced step by step, but absolute zero probably never attained, for the purpose in mind at the commencement of the experiment—if it can be attained at all. The matter or substance under investigation will change its nature before its temperature reaches absolute zero.

It is well known that the frequency band of electro-magnetic oscillations which give rise to radio phenomena are only a portion of a much wider band of the same kind, and that the whole band, in its entirety, includes the phenomena of heat and light amongst other things ; and, for all we may know, every phenomenon of which our senses make us aware. We know that the higher we get in the radio frequency band the closer we approach the frequencies of heat and light. Even if we did not already realise this from theoretical considerations, it would not be possible to experiment for long on very short wave-lengths without discovering it practically.

I well remember some time ago experiencing a sensation of considerable heat round the closed circuit inductance of my transmitter whilst it was functioning on what I estimated to be something under five metres wave-length, and, on switching off the set, how interested I was to find the inductance quite cold. Similarly, how various glows have occurred at different times in different parts of the circuit.

Given such simple facts we have grounds for assuming that a lot of our reasoning and conjecture may be based upon confirmed knowledge of light phenomena. If we are still in doubt, we have only to follow Clerk Maxwell's mathematical investigations, during last century, of the connection between electricity, magnetism and light. Light, as it is at present understood, is a composite phenomenon due to certain very high frequency oscillations, of slightly different values. One frequency causes our optic nerve to register the sensation of a red colour, another a green, another a violet; while the simultaneous effect of all the visible colours of the spectrum manifests itself as what we call white light. It is well known that by passing a beam of white light through a glass prism its component colours are "bent" in varying degrees and become, in effect, separated from one another. It is well known that light can be reflected by a mirror or other polished surface; and that it can be absorbed by some media and substances. And it is known that interference between two or more light frequencies can occur. Again, it is known that certain very short waves of this band can penetrate certain matter which others are not able to do. For instance, metals opaque to visible light become translucent or transparent under the agency of X-rays. And there is the property of radio-activity. All these phenomena obey very definite laws.

Here is a wealth of data to assist our work with short radio waves. Surely we are not unjustified in expecting the laws of light to have application in the case of radio frequencies so closely situated in the band of general frequencies. In fact, as long ago as the first days of wireless, Senatore Marconi proved the reflection of waves longer than those we are now considering; and more recently has put forward the system of beam transmissions. More recently, again, there has been expounded the theory of the Heaviside layer, by which there is supposed to exist, at a varying number of miles above the earth's surface, a layer of ionisation, constituting a medium of different nature to that of the adjacent electro-magnetic medium. Reverting to our light analogies, we are led to expect, and, in point of fact do find, radio results in conformity with the assumption that the Heaviside layer may be regarded as a medium capable of reflecting, refracting and absorbing electro-magnetic waves. In the first place, you will know that the angle through which a light ray is bent by any given medium, varies according to the colour or wave-length of the light in question. Therefore we expect that a short wave of, say, 10 metres length, will not behave in the same fashion as one of, say, 100 metres, and that is exactly what we do find. You will have found out, or heard, that whereas a transmission is audible on 10 metres at some distance, it may not be at all audible at some point nearer the transmitter; and that while enormous ranges are obtainable during darkness with a 100-metre wave-length, a 10-metre wave-length is not capable of working any real distance; and that the reverse of such case may obtain during daylight.

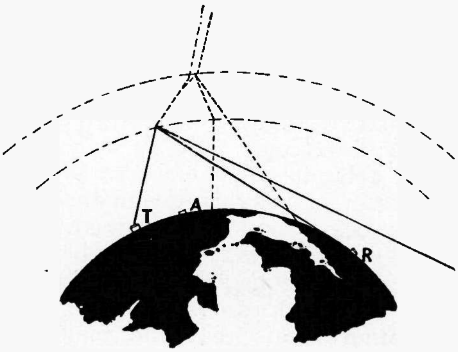

Fig. 1.

An explanation of such occurrences may be given on the basis of the Heaviside layer. The short wave-length, inaudible nearby, has probably acted in the way shown in Fig. 1. *T* is the transmitter, *A* the nearby receiver, and *R* the distant receiver. The wave (thick line) has been reflected by the layer in a similar manner to that in which a light wave is reflected from a concave mirror. But certain portions of the radio wave also enter the Heaviside medium and become refracted through it and either again refracted through into space beyond or internally reflected or refracted back again to earth (dotted lines). Naturally, these reflections and refractions will all depend upon the wave-length, the nature of the reflecting and refracting medium and its thickness. Probably, it is not a uniform medium; probably, also, it is changing constantly. Therefore it appears that we have an explanation of fading, of "distortion," and of the variation of direction finding bearings, amongst other things.

There is little doubt that in the same way that the visible light ray and the invisible light ray are able to penetrate a given medium to varying extents, so one radio wave can penetrate the Heaviside layer to a different depth from that of another. Probably also the reflection and refraction vary to a similar extent. If such is the case, and at the same time we assume that the degree of penetration reflection and refraction is varying, we must certainly have a useful explanation of fading and distortion, and, to a like extent, variation of direction finding bearings.

Various experimenters tell us that fading occurs regularly, that is to say, that we get constantly varying time periods during which we can, or cannot, receive signals from a given station; which would rather indicate that the variations occurring in the layer are uniform in their action at any given period of the 24 hours. It seems probable that in order to receive the very short waves at comparatively short distances during daylight it is necessary to raise the height of the receiver from the ground, either by moving it up a mountain or taking it up in an aircraft. A possible explanation of such a phenomenon has already been outlined by Captain Miles in his paper last month, so that it is possibly unnecessary for me to enlarge upon it.

I am not going to attempt, beyond what has been said above, either to confirm or contradict the explanations afforded by the Heaviside layer theory. If they are correct they serve our purpose admirably for the time being, and, if incorrect, there is at least some vestige of truth in them which is decidedly helpful to us as we are situated to-day. One fact stands out very clearly in all this work and that is that the reactions taking place are profoundly delicate. I have said that whereas a 10-metre wave-length is not apparently capable of working any great distance during darkness, a wave-length of five to ten times that value is quite capable of doing so; and, conversely, while a 60-metre wave-length is not suitable for daylight working, a 10-metre wave-length is receivable at very great distances.

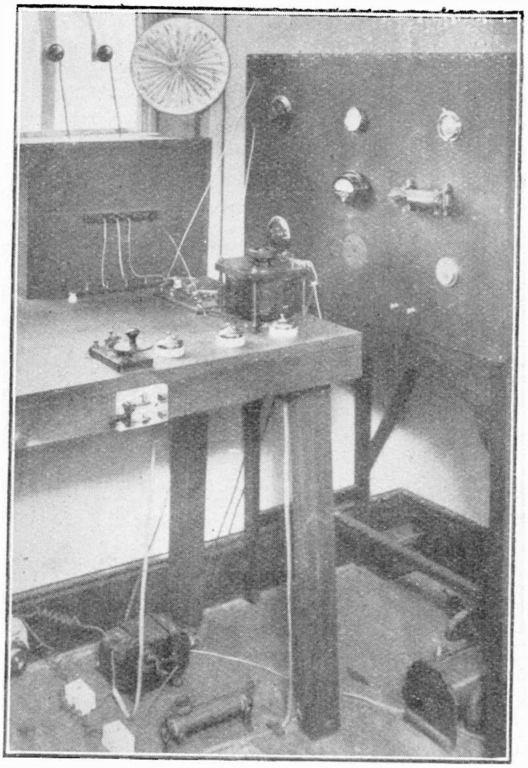

Fig. 2. *Photograph showing the transmitter* (20-30 *metres*) *on the right. One broken down condenser was removed at the time the photo was taken, and an oil immersion condenser* (*on the bench*) *substituted.*

On the face of it it would seem that as we decrease our wave-length we pass firstly through a band of wave-lengths which are suitable for night work only, and secondly, into a band of wave-lengths which are suitable for daylight work only; and, therefore, we have reason to suspect that at some point we shall find a very small band of wave-lengths, or even an actual wave, which is good for both day and night signalling over a certain distance.

In practice it is easily ascertained that for given periods during one day we can quite readily receive signals from, let us say, a given station in the United States, while the next day we are unable to do so on that wave-length, but can readily do so if the wave is changed by one or two metres. This phenomenon most usually occurs in the neighbourhood of 30 to 40 metres ; so that it would seem that the wave-length good for both day and night working would lie somewhere within this band.

As far as I am personally able to see at the present time, I rather expect to find that several wave-lengths, or even many in or very near this band will prove suitable for regular constant working, taking into account the distance separating the two communicating stations in each case.

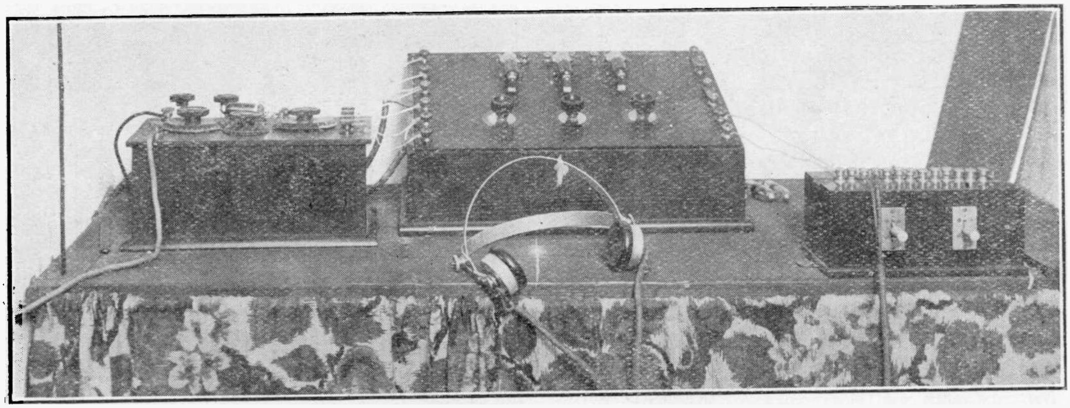

Fig. 3. *The general purpose long-wave receiver, consisting of detector and tuning unit, and 3-valve resistance coupled amplifier. The box on right controls H.T. and L.T. and phones, loud-speakers (for broadcasting), etc.*

If we may, with some justification, assume the existence of an ionised layer at a distance of several miles above the earth's surface, we may also expect to find ionisation taking place nearer and especially on the earth's surface, more particularly in those areas where the nature of the soil is of assistance to the process of ionisation. Take, for example, soils which contain radio active substances. Naturally the structure of the earth's surface is not entirely uniform all over, and therefore it seems that the areas of ionisation on this basis alone are not in themselves quite uniform. It seems an established fact that over the surface of the sea the conductivity of the atmosphere is somewhat better than that over land. It seems that ionisation is much more uniform over sea than over land. We know that at difficult times for direction finding work bearings may be taken over the sea with possibly half a degree's error up to distances of perhaps 100 miles, while during the same periods over land the limits approximate to 15 miles and the errors to 5 or even more degrees.

If we allow that the intensity of illumination by the sun's rays may be a factor in ionising the atmosphere and that the intensity of ionisation may vary at the lower altitudes depending upon the amount of cloud, etc., it is quite evident that the amount of ionisation occurring in any particular area is by no means a definite quantity at any given moment even over sea.

It has been established that the mean values of the conductivity of the atmosphere in the neighbourhood of the ground vary very considerably for different regions as much as three or four times their standard value. The variation most surely depends on local conditions. Various independent scientists seem in agreement that the conductivity of the atmosphere increases with height above the earth's surface. They tell us that although the first few hundreds of metres above the ground surface have not been sufficiently explored, from the point of view of ionisation, the fact is certain that above certain altitudes and in the highest regions where they have been able to make measurements, the conductivity may reach values of 25 to 30 times greater than on the earth's surface. Their research leads also to the conclusion that the mobility of ionisation varies sensibly inversely as the atmospheric pressure. The ionisation of the air must then increase at great altitudes. The assumption seems, therefore, that there is a layer of ionisation immediately surrounding the earth's surface and extending for some distance above it, decreasing in density as height increases. This layer would appear to be only of a few hundred feet in height and would vary quite considerably in accordance with local conditions. This assumption so far seems to me to explain why it is that at certain points we are never able to receive wireless signals properly. In other words, it seems to explain the problem of the "blind spot" because it is quite conceivable that the ionisation at any given point may be such as to preclude considerably, if not entirely, the possibility of oscillations of a given frequency taking place. I think it is justifiable also to assume that, beyond the temperature and illumination effects of the sun's rays on ionisation, atmospheric pressure may also play a part, or, in other words, that meteorology has quite a definite connection with wireless. Subsequent to forming this opinion I learnt recently whilst in Germany that there are certain parts of that country in which it is impossible to carry out accurate direction finding work during such times as there is bright sunlight accompanied by a sudden fall of atmospheric pressure.

On the assumption that ionisation increases directly in proportion to solar illumination we might expect something of the state of affairs shown in Fig. 4. In the case of perfectly uniform daylight over very wide areas the ionised layer is, perhaps, held in a position of stable equilibrium by some electromagnetic field due to the solar rays; thereby giving rise to comparative density of ionisation and consequent impedance to signals on wave-lengths above the lower limit. We remember, years ago now, before short waves were discussed, how ranges were generally reckoned to be increased by night.

In practice absolute uniformity of daylight over great distances does not occur, owing to the comparatively slight alteration of other factors; and, therefore, we may have a condition of affairs where, for long distances during daylight, the medium, speaking generally, is uniform, semi-obstructive and somewhat variable to signalling, but for shorter distances is to all intents and purposes uniform. To quote an example, D.F. on wave-lengths of 600 to 900 metres during almost any sort of day is good at ranges of, say, 100-150 miles. It remains to determine practically whether the error increases as the distance between the two operating stations, or whether we find larger errors at greater ranges, or, again, whether D.F. on the shorter waves, *i.e.*, those below the suspected low limit, is impossible. From my present experience this latter is the case.

At night time, the ionisation may become considerably non-uniform, even, in fact, being widely broken up, and that waves of lengths above the low limit are vastly affected; while the shorter ones are unaffected. I know, too well, that ground stations taking bearings of aircraft during night flights are very often considerably in error, working on the wave-lengths previously mentioned, but have no experience as to whether the shortest waves, if it is possible to use them for D.F., would prove reliable. It would not be surprising to find that they would prove so since they very likely behave in the way that a series of machine gun bullets behave with most obstructions in its path. The "pick up" is so large in a short space, for these very short waves, that D.F. does not seem hopeful at present.

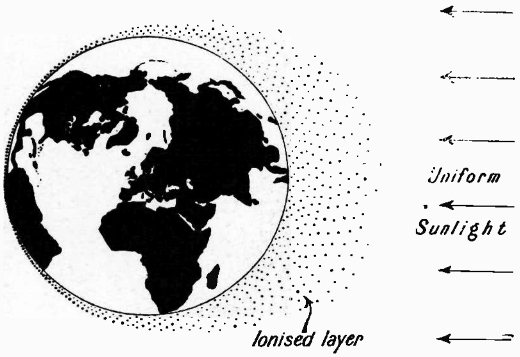

Fig. 4.

It does seem that the stronger the signal the less interference it experiences at night, and the straighter the path it follows. If this is true, and if also it is true that the density of ionisation increases the nearer we approach the ground surface, then the signals received on the ground from an aircraft are much more liable to variations than those received on the aircraft from a ground station. In the latter case the maximum transmitted energy exists in the region most liable to cause trouble and, therefore, forces its way through with little or no damage being done. As it approaches the aircraft, and gets weaker and weaker, it enters a more and more uniform medium and, therefore, though in a more suitable condition to be affected, is actually less so. This again is mere assumption, but, if true, will enable bearings to be taken, during night flights, on board aircraft and will afford a really invaluable aid to air navigation.

The breaking up of the regular ionisation at night would also seem indicated by another phenomenon. A comparatively strong magnetic field would doubtless have some influence over ionisation, and if the connection between light and magnetism is as close as I have assumed, the attraction or repulsion of huge groups of ions by terrestrial magnetism to or from the poles, with all the consequent re-arrangements and re-adjustments of the particles giving rise to vast changes of frequencies, might conceivably so alter matters as to give rise to light phenomena such as we get in the Aurora Borealis. All that I know from experience is that, some years ago, when I had charge of a group of wireless stations in the far north of Russia, most extraordinary noises became apparent in the receivers after nightfall whenever there was a display of Aurora—especially on one occasion when the station concerned was in the apparent centre of the disturbance. During that evening, for a matter of a very few minutes, Seychelles was read at *tremendous* strength by one of my operators, just before the display. We spent the whole of the night subsequent to the display in trying to work another station in the group only 160 miles away. Remember that this was in the days when the idea of working to the remote ends of the earth seemed really an utter impossibility.

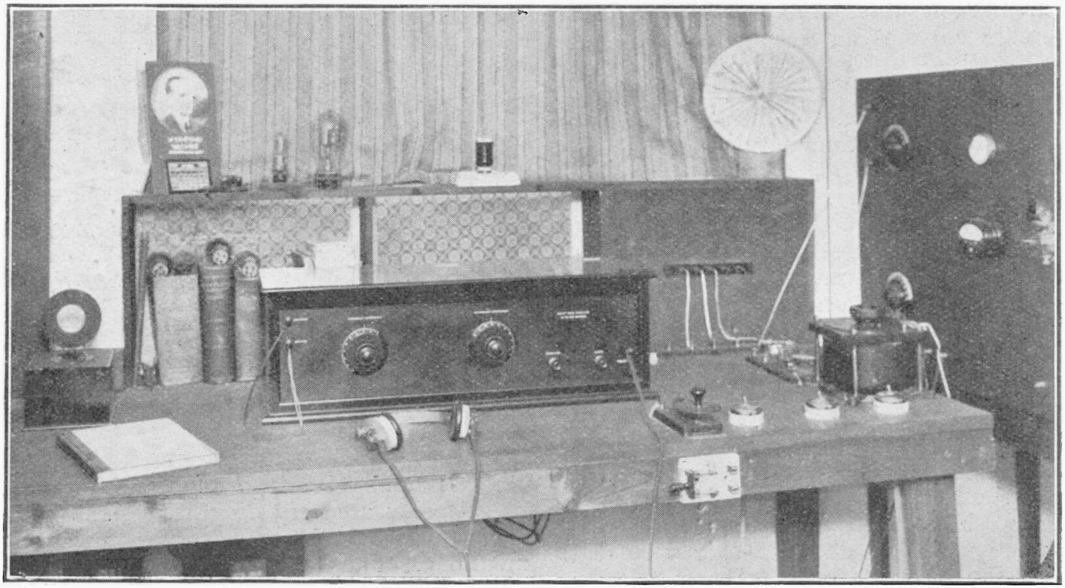

Fig. 5. *General view of short-wave apparatus. The receiver* (10-150 *metres*) *is seen in the centre, and the synchroniser on the left.*

As a mere hypothesis affecting wireless and *not* as a theory perhaps it may be said that the *ground surface* definitely plays a highly important part in the process of ionisation. This surface ionisation decreasing in intensity with height and extending only a few hundred feet above the surface, varying with local conditions, and with the effects of day and night, affords, I suggest, a possible explanation of many of the well-known phenomena existing in our present state of development, and which are not so easily explainable on the theory of the Heaviside layer. It may be that sunlight is not capable of ionising the atmosphere near the ground owing to the fact that those rays from the sun, capable of ionising gases, are not able to penetrate the entire depth of the atmosphere and become absorbed before they reach the earth. It is quite possible that, on the other hand, during certain times of low pressure these rays are able to penetrate to the ground. We all know the effect of reducing the pressure between two electrodes in a glass vessel.

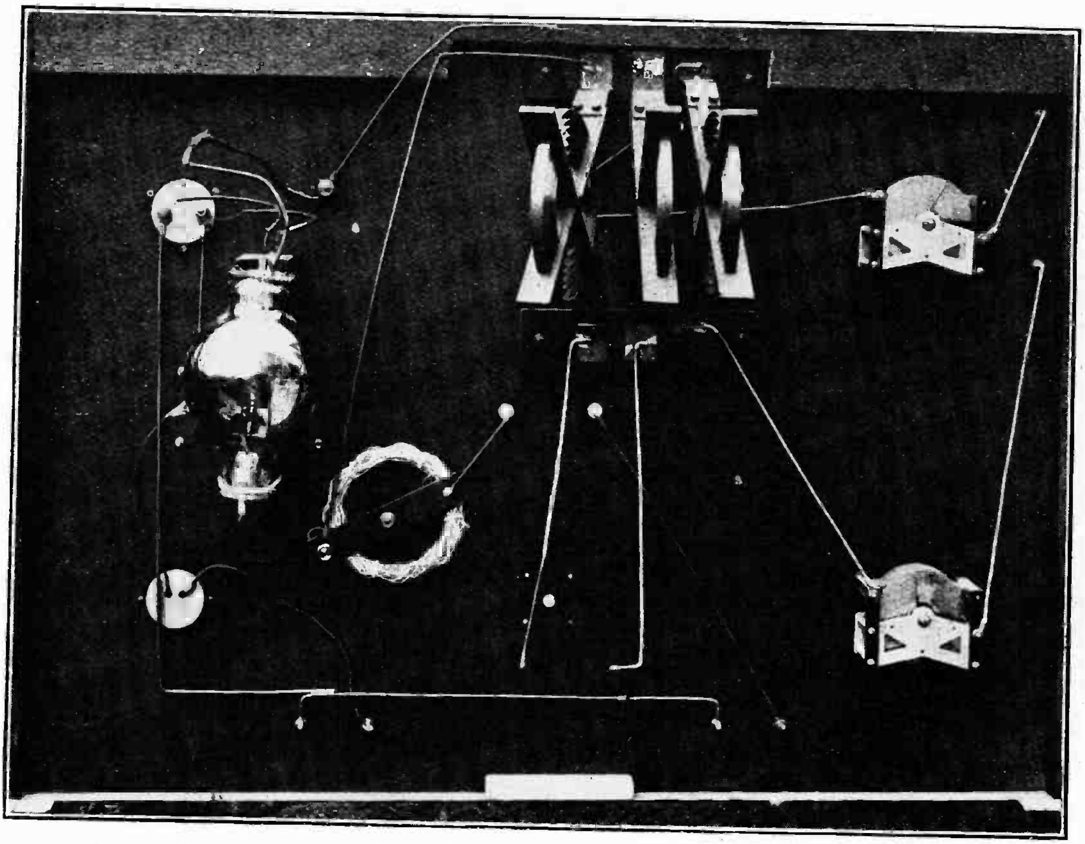

Fig. 6. *Back of the transmitter. The "broken down" condenser is removed* (*see Fig.* 2).

I have been most fortunate in getting for this paper a preliminary reading by a leading authority on atmospheric electricity. This gentleman has referred me to a document in French under the title of *Electricité Atmosphérique*, by M. B. Chauveau, published by Librairie Octave Doin, Paris. I can thoroughly recommend those deeply interested in this subject to a thorough study of this publication. At the same time there are about to be published the results of some very recent researches, based upon Larmor's work, which will throw a great deal of light on matters at present in the dark. In certain respects its publication would lead me to modify several points in this paper. But it is not yet public property and these views, although only two to three months old, must stand. It is only another example of the speed of development of our science.

I think I am safe in saying that as we stand to-day wireless theory generally leaves very much to be. desired. We continue to explain phenomena by theories which do not really afford explanations and I think that we must search farther for the true meaning of what is going on. I have put forward in these facts and notions a hypothesis which may lead to the establishment of a definite theory, but I hope sincerely that what has been said will give rise to helpful discussion and that this discussion will lead us to the determination that existing theories need revision. Personally, I shall welcome discussion, both constructive and destructive, but I hope that the discussion will be constructive. It is only too easy these days to present a destructive criticism, especially on so highly controversial a matter. We have, all of us, professional and amateur, an enormous amount of research work before us in this respect, and we must search for positive and definite explanations before we can even begin to understand where our work is leading.

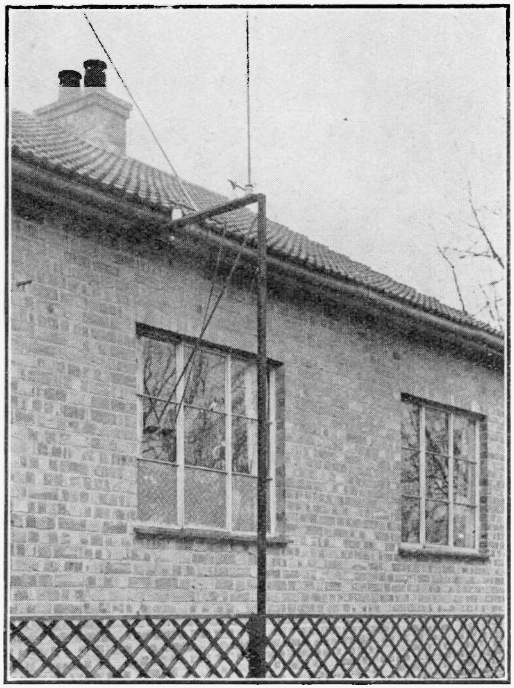

Fig. 7. *The leading-in system. The masts,* 40 *ft. and* 70 *ft., are not shown. The short-wave aerial is a* 25 *ft. vertical wire, and the long wave aerial* 70 *ft. horizontal with* 40 *ft. down lead, average height* 55 *ft.,* L*-shaped.*

It may be interesting to see one or two views of my private station, 2OC, at Shepperton-on-Thames, which is illustrated in Figs. 2, 3, 5, 6 and 7. These have little or nothing actually to do with the paper, but is perhaps of passing interest to you.

Some time ago the British amateur was given a band of short wave-lengths to experiment with, and the results he obtained were startling to say the least, in so far as ranges and powers were concerned. His work quickly led to minute investigation by the professional radio-engineer, with the result that our present knowledge was the outcome.

An enormous amount of credit is due to those amateurs—those heroic souls who sat far into or even through the night often in complete personal discomfort—to determine what we now know. For once the amateur has shown us the way.

I can only say that we are quite willing again to be led in such a manner.

DISCUSSION.

Mr. P. R. Coursey: It is always interesting to have a paper which gives us another view of what may be a more or less commonplace matter. We are all now in the habit of speaking very freely of ionised layers, Heaviside layers and so on, and that being so it is all the more interesting to have someone come forward with at least some modification of the well-known ideas on that subject. The suggestion of a fairly low ionised layer, although it may not be substantiated—I cannot say as to that —is valuable and it is certainly interesting to learn that some data is probably forthcoming for publication very shortly and will tell us a little more about it. That suggestion of a low ionised layer seems distinctly useful. I would suggest that it might probably follow from that, that there are existing streams of conducting and semi-conducting gas moving about in the atmosphere, at apparently low levels, and if that is the case, these streams of conducting gas—winds of conducting gas—might be a very easy means for accounting for the directional changes which are observed. It is like putting a conducting loop near a D.F. receiver. We all know what errors are produced in that way, particularly, of course, on board ship or aircraft if there is any conducting metal in the neighbourhood of the D.F. receiver. I think, also, that such conducting streams of ionised gas might help to account for the earth-air currents which are observed by meteorologists. The fact that these currents are known to exist means that there must be a movement of electrons near the earth's surface and it is possible that they may exist in comparatively large bulk. The suggestion that these patches of low level ionised gas may account for blind spots, also seems very instructive and I am sure we are indebted to the author of the paper for these very useful and instructive suggestions. Personally, I often feel that the pictures that are shown illustrating or purporting to illustrate the reflections and refractions in the upper ionise layers, seem a little far fetched. They are always so obviously drawn to illustrate what the lecturer wants to talk about, and whilst they are prettily drawn to show rays sent from a station at *A* to another at *C* and missing out another at *B*, it is perfectly obvious that one could draw another line which would connect up with *B* and *C*, and whilst considerations of reflections and refractions, critical angles, etc., obviously lead to limitations as to where one can draw rays, there seems to be a very wide range of possibilities that are not altogether satisfactorily explained by the generally accepted theory, as far as I am aware. This, all the more, seems to render useful the suggestion of other patches of ionised gas at a very much lower level, which may move about, and probably fairly rapidly, by ordinary meteorological convection and wind circulation. Finally, we must thank the author for a very interesting paper which we hope will lead to useful results.

Admiral Sir Henry Jackson: I think the author has given us a very interesting paper, but it is one that must be read carefully to get to the bottom of all the points raised. He puts forward what may be considered the accepted theories of most people, although there are scientists who differ from this accepted idea of the Heaviside layer theory. With regard to the different results from different wave-lengths, it is rather early to speak. Plenty of people are working on wave-lengths from 20 to 100 but very few are doing much with 19 metres and it may be that the 10-metre wavelength will do just as well as the others, but that there has not yet been a receiver at the right place tuned to receive it. Therefore it is a little hasty to say that a 10-metre wave will not go far. Perhaps that will come out in the course of further trials. In my opinion there is no doubt—and also in the opinion of the Radio Research Board —that the deflection of waves from the upper atmosphere has actually been proved by scientific measurements. The paper published before the Royal Society a week or two ago by Prof. Appleton is one proof, and there is proof from a gentleman in America who did it with low frequency. There are other papers by the staff of the Radio Research Board which will appear shortly and will corroborate this. There are really four different methods which have been tried and which prove this, and these down-coming waves can practically be proved to be the cause of fading and other variations. Where they come from and whether they are refracted or reflected is not yet proved, but there seems to be general agreement that the height of this reflection is about 80 kilometres. although it varies. I think it probably varies by day and by night and that is a point I hope we shall be able to take up. There is no doubt it plays an important part in radio communications and it may have an important bearing on the results obtained from aeroplanes. It is rather early to speak, however, until definite systematic experiments have been carried out.

Mr. E. H. Robinson: I am very interested in the paper, but I do not think I can add very much because the author has summarised the present state of our knowledge with regard to short-wave work fairly completely. At the same time we ought to be rather careful about arriving at conclusions with regard to the theory underlying short wave transmission. The period during which amateurs and others have been studying waves below 100 metres is very short, and even our experience of 45 metres dates back barely a year. Therefore I think it is a little premature to go too far on the theoretical side before a larger quantity of practical data has been collected. Thanks to the activity of the T & R Section, I believe the efforts of the amateurs are going to be co-ordinated and their data collected as far as possible, but so far, over a period of a year or two, very few experimenters have actually made consistent observations which they have published. The theories which have been put forward have really been put forward by one or two people and have been reproduced here and there in different periodicals and papers, and we get the idea that they are more generally accepted than they really are. In addition to the phenomena of reflection, refraction and the Heaviside layer effect on short-wave transmission, I should like to draw your attention to the possibilities of the effects of polarisation, which are known to exist but which are not remarked upon so much as is reflection. Any ordinary aerial will set up a polarised wave, *i.e.*, the oscillation will take place in one direction in a plane perpendicular to the direction of propagation. After travelling some distance, the plane of polarisation may alter or a different component in a different plane of polarisation may be introduced by the waves coming via different paths, and you get at the distant point an elliptically polarised wave. In general, what you get over big distances is an elliptically polarised wave and that means that whichever way you send a signal into the aerial it will have some effect. If you consider, however, a fixed aerial and a signal arriving, the inclination of the plane of polarisation may be varied by something in the intervening medium. Here again you have a totally different explanation of fading, and one which probably operates simultaneously with and independently of the reflection effect. I thought it would be interesting to point that out because I think it is very important.

Mr. H. Bevan Swift: The paper has been excellent and full of interest to those concerned with short wave work. I should like to start by thanking the author for the statement he made at the end of the paper, that the amateurs have helped to show the way. I think we are all very grateful, especially the members of the T. & R. Section, for that remark. The author's reference about the simplicity of apparatus for short wave working is very important. The simpler the apparatus the better both as regards the circuits for transmission and for reception. Too many people are coming down to short waves using supercircuits. If you listen down on 45 metres you can hear them tuning on their superheterodynes and it is going to be a great nuisance, and a great source of trouble and interference to those who are trying to do serious work down there. Again, I should like heartily to endorse Mr. Robinson's remarks that it is extremely premature to try to come to any definite conclusions about short wave work yet. We do not know where we are with regard to the Heaviside layer. As I think I remarked in the discussion on Captain Mills' paper at the last meeting, this is a thing which wants organising but we cannot organise it. It is there, and we have got to put up with it and the best thing is to find some means of combating it. To my mind, we shall have to do something on the lines that Mr. Robinson has laid down, *i.e.*, directional transmission. We shall have to treat the Heaviside layer as a sort of cushion against which we shall have to take aim for a rebound in order to get the station we want to send to. It is possible to conceive that some years hence we shall be taking aim with our receivers. We know the locality of the station we want to speak to and by some means, with a directional beam, we shall be shooting at the Heaviside layer, knowing its latitude, much after the fashion that you take aim with an artillery gun. I do not know whether that theory will ever come about but it is something which arises in my mind for the time being. One station with which I work stated they could only receive me at sunset and I arranged a test with that station to try and corroborate this. As the sun was sinking one night I carried out a series of tests for over half an hour and he was able to gauge reception and found that it gave a very peculiar curve, not in the form of a sine curve or an ordinary single peak, but in rather a series of peaks which, of course, I am quite unable to understand. That was only one test, of course and possibly it might not be repeated and if the author can offer an explanation I shall be glad to hear it. The other thing I should like to ask the author is whether he has made any tests with the Hertz type of aerial. It seems to me that this simple aerial offers great possibilities for short wave working and many of our prominent transmitting amateurs are now using that system.

Mr. Maurice Child: There are one or two points I should like to refer to. First of all the lecturer stated that it is possible for heat to be felt from a number of inductances in which oscillations at very high frequency are taking place. You might be interested to know that I can fully endorse that, having had experience of exactly the same phenomenon, in a transmitting set constructed for a local society in the summer of this year. The wave-length used was 45 metres and with an oscillating energy of approximately 100 watts it was possible to feel quite a warming up of the skin with the hand about 2 in. off a three-turn inductance coil about 5 or 6 in. diameter, yet the coil in question was absolutely at atmospheric temperature, as far as one could judge it, during the time the power was expended in it. That is interesting although it does not throw very much light on the main problem which the author has spoken about to-night. I remember reading in a scientific journal some years ago something which has just come to my mind whilst listening to the paper, and I mention it as a matter of interest although it may have no practical utility; on the other hand, one never knows; it may have some bearing. I remember reading an account of a very large volcanic eruption which took place on the Island of Krakatoa in the Japanese Group. I think 1876 was the date, and it was a tremendous eruption. It was recorded that some few days afterwards an appreciable quantity of dust—which was analysed and shown to be similar in character to that thrown out during this volcanic eruption—was discovered in the neighbourhood of London. I remember it was suggested that a certain height above the earth—I believe 40 or possibly 100 miles—there is a perpetual wind, going at a tremendous velocity and that somehow or other these dust particles from this volcanic eruption were caught up and eventually reached England. How much of this stuff is carried about in this way, of course, has never been determined nor have we any information as to how much comes down. Similarly, we have no information as to how much is still up in that wind, but it does suggest to me that if you can get a large quantity of dust carried about in that way the particles will carry electric charges and we do not know what sort of disturbances are taking place at great heights due to the bombardment of these masses of particles of dust against each other or with other matter. It may, as I say, have no important bearing on the subject but if it is a fact that there is this wind, it may possibly account for certain things. When all is said and done, however, we should very much like to know the complete theory of the transmission and reception of waves through space, but I cannot help thinking that even when we know it we are going to have a Dickens of a time regulating our space in such a way as to bring these waves within our absolute control, so that we can positively put a station at a certain point and say the wave-length is to be so-and-so and that it is going to reach that point and no farther. That is the sort of thing we want but it will take a long time. In that connection I think the paper we have had to-night is one we want to make a great deal of use of, because our lecturer has made out a very good case for the work of this Society to be extended and assisted in every possible way. I have in mind particularly the official assistance that is required from the Post Office and from various Government Departments concerned. It is a very serious thing, if, whatever is done in the commercial world to benefit the masses of the people from the purely entertainment point of view and even in the transmission of commercial messages, etc., anything is is done to give those wider facilities at the expense of research into this phenomena. That would be most disastrous. The paper has mentioned work which the amateur has done on certain specific wave-lengths, and a point was mentioned regarding the possibility of 30 metres being the best for signalling between this country and the States. But it is suggested—and I think quite rightly—that there may be a certain particular wave-length which the amateurs could use for stations close by and another wave-length for stations at a distance. If this is the case, it indicates that the amateur experimenter as well as the professional must have good facilities for quickly changing his wave-length when the opportunity occurs at the right moment in his experiments, so that he can say to the man at the other end that he is getting through. He should be able to change his wave-length, say five metres one way or the other. We must have facilities of that kind if we are going to carry on our work with the maximum efficiency and in the best interests of scientific discovery. At the present moment we have the Government Departments telling us we can only work on 45 metres plus or minus $\frac{1}{2}$ metre, and we get little pinpricks in the shape of letters saying we are going one metre over the proper wave-length. That is antagonism to scientific work. We want reasonable control but it must be reasonable and not simply muddling officialism.

Mr. F. S. Mockford: In the hope of one day being able to give correct bearings in any direction, we have collected a vast amount of information at Croydon by recording D.F. bearings on aircraft, the actual positions of which were known, but we find it almost impossible even in daylight to hope for such accuracy. For instance, the bearing of an aircraft over Maidstone may shift over several degrees within a minute and, in consequence, our D.F. positions have an average error of two miles. One interesting case was that of a machine over Lympne aerodrome, the bearing of which was 5° different to that of Lympne taken at the same time. If anybody can explain that I shall be very pleased. It is rather interesting in view of Mr. Coursey's remarks on streams of ionised gases.

Mr. F. L. Hogg : We have been rather prone to discuss the Heaviside layer and say that a wave will go up in a certain way and be reflected, but are we justified in assuming that we are getting such reflection from the Heaviside layer or that there is such a thing as a critical angle for the Heaviside layer as for glass or any other medium ? I cannot help feeling that we are pursuing the analogy between that and reflection and refraction a little too far, because the reflection and refraction of the Heaviside layer which we are assuming is not true reflection and refraction but a gradual bending effect. I thought I would like to mention that point as one worthy of further consideration.

Captain Duncan Sinclair, replying to the discussion, said : I think that Mr. Coursey has very aptly hit the nail on the head when he spoke of streams of ionised gas. I can call to mind one particular case where we were getting perfectly correct wireless transmission and reception over an enormous ravine, some 40 or 50 miles long. We were situated comparatively high at one end, but from another station which was slightly on the flank, we were getting no signals at all. In other words, there was some definite " stream " in that valley. I quite agree with Mr. Coursey when he says that the picture of the Heaviside layer as it is often drawn is a very pretty one, but why does station *A* transmit to *C* and miss *B* ? I have never been to the Heaviside layer and I do not suppose I ever shall get there and I am unable, in the present stage of my experience, to answer the question. But there seems to be very little doubt—I am including the remarks of the last speaker in this—that at any given moment the angle of reflection or of the rebound, if you like so to call it, is fairly critical, and that the penetration of a given wave-length into the Heaviside layer varies very much from time to time. Sir Henry Jackson did something which I hesitated to do. He mentioned the Radio Research Board as a possible correcting authority on certain points which I have mentioned to-night. You will remember that I said in my concluding remarks that there is about to be published the results of some recent researches based upon Larmor's work, and it was this particular research that I had in mind, coupled with Dr. Appleton's, for the proof of the existence of the Heaviside layer. As to my remarks about reception on an aircraft as against reception at a land station from an aeroplane, I hope to have something more to say on this later, because early next month we are carrying out tests of night flying with a Vickers machine, with the object of determining whether we cannot get rid of these troublesome errors which the Bellini Tosi system gives during the night, in order to render night navigation by direction finding wireless an absolute possibility. Mr. Robinson called for the co-ordination of the work of amateurs, and I remember that Captain Miles, in his paper last month, besought you all to put down what you do and keep a record. Therein lies the value of your work. We are struggling to find out something about short waves and all of you can help enormously by keeping a log and classifying the evidence on every point. Mr. Swift spoke of peaks of signal strength. A previous speaker requested me not to be too hasty in coming to any decision and I assure him that I refuse to come to any unduly hasty decision on this point. I would just like to make one point. You know that as the sun sinks over the horizon you get a changing series of skies which go practically through the whole scale of the spectrum, under certain meteorological conditions, culminating in the red. As each one of these colours changes, it may be that you get a certain phenomenon taking place which will give you first of all a strong signal and then a weak signal. I give you the idea for what it is worth. Mr. Child wants an extension of facilities, as far as the Society is concerned, as regards wave-lengths and he mentioned the Government Departments. I am connected with the Air Ministry and I can only speak in a personal way. For the Air Ministry, I can say this : we are inclined at least to be friendly, we wish you well and as amateurs we have faith in you.

Mr. H. Bevan Swift : It gives me very great pleasure to propose a vote of thanks to the author for his excellent paper. We cannot have too many papers on the subject of short waves as it is a matter which interests us all. I should like to assure Mr. Sinclair that so far as the remarks he made just now are concerned, we of the T & R section shall be only too pleased to co-operate in gaining information and in carrying out tests in any way we possibly can to increase the knowledge of short waves and help us to progress as a nation. It is with very great pleasure that I propose a cordial vote of thanks to Mr. Sinclair.

Mr. R. J. Hibberd : I have very great pleasure in seconding the motion. There is one thing which struck me in the lecture and that is the close association of meteorology with wireless. I am sure there is much to be done in this direction and in seconding this motion I appeal to all members to carry out some research work in this branch of wireless. I am certain there are many things waiting to be discovered. I have done a little in a small way myself and it has made me realise what a vast field of research it offers. I have no doubt that the next time Mr. Sinclair comes here he will tell us about wave-lengths on the minus side of zero. I feel that we are getting down there.

The vote of thanks was carried unanimously.

The Rectification of Small Radio-Frequency Potential Differences by means of Triode Valves.—Part IV. [R134

By F. M. Colebrook, B.Sc., A.C.G.I., D.I.C.

C.—Autodyne (Homodyne) Reception.

20. In addition to grid and anode rectification there is a third way in which a valve can be used for the detection of either continuous wave or modulated continuous wave signals. The distinctive feature of this method is that the circuits associated with the valve are in a state of continuous self-generated oscillation during the reception. Almost any convenient form of self-oscillating circuit can be applied to this method of reception, one of the simplest being that illustrated in Fig. 29, which is seen to be the same as the ordinary grid-rectifying circuit with reaction except that the grid-circuit resistance and grid condenser are omitted.

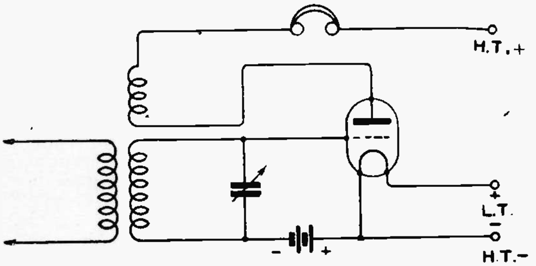

Fig. 29.

A full account of this subject will not be attempted in this paper, as the analysis involved is of so complex a character that it is doubtful if any useful form of mathematical statement could be arrived at. A brief description is included for the sake of completeness, and it is hoped to return to the subject at some future date.

Under suitable conditions of coupling between the grid and anode coils, self-maintained oscillations will be produced in the grid circuit. A milliammeter included in the anode circuit will show a considerable increase of anode current when this condition obtains. Since there is no appreciable change of mean grid potential (the external resistance in the grid circuit being negligibly small) this change of mean anode current must clearly be due to the rectification of the alternating components of the anode current by means of the curvature of the anode characteristic over the range of alternating grid voltage, which may be of considerable amplitude. The conditions are essentially those of anode rectification and heterodyne reception, operating at the maximum sensitivity. The magnitude of the change of mean anode current is very sensitive to the amplitude of the oscillation of the grid circuit. If the latter is tuned to a frequency nearly equal to that of a continuous wave signal E.M.F. induced in the coil by coupling to an aerial or other receiving system, the interference between the two E.M.Fs. will give rise to a periodic variation of the amplitude of the grid circuit oscillation, which variation can be made to occur at an audible frequency. This variation will in turn produce in the anode circuit a corresponding variation, at the same frequency, of the magnitude of the change of anode current, *i.e.*, it will give rise to a "beat frequency" change of anode current which can be made to operate telephone receivers either directly or after low frequency amplification in the ordinary manner.

The above process is usually described as autodyne reception. It is characterised by very great sensitivity. As an example it may be mentioned that an 18-in. frame with a centre tapping connected as a "shunt-feed" Hartley circuit with single plate condenser control of the reaction, *i.e.*, of the oscillation amplitude, followed by one low-frequency amplifying valve, will pick up the carrier waves of all the B.B.C. stations.

The application of the above method to the reception of modulated continuous waves is as follows: A carrier wave of frequency $\omega/2\pi$ modulated with a single pure tone of frequency $n/2\pi$ can be shown to be equivalent to three continuous waves of frequencies $\omega/2\pi$ and $(\omega \pm n)/2\pi$ respectively. If an E.M.F. of this character is induced in an oscillating grid circuit of which the oscillation frequency is kept constant at $\omega/2\pi$ then the carrier wave will be in synchronism and will not produce any beat tone. Each of the other two waves, however, will give a beat frequency $n/2\pi$ and a corresponding modulation frequency component of current in the anode circuit. Similarly, of course, for a modulation of any character. Thus, provided the grid circuit oscillation is held accurately synchronised with the carrier wave, there will be produced in the anode circuit a current having the same, or nearly the same, wave-form as the modulation, the faithfulness of the reproduction depending chiefly on the extent to which the change of mean anode current is a linear function of the amplitude of the grid circuit oscillation.

The term "homodyne" has been given to this method of reception. It is a convenient name to distinguish it from the autodyne reception of continuous waves. The success of the method obviously depends on the extent to which exact synchronism can be maintained between the carrier wave and the local oscillation. In practice it would probably be impossible to maintain the exact synchronisation required if it were not for an effect described and analysed by E. V. Appleton.[1] Under suitable conditions of relative amplitude, *i.e.*, if the carrier wave E.M.F. is of a certain intensity compared with that of the local oscillation, the latter will be controlled as to frequency by the former, automatic synchronisation being obtained. The phenomenon is a familiar one in another connection, namely, the wide silence band obtained when using a heterodyne wavemeter too closely coupled to the source of oscillation which is being measured—sometimes an apparent failure of the beat tone altogether, due to the same cause.

This automatically synchronised condition is quite easy to realise at distances up to 15 miles or so from a main B.B.C. station, and, of course, at even greater distances if preliminary high-frequency amplification is employed. In fact it is very probable that many persons habitually receive broadcast transmission in this way, for although a "grid-leak" and condenser are not required for the process, they will not prevent its occurrence, the only difference being that the oscillating condition will produce a decrease of mean anode current instead of an increase. This is due to the fact that the rectification of the alternating grid potential will give rise to a lowering of mean grid potential which more than counterbalances the increase of anode current produced by anode rectification. A distinctive feature of this type of reception is that a small displacement of the tuning of the receiving circuit in either direction will cause a heterodyne howl, since the local oscillation is pulled out of step with the carrier wave. As a matter of practical politics the method is not one to be encouraged, for though the oscillating valve is quite harmless in its controlled state, this condition cannot be reached without a certain amount of preliminary howling, and the local oscillation is always liable to be pulled out of step by subsequent adjustments of the receiving circuit. It is, however, a very interesting as well as a very sensitive method of reception, and one of which the possibilities have not yet been fully explored—probably because it is not very well known.

21. *General conclusions.*

In the present and the preceding paper already referred to, the author has analysed closely the behaviour of crystal rectifiers and various valve circuits as applied to the rectification of various forms of wireless signals. It should therefore be possible to draw up a complete quantitative comparison of the various arrangements on the basis of their efficiency in converting wireless signals into continuous or audible frequency energy changes.

In practice, however, no such comparison of general applicability can be made, since so much depends on the actual conditions of operation and the requirements to be fulfilled in any given case.

Suppose, for instance, that it is a question of converting a continuous wave signal into continuous current energy. It has been shown in the course of these papers how the

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[1] *Proc. Camb. Phil. Soc.*, 22, pp. 231-248.

maximum continuous current energy obtainable from the rectification of a given high-frequency E.M.F. can be calculated. In every case it is an expression of the form $E_c^2/4R_c$, where E_c is an effective rectified E.M.F. associated with an apparent internal resistance R_c. The following table gives typical results for various rectifying arrangements with a signal amplitude of .7 volt.

Method of rectification.	Opt. resistance load.	Max. D.C. power.
Galena Crystal ..	100 ohms.	200×10^{-6}
Zincite-Bornite ..	1 000 ohms.	20×10^{-6}
Valve. Anode rect.	100 000 ohms.	$.7 \times 10^{-6}$
Valve. Grid rect. ..	50 000 ohms.	50×10^{-6}

These are, of course, round figures, and different individual specimens, particularly of the crystals, might give results differing by 100 per cent. or more from the above values. They do, however, indicate the general nature of the comparison under the stated conditions.

But these conditions are so remote from those likely to obtain in actual reception that the comparison can only be described as academic. In the first place, it assumes a given fixed E.M.F. acting on the rectifier terminals. In practice the E.M.F. available for rectification will depend very greatly on the load imposed on the receiving circuit by the rectifying arrangement, and a much more reasonable basis of comparison would therefore be the energy transformation efficiency under specified conditions, *i.e.*, the ratio of the D.C. power to the high-frequency power necessarily consumed in the process of rectification. Here again, however, the comparison breaks down, as between valves and crystals, for in almost every valve rectifying arrangement the use of reaction gives a wide control of the load imposed on the receiving circuit. With direct crystal reception this control of the load cannot be obtained, so that for any given case there is a fixed optimum circuit condition, a feature which has been discussed elsewhere by the writer.[1]

In connection with this question of energy transformation a point of difference should be noted between the two chief methods of valve rectification. Grid rectification operates in virtue of a load imposed on the receiving circuit. Anode rectification, on the other hand, does not *require* the absorption of any power from the receiving circuit. But again, in practice, inter-electrode capacity minimises this distinction at all except very long wave-lengths, and, in both cases, the load can be controlled by reaction.

At present the most general, though not, perhaps, the most important, application of rectifiers is to the reception of broadcast telephony. In this connection the most important consideration is not the efficiency of the process but the purity of the reproduction of the modulation. From this point of view, crystals have a superiority over valves in two respects. In the first place their rectification characteristics (*i.e.*, rectified current plotted against high-frequency E.M.F.) are very nearly straight lines for signal amplitudes greater than a few tenths of a volt. This ensures that extraneous frequencies introduced by the rectification process shall be of very small amplitude compared with the original frequencies. In the second place, the effective internal resistance to the modulation frequency currents is much smaller than that associated with either type of valve circuit. The significance of this fact from the point of view of purity of reproduction has been dealt with in the first part of the paper. Further, as compared with grid rectification, crystals have the advantage of freedom from the inherent distortion effect described in Section 13. Once more, however, no general comparison can be made, since the acoustic characteristics of the load would also need to be considered.

As between the two different types of valve rectifying circuit, anode rectification is intrinsically far less sensitive as judged by the D.C. power obtainable from the rectification of a given signal. On the other hand, it is free from the grid condenser effect in the reception of modulated continuous waves or in heterodyne reception, and may therefore give an equal or even greater sensitivity at comparatively long wave-lengths (say, 3 000 metres or over).

On the whole, it would seem that the best arrangement of all is one which combines the intrinsic sensitivity and purity of the crystal with the energy efficiency of the valve. An arrangement of this kind is

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[1] "What is the Best Circuit for Crystal Reception?" F. M. Colebrook. *Wireless World*. 30th April, 1922 (p. 122).

illustrated in Fig. 30. This is essentially direct crystal reception with the load effect of the crystal neutralised by valve reaction. Using a frame aerial with an 18-in. side, 2LO can be received at comfortable intensity at a distance of over twelve miles. A large number of similar arrangements are possible, their effectiveness depending chiefly on the fineness and smoothness of the reaction control.

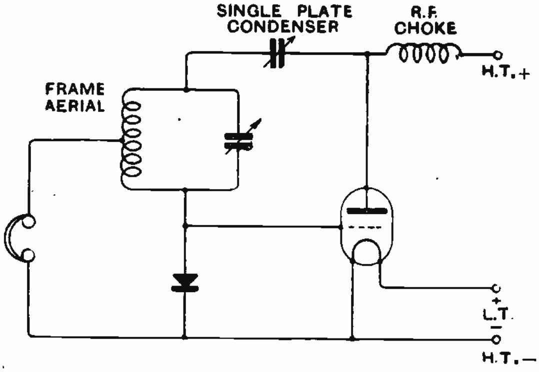

Fig. 30.

Finally, at the end of a somewhat lengthy paper, the writer is conscious of the fact that his treatment of the subject, though fairly comprehensive, is very incomplete This was almost inevitable, on account of the wide extent of the ground to be covered. He hopes, however, that he has succeeded in reducing the analytical side of the subject to manageable form, on the basis of which the fuller elucidation of particular cases can be carried out at some future time.

APPENDIX IV.

$$f(e) = a\epsilon^{be}$$
$$f'(e) = ab\epsilon^{be}$$
$$f''(e) = ab^2\epsilon^{be}, \text{ etc., etc.}$$

Therefore

$$F_1(E) = abG\,(bE/2). \quad \text{(See equation (7.8).)}$$
$$F_2(E) = ab^2G\,(bE/2)$$
$$F_3(E) = ab^3G\,(bE/2), \text{ etc., etc.,}$$

and from equation (13.10)

$$\frac{1}{R_c} = abG\,(bE/2)\left\{1 + b\,(v - v_0 - v_c) + \frac{b^2}{2}\,(v - v_0 - v_c) + \frac{b^3}{3!}\,(v - v_0 - v_c)\ \ldots \text{etc., etc., } \textit{ad inf.}\right\}$$

$$= ab\epsilon^{bv}\epsilon^{-b(v_0+v_c)}G\,(bE/2)$$

and since, from equation (7.7)

$$b(v_0+v_c)\epsilon^{b(v_0+v_c)} = abR\epsilon^{bv}G(bE/2)\,.$$

Therefore

$$\frac{1}{R_c} = \frac{b\,(v_0+v_c)}{R}$$

Standard Specifications for Vulcanised Fibre and Press Boards.

TOGETHER with the specification for ebonite recently issued, two other specifications* governing electrical insulating materials have recently been compiled by the British Engineering Standards Association which are of particular interest to the wireless industry. The preparation of the specifications has undoubtedly entailed considerable research work on the properties of insulating materials, and the values given, as well as details of the methods of testing, are based on the results of the researches carried out by the Electrical and Allied Industries Research Association.

The methods of testing have been devised for the guidance of all interested to secure, in the first place, the use of the best available methods of test in the development of improved material and, in the second place, uniformity of practice so that data obtained by different observers may be comparable.

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* B.E.S.A. Specification No. 216/1926. Vulcanised Fibre for Electrical Purposes. B.E.S.A. Specification No. 231/1925. Press Board for Electrical Purposes, obtainable from the British Engineering Standards Association, Publications Department, 28, Victoria Street, London, S.W.1. 1s. 2d. each post free.

The Piezo-Electric Effect and its Application to Wireless.

By C. W. Goyder (2SZ—2HM). [R351·218

The recent experiments of the writer are given in the following article, with a general outline of the subject. The field is new, and any other amateurs wishing to investigate it will find it most interesting and useful in its practical application.

THE piezo-electric effect was discovered in 1890 by the Curies, but it was of no practical use until very recently, when it was utilised for converting sound waves to electrical impulses (microphones), and conversely for changing electrical impulses into sound waves (loud-speakers). In 1922, Professor W. G. Cady, of the Wesleyan University, Connecticut, developed the theory of the use of the quartz crystal as a resonator, and applied the principle to frequency measurement. More recently G. W. Pierce and Dr. Hoyt-Taylor of America have extended and improved the practical application.

The Piezo-Electric Effect.

The principal substances exhibiting this property are Rochelle salts, cane sugar, tourmaline, zinc silicate and quartz.

Rochelle salts give the greatest piezo-electric response, but, unfortunately, it is difficult to work Rochelle salt crystals, and also, as they are affected by atmospheric conditions; they cannot be relied upon for constancy. For other than constant frequency work they are very suitable, as the crystals can be easily made by evaporating slowly a solution of Rochelle salts.

Quartz is the only piezo-electric substance used in radio work, as it is not appreciably affected by atmospheric conditions and is very strong physically. Its hardness is intermediate between that of glass and diamond.

It is interesting to find what difficulty there is in obtaining a suitable specimen of quartz when such large quantities of it exist. Over one-tenth of the earth's crust is made up of quartz. Silica is chemically identical with quartz, flint and sand are both modifications. When coloured by various impurities it occurs as amethyst, agate, jasper, etc.

The quartz crystal for radio work should be quite clear, without flaws or bubbles. The surfaces should be smooth. A crystal such as shown is a convenient size, from which several slices may be cut. The width is approximately 2 in., length 3 in. Unfortunately, it is not possible to tell by mere inspection whether the crystal will be suitable for radio work. It may be a twin crystal.

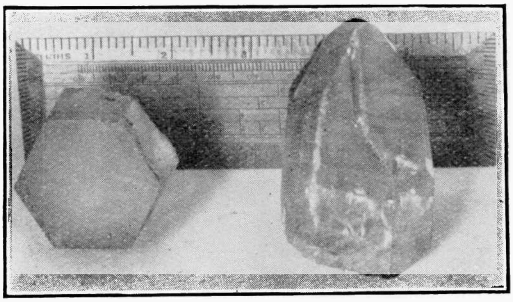

Fig. 1. *A natural quartz crystal (right) and (left) one cut perpendicular to the optical axis.*

Twin Crystals.

The optical property of a plate of quartz cut perpendicular to the optical axis is that it rotates the plane of polarisation of light. However, some pieces of quartz rotate the plane in a clockwise direction. By a "twin" crystal is meant one in which both types of quartz are present in one crystal. It can be seen that the mixture of the two kinds of quartz will diminish or annul the rotation effect. The only method to determine whether the crystal is pure or twin is to cut off the top and bottom of the crystal and polish the surfaces so that it is possible to see through. Then, if reflected light transmitted through the crystal is viewed through a Nicols prism, when the prism is rotated the light is completely cut off in one position if the crystal is pure. If it is twin, the interference between the two types of quartz crystal will allow light to pass through in some parts of the crystal and not in others, while the surface junction between the two forms shows up as a jagged outline resembling a large crack perpendicular to the optical axis. A pure crystal gives the greatest piezo-electric response and is essential for resonator work. For an oscillator it is not so necessary. Mention is made of this later.

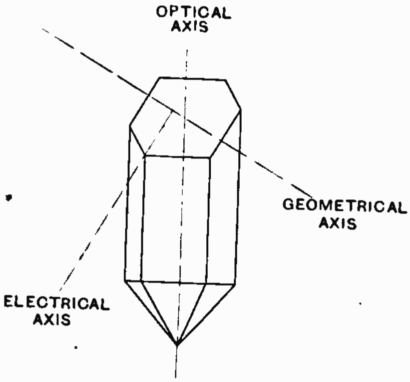

Fig. 2.

The axes of a quartz crystal are shown in Fig. 2. It is evident that there are three geometrical and electrical axes, therefore in choosing a slice of the crystal for a resonator or oscillator, use is made of the axis which allows the slice to be taken out at the greatest distance from any flaw or bubble. The position of a slice required in a crystal is shown in Fig. 4.

If the crystal is placed upon a flat surface, and a 30 degree set square is held against its face, the line of the set square will indicate the direction of the length of the crystal (Fig. 5). The thickness will, of course, depend upon the frequency required.

It has been stated that it is possible to cut out a slice with the back of a hacksaw blade and carborundum powder, but the author certainly does not recommend anyone to try it! Apart from physical considerations, the difficulty of keeping a saw blade at the correct angle for several hours should be sufficient argument.

When the slice has been cut out, it is comparatively simple to grind the crystal down to the required thickness, using fine carborundum powder, and testing frequently with a wavemeter until the required wavelength is reached.

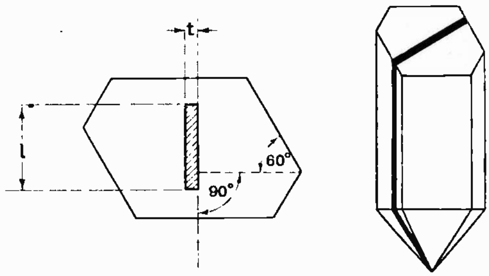

Fig. 4.

A slice of quartz crystal cut in the correct axis and approximately three-quarters of an inch square may be obtained from the J. W. King Lapidary Co., Ltd., 1, Albemarle Street, St. John's Square, London, E.C.1, for £1. The crystal is not guaranteed pure,

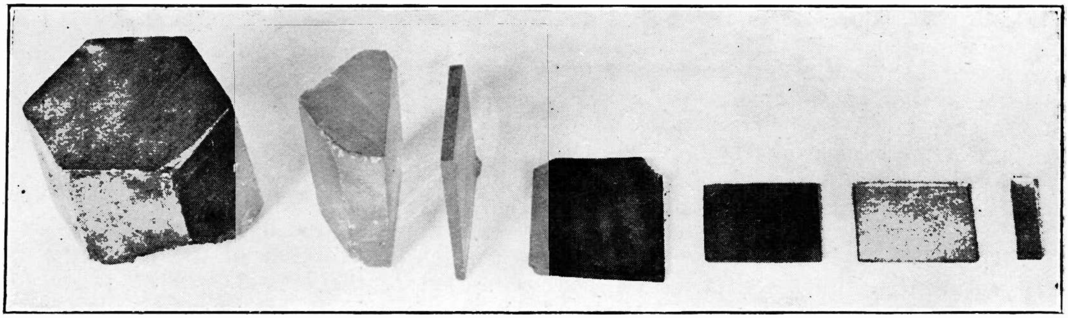

Fig. 3. *Showing how a slice is cut out and subsequently ground. The square crystals are for oscillator work and the small one is a resonator.*

or to have oscillating properties. The slice is about 1.5 millimetres thick. While these crystals are very useful for experiments using the quartz crystal as an oscillator, they are not usually suitable for resonators, due to the doubtful quality of the original quartz. The author took his chance with two of these crystals, and they both worked perfectly well as oscillators. One is at present in use in the crystal-controlled transmitter.

A crystal cut accurately and ground with the faces parallel to one or two hundred-thousandths of an inch and within 5 per cent. of a specified wave-length, may be obtained from Adam Hilger, Ltd., 24, Rochester Place, London, N.W.1. These are about half an inch in diameter, are guaranteed to have oscillating properties, and are specially suitable for crystal control transmitters. Small crystals for resonator work only, 2.5 centimetres long, 3 millimetres wide, and 1.5 millimetres thick in the electrical axis can be had for about 17s. 6d.

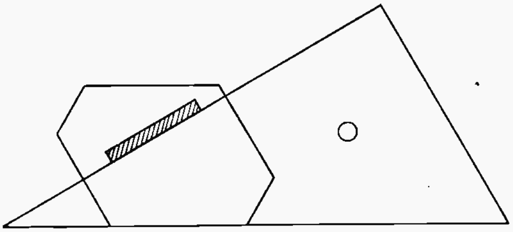

Fig. 5.

The Curies found that when a crystal of Rochelle salt was placed between two metallic plates and pressure was applied, an electrical charge was produced on the plates. They called this piezo-electricity (*piezo* from the Greek, meaning to press). In Fig. 6, force acting in either direction of the arrows will produce a charge on the plates. If the force is reversed so that it acts in a direction opposite to that of the arrows, the charges on the plates will be reversed also. Here we have a mechanical effect giving rise to an electrical effect. It might be expected that by reversing the conditions an electrical effect will give rise to a mechanical effect. This is found to be true.

If a potential difference is established between the plates, the crystal will shorten in the direction *AB* and lengthen in the direction *BC*.

Consider a slice of quartz cut from a crystal as shown in Fig. 2. It is a solid body having a certain degree of elasticity. Therefore, if pressure is applied it will be compressed. Potential energy is stored up in it which is tending to restore it to its original position. If the pressure is suddenly released, the slab will try to return to its original shape, but due to its inertia will swing past the position and oscillate until its energy is expended in an exactly similar manner to that in which a violin string, if moved from its normal position and released, will vibrate. The violin string vibrates at an audio frequency, but the quartz crystal vibrates at a radio frequency.

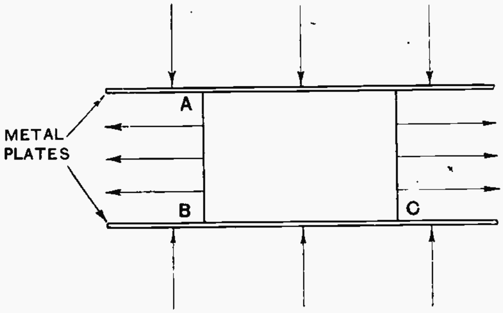

Fig. 6.

Therefore, if the slab of quartz is struck it will vibrate for a few moments at a definite frequency determined by its density and modulas of elasticity. But, due to the piezo-electric effect just mentioned, during the vibration (which consists of a compression and extension of the faces of the crystal), an alternating potential of a frequency, equal to that of the frequency of vibration of the crystal, will be produced on the faces. If we now maintain the mechanical vibration of the crystal we have a constant frequency alternating potential.

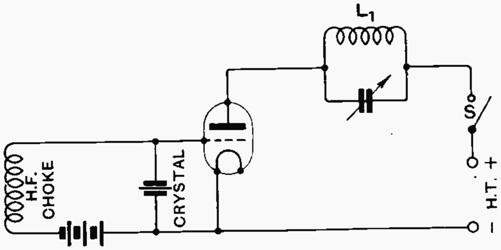

Fig. 7.

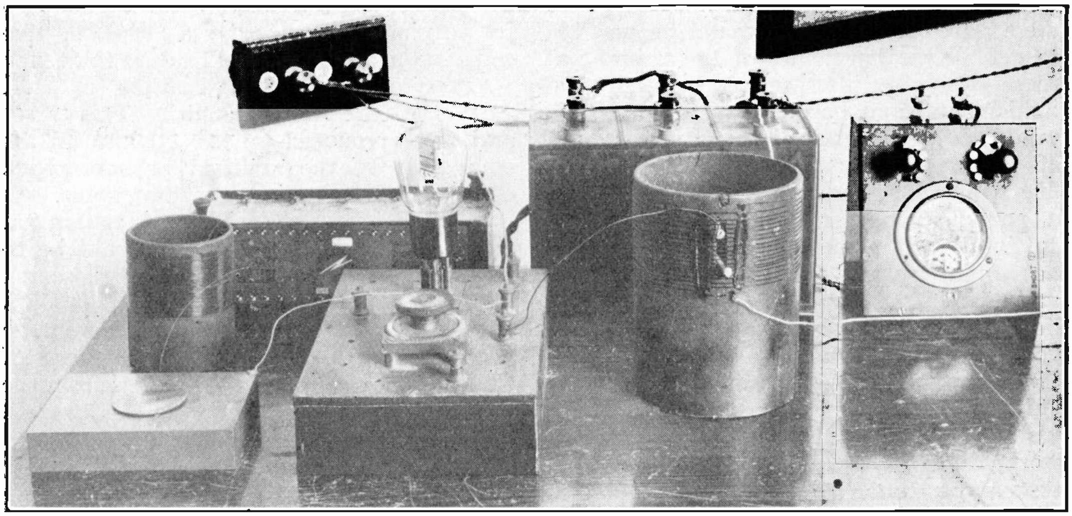

Fig. 8. *The crystal-controlled valve. The crystal is between the brass disc and steel block (left).*

Consider a quartz crystal in the circuit as shown in Fig. 7. If the switch *S* is closed, the first surge of current produces a potential difference across the inductance L_1. This P.D. is impressed on the crystal by the plate to grid capacity of the valve, and causes the crystal to contract. When the crystal expands, it, by virtue of its piezo-electric property, sets up a P.D. between the grid and filament of the valve, which in turn affects the plate circuit, and so the circuit oscillates. The frequency is dependent upon the mechanical vibration frequency of the crystal, which is inappreciably affected by the variation of anything external to the crystal itself.

For the short wave-lengths used in amateur radio, the frequency dependent upon the dimension "*t*," Fig. 4, is employed. The crystal will also oscillate at a frequency dependent upon its length and width, which is, of course, a much lower value. The constants of a quartz crystal are such that it will vibrate at a frequency corresponding to approximately 105 metres per millimetre thickness of crystal, but this is, however, quite an indefinite value. Mr. Underhill, of Adam Hilger, Ltd., who is in charge of the work there, has found crystals varying from 100 to 113 metres per millimetre thickness of crystal. The purer specimens apparently have the larger values. The crystal used by the author responded to a wave-length of 104 metres per millimetre thickness.

If a crystal 1 millimetre thick was connected in an ordinary regenerative circuit as shown in Fig. 9, a response would be expected at, say, 105 metres, and another at 52.5 metres, the second harmonic of the crystal. When the condenser is varied to pass through 105 metres, the expected click is heard when the set stops oscillating, due to the greatly increased damping at the resonant frequency. Similarly at 52.5 metres. But, with one crystal, contrary to expectations, over 100 smaller resonances were found between these two wave-lengths! Some were very weak, others quite strong, and in a small band of two metres 28 were found packed closely together. There was apparently no symmetrical relation between the strengths or the wave-lengths of the

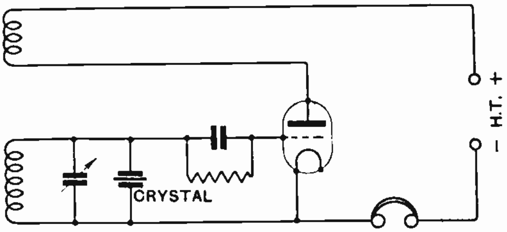

Fig. 9.

resonances. It is difficult to imagine where these all come from. It would be possible to get all the harmonics of the crystal and those due to interference between the fundamental and harmonics, etc., but this would not explain the unsymmetrical position of the many others. They are probably due to irregularities in the quartz, such as a partial twin formation, inaccuracies in cutting the slice from the correct axis, faces not quite parallel, or possibly a slight variation in density of the quartz. This is borne out by the fact that very pure and accurately cut specimens are almost devoid of these minor resonances.

It is obvious that a crystal such as that just mentioned would be no use for resonator work, due to the difficulty of finding true resonances, but for oscillating work it apparently is immaterial. This same crystal is now in use as the oscillator in the crystal-controlled transmitter. It oscillates at only one frequency, which may be assumed to be the fundamental, the others being too weak to produce oscillation.

Practical Application of the Quartz Crystal for Resonators.

It is very difficult to build a wavemetre which will maintain a reasonable degree of accuracy, due to the many factors which may change and vary the calibration, such as :—

(1) Variation in H.T. or L.T. voltage.

(2) Effect of damp and temperature variations on coils.

(3) Variation due to necessity of changing a burnt-out valve, or that due to sagging of the filament or other element.

(4) Slipping of dial on spindle.

(5) Variation in condenser capacitydue to inevitable warping of the plates.

Considering the effect of error (1)—The grid tuning condenser has the grid connected to one side, the filament to the other. This adds the inter-electrode capacity of the valve to that of the grid tuning condenser, and also the grid to filament resistance of the valve. Variations in the L.T. supply to the filament vary this resistance, which is in parallel with the capacity in the circuit. A resistance in parallel with a capacity increases the effective value of the capacity. Therefore variations in the internal resistance of the valve produced by varying L.T. supply vary the effective capacity in the circuit, and so the frequency is affected. Similarly, variations in H.T. vary the effective capacity. This error and those produced by (2), (3) and (4) are equivalent to the addition or subtraction of a small fixed capacity in the circuit.

The effect of this on a wavemeter calibration curve as shown in Fig. 10 (*a*) would be to cause a symmetrical movement of the curve to position (*b*) or (*c*).

What is now required is a fixed point on the wave-length scale not subject to any of these errors, so that any deviations from the original calibration curve may be detected. This is afforded by the quartz crystal.

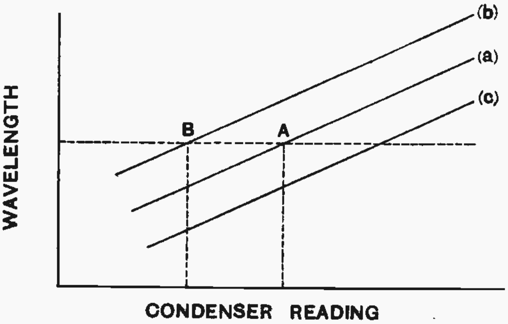

Fig. 10.

If a quartz crystal of known wave-length is included in the circuit, a resonance click will be obtained at the appropriate setting of the condenser, giving the wave-length, say at *A*. If now a reading is taken on the condenser of an unknown wave-length, before looking this up on the graph it should be seen whether the resonance click of the crystal corresponds with reading *A*. If it is found that, due to any of the first four errors, the resonance click occurs at *B*, then the difference in condenser reading represented by *AB* should be added to the condenser reading of the unknown wave-length to give the true condenser reading, from which the correct wave-length may be found.

The resonance click will always correspond to the true natural period of the quartz crystal, independent of the variations in the external circuit. Variations due to errors (1) to (4) will cause the response to occur at different condenser readings, but by inspection of Fig. 10 it is evident that if a certain wave-length is ten condenser divisions above the resonant point of the crystal in curve (*a*) it will still be ten degrees above in curve (*b*) or (*c*), even though errors due to (1), (2), (3) and (4) have been introduced.

The author has found it better to calibrate the wavemeter in terms of wave-lengths per condenser scale divisions on either side of the crystal resonance point. If this is done, errors due to the dial slipping, etc., do not come into question, as any wave-length is a definite number of scale divisions above or below the resonance point, immaterial of the initial position of the dial.

In the case of a particular wavemeter for the 44-46 metre band it was found that the heterodyne note of the wavemeter in the receiver or that of the transmitter in the wavemeter was so strong that a considerable error was introduced, due to the difficulty of getting an accurate zero beat position, and to the tendency of the oscillations to draw each other in step. In the end it was found better to calibrate the wavemeter from 88-92 metres and use the harmonic. In order to minimise the error due to the assumption that a small added capacity produces a symmetrical movement of the calibration curve, the minimum capacity in the circuit should be large. This was afforded by a .000 1 μF fixed condenser which was connected across the inductance. The inductance was arranged so that the minimum wave-length was 70 metres. The variable tuning condenser was connected in series with another fixed condenser so arranged that the variable condenser gave a variation from 70 to 92 metres, thus covering on harmonics the most used amateur band from 35-46, 18-23, and also 23-30.

The errors in a wavemeter of this type are very small. There is the 100 cycle error stated on the N.P.L. certificate, if the crystal is calibrated by them. An error of 100 cycles may be allowed in the measurement of the resonance click. Then, if there are 100 condenser scale divisions which may be read in tenths by means of a vernier, the scale may be read to 1 part in 2 000 by judging half a vernier division. This will give an accuracy of about one-third of 1 per cent. over any of these ranges. The accuracy may, of course, be increased by reducing the wave-length range. The main error is introduced by the difficulty of reading the dial.

In Fig. 11 is shown the connection of a wavemeter such as described above. There are several ways in which the crystal may be incorporated in the circuit, in parallel with condenser C_1 C_2 or C_3. In parallel with C_3 the crystal is across the whole coil. This causes it to give too great a response at the fundamental frequency. In parallel with C_1, the response is much weaker; just sufficient to give a well-defined click. The most convenient method was found to be to incorporate the crystal in the construction of the fixed condenser C_2. This condenser must be constructed so that the variable condenser C_4 has the required range. It may be made from two parallel copper plates, the size to be found by experiment, and the crystal may be fixed in between the plates. The response will vary in different wavemeters, due to the various capacities in this circuit, and also, to some extent, with the quality of the crystal. This is a matter for experiment. It is quite simple to change the position of the crystal to find the best position.

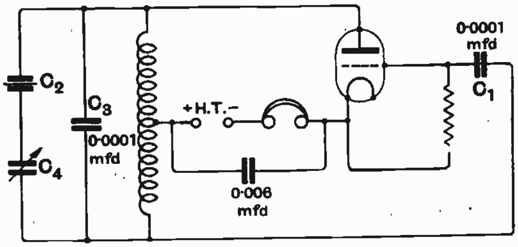

Fig. 11.

With regard to the mounting of crystals for resonator work. The crystal may be placed between two plates which actually touch both surfaces of the crystal; or a clearance of a few thousandths of an inch may be allowed. On the longer wave-lengths the latter method appears to give a greater response. On the short waves the former has been found by the writer to be better. It has the advantage that the crystal may be easily held in place by the weight of the plate or by means of a light spring. It is true that the frequency of the crystal resonance is changed slightly if it is mechanically constrained, but this error is quite negligible in comparison with the other errors present in the wavemeter. The mounting should be solidly built. When connecting the mounted crystal in the circuit it must be remembered that the parallel plates of the crystal holder act as a small condenser with the quartz crystal as dielectric, therefore removing the mounted crystal or the crystal from the holder will vary the capacity in the circuit.

Calibration of the Wavemeter.

There are two ways in which the wavemeter may be calibrated.

With the crystal in the circuit the wavemeter may be calibrated against a standard wavemeter. The resonant wave-length of the crystal may then be immediately determined from this calibration before there has been time for errors to be introduced, due to deterioration of the H.T., etc. A curve may now be made in terms of wavelength per condenser degrees on either side of the resonant point, as mentioned before, and this will be independent of the first four errors mentioned.

In the second method it is assumed that the resonant frequency of the crystal is known.

This gives one fixed point on the wavelength scale, but this is really no use unless the form of the calibration curve is known. If the curve is a straight line (*i.e.*, wavelength proportional to condenser reading), then the one fixed point allows the whole scale to be fixed. In almost every case this is not true. It then becomes necessary to calibrate the wavemeter roughly in order to obtain the form of the curve. It is usually symmetrical, but if the condenser is placed so that the moving plates come into the field of the inductance during some portion of their journey from maximum to minimum capacity, an irregular curve will result.

It would be possible to use the calibrated crystal as an oscillator and then to use the various harmonics as calibration points, but as several points are required in a range of ten or so metres the production and especially the fixing of the harmonics would entail considerable work and difficulty.

The error due to variations in condenser capacity, such as produced by warping of plates, loose bearings, etc., has not yet been considered. It is very difficult to get a suitable condenser for a wavemeter. After examining many types, the Igranic low loss was found to have the most sturdy construction. There is practically no chance of a loose bearing and the plates are unusually thick. The extremities of the movable plates are rather flexible, due to their large distance from any support. The principal error would be introduced by the movement of these ends. This was eliminated by soldering a strip of brass across the plates at the point most remote from the centre. This can be done without affecting the action of the condenser. The errors now introduced with age and use will be small. It seems impossible to correct them, as they produce no symmetrical variation on the calibration curve.

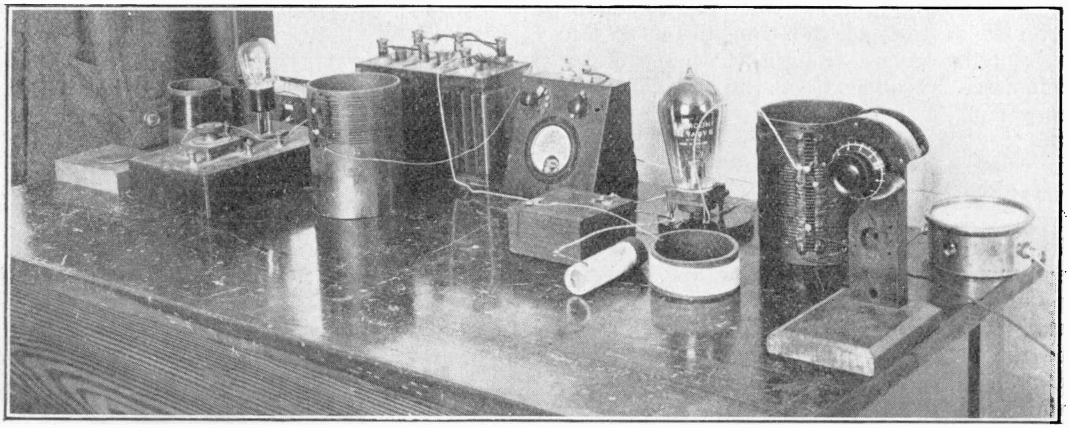

Fig. 12. *The crystal-controlled valve and first amplifier. The grid lead to the next amplifier is seen on the right.*

(*To be concluded.*)

Frequency Variations in Thermionic Generators.

[R344·2

Paper read by Lt.-Col. K. E. EDGEWORTH, D.S.O., M.C., before the Wireless Section, I.E.E., on 6th January, 1926.

Abstract.

THE paper is divided into six parts. In Part I. is considered the mechanical analogy of maintaining a pendulum in oscillation, with especial reference to the timing of the maintaining impulses, and the effect of the timing on the rate of the clock.

Part II. considers frequency variations (in a triode oscillator) due to changes in the anode resistance occurring as the result of changes of operating conditions. The author first considers the normal coupling resulting from electrode capacity, and then defines *normal* inductive coupling as that in which the coupling assists that due to the valve. The name *Orthodyne* is suggested to describe such a system. An opposite sense of inductive coupling is defined as *reversed* coupling and the name *Antidyne* is suggested. It is pointed out that oscillations may be generated by either normal or reversed coupling, the latter requiring a stronger coupling coefficient.

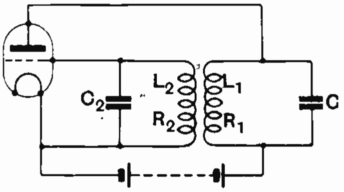

Fig. 2.

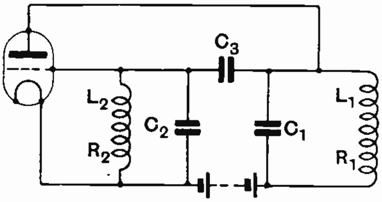

Fig. 4.

Resistance coupling between the grid and anode circuits is described as *Rheodyne*. The paper next considers inductive coupling with two tuned circuits (as in Fig. 2*) and its reduction to an equivalent network.

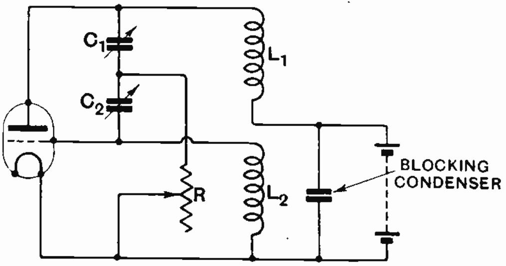

Fig. 5.

An expression is derived for the frequency. The capacity coupled system of Fig. 4 is then considered, with an equation for frequency.

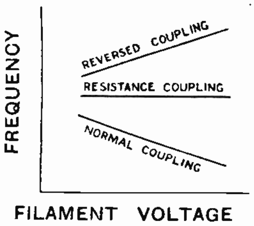

Fig. 6.

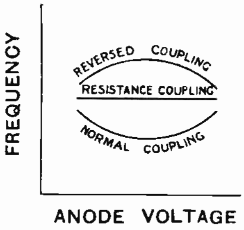

Fig. 7.

The resistance coupled circuit of Fig. 5 is then discussed, and it is pointed out that when $L_1C_1=L_2C_2$ the frequency is determined by the values of L and C, and is independent of the magnitude of the various resistances.

The effect of changes in the anode impedance (due to changes in L.T. or H.T. voltage) is then dealt with. From the expression for frequency, it is shown that an increase of anode resistance involves an increase in frequency, and Figs. 6 and 7 are given as showing the type of variation to be expected.

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* The author's original figure numbers are used throughout this abstract.

The words "normal" and "reversed" apply to inductive and capacitative couplings. Variations of this kind are described as "variations of the first type." The effect of changes in grid potential is discussed and it is concluded that under normal working conditions the damping due to grid current is usually smaller than that due to other causes, and can be kept down to a very low figure, *e.g.*, by keeping down grid current.

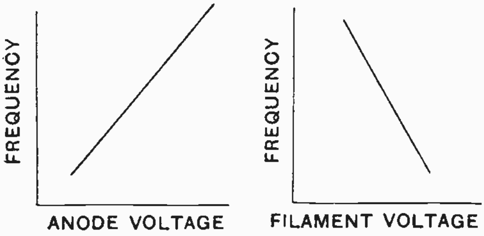

Fig. 8.

Part III. considers irreversible variations, such as do not depend on the nature of the coupling, but on the fact that the E.M.F. generated by the *valve itself* is not a true sine curve, approaching more or less closely to a succession of positive half cycles with the negative half cycles omitted.

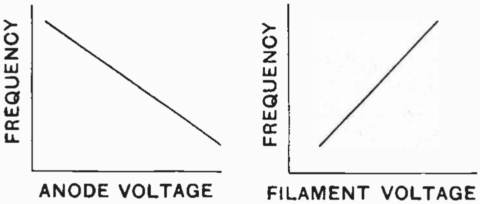

Fig. 9.

An oscillator with tuned grid and untuned anode is shown to undergo further variation due to this cause, of the form shown in Fig. 8, these being called variations of the second type.

In the case of tuned anode and untuned grid, the form is as shown in Fig. 9, these being described as variations of the third type. This type is always present with tuned anode circuits, whether the grid circuit is tuned or not.

A demonstration of many of the points in the author's remarks were given here. The effect of coupling variations, and the possibility of producing oscillations with reversed coupling, were illustrated. Changes of L.T. and H.T. voltage were shown to produce variations, and it was shown that these were less pronounced with small grid damping. Resistance coupling was substituted for inductive, and it was demonstrated that this method gave much less variation of frequency on varying the voltages.

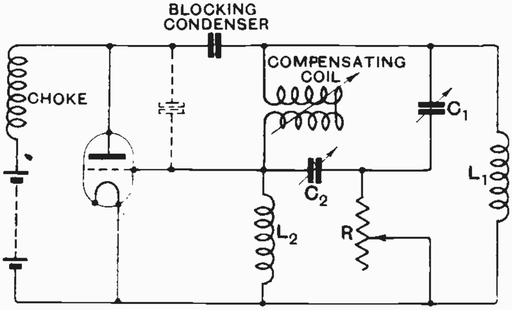

Fig. 10.

Part IV. then proceeds to discuss the design of a Rheodyne oscillator to cover a wide band of frequencies. Arrangements of resistance coupling (additional to that of Fig. 5) are shown in Figs. 10, 11 and 12.

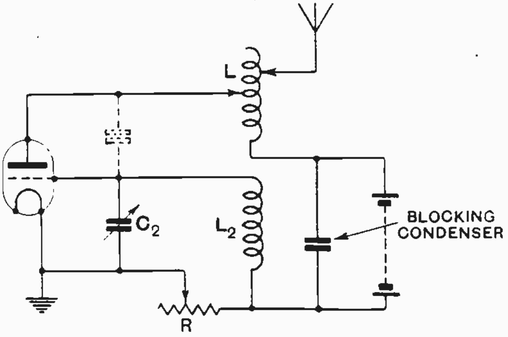

Fig. 11.

It is essential that the capacities of the two oscillatory circuits should be connected to one end of the resistance and the two inductances to the other. It is also necessary that coupling due to stray capacity should be neutralised. This may be done (1) by reversed inductive coupling as in Figs. 5 and 11; (2) by providing an inductive path between grid and anode as in Fig. 10; (3) By a neutrodyne method as in Fig. 12.

It is stated that the energy absorbed by the coupling resistance is approximately equal to that absorbed by the grid of the valve, and that the circuit generates oscillations of full amplitude and is applicable to transmitters, master oscillators and wavemeters.

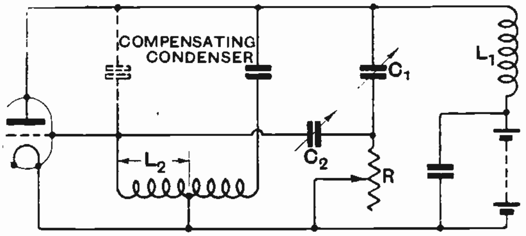

Fig. 12.

For efficiency it is necessary that (1) the grid potential should be approximately independent of frequency, and that (2) stray capacity should be correctly neutralised. Remarks follow on these points, the chief conclusions being (1) for a generator without anode tap, the same tuning method must be adopted for each circuit, while for generators with anode tap it is immaterial whether the grid circuit is tuned by condenser or variometer; (2) For generator tuned by variable condenser, it is desirable to work with small coils and large condensers, while the ratio of mutual inductance to anode coil inductance must be kept constant.

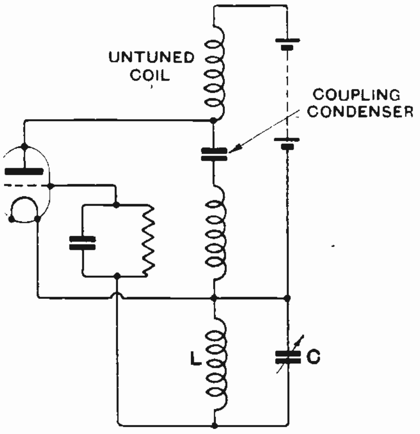

Fig. 13.

In Part V. is discussed the design of an Antidyne oscillator to cover a wide band of frequencies. This should make use of a combination of reversed capacity coupling and reversed inductive coupling. The reversed capacity coupling is most effective at the higher and the reversed inductive coupling at the lower frequencies. Figs. 13 and 14 are suggested as suitable for untuned anode and untuned grid respectively.

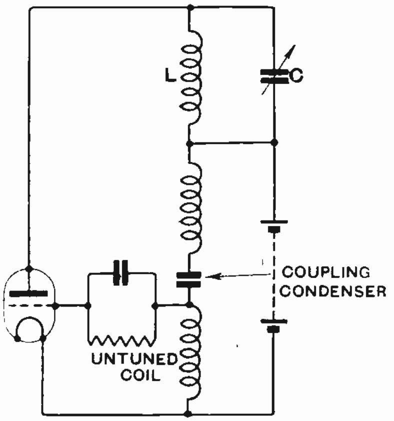

Fig. 14.

In Part VI. the author deals with experimental results and practical applications. With an oscillator of about 5×10^6 cycles, results are shown of the variations (cycles per second) against (1) anode voltage and against (2) filament voltage for various types of coupling, *i.e.*, normal, reversed, half normal, half resistance, resistance coupling, etc. The effects of grid damping are then shown in a further series of curves, where frequency variation is plotted as before for various values of grid leak and different types of oscillator, *i.e.*, Orthodyne, Antidyne, etc., with different arrangements of tuned and untuned circuits.

Lastly are considered practical applications of the results, these applications being chiefly wavemeters and transmitters, including master oscillators. Three solutions of the problem of a constant frequency generator are offered from the results; (1) Rheodyne; (2) Orthodyne, with grid bias and grid circuit untuned; (3) Antidyne, with grid bias. The relative merits of these are briefly discussed. Future possibilities are also briefly mentioned, including the possibility of a generator constant within the part in 50 000 for wave-lengths of the order of 30 metres.

An appendix deals with several of the mathematical expressions used in the text.

DISCUSSION.

The paper evoked considerable discussion which was opened by **Major A. G. Lee.** Major Lee referred to the tuning fork control as a satisfactory solution of frequency stabilisation, but thanked the author for having tackled the subject from a really fundamental basis. He considered that a physical interpretation of the circuits was helpful and illustrated his remarks more particularly with the Colpitt's circuit. He also suggested that the Dynatron circuit might be useful as a constant frequency system.

Mr. G. Shearing discussed some of the experimental results and the use of the Rheodyne as a master oscillator. He illustrated a resistance coupling method for applying a master oscillator to a power system, and referred to M. Fromy's circuit for constant frequency (see abstract in E.W. & W.E., December, 1925). Finally he took exception to some of the author's nomenclature.

Dr. R. L. Smith-Rose spoke on the subject of frequency variations in relation to interference, quoting observations made at the N.P.L. He also took exception to some of the nomenclature.

Mr. Warren criticised the author's mathematical treatment of inductive coupling, suggesting an alternative solution, which he illustrated with equations.

Mr. G. W. N. Cobbold referred to previous work on the subject by Eccles and Vincent, and suggested that apparent discrepancies between these and the results of the author were possibly due to differences of voltages according to the couplings used. He also discussed the change of frequency due to a change of valve.

Lt.-Col. H. P. T. Lefroy spoke of the importance of the subject, more especially from the point of view of small portable sets for Service purposes.

Prof. C. L. Fortescue dealt with the effect of the resistance in a resistance coupled system. He also discussed the mathematical expressions both of the author and of Mr. Warren.

Mr. Phillips spoke briefly on the practical difficulties involved in the apparatus for the 1 in 50 000 accuracy suggested by the author.

Col. Edgeworth briefly replied to the discussion, and a cordial vote of thanks to the author was accorded on the motion of the Chairman (Major Binyon).

Universal Testing Volt-Ammeter. [R260·251

THE home experimenter invariably feels the need for a testing instrument fitted with suitable interchangeable shunts and series resistances and calibrated so that the currents and potentials customarily met with can be determined with reasonable accuracy. A measuring instrument of the moving coil type can, of course, be easily fitted with shunt and series resistances to read over a wide range, but if accuracy is to be maintained, the resistances must be robustly built and a reliable form of construction adopted for attaching them to the terminals of the moving coil instrument.

Messrs. Fonteyn, Gilbert & Co., Ltd., 6, Blandford Mews, Baker Street, London, W.1, are marketing a universal continuous current meter provided with numerous terminals and shunts so that the scale, which carries three divisions, and is subdivided into tenths has maximum scale readings of 6 and 120 volts, 3, 30 and 120μA and with external shunts 1, 6 and 30 amperes.

Compared with a laboratory instrument, the scale reading can be taken as reliable within ± 5 per cent., which is a satisfactory degree of accuracy for general testing.

Rectifiers for High-Tension Supply.

By R. Mines, B.Sc. **[R355·5**

Part VI: Vacuum Arc Rectifiers

(*continued*).

XI.—The Use of an Independently-heated Cathode.

IT was hinted in Part IV. that independent means may be used for keeping the cathode in an arc hot enough for thermionic emission. Such a procedure adopted in the mercury arc rectifier, for example, would eliminate the difficulty of extinction of the arc and hence overcome its greatest disadvantages.

For working in a vacuum the only practicable way of producing heat at the cathode is by passing an independent electric current through it. The convenience with which this may be done is dependent upon the electrical resistivity of the cathode material as well as its thermal properties—in most cases it is necessary for the cathode to be shaped into filament form, the rise in temperature produced being uniformly distributed over the cross-section.

There are obvious difficulties in arranging a cathode of liquid mercury to fulfil these conditions. But apart from these the temperature limit renders the use of mercury impossible—for, under the conditions of the vacuum arc, mercury boils at about 70°C. (the working temperature of the mercury vapour arc), whereas a " yellow heat " at least is required for effective thermionic emission. However practicable then the attainment of such a temperature may be under the extremely localised " hot-spot " condition of the mercury arc, it is not possible to realise it with a filament of which the whole cross-section is heated.

XII.—Application to the Vacuum Arc.

If a heated filament is to be used as cathode in a " vacuum arc " it is necessary then that it shall be made of a more refractory material—this carries with it the disadvantage that there will be no " vapour of the electrode material " available to carry the discharge with a low ionisation potential, because of the temperature limit imposed on the discharge by the containing vessel.

In fact, the discharge ceases to be an *arc*, according to the definitions that we have given: it can be carried only by the residual gas in the tube.[1]

This modification, therefore, would appear at first sight to be a retrograde step in developing the utilisation of gas discharge phenomena for rectification. Actually there are some further apparently backward steps awaiting our consideration before we shall be in a position to discuss the latest improved apparatus.

XIII.—The " Metastable State."

It has been stated that normally the velocity (or rather the energy) of a bombarding electron must not be less than that corresponding to the *ionisation potential* of the atom it strikes. However, if it is less, but still does not fall short of the *resonance potential*, an internal change may take place in the atom, accompanied by absorption of the amount of energy corresponding to this potential. Further, an atom which has suffered such a change (it is said to be " excited ") tends to revert to its original state, the extra energy being rejected in the form of electro-magnetic radiation.

Recent experiment[3] reveals the fact that an atom may remain in the excited state for a definite average time—hence the term " metastable." In the case of mercury, for example, the average life in this state was found to be $\frac{1}{22}$ of a second.[4]

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[1] Incidentally it is quite feasible for experimental purposes at least to distil some mercury into the tube so that the " residual gas " is mercury vapour. Actually the later developments to be described prohibit the use of mercury vapour in this manner.

[2] See E.W. & W.E., Vol. 2, p. 844, Oct., 1925.

[3] By M. Marshall; see *Science Abstracts " A,"* No. 641, March 1925.

[4] This corresponds to the first resonance potential at just under 5 volts; mercury also has a second resonance potential (about 6 volts) occurring before the ionisation potential of 10.4 volts is reached: the life of this second metastable state was found to be 1/170 of a second.

XIV.—"Cumulative" Ionisation.

The useful result of this state of affairs is that if an atom in a metastable state is bombarded by a second electron before it has had time to revert to its normal state, the energy this electron need possess to complete ionisation of the atom, is only that corresponding to the difference of the ionisation and resonance potentials. The ionisation of the atom is therefore accomplished virtually in *two stages*, by two electrons neither of which possesses energy as much as corresponds to the ionisation potential. In the case of mercury, having two metastable states at energy levels lower than that of the ionised atom, ionisation may be accomplished in *three* stages.

It is at this point that we see a real advantage in an independently-heated cathode—with it the supply of electrons is under control, and with a good size filament intensely heated the stage may be reached where this "cumulative" ionisation can take place: for obviously it requires the bombarding electrons to be very much more numerous than suffices for an ordinary type of arc discharge. When this occurs, the discharge can be maintained with a very much smaller P.D. than was previously necessary.

XV.—The Correct Gas Pressure.

As we have said, the type of discharge under consideration is unlike the atmospheric arc, for example, in that it is not necessarily electrode vapour that takes part in the discharge by becoming ionised. Further, so far the use of an independently-heated cathode has been postulated as an essential condition; under this circumstance, the discharge may well be called an "*assisted arc*" (in contradistinction to a *self-maintaining* arc).

Nevertheless the mechanism of the discharge is similar. The positive ions bombard the cathode (now the filament) as before; and though the electron emission may be unaffected by it, such bombardment usually causes disintegration, and with an electrode of filament shape this is particularly disastrous.

One way of reducing the amount of bombardment is to reduce the gas pressure and hence the positive ion density. This method, however, is inadvisable partly because it involves a corresponding reduction in the current carried by the discharge.

Increasing the gas pressure conversely increases the ion density and so tends to augment the intensity of bombardment to which the cathode is subjected. On the other hand, the increased density of the gas reduces the mobility of the ions (by reducing their mean free path) and hence their destructiveness.

This reduction, however, is not a linear relation, and it has been shown as a result of painstaking investigations that by a proper adjustment of the gas pressure disintegration can be almost eliminated—it appears that the product of the average destructiveness of each ion and their total number, representing the total destructive effect on the cathode, passes through a minimum value as the gas pressure is varied. This optimum point occurs at rather less than $\frac{1}{10}$ of an atmosphere (in the case of argon).

XVI.—Choice of Working Gas.

A reason is now apparent why mercury vapour is unsuitable if the conditions described above are to be established. The pressure stated is about one thousand times that at which the mercury vapour arc operates; and at this higher pressure the boiling point of mercury, and hence the *minimum* operating temperature in the arc, is about 240°C.

Unfortunately most gases exert a deleterious effect on the electron emission from a hot cathode, some (for example, oxygen, either alone or in combination) almost inhibit it. Of the gases that have been tried in developing gas conduction rectifiers, *Argon* has proved the most successful, largely because it does not lower the emission from a tungsten filament. (Probably other gases from the "inert" group would answer; but argon is the most suitable).

XVII.—Purity Essential.

The effect of certain gases on thermionic emission is sufficient reason that every effort should be made in constructing an apparatus to operate on this principle that the working gas should be kept in as pure a state as possible—if necessary means of purification must be provided that shall remain in the apparatus after manufacture and account for any impurities that may appear during its operation.

In addition to the above effect there is the effect on the discharge itself to be guarded against. Unless the gas is pure it is not possible fully to realise the condition of low arc drop that ensues from the cumulative ionisation of atoms in the metastable state. But more important than this is that, with the filament hot, on application of the P.D. between anode and cathode a much higher value is necessary to *start* the "assisted arc" discharge than is required even to maintain it. This introduces an undesirable factor of erratic behaviour apart from increasing the power losses in the device. (See Fig. 1.)

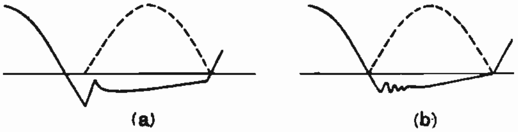


Fig. 1. *Oscillograms of "Tungar," rectifier on A.C. supply (a) without purifier, (b) containing an active purifying agent. The full line curve represents the P.D. across the arc and the dotted line the current.*

XVIII.—The "Tungar" Rectifier.

This rectifier is an apparatus in which the conditions whose development has been outlined above are realised as fully as is possible in a commercial production. Fig. 2 illustrates the construction. The filament is of plain tungsten (*i.e.*, the "bright emitter" type) and is made very thick, partly to withstand the residual ionic bombardment, but primarily to provide a sufficient thermionic emission. Connection is made to it through a standard screw lamp cap. The anode is a disc of graphite mounted in proximity to the filament.

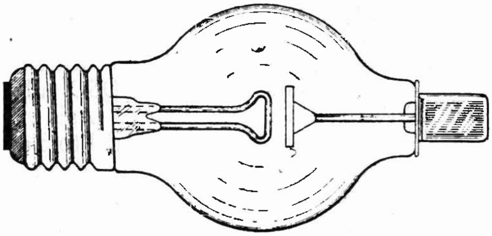


Fig. 2.
Arrangement of electrodes in the "Tungar" rectifier.

After assembly, the containing glass bulb is exhausted as completely as possible, the process being assisted by "baling out" the bulb and both the electrodes, just as for a high-vacuum device. When this is complete, argon is introduced into the bulb at exactly the correct pressure.

In addition to this careful exhaustion and filling the early models at least were provided with an active purifying agent, which was either attached to a filament lead or incorporated in the anode material. Thus when impurities became excessive, the arc drop increased, the electrodes both rose to a temperature higher than normal—this caused some of the purifying agent to be evaporated and so to do its work by combining with the unwanted gases. Magnesium is a good purifier, owing largely to its affinity for oxygen; it is the condensation of some such material as this that causes the mirror-like deposit on the inner surface of the glass of many of these rectifiers. (The same appearance is commonly found in "dull-emitter valves," and is due to a similar cause.)

XIX.—The Rectification Action.

The manner in which rectification is effected by the "Tungar" bulb is very similar to that of the arc, either atmospheric or under reduced pressure. There is one electrode maintained hot enough to supply negative ions that ionise the residual gas, while the other remains cool enough to be unable to supply them; and in the interval between successive conducting periods, that is while the applied alternating potential is reversed in direction, the gas de-ionises by recombination of the ions and so ceases to conduct.

In this connection it is appropriate to mention some decided advantages that the Tungar rectifier has over other forms, for example, the mercury vapour rectifier. Pure argon gas it is found shows an extremely rapid recombination of its ions on withdrawal of the stream of bombarding electrons, no matter how heavy the preceding discharge may have been. This factor makes for certainty in operation and reduces the risk of "flash-over."

Further, due to the small drop of potential in the arc, the heating of the electrodes by either positive or negative ions is relatively small. Thus, for a given construction and material for the anode, for example, the safe limit of current at which it will remain sufficiently cool is considerably higher.

XX.—Manner of Use.

This rectifier is always connected to the A.C. supply through a transformer, for two main reasons: First, the reactance of the windings (which, if necessary, may be increased above that ordinarily used in a transformer) acts as the stabiliser that is always necessary with a device not having a "rising characteristic" (see Fig. 3). Secondly, owing to the fact that the arc current is distributed over the length of the filament, it is necessary that it should be heated with A.C., to avoid unequal heating of the two ends and consequent shortening of its life.

When a rectifier is run near its full current rating it is found practicable to cut off the filament heating current, since the residual ionic bombardment is sufficient to maintain the temperature of the cathode. Under these circumstances the discharge is *self-maintaining* and is nearer to being a true arc. The "arc" usually concentrates on one part of the filament, making a "hot-spot" which moves towards the filament lead connected to the output circuit as the current is increased.

This localisation of the discharge becomes more pronounced at smaller current values, and this tends to shorten the life of the filament unduly. Further, the arc drop under the self-maintaining condition is much increased, and the larger power loss that results more than offsets the saving of filament heating power. Hence it is usually more economical to keep the filament excited continuously.

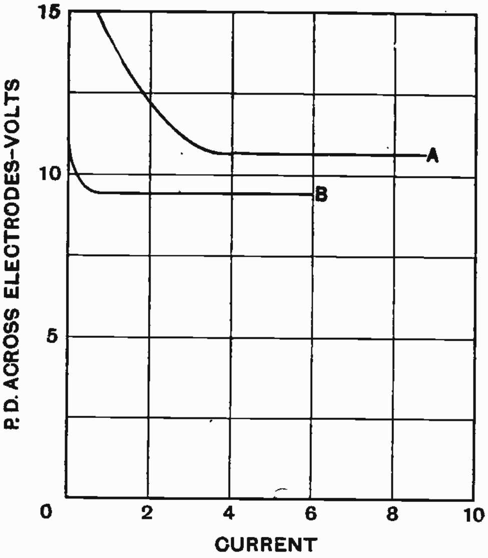


Fig. 3. *Characteristic curves of a "Tungar" rectifier showing relation between the anode current in amperes and the P.D. across the electrodes (A) without filament current and (B) with filament heated independently.*

Amateur DX in South America.

By Ch9TC (Los Andes, Chile). [R545·009·2

THINGS move so fast nowadays that my note in E.W. & W.E. of November (p. 930) already needs supplementing.

Argentine.—The station BA1 (Buenos Aires) has recently come very much to the fore, having worked England, Tahiti, New Zealand, Japan, and, of course, U.S.A., etc., with 20 watts nominal.

Uruguay.—New Zealand with 10 watts and 5 watts nominal, Chile with 2 watts actual; this country seems to specialise in low-power work.

Chile.—I am glad to say this country is well to the fore. Ch1EG (now temporarily closed) claims the world record for work with China on the 40-metre band, since signals must have gone "the other way round." 2LD has now been QSO all five continents, PI1HR representing Asia; two "five-watt" tubes (60 watts actual to plates). Some interesting work has been done with U.S.A. on receiving tubes U.V. 201a, about 2 watts actual, by 2RM and 2RE.

Ecuador.—There is, at any rate, one receiving station on short waves, at Santa Elena (QRA "c/o All America Cable Co.") since tests with the writer were made.

All above are "40-metre band," except 9TC, which is on 48 metres (but works little except Tuesdays and Fridays, 21.15 to 21.45 G.M.T., to RMA1, using call CLAA.)

Long-Distance Work.

By Hugh N. Ryan (5BV).

[R545·009·2

READERS will remember my promise to give, or at any rate to attempt to give, in this month's article, a summary of the observations made by those experimenters who are keeping records of signal strengths and weather conditions, over long periods, with a view to establishing some sort of a relation between them.

I have found, from the correspondence which I have received on the subject, a much greater degree of interest in these investigations to exist than I had thought. Quite a number of amateurs are keeping more or less systematic observations, and though I fear that the observations made by some are too random to be of any great value, a certain number have obtained a great deal of data, and three men in particular (including Mr. Lewer, whose work is already known to readers) have tackled the job in a very complete and systematic manner.

Results.

The results obtained by various observers conflict greatly on some points, but agree remarkably well in other (in fact in most) respects. This month I will just give the main points in the observations which have been brought to my notice, but before doing so I should like to make a few remarks of my own, which are suggested by the lines upon which experimenters are working.

I have heard many people say that DX conditions cannot possibly be affected by the weather, since, to quote one of them, "the ether is not dependent on the air." Other people, on the other hand, will point out that it is a matter of common experience among DX workers that an appreciable alteration in general signal strength is usually accompanied by a change, of some sort, in the weather. The point often overlooked, I think, by partisans of both sides, is that to assume a *connection* between two things is not necessarily to assume that one is *directly dependent* on the other; they may both be dependent on a third variable influence. If, therefore, observation tells us that changes in long-distance signal strength do coincide with changes in certain aspects of weather conditions (which I think is so) and other considerations tell us that the one cannot be influenced by the other (of which I am less sure), then, having established the connection beyond dispute, we shall have to assume that they are both under the control of some third influence (perhaps in this case an astronomical one).

Another, and similar, point arises from this. Many observers find that good and bad conditions recur in regular cycles. Some find that small cycles (of perhaps a few days) are superimposed on larger cycles (of perhaps a few weeks). Now it happens that some observers find that the most pronounced cycle has a length of about four weeks. The cycle of the moon's phases is of a similar length. Several have not expressed their results by saying that conditions recur in a four-week cycle, but have just said that conditions are controlled by the moon. I am not denying the *possibility* of this theory turning out to be correct, but I am pointing out that there is very little justification for it at the present stage, and that its adoption by an observer is not compatible with the open mind which he requires. The only possible test of it is to continue observations for a long time, and observe whether the "signal" and "lunar" cycles remain always in the same phase relation to each other, or whether they gradually drop out of step, as some of the curves I have seen rather seem to be doing. I have seen none which extend over more than three or four lunar months. If the curves do stay in step, then the protagonists of the theory will have a strong case.

Weather Conditions.

Most observers appear to be recording only the weather conditions at their own stations. While one appreciates the difficulty of obtaining data, over long periods, of conditions at the "other end" and in the intervening space, it does not appear likely that completely satisfactory results will be obtained until the observed as well as the observing stations keep weather records, and records of the intervening conditions are obtained. The last-mentioned records are actually being obtained and used by at least two observers, and I believe a third is arranging for observations at one distant station, but many are neglecting these points, even to the extent of plotting strength of Australian signals against barometric pressure in, say, London. The surprising thing is that their curves are extraordinarily good!

Mr. Lewer is continuing the observations which he recorded in this paper two months ago, and as his further work will probably form the subject of another article from his own very able pen in the near future, I will not detail it here. I may say, however, that his recent work is checking up very well indeed with his previous theory; so much so that he is apparently assuming the mantle of a DX-prophet, and telling the "gang" when it is worth attempting DX work. The oracle (to mix the metaphor a little) has spoken with some success up to the present.

The other two experimenters whom I mentioned as having obtained some very complete data indeed are Mr. Erith, of Sutton, and Mr. Charman (6CJ) of Bedford. Each of these gentlemen has sufficient results to make a complete and very interesting article, and I have suggested this to them. I will not, therefore, give their data here (in any case I have not the space), but will give an outline of the chief results derived from it. Readers will be able to note on what points they check up, and where they disagree. I may say that they are not in touch with each other, so the results are entirely independent, and the observations of each have extended over a period of several months, so that the element of conincidence is largely eliminated.

Mr. Erith's results, boiled down, yield mainly the fact that conditions go in cycles of six days (these cycles being, of course, superimposed on the more familiar yearly cycle, if not on others). He has given me a graph showing the "goodness" of DX reception (drawn to scale), barometric height, and temperature. He has drawn an ordinate every six days, and certainly this ordinate coincides in nearly all cases with a peak in the reception curve, there being no intermediate peaks of any size. In the few cases where the reception peak does not appear, either the temperature curve is very much above or below the average, or the barometric curve shows pressure to be very unstable, and these phenomena also do not appear anywhere in the normal part of the reception curve. Altogether, there is sufficient evidence to show clearly that some sort of connection exists, though any theory formed from these results alone will have to be modified before it will entirely fit all existing facts. The "excess temperature" idea was suggested by Mr. Lewer, but I think that while Mr. Erith's curves tend to confirm it so far, Mr. Erith himself distrusts it, and is well on the way towards getting a theory into which temperature, as such, does not enter, since it is itself bound up with pressure variations.

Graphical Records.

Mr. Charman's observations are mainly incorporated in a set of graphs which he has given me. These are very complete indeed, and I hope he will publish them and their future extensions. They cover a period of four months, but I think they will have to be carried on a good deal longer before any definite theory can be formed from them. The results for the four months, however, are extremely interesting. They comprise the usual barometric curve, several curves for the strength of a number of individual Australian and New Zealand stations in regular operation, a curve representing the strength of North American signals in general, and a very neatly made "composite curve" of two New Zealand stations whose signals have always been found to be of similar strengths, but who are not both always in operation. His own main conclusion from these observations is that a low, steady barometer is favourable for DX. He will forgive me if I suggest that he is prejudiced in favour of a low barometer as a "DX-bringer," perhaps from previous observations, and tends to read this result into his curves; the curves do not seem, to me at any rate, to justify this, though they do suggest it in places. His other point, however, that of the barometer being steady, seems to me to be the one definite fact which emerges both from his curves and from all others. While they disagree on some points, every one of them shows that when the barometer is jumping up and down, then signal strength becomes very erratic or falls off altogether. This alone is sufficient justification for continuing observations on these lines, and perhaps curves covering longer periods will yield more consistent results on other points.

2BOW (Cheshire) sends me a graph, extending over some four months, having one curve showing strengths of North American signals, and another of signals from South of the Equator (South Americans, A's and Z's). These are plotted to a time base, on which the phases of the moon are noted, the deduction suggested being evidently that the two are related. I have already mentioned the "moon" theory near the beginning of this article, so I will only mention here what is to be seen from this graph. North American signals do undoubtedly recur in it almost exactly in step with the moon's phases, best reception occurring at full moon and worst at new moon. Signals from South America and the Antipodes do not follow the moon's phases quite so well (as, I must admit, one would expect if the lunar influence theory is correct). There are quite definite peaks in the reception curve about each last quarter, but otherwise there is little relation.

General Reports.

I have no other reports of systematic work over long periods, but a few other well-known operators have made more than casual observations, and formed opinions in their own minds. I will quote a few of these, and would point out that though they are not to be looked upon as systematic research they all come from well-known experimenters, who do not form opinions at random, and who spend so much time in successful DX work that their observations are likely to be reliable.

5QV says he has noticed that a bright moon and a clear sky usually means a good DX night. This checks up with 2BOW's lunar observations as far as U.S.A. signals are concerned. The "clear sky" part may alternatively be expressed as "absence of cloud," and thus agrees with a number of letters I have received, whose writers give as their chief observation that the appearance of heavy clouds will spoil conditions on a promising night. This is, of course, mainly a local effect and is rather to be expected. 5QV also finds that a high, steady barometer is favourable. (Compare this with Mr. Charman's "low, steady barometer," and note that while they disagree absolutely on the question of low or high, which I mentioned as the weak point in Mr. Charman's interpretation of his own curves, they agree that steadiness of pressure is favourable, which seemed to me to be the chief observation to be made from the curves).

It will be remembered that, according to Mr. Lewer, good or bad conditions affect all wave-lengths simultaneously. This is very important if it is true, as it would mean abandoning the idea of trying to change wave-length from time to time, to "get round" bad periods. It is, therefore, interesting to note what other observers find. Messrs. Studley, of Harrow, who are very consistent DX listeners, consider that wet weather favours 90-metre work, dry cold weather favours 45-metre work, and weather has no effect on 20 metres. My own recent observations, such as they are, tend to confirm this.

It will be noticed that I have neglected two important effects, atmospherics and "sunset and sunrise bands." The first is omitted because I have as yet very little data on it. Such as I have seems to have no bearing on any other effect, and the several sources of data conflict greatly with each other. I hope to deal with it in the future. The question of sunset and sunrise I have neglected because nothing new has arisen beyond what is already well known. Briefly, as sunset or sunrise passes over the transmitting or receiving station a disturbance of signal strength takes place, usually taking the form of a considerable increase preceded and followed by a reduction below normal. In cases where sunrise passes one station at the same time as sunset passes the other, very "freak" results are to be expected, and do occur. An example of this is the recent very fine reception of Japanese 1PP's speech and music all over Britain.

I have received many other reports on these investigations, but they have either just confirmed points in those I have mentioned, or else covered such short periods or been so random as not to be of very great value by themselves. I hope those concerned will therefore forgive me for not including them in an article which has already become too long.

"DX" Reports.

The weather "effect" part of the article having taken up so much space this month, I fear the DX report will have to be very short. I will include, therefore, the more important stations only, again with apologies to those omitted.

2SZ has now a crystal-controlled transmitter in operation on 45 metres. This is believed to be the first successful crystal-controlled set in Europe. and is giving very fine results. It has been used for work with all parts of the world, and all stations report it as giving an exceptionally good signal, with a perfect note. A Canadian "Five" says it is the first British signal ever heard in his part of the country.

5QV has reached the end of his three-month schedule with C4GT, with the object of establishing touch with the Canadian fourth district. Unfortunately they had no luck whatever, the nearest they ever got to success being one morning when G2SZ heard C4GT calling 5ZV and took a message for him, though he could not raise the Canadian. This is interesting as 4GT is a good station who can work New Zealand any morning.

6LJ has worked 8QQ (Indo-China) and been heard near Calcutta.

2GO has been heard in India, with 12 watts input.

6QB has at last got his little generator going, and is reported as QSA all over Europe, but still cannot reach America with his small power. No doubt he soon will.

6VP has now changed his note to pure D.C., and though rather weaker, his signals are reaching further. He gets about 1 000 miles with 10 watts.

2BOW reports reception of two American Canal Zone stations, and of Australian 3KB, who was only using 30 watts and had been in operation less than a week.

2MA and his near neighbour 2OF are both doing excellent European work on low power, the former using a Hertz aerial.

6CI, of Coventry, has been in reliable contact over 80 miles in daylight, using 45 volts on a receiving valve.

A new station, 5WV, has started up in Essex with 6 watts, and has already covered most of Europe.

5OC (South Wales) is now on 45 metres, and worked an Australian the first night he used that wave-length.

5NJ (Ulster) now only works at week-ends, but has worked South Africa and South America, and is still in frequent contact with Australia and New Zealand.

Messrs. O'Dwyer, of Dublin, send me a very good log or reception from most parts of the world, including Japan and Hawaii.

An interesting report of low-power work comes from Australian 5BG, who has worked seven U.S.A. stations, with his inputs varying from 5½ to 7½ watts, on 35 metres.

Spain is now QSO New Zealand, EAR1 and EAR21 having worked Z2AC.

Three of the leading Belgians, P2, B7 and S2, have been out of action with blown tubes or condensers. B4YZ and B4RS are in regular touch with America. While BS4 was transmitting recently, the River Sambre suddenly rose in flood, and S4 was nearly drowned in his station. He says he thought of calling SOS, which would, I think, have been a unique event in amateur history !

Patent Infringement and Experimental Use.

By a Patent Agent. **[R347**

LETTERS Patent for invention confer upon the patentee, *i.e.*, the owner whose name is for the time being entered on the Register of Patents, the sole and exclusive right to make, use and sell under the patent. The patentee may share this monopoly with others in many ways. For example, he may grant licences to others to make, use or sell or to do all these things. Again he may merely manufacture and sell and a straightforward sale without restrictions is an implied licence to use. The manufacture, sale and use of a patented article, or process without the patentee's permission, however, constitutes infringement and is actionable at law. Manufacture of a single article for personal use undoubtedly constitutes infringement just as much as does manufacture in quantities for sale. It is a mistake therefore for manufacturers to imagine, as is too frequently the case, that if they buy a patented machine they are entitled to build and use replicas in their own factory. In a successful action for infringement the court grants the patentee an injunction to restrain the infringers from committing further infringement together, in the absence of peculiar circumstances, with damages. These damages are based not so much on the profit which the infringers have made but the damage which their action has done to the patentee.

To the foregoing there is, however, one notable exception. Use by way of bona fide experiment is no infringement. To quote Frost: "It is no actionable invasion of a patentee's rights for another person to use the invention, and thereby produce the finished product by way of bona fide experiment or amusement, without the intention of selling or making use of the thing so made for the purpose for which the patent was granted, but with the view merely of improving upon the invention the subject of the patent, or with the view of seeing whether an improvement can be made." The term "amusement" needs some qualification. One might build a wireless set for the enjoyment of building it, but assuming the set to be in accordance with an existing patent then the use of the set when once built for receiving broadcast programmes would unquestionably constitute infringement. The reasoning is simple. In building the set no damage or injury was done to the patentee and the set, being built merely for the fun of building it, there was no intention of selling the set and therefore no intent to evade the patent. If the set is used however, probably for amusement, the patentee's interests are injured. That is to say he has the sole right to supply sets in accordance with his patent and would presumably have made a profit on the sale. To build a set in accordance with a patent and satisfy oneself that it works does not constitute infringement, but it is the use after this satisfaction has been obtained that is an infringing action. If after the set has been made one experiments with it for the purpose of trying to improve upon it then again there is no infringement. The experiments may conceivably be quite exhaustive and extend over an appreciable period and the behaviour of different apparatus may be tested in conjunction with the set provided that the experimental work has a direct bearing upon the set itself. To build a set in accordance with a patent, however, and then use the set for testing the qualities or properties of apparatus without relation to the set cannot be regarded as experimental use under this heading. As an example, the properties of loud-speakers might be tested with a set built in accordance with an existing patent. Assuming these tests could be carried out with practically any other type of set then obviously there is no experimental use in so far as the set is concerned and the use of the set for this purpose must be regarded as infringement.

Experimental use is a term which in common with many others is inclined to have somewhat different meanings applied and a case, decided in 1885, is worthy of note. An English electrician purchased and imported certain articles made abroad in accordance with a British patent. In an action for infringement brought by the patentees the electrician defended his action on the grounds that the articles were bought for the purpose of experiment and examination by himself and his pupils. The articles were never sold or used for any other purpose, and it was submitted that the articles produced in this country under the patent were too costly to be used for taking to pieces. The Court, however, held that the use complained of was infringement, and an injunction was granted restraining the continuance of it.

In another case, decided in 1889, a number of infringing machines were purchased on the understanding that if they were unsatisfactory or unsuccessful they were not to be paid for. These machines were installed by the purchasers in their factory and were used for a few months after which use was discontinued. In a subsequent action for infringement the Vice-Chancellor of the Court of the County Palatine of Lancaster held that such use was not experimental and granted an injunction. The case was taken to appeal when the injunction was dissolved, the ground being that even though infringement were proved the defenders were not manufacturers but only users, and the use complained of had not only been discontinued an appreciable time, but further, there was no evidence of further use or intention to continue in the act complained of.

Concerning the question of manufacture without sale, it is to be noted that this may constitute infringement. Two precedents are of interest. Firstly, if a retailer exposes for sale certain infringing articles but effects no sale, then there is no infringement. If the retailer was unaware at the time of purchasing the articles that they were infringing articles he would under no circumstances be liable provided no sale was effected, although innocence after sale is no excuse. To be a party to the manufacture, however, renders one liable, and in a case tried in 1860 infringement was held to have been committed when a defendant had manufactured articles and his traveller had offered them for sale, although none had been actually sold. A decision of the House of Lords in 1900 should, however, be considered in connection with this latter case.

Concluding, it must of course be recognised that each case is one of fact and must be treated on its merits, but in the words of Jessel M.R., "patent rights were never granted to prevent persons of ingenuity exercising their talents in a fair way. But, if there be neither using nor vending of the invention for profit, the mere making for the purpose of experiment, and not for a fraudulent purpose, ought not to be considered within the meaning of the prohibition, and, if it were, it is certainly not the subject for an injunction."

[NOTE.—The Editors do not hold themselves responsible for the views expressed above.]

The Past, Present and Future Developments of Wireless Telephony. [R550

A Lecture by CAPT. P. P. ECKERSLEY, at an Ordinary Meeting of the I.E.E., on 7th January, 1926.

THE lecture was on more or less popular lines, and was delivered in the brisk and fluent manner which broadcast listeners have come to associate with the B.B.C.'s Chief Engineer.

Capt. Eckersley first reviewed the general principles and methods of wireless communication, and of the transmission and propagation of waves, including the effects of the Heaviside layer. He then discussed methods of modulation, illustrating the well-known choke control system, as well as absorption modulation and grid modulation. Next he reviewed the work and technical policy of the B.B.C., expressing himself as an unrepentant believer in the principle of providing transmissions capable of crystal reception by a maximum of the population. Dealing with the High Power Station, indeed, he suggested 100 per cent. as the desirable goal.

The lecturer then turned to the matter of international interference, pointing out the possibility of mutual heterodyning even though the modulation of the interfering station was not received. The allocation of wave-lengths and power was then discussed on an international, or at least Western European, basis. The matter of 200-600 metres on an "exclusive" and "non-exclusive" division was dealt with, and experiments were described with Bournemouth and London working on precisely the same wave-length. The Piezo-electric crystal oscillator was suggested as a drive control for the great accuracy required if two—even low-powered—stations were working on the same wave-length.

Lastly, the lecturer discussed the matter of quality in both transmitter and receiver, dealing with the microphone, the loud speaker and coupling transformers. The latter point was illustrated by slides of curves showing the flat output of the transformers used by the B.B.C.

A discussion followed, opened by Prof. C. L. Fortescue, and the lecturer was cordially thanked.

I.E.E. Wireless Section.

An Announcement.

[R060

THE Committee of the Wireless Section of the Institution of Electrical Engineers has further carefully considered the proposal for a New Institute of Wireless Engineers and is definitely of the opinion that the interests of qualified professional wireless engineers are best served by the Institution of Electrical Engineers. The Wireless Section of the Institution has already deprecated the formation of a new Institute of Wireless Engineers, and feels confident that it is unnecessary, and will not be supported by representative and qualified professional wireless engineers.

The Institution of Electrical Engineers has already explained that an engineer with adequate wireless qualifications can become a Corporate Member of the Institution, and that other wireless engineers not reaching that standard are eligible as Graduates and as such can attend all meetings of the Wireless Section as well as those of the Institution.

The Committee has taken into consideration the suggestions arising out of the previous correspondence on this subject which appeared in the Press, and with a view to improving and extending the activities of the Wireless Section, and making it more definitely representative of professional wireless engineers, the Committee submitted the following recommendations which have been approved by the Council of the Institution:—

(1) While it is essential that the standard of qualifications for membership of the Institution should be maintained, more opportunity is to be afforded to the physicist engaged in wireless work to become a member of the Institution.

Applications for membership of the Institution based upon the usual general scientific training and wireless professional qualifications to be referred by the Secretary to a Wireless Section Membership Sub-Committee which will make reports and recommendations for the guidance of the Membership Committee of the Council.

(2) The qualifications for membership of the Wireless Section to remain as at present, viz.: "That he is a member of the Institution and is actively engaged in the study, design, manufacture, or operation of Wireless or High Frequency Engineering Apparatus," and the Wireless Section Membership Sub-Committee to scrutinise all new applications for membership of the Wireless Section and decide who shall be admitted to it.

The Sub-Committee to be authorised to call for full particulars as to the nature of the study undertaken by an applicant or for particulars of his work in design, manufacture, or operation in order to satisfy themselves that the applicant is properly qualified in wireless engineering.

(3) The fact to be emphasised and more widely published that the meetings of the Wireless Section are open to all members of the Institution.

(4) The Wireless Section Committee to get into direct touch with the Local Centre Committees for the purpose of ascertaining the possibility of:—

(*a*) Starting Local Wireless Sections.

(*b*) Stimulating efforts to produce local Wireless papers.

(*c*) Suggesting the reading of available suitable papers, or giving of lectures at Local Centres.

(5) Each Local Wireless Section, when properly constituted, to be entitled to elect or nominate one Wireless member to the Wireless Section Committee. For this purpose, a Local Wireless Section shall consist of at least 15 members, who must already be members of the main Wireless Section.

(6) The papers and the discussions of the Wireless Section and other Wireless papers, in addition to appearing in the Journal of the Institution, to be issued separately in the form of "Proceedings of the Wireless Section."

(7) The Chairman of the Wireless Committee to be an ex-officio member of Council, in the same way as are the Chairman of Local Centres, as soon as the necessary alterations to the Bye-Laws can be made, but in the meantime he will be invited to attend all meetings of the Council.

Radio Society of Great Britain.

A Review of Last Year's Activities.

[R060

A HIGHLY satisfactory report of the work carried out in 1925 was read at the Annual General Meeting of the Radio Society of Great Britain, held at the Institution of Electrical Engineers on Wednesday, 16th December, under the Chairmanship of Brig.-Gen. Sir Capel Holden, K.C.B., F.R.S.

This review of the work of the Society constituted the Annual Report of the Council for the year ended 30th September, 1925.

Early in 1925 (said the Report) the Society was called upon to render valuable service in connection with the "Wireless Telegraphy and Signalling Bill, 1925," which then came before Parliament. The Society prepared a memorandum upon the clauses of the Bill incorporating proposed amendments and additions which the Council considered would be in the interests of the membership. The ultimate withdrawal of the Bill was, it is considered, largely due to the Society's efforts.

Ample evidence has been forthcoming from all parts of the country showing that the Society continues to be recognised as the most important national organisation in the amateur movement. It is also interesting to note that Societies in Africa, Belgium, France, India and Malta are now affiliated with the parent Society.

In December, 1924, arrangements were completed whereby EXPERIMENTAL WIRELESS became the official organ of the Society.

The fact that a standard specification of ebonite is now available is consequent upon the efforts of the Society in co-operation with the British Engineering Standards Association. This important achievement is being followed up by proposals for the standardisation of wireless components and apparatus.

The decision to adopt a distinctive emblem had been put into effect, and badges are now available to members.

With the formation of the International Amateur Radio Union the Society received further distinction, three of its members being appointed officers of the Union. In connection with the formation of the British Section of the Union, the Society had the honour, in April last, of entertaining Mr. Hiram P. Maxim, President of the American Radio Relay League, and several American colleagues.

The social side of the Society's activities was not neglected, and both an Annual Dinner in London and an Annual Excursion were organised. The latter took the form of a visit to the Marconi Wireless Transmitting Centre at Ongar, Essex.

The formation of a Technical Advisory Committee was also recorded in the Report. The assistance of this Committee is now available to all members desirous of help.

During the session six important lectures were delivered, the speakers being Sir Oliver Lodge, D.Sc., F.R.S., Prof. C. L. Fortescue, Mr. P. K. Turner, Mr. G. G. Blake, M.I.E.E., and Mr. F. M. Colebrook, B.Sc. In addition a number of profitable informal meetings were held.

A certain amount of difficulty was experienced in securing a sufficient number of lecturers to visit the affiliated societies, despite the fact that nearly fifty gentlemen volunteered their services. It was hoped that during the current year more members would be able to undertake this duty occasionally. In the meantime the Society thanked all those gentlemen who had so generously given their assistance.

The advantages accruing from the consolidated opinion of all the affiliated societies was shown at the meetings of the General Committee of the affiliated societies. As a testimony to the value of the considered opinion of the Society it is interesting to note that when the Imperial Communications Committee proposed the drafting of technical wireless regulations the Society was invited to send a representative.

Thanks to the courtesy of the B.B.C., fortnightly talks on behalf of the Society were given regularly from the London Broadcasting Station.

It was reported that a scheme for the registration and certification of wireless dealers was now under consideration, but that the Society recognised the inherent difficulties in working such a scheme and very careful investigations were being made.

Membership in all branches showed a decided growth during the year, notably in the Transmitter and Relay Section.

The extension of the " T. and R. Section " was remarkable. There were now members in Africa, Spain, France, Italy, America, Canada, and India, with the result that much useful co-operation had been possible in the direction of experimental tests. Informal meetings held at regular intervals proved a steady attraction, and while the lectures were mainly devoted to transmission questions, the receiving side had not been entirely neglected.

In July the important decision was made to issue a monthly publication known as *The T. and R. Bulletin* devoted entirely to the interests of transmitting amateurs. This publication, which is circulated only among members of the Section, had been well received, and was responsible for a large influx of members.

The Society's report concluded with a reference to the Schools Radio Section, which had also shown good work in the past year, notably in the organisation of a Schools Radio Exhibition in the summer, and in collaborating with the Education Department of the British Broadcasting Company.

The Treasurer's Report, which was also read at the meeting, showed a satisfactory state of affairs.

Among the interesting points raised was the expenditure on postage, which revealed an increase of £23 4s. 0½d. over the previous year. This clearly demonstrated that the Society's correspondence had expanded considerably.

The accounts showed that a satisfactory balance was carried forward.

Correspondence.

Letters of interest to experimenters are always welcome. In publishing such communications the Editors do not necessarily endorse any technical or general statements which they may contain.

Howling in Short-Wave Receivers.

The Editor, E.W. & W.E.

SIR,—I notice that one of your recent contributors has experienced the difficulty, when working on waves below about 50 metres, of the set howling when tuning down to the silent point just prior to the cessation of oscillation. He suggests connecting the grid-leak to L.T. I find that apparently this can be cured completely by shorting the L.F. transformer secondary by a grid-leak (the higher the effective resistance the better). It would therefore appear that this is a trouble caused by some sort of interference by the oscillating detector valve with the low frequency circuit.

C. R. BETTS.

Sandy Knoll,
Radlett, Herts.

The Beverage Aerial.

The Editor, E.W. & W.E.

SIR,—I am sure I am not the only wireless amateur to regret the lack of real, useful information on the Beverage aerial. What I have been able to gather is the following :—

The Beverage aerial, the invention of Mr. H. H. Beverage, of U.S.A., is a curious form of aerial, remarkable for the fact that its length is about equal to the wave-length of the signals to be received, that its height is about 6 ft., that it was used by Mr. Paul F. Godley, also of U.S.A., a few years ago, in the then remarkable feat of receiving 1kW. transatlantic signals with a supersonic receiver, using only ten " toobs." The directional effect of the Beverage aerial is stated to be very great. It is earthed at the far end, through a resistance, to get uni-directional reception. (It also appears that the Beverage aerial is unsuitable for erection in the average suburban garden !)

To save the operator the trouble of a two-λ walk, a method has lately been devised whereby the earthing resistance is moved to the receiving end of the aerial.

This information (*sic*), appropriately rearranged and polished, has from time to time adorned the pages of the " popular " periodicals. Seeing that many patent specifications are given in every number of your excellent magazine, would it not be possible to include those relating to the Beverage aerial in an early issue, giving circuits and data ?

May I say at the same time how heartily I agree with Mr. Anson in his recent remarks regarding the " R.I." amplification curves. My opinion is that the firm in question ought to leave that kind of thing to firms who have not such a good reputation to take care of. In its capacity as the leading English wireless publication, E.W. & W.E. ought to impress upon the wireless fraternity the necessity of giving amplification curves with a logarithmic frequency scale. The experiment of superimposing a piano keyboard on the frequency scale of the average transformer curve might set some people thinking. It cannot be done ! The frequencies ought to read : 16, 32, 64, 128, 256, 512, 1 024, 2 048, 4 096, 8 192, and not : 0, 500, 1 000, 1 500, 2 000. 2 500, 3 000, etc. " Truth in Advertising," please.

KAYE E. WEEDON.

21, Parkveien,
Oslo, Norway.

Screening of Small Variable Air Condensers.

The Editor, E.W. & W.E.

SIR,—With reference to the letter of Mr. R. Kay Gresswell, F.R.A.S., in the January issue of your journal, I should like to discuss briefly one or two of the interesting comments expressed.

The object of the article was to show that for *quantitative* work a screened condenser only has a strictly definite capacity between terminals, independent of the absolute potentials of the plates, when one set is connected to the screen. Thus the cases for the screen being—

(1) Separately earthed ;
(2) Left quite free and insulated ;

were dealt with in the last section (p. 971) and the first section (p. 972), but purely from the point of view of definiteness of capacity.

I quite agree with Mr. Gresswell for the circuit cited, for in the sketch one set of plates is earthed, and then to earth the screen is equivalent to connecting the screen to one set and earthing the combination.

In this condition, the capacity is independent of applied voltage and " hand effect " (as far as the terminal and leads from the insulated system will allow).

When, however, both plate systems are at potentials different from that of earth, and the screen is separately earthed, the effective capacity between terminals at a given setting is by no means necessarily constant, and although unsuitable for quantitative work, is probably quite satisfactory for ordinary tuning purposes.

D. A. OLIVER.

4, St. Dunstans Gardens, 5th January, 1926.
Acton, W.3.

Patents.

The Editor, E.W. & W.E.

SIR,—With reference to your editorial on patents, appearing on page 935 of your issue of December, 1925, the answer to the question contained in the last three lines of this paragraph is a most emphatic "No," as such an amateur would infringe our patents and we have not presented amateurs with a "free gift" up to the present.

As I think we have made clear in advertisements and otherwise, we are willing to issue a licence to all and sundry under the Supersonic Patent on receipt of the standard royalty of 30s.

H. A. DISNEY, Secretary,
Standard Telephones & Cables, Ltd.

63, Aldwich,
London, W.C.2.

4th January, 1926.

The Editor, E.W. & W.E.

SIR,—With regard to the letter by Mr. Disney —in November—and your Editorial in December:—

Firstly, a patent does not, so far as my knowledge goes, affect *use* (*vide* your Editorial) but only manufacture, and I do not think the view you take is a correct interpretation of the existing enactments.

"The principle of our patent law is that nobody is given an exclusive right to an idea but only the exclusive right to *exploit* an idea for profit and an idea is only patented as part of the machinery for accomplishing that end.

"Mere making of any object which comprises ideas covered by patents does not, therefore, constitute an infringement of the patent as long as such making is not used as a source of gain or profit."

This was my impression—and I had intended to write you in time for the December issue. However, I submitted it to legal opinion as a matter of precaution, and I have given you his own words—my only excuse for adding to the volume of letters on the subject.

Dover. C. E. B. WILKINS, B.A.(Cantab.).

Esperanto.

The Editor, E.W. & W.E.

SIR,—With reference to your note in your editorial of current issue of your journal with regard to the continuation of your Esperanto section, as a reader of your excellent journal since its first number, I would be very sorry to see you drop this section, as I am sure that you fully realise the importance of the language to Radio use, and especially in its capacity as a means of communication between the radio amateurs of different nationalities, for whom you cater so efficiently.

F. R. A. MCCORMICK.

5, Mount Eden Road,
Donnybrook, Dublin.

2nd January, 1926.

Selective Amplifiers.

The Editor, E.W. & W.E.

SIR,—I read with great interest Mr. Turner's article on Selectivity, but I should like to point out a small error, which, luckily, does not affect the value of the numerical results.

In Appendix I. he takes $\psi = \frac{R}{\omega L}$, $m = \frac{a}{\omega L}$. Substituting these values in equation (11), we do not get equation (12). I deemed this to be a misprint at first, but noted afterwards that he says that although ψ, R both vary, ψ is the more nearly constant.

It would appear better, therefore, to take as constant ψ and L, when we have

$$\psi = \frac{R}{\omega L},\ m = \frac{R_a}{\omega_0 L};\ \frac{R_a}{\omega L} = \frac{m}{p}.$$

In this case we get for equation (11)—

$$\frac{1}{\eta^2} = \left(\frac{R_a}{R_e} + 1\right)^2 + \frac{2m\psi}{p}\left(\frac{R_a}{R_e} + 1\right) + \frac{m^2\psi^2}{p^2} + \frac{m^2}{p^2}(1-p^2)^2$$

$$= \left(\frac{R_a}{R_e} + 1 + \frac{m\psi}{p}\right)^2 + m^2\left(\frac{1}{p} - p\right)^2.$$

This is without approximating by neglecting the effect of p in the first part. Now, so long as ψ is small, the first part is small compared with the second, and the numerical results given by the two equations for η will be sensibly equal. Only if ψ is increased greatly does the effect of the p in the first part (A) become sensible.

T. IREDALE WILLIAMS.

Collinson House,
Mill Hill, N.W.7.

12th December, 1925.

The Editor, E.W. & W.E.

SIR,—I have read with interest the criticism of Mr. Williams in connection with my article on selective amplifiers.

The discrepancy which he mentions in his second paragraph is due to the fact that in Appendix I of the original article there was a misprint. In the line which reads:—

"And lastly, let $\frac{R}{\omega L} = \psi$ and $\frac{R_a}{\omega L} = m$,

ω_0 should be substituted for ω in the denominators in both cases.

Having made this correction, Mr. Williams will find that the form of equation 12 as given by me is correct.

It is obviously desirable to express the two ratios ψ and m in terms of ω_0 rather than ω, for the former is a constant (*i.e.*, the frequency of resonance), whereas the latter is not.

P. K. TURNER.

13, Norland Square,
London, W.11.

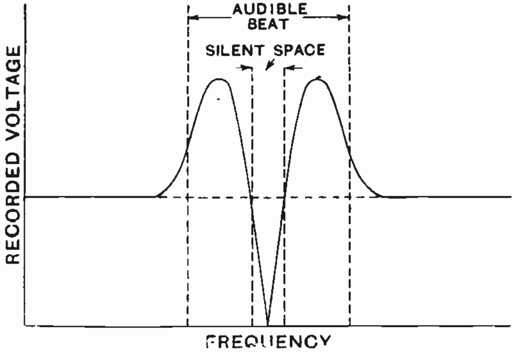
7th January, 1926.

"Wipe Out."

The Editor, E.W. & W.E.

SIR,—Replying to the letters which have appeared with reference to the oscillator phenomenon which I have previously described, it does not seem to me that either explanation covers my observations. In order to avoid possibility of error I have repeated the experiment, taking special care to observe the effect from *both* sides of the resonant frequency, and the results confirm the previous observations in every respect but one, namely, that there is a sharp drop in voltage *just before* synchronisation. In every case, however, the curve of recorded voltage is symmetrical, and returns to zero (unlike that shown by Mr. de Burgh).

The explanation given by Mr. de Burgh describes the sequence of events which I would have expected, and it was the different result which occasioned surprise.



The silent space is greatly exaggerated on the diagram, as with the sets of oscillations being of equal strength it is reduced to an extremely minute band of frequency.

It will be noticed that the curve shown is similar to those obtained from quartz resonators across a tuned circuit. From what little I know of the properties of these crystals it seems to me that the action is analogous in some respects.

Referring to Mr. Browning's remarks, am I to gather that the inductance of the oscillator is affected by the distant signals (from 2LO)? It does not seem that this could be possible, as the voltage set up in the oscillator coils would be very much less than that in the receiver, which is connected to an aerial, and this again would be much less than that generated in the oscillator coils by its own action. The only appreciable effect on the oscillator from the distant transmitter would be through the medium of the receiver.

In fact, it is the interaction of *three* circuits which makes the matter so complex, as the effect of one train of oscillations on the other through the medium of the receiver circuit would appear to play an important part in the action of the experiment.

Marcus G. Scroggie, B.Sc.

Lee, S.E.12.

Alignment Principle.

The Editor, E.W. & W.E.

Sir,—Mr. Barclay's article on the Alignment Principle is a most useful contribution in December number showing how Projective Geometry can be utilised.

The formula at top of column 2, p. 940, and in appendix, p. 942, should be corrected by substituting the following expression for the "*I*" (last term) :—

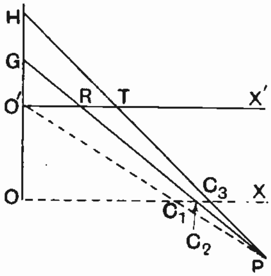
$$\frac{\lambda_3^2-\lambda_2^2}{\lambda_3^2-\lambda^{2}}\cdot\frac{\lambda_1^2}{\lambda_2^2}$$

J. Hart.

Harrow.

The Editor, E.W. & W.E.

Sir,—I cannot understand the significance of your correspondent, Mr. Hart's, "correction" to my formula, and for his satisfaction as well as for my own, I herewith append a simple proof of it :—



Our object is to find the points $C_1\ C_2\ C_3$ on OX such that

$$\frac{C_2C_3}{C_1C_2}=\frac{r_3-r_2}{r_2-r_1}\text{ and }\frac{HG}{GO^1}=\frac{\lambda_3^2-\lambda_2^2}{\lambda_2^2-\lambda_1^2}.$$

Change now the origin to the point O^1, and let O^1X^1 be the new axis of X.

Then, y-co-ordinate of G is $\lambda_2^2-\lambda_1^2=a$ (say)
y-co-ordinate of H is $\lambda_3^2-\lambda_1^2=b$ (say).

Let PC_2, PC_3 meet $O'X'$ in R and T respectively.

Then, x-co-ordinate of R is $k(r_2-r_1)=kc$ (say)
x-co-ordinate of T is $k(r_3-r_1)=kd$ (say)
where k is a constant.

For the lines GR and HT we have then the equations,

$$\frac{x}{kc}+\frac{y}{a}=1\text{ and }\frac{x}{kd}+\frac{y}{b}=1$$

in which, putting $y=-\lambda_1^2$, we obtain for the x-co-ordinates of C_2 and C_3,

$$OC_2=kc\left(1+\frac{\lambda_1^2}{a}\right)=\frac{kc\lambda_2^2}{a}$$

$$OC_3=kd\left(1+\frac{\lambda_1^2}{b}\right)=\frac{kd\lambda_3^2}{b}$$

Whence,

$$\frac{C_2C_3}{OC_2}=\frac{\lambda_3^2ad}{\lambda_2^2bc}-1=\frac{(r_3-r_1)}{(r_2-r_1)}\cdot\frac{(\lambda_2^2-\lambda_1^2)}{(\lambda_3^2-\lambda_1^2)}\cdot\frac{\lambda_3^2}{\lambda_2^2}-1$$

W. A. Barclay.

Murtle,
Aberdeenshire.

Readers' Views.

The Editor, E.W. & W.E.

Sir,—You invite expressions of opinion from your readers as to the contents of your magazine.

Might I suggest that the best policy would be to let the contents correspond as far as possible with the title? That is to say, I think the contents should be about experimental work in wireless telegraphy and should not deal with index-making or Esperanto, useful and interesting though these subjects may be.

I should also like to enter a plea for the reader whose mathematics have not gone beyond the point of (say) the matriculation examinations at Universities. Not that I would desire to eliminate the more highly specialised articles. Far from it. I think they seem to be very useful, though I comprehend them only in places. But surely there is room in the field of experiment for the, relatively, non-mathematical amateur. Further information as to how such an one might help to augment the general fund of knowledge would be very useful, and if the pith of the mathematical articles could be given for his benefit, I think it would be well. I am sure that this is already done to a considerable extent, but I think it might be done a little more fully.

Another point. Could not more references be given to authoritative text-books so that the relatively unmathematical reader might be put in the way of brushing up his mathematics and so, perhaps, eventually, be better able to appreciate the more highly technical articles?

Experiment and education in experiment. These should, I think, be your watchwords.

E. C. R.

West Byfleet.

Transformer Curves.

[Under correspondence in the issue of E.W. & W.E. for January, 1926, two letters were included over the initials A.B. and C.D. respectively. We have been asked to make it clear that these initials were put in substitution for the names of two firms commercially interested in the manufacture and sale of L.F. transformers.—EDITORS.]

The Editor E.W. & W.E.

SIR,—I have followed the Transformer Curve discussion with great interest. To my mind the correspondence columns are one of the most interesting features of your paper. Also I should like to congratulate you on the policy upon which you run these columns as expressed in the December editorial. May I contribute something to the discussion?

There is one fact that Mr. Appleton's critics seem to have ignored: the response of iron to a magnetic field is partly mechanical. There is an expansion of the iron, for instance.* Now in any operation into which mechanical friction enters there is a limiting value for the applied force below which there is no response at all. I don't think that there can be much doubt that the "Appleton effect" does exist, though I am rather sceptical about its importance under normal conditions. I should like to see the figures obtained in the experiments upon which Mr. Appleton's theory is based.

D. F. VINCENT.

Reading. 14th January, 1926.

The Editor, E.W. & W.E.

SIR,—I am not surprised that Mr. Appleton should devote more attention to my introductory anecdote than to the body of my letter, because he had once before proved his reluctance to facing the facts. But I am surprised that he should throw suspicion on my motives.

In these circumstances I am compelled to state that I am not connected, either directly or indirectly, with the radio industry—except as a purchaser of transformers and accessories, for my own use.

Further, like many other independent experimenters, I am constrained to inform the "powers that be" that there are limits to which the licence we readers allow advertisers can be stretched. I would sooner deny myself the undoubted benefits that I derive from subscribing to technical publications than submit uncomplainingly to an advertising campaign that gratuitously insults my intelligence each issue.

WILLIAM D. OWEN, A.M.I.E.E.

Palmer's Green, N.13.

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* [Mechanical friction of a molecular character —and it is molecular friction which is involved here—enters into the deflection of a spring, but we have always understood that the deflection was proportional to the force, however small.— G.W.O.H.]

From the World's Wireless Journals.

Abstracts of Technical Articles.

R000.—GENERAL PRINCIPLES AND THEORY.

R060.—I.E.E. Wireless Section—Chairman's Address.—Maj. B. Binyon, O.B.E. (*Journ. Inst. E.E.*, Dec., 1925).

Address delivered 4th November, 1925, an account having appeared in E.W. & W.E. of December, 1925. The address dealt chiefly with marine wireless, under the sectional headings: Spark Sets, Spark Gaps and Systems of Transmission, Emergency Sets, Lifeboat Installations, Ships' Aerials, Automatic Call Devices, Direction Finding, Demonstrations of an Automatic Call Device and of an Automatic Direction Finding System were given.

R111.—Ether Drift.—G. W. de Tunzelman (*Electrician*, 18th Dec., 1925).

An article discussing the recent change of front on the question of the relative motion of earth and ether. Observations made by Dr. D. C. Miller at Mount Wilson Observatory, California, 6 000 feet above sea level, indicate that at this altitude the earth and the adjacent ether do not move together. By comparison with previous observations made by Miller and Morley at Cleveland, Ohio, at only slightly above sea level, this suggests a partial drag of the ether by the earth, which decreases with altitude, or in other words, with greater altitude the earth loses its grip on the ether. This article reviews classical work on the subject from the original interference experiments of Michelson up to the 1904-5 work at Cleveland, Ohio. The recent work of Miller at Mount Wilson from 1921-25 is described with some curves of results. Further work is in progress.

A short note on the same subject in the Journal of the Franklin Institute, November, 1925, quotes views of Lorentz, Einstein and Eddington on Miller's results, in so far as they are completed. The subject is of considerable importance in connection with the theory of relativity.

R113.—Notes on the Conditions Governing Transatlantic Reception.— S. K. Lewer (E.W. & W.E., Dec., 1925).

R113.6.—On Some Direct Evidence for Downward Atmospheric Reflection of Electric Rays.—Prof. E. V. Appleton and M. A. F. Barnett (*Proc. Roy. Soc.*, A. 109, 1925).

A paper dealing both theoretically and experimentally with the effect of the upper ionised layer.

Experiments are described indicating that for short-distance transmission (*e.g.*, 100 miles) of waves of broadcasting length, the fading of signals is due to interference between two sets of waves, one the direct ray from the transmitter, and the other a ray deviated by the ionised layer. According to the instantaneous phase between these they may mutually assist or oppose. A method of estimating the height of the layer is shown, the preliminary experiments indicating a height of about 90 km. Subsequent experiments are described for the determination of the angle at which waves deviated by the upper layer reach the ground. It is shown that waves arriving at the ground from the ionised layer produce a "stationary wave" system. From considerations of the values of the electric and magnetic vectors, it is proved that, with a downward ray, greater signal variations will occur with a loop receiver than with a vertical aerial receiver, and that from the ratio of these variations the angle of arrival can be calculated. A loop receiver and a vertical aerial receiver were both adjusted for equal (galvanometer) signals for the direct ray transmission of day time. When night time fading set in the ratio of signal current variations was observed, and it was found that fading was more pronounced on the loop set. At Cambridge the ray from the London (B.B.C.) transmitter returned from the layer arrived at an angle of 60°—70° with the ground.

Examination of the stationary wave system referred to shows that the atmospheric rays are, generally, elliptically polarised, and that this may be sufficient to cause errors in direction finding systems at distances of 30 or 50 miles for overland transmissions.

Approximate values are finally deduced for the reflection co-efficient of the ionised layer for low angles of incidence, and for the inferior limit of electronic density. The reflection co-efficient is estimated at between 0.2 to 6 per cent., according to the attenuation factor assumed for the direct ray, and the electronic density is placed at a minimum of 10^5 electrons per cubic centimetre.

R113.6.—Wireless Signals Variations.—Prof. E. V. Appleton and M. A. F. Barnett (*Electrician*, 11th Dec., 1925).

An article dealing with the same general matter as the foregoing abstract, and more especially with the measurement of the angle at which the reflected waves reach the ground. Variations with height of the three dimensional electric and magnetic vectors of the "stationary wave" system are illustrated by diagram and evaluated in a table.

R.134—The Rectification of Small Radio Frequency Potential Differences by Means of Triode Valves. Part II.—F. M. Colebrook (E.W. & W.E., Dec., 1925).

R200.—MEASUREMENTS AND STANDARDS.

R240.—Rating Circuit Resistance.—G. H. Browning (*Q.S.T.*, Dec., 1925.)

An attempt is made to specify a quantity "to give an easy way of rating the worth of a circuit used in the reception of radio signals." The ratio R/X is suggested as suitable, R being the *H.F.* resistance and X the inductive reactance ($2\pi fL$) of the circuit. Curves are given for this ratio against wave-length for a number of coils of spaced and of non-spaced winding, while an extended curve for a single coil shows a considerable tuning region where the ratio is a minimum and almost constant. The article concludes with remarks on the choice and design of coils.

R261.—MESURE DE LA DIFFÉRENCE DE POTENTIEL MAXIMA AUX BORNES D'UN CIRCUIT PARCOURU PAR DU COURANT ALTERNATIF.—F. Bedeau (*Onde Elec.*, Nov., 1925).

An article dealing with the well known "slide back" method of measuring the maximum of an oscillating or alternating E.M.F. After considering the general principles and advantages of the arrangement, the author points out the disadvantage and the difficulty of obtaining an absolutely zero value of anode current due to the very slight slope of the anode current curve at the region involved. A satisfactory solution is recommended by the resistance coupled arrangement shown in Fig. 1, where V_x is the input to be measured. If the second valve be already lit, switching on the first valve will give a P.D. across *R*, causing a diminution of current through *mA*. In the absence of the input voltage V_x, the mean voltage of the first grid is made negative by the battery (and potentiometer for fine adjustment) until the reading of *mA* is again a maximum. This is repeated with V_x applied, in the usual manner of the slide-back method. The resistance *R* can conveniently be of the order of a megohm, on account of the high internal impedance of the valve round the region of zero current. A note follows on calculation, the choice of valves, and calibration.

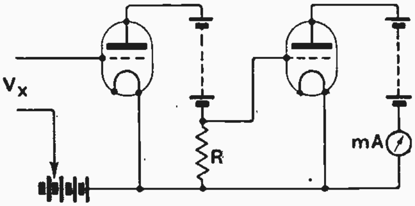


Fig. 1.

A supplementary note by P. David emphasises the advantage of the arrangement in determining peak values when the positive and negative half cycles are not of equal amplitude. This is readily done by reversing the connections of V_x.

R283.—IRON CORED CIRCUITS :—

(*a*) The Inductance and Loss Resistance of an Iron-Cored Circuit.
(*b*) Damped Oscillations in an Oscillatory Circuit with Iron Core.
(*c*) Switching-in Effects in an Oscillatory Circuit with an Iron Core.
(*d*) Oscillations in Coupled Circuits when the Secondary Circuit contains an Iron Core.
—H. Plendl, F. Sammer and J. Zenneck (*Zeit. f. Hochfreq*, 26, No. 4, 1925).

Four articles on the behaviour of circuits containing an iron-cored inductance, all the measurements being made at frequencies of the order of 500.

In (*a*) measurements of the inductance and loss resistance are described. The alternating current through, and the voltage across the iron-cored coil, were measured, and a dummy circuit of a variable resistance and an air-cored inductance was substituted and adjusted to equality. Different values of steady current up to 2 amps were also sent through the iron-cored circuit with the A.C. superimposed. Result curves of the inductance and loss-resistance under the various conditions are given.

(*b*) Deals with the effect of the iron-cored circuit on oscillations set up in a secondary circuit by a freely excited primary. The effects are well illustrated by a series of oscillograms, taken with a Braun tube. A switch permitted the substitution of a dummy circuit, as in (*a*), similarly adjusted to equality with the iron-cored circuit. The primary was excited into free oscillation by switching discharge of its condenser after charging to known potentials. An extensive series of oscillograms show the comparison between the secondary oscillations with and without the iron-cored, when the primary condenser was charged to various voltages and its circuit variously coupled to the secondary.

In (*c*) are considered the effects of switching in a generator to a circuit containing a condenser and an air-cored inductance (for resonance adjustment) and the iron-cored circuit under consideration. Curves are given for the current-voltage characteristic of the circuit under different conditions, and Braun tube oscillograms show the transient effects on switching.

In (*d*) a generator of the order of 500 cycles is included in a primary circuit with a condenser and two air-cored coils, one of these being coupled to the secondary, which contains a condenser and the iron-cored circuit. Curves are given for the induced secondary voltage against the current in that circuit, and for primary and secondary current with machine voltage for various values of coupling between the circuits. Oscillograms are also given for primary and secondary currents for different couplings and machine voltages. Many of these show the beating phenomena of coupled circuits, and these are discussed with reference to the conditions.

R300.—APPARATUS AND EQUIPMENT.

R342.7.—CONSTRUCTION D'AMPLIFICATEURS DE PUISSANCE SANS DISTORTION.—E.W. Kellogg. (*Onde Elec.*, Nov. and Dec., 1925). Translation and Reprint from *Journ. Amer. I.E.E.*, May, 1925.

The paper deals with the problem of obtaining the maximum possible output from a given amplifying valve, with negligible distortion. An amplifying valve can conveniently be rated in terms of the output in watts obtainable when a sinusoidal voltage, of as great amplitude as can advantageously be utilised, is applied to the grid. This rating is much less than that of the same valve used as an oscillator.

From a set of static characteristics for a given valve, the dynamic characteristics for any load can be plotted. From this the power output and distortion can be evaluated, examples being given.

An optimum value of load resistance is given as twice the internal impedance of the valve, with the H.T. voltage limited to a certain value. An increase of this value would lead to excessive heating of the anode, and different rules apply, calling for greater grid bias and load. The symmetrical circuits of the "push-pull" system are discussed, and it is stated that while this reduces distortion, it will not make up for incorrect operation of the valves, nor will it greatly increase the output per valve.

The dynamic characteristics for a reactive load are not readily plotted. For most practical purposes of design, however, it is sufficient to determine the optimum conditions for a resistive load, and then to make the impedance of the reactive load high enough to maintain the anode current variations within the limits determined for the resistive load.

An important application of the principles dealt with is in the design of complete transmitters for telephony. Here serious distortion results from over-working the modulator valves. For moderately deep modulation it is stated that there should be from two to four modulator valves for each oscillator.

The closing paragraphs discuss certain details of design.

R342.6.—A Three-Tube Neutrodyne for Short Waves.—D. Ablowich, Jr. (*Q.S.T.*, Dec., 1925).

A description of a three-valve receiver set employing the arrangement of neutrodyne shown in Fig. 1. The anode coils have 8 turns of 22 D.C.C. wire wound on a 1¾ in. diameter tube. The grid coils have 40 turns of the same wire wound on a 2⅛ in. diameter tube. The coil is tapped at 8 turns for connection to the neutralising condenser, while an auxiliary tuning tap is taken off at 25 turns. With a condenser of $.000\,25\mu F$ the 25 turns tune from 80 to 170 metres and the 40 turns from 145 to 235 metres.

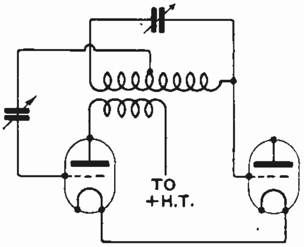


Fig. 1.

R344.1.—Valve Maintained Tuning Forks.—T. G. Hodgkinson (*Proc. Phys. Soc.*, London, Dec., 1925).

The paper first considers the original (and more usual) case of a fork system with the magnet coils directly connected to the valve electrodes. Attention is devoted to the effect of the valve electrode conductances, and to the direction of the coil windings, with expressions for the various cases. In the second part of the paper is discussed the case of a fork system with transformers between the valve electrodes and the fork magnet coils. It is shown that this arrangement has considerable advantage—especially for low frequency forks—in that it permits more winding space for the valve coils thus letting the impedance of the anode coil be comparable to the impedance of the valve. It is also shown that it is possible to adjust the phase of the driving current in such a way as to reduce the difference between the note of the maintained system and that of the true fork to zero.

R345.—Note sur un Nouveau Procédé de Modulation des Emetteurs à Lampe.—P. David (*Onde Elec.*, Nov., 1925).

The method described is an interesting expansion of the standard "constant current" or "choke control" system of modulation.

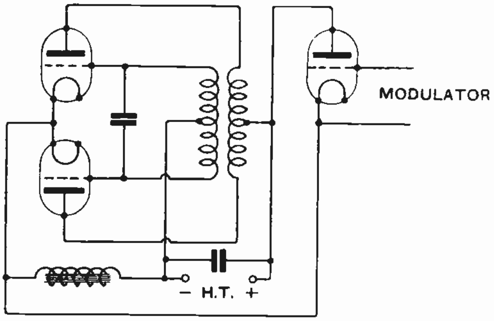


Fig. 1.

The method is primarily illustrated by Fig. 1, showing two valves employed symmetrically as oscillators. The choke is placed between the negative H.T. lead and the filament, so that, without modulation occurring, the steady drop of voltage across the ohmic resistance of the choke makes the oscillator grid negative. The incidence of modulation causes the usual audio-frequency voltage fluctuations across the choke. These variations are applied, part to the anodes (as in

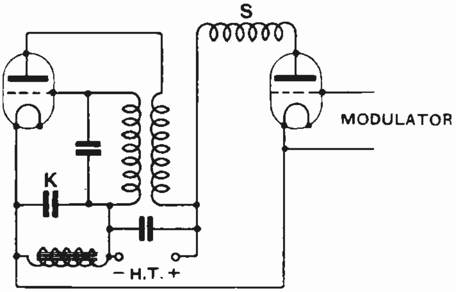


Fig. 2

the normal method of choke control) and *part to the grids of the same valves*. The grid variation being preponderant gives a more powerful modulation.

The system is equally applicable to an oscillatory system not symmetrically disposed, as shown in Fig. 2. It is then necessary to shunt the choke by a condenser *k* by-passing for radio frequencies, and to insert an H.F. choke *s* in the modulator circuit to keep the high frequency current out of that circuit.

It is also pointed out that only part of the oscillator H.T. voltage may be applied to the modulator.

If iron cored, the choke should not be saturated by the steady anode current, and the H.T. source should be shunted by a condenser passing musical frequencies.

Several oscillograms are given showing modulation results with the arrangement of Fig. 1.

R374.5—THE NEW CARBORUNDUM DETECTOR.—M. L. Hartmann and J. R. Meagher (*Q.S.T.*, Dec., 1925).

The paper describes work in the Research Laboratory of the Carborundum Company, Niagara Falls, with a view to finding carborundum of the most efficient rectifying qualities, the best type of mounting and the correct pressure between crystal and contact. It is stated that with a particular variety of carborundum never previously used for rectification, it was found desirable to press the crystal against the hardened metal plate with a pressure of more than 5 lbs. A detector of fixed pressure contact is illustrated and described, with a diagram of its characteristic curve. The complete unit comprises crystal, potentiometer with battery, and by-pass condenser. Mechanical pressure being fixed, the only adjustment is that of the potentiometer for the position of rectifying bend.

R376.3—DEMONSTRATIONS OF THE PUBLIC ADDRESS SYSTEM IN EUROPE.—A. F. Rickard (*Elect. Communication*, Oct., 1925.)

An historical account of the progress of the Western Electric Company's public address system in Europe, more especially in Great Britain. Descriptions are given of various occasions when the system has been used since its first appearance in November, 1922. Considerable prominence is given to its use at the opening of the Wembley Exhibition by H.M. the King. Another notable occasion mentioned was the use of an extra large wooden projector in a demonstration at Southampton to show the utility of the system in directing ships into dock berths. With this projector speech could be understood up to one and a half miles from the berth. A note is given on its use at sports meetings, etc., and a short concluding section names a few of the occasions when the system has been used on the Continent.

R382.—TOROIDS.—F. J. Marco (*Q.S.T.*, Dec., 1925).

After considering the disadvantages of solenoidal pattern windings in H.F. amplifier circuits, the author points out the theoretical advantage of the torus in that it has practically no external field. He then considers the design of toroidal coils, working from the Bureau of Standards formula

$$L = .004\,606\,N^2\,H\,log\frac{r_2}{r_1}.$$

Where N is number of turns, H thickness, r_2 the outer and r_1 the inner radius. Curves are given for the ratio r_2/r_1 for maximum inductance and for maximum L/R. The use of toroidally wound coils for inductively coupled H.F. transformers is then considered, the author concluding that it is desirable to wind the secondary as a complete torus with the primary inside it to about one-third of its circumference. It is pointed out that the toroid is not immune from electrostatic "pick-up," but it is claimed that this pattern of coil is more easily shielded without increase of effective resistance. Illustrations are given of several commercial forms of toroidal windings.

R386.—AMATEUR FILTER PROBLEMS.—F. S. Dellenbaugh (*Q.S.T.*, Dec., 1925).

An article dealing with the design of filter circuits for smoothing a rectified 25-60 cycle supply. An extensive table of design data for inductance coils with iron cores is given, with diagrams of suitable complete filter circuits. Instructions are also given for testing, especially with regard to the best air gap in the magnetic circuit of the iron cored coils.

R388.—A NOTE ON THE CATHODE RAY OSCILLOGRAPH.—F. Richard Terroux (*Journ. Frank. Inst.*, Dec., 1925).

The author suggests the use of a quartz window to facilitate photography of the fluorescent pattern. It is pointed out that with the fluorescent material on the inside of a glass bulb, relatively long exposure of a steady pattern is necessary, while with instruments (*e.g.*, that of Dr. A. B. Wood) photographing inside the vacuum, considerable handling and pumping are necessary. The author describes an arrangement using a quartz window, with a highly sensitised plate mounted externally to it. Exposures down to about 1/100th second are illustrated. The fluorescent material was Willemite, but it is suggested that a mixture of calcium tungstate (as in the Western Electric tube) would be superior, while the quartz sheet could be made thinner than that actually employed. It is pointed out that as the photographic plate is placed against the quartz, no lens system is necessary, and that the arrangement should prove suitable for the convenient photography of impulsive phenomena.

CORRECTION.

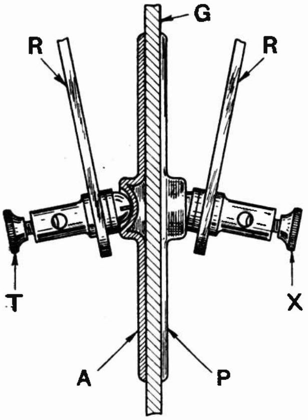
In the advertisement pages of the December issue an error occurred in the announcement of Messrs. A. Vandam, Caxton House, Westminster, S.W.1. The O'Keefee coil, which is referred to, is described as the smallest coil with the greatest *capacity* instead of the greatest *efficiency*.

Some Recent Patents. [R008

A CAPACITY LEAD-IN.

(*Application date*, 19*th November*, 1924.
No. 243,517.)

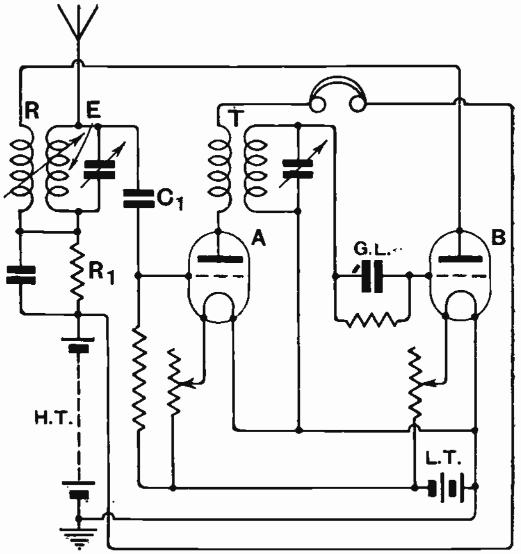
An attempt to solve the lead-in problem is disclosed in the specification of British Patent No. 243,517, granted to A. H. Guiness, the accompanying diagram illustrating the idea of the invention. It is well known that it is not always easy to obtain a really efficient lead-in when the aerial has to be taken to some room in an ordinary house not provided with commercial lead-in insulators, and similar fittings. According to the invention, instead of passing the wire through the window or wall, connection between the aerial and the receiving system is capacitative. Two metal plates are used, one on either side of the glass window, which forms the dielectric. Thus, in the accompanying illustration, it will be seen that one plate *A* is provided with a terminal *T*, which is connected to the aerial lead-in, while another plate *P*, provided with another terminal *X*, is connected to the aerial terminal of the receiver. The glass window shown at *G* constitutes the dielectric. The two plates are kept in position by means of two arms *R* of insulating material which are fastened together by means of a U-shaped spring, not shown, of course, in the illustration. This spring is placed over the top of the window, which thus supports the two plates and the spring action causes the plates to keep in contact with the glass. We should imagine that a lead-in of this nature would be more useful on the shorter wave-lengths.



A RESISTANCE COUPLED REFLEX RECEIVER.

(*Application Date*, 18*th June*, 1924.
No. 243,039.)

A modification of the well-known form of reflex receiver, in which the valve is made to amplify both high frequency and low frequency, is disclosed in the British Patent No. 243,039 granted to J. Scott-Taggart. The accompanying illustration indicates the broad idea of the invention. The receiver employs two valves *A* and *B*. The first valve *A* is employed as a high frequency and low frequency amplifier simultaneously, while the second valve *B* merely acts as a detector. The aerial circuit *E* consists of an ordinary inductance and variable capacity in parallel, which is virtually connected between the grid and filament of the first valve. Actually the grid-filament circuit of the first valve includes a condenser *C*1, of the order of 0.25μF, a high resistance *R*1 of the order of 70 000 ohms shunted by a condenser to act as a high frequency by-pass, and also the H.T. The anode circuit of the first valve *A* contains the primary winding of a high frequency transformer *T*, the secondary of which is shown tuned and connected between the grid and filament of the second valve *V*, a grid leak and condenser *GL* being included in order to make the second valve rectify. The anode circuit of the valve *B* contains a reaction coil *R* and also a high resistance *R*1, while the anode circuit of the first valve *A* also contains the telephones or loud-speaker. The mode of operation is briefly as follows: High frequency potentials due to incoming signals will be impressed between the grid and filament of the first valve, where they will be amplified and passed on by the high frequency transformer *T* to the grid filament circuit of the second valve. Here the amplified potentials will be detected, and the anode circuit of the second valve will then contain low frequency components. As this anode circuit contains the high resistance *R*1, low frequency potential will be set up across it which will be passed on to the grid of the first valve, by means of the 0.25μF condenser *C*1, where they will be amplified by the valve *A*, and cause the telephones to be operated. An additional feature of the circuit, of course, is the regenerative effect of the detector valve, and the aerial circuit by means of the reaction coil connected as shown in the diagram.

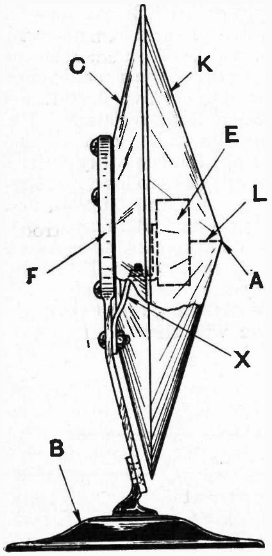


The circuit is really a normal reflex arrangement in which the resistance replaces the low frequency transformer or choke.

THE "KONE" LOUD-SPEAKER.

(*Application date, 9th August*, 1924. *No.* 240,596.)

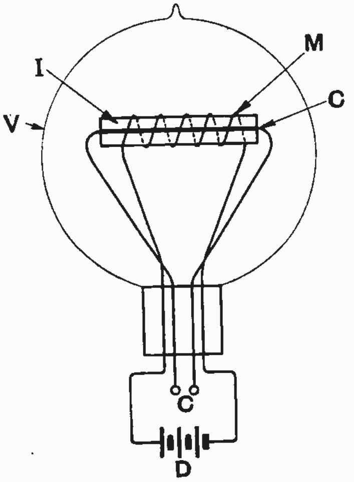
The above British Patent, No. 240,596, granted to the Western Electric Company, Limited, appears to be one of the specifications relating to the well-known "Kone" loud-speaker. The accompanying diagram illustrates the general idea and construction of the "Kone," which will be seen to comprise a metal framework *F* supported by a base *B*, the framework *F* being essentially circular in shape and carrying a truncated cone *C*. To the base of this cone is attached another complete cone *K*, the apex *A* of which communicates with the electromagnetic system. That actually used is the Baldwin or balanced type, shown at *E*, and supported by a bracket *X* attached to the framework *F*. The armature of the system is connected to the apex *A* by means of a link *L*. When the armature is caused to vibrate by the passage of speech currents the link *L* is pulled in and out, and the movement is imparted to the two cones which vibrate as a whole, and cause a similar vibration in the air. The diameter of the cone is of the order of 12 to 18 in. and is made of specially treated paper or similar material. The advantage of this type of receiver is that certain frequencies, especially the lower ones, are not likely to be cut off as is usually the case with the normal type of sound conduit and horn.



AN INTERESTING FILAMENT.

(*Convention date, France, 26th May*, 1924. *No.* 234,480.)

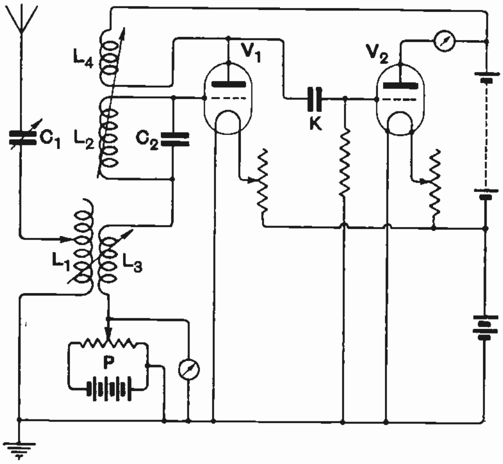
The above British Patent, granted to J. Hawadier, describes yet another method of dispensing with large storage batteries for filament heating of small valves. It is well known that if alternating current is used directly to heat the filament many undesirable effects are obtained. Hitherto equipotential cathodes have been employed in which the A.C. element heats a large surface, which is used as a cathode, but this method necessitates a rather large current consumption. According to the present invention, however, a valve *V* is provided with two filaments, one filament *C* is heated by alternating current at any desired voltage, and the filament is then covered with some non-conducting substance *I*. It is pointed out in the specification that before any substantial emission is obtained it is necessary to raise the temperature of the filament to a certain degree, dependent upon the nature of the substance. There is actually a particular value at which practically no emission takes place, and yet if increased by a few degrees immediately causes electronic emission to occur. Accordingly, the insulating material is raised to some predetermined temperature by the alternating current, and an auxiliary filament *M* is wound over the insulating material, the filament being provided with means of connection to an accumulator. The current passing through the filament from the accumulator is sufficient to raise the temperature to the desired additional number of degrees to produce sufficient emission. It is stated that it is preferable to employ for the auxiliary filament a material having a negative temperature co-efficient. Although the scheme appears to be very ingenious we do not see the advantage over the ordinary equi-potential cathode, since it would appear to be quite possible to make the insulating material a few degrees hotter by passing more alternating current through the main filament, thereby obviating the necessity of any direct current through the auxiliary filament.



AN INTERESTING RECEIVING SYSTEM.

(*Application date, 22nd July*, 1924. *No.* 243,407.)

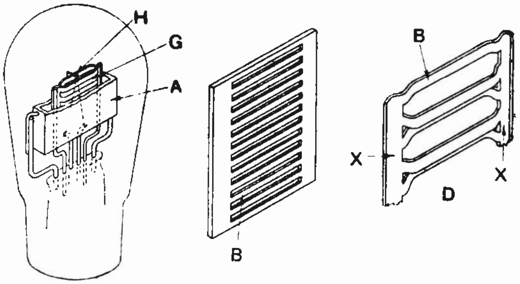
A rather interesting method of receiving signals by stopping and starting the generation of continuous oscillations is described in the above British Patent Specification by D. B. Fletcher, D.Sc., and S. Brydon, D.Sc. It is well known that the condition of a valve determining the production of oscillation is dependent upon the grid potential and the coupling existing between the anode and grid circuits. As soon as the valve commences to oscillate there is a very considerable change in the anode current, and consequently if incoming signals can be influenced to control the oscillation of a valve a very efficient method of detection is assured. The circuit shown in the accompanying illustration indicates one method of arranging a series of valves to act in this manner. It will be seen that the aerial circuit consists of the usual condenser $C1$ and inductance $L1$. The grid circuit of the first valve contains the tuned circuit $L2$ $C2$, and also the inductance $L3$, which is coupled to the aerial inductance $L1$. The anode circuit of the valve $V1$ contains an inductance $L4$, which is coupled to the inductance $L2$ so as to bring about a regenerative effect. The potentials produced by the anode of the valve $V1$ are communicated to the grid of the valve $V2$ by means of a condenser K, the valve $V2$ acting as a rectifier. The lower end of the inductance $L3$ in grid circuit of the valve $V1$ is connected to the slider of a potentiometer P provided with a battery and a voltmeter. The successful operation of the scheme depends upon the correct adjustment of the grid potential and coupling existing between the grid and the anode circuits. This has to be found by a method of trial and error, so that the slightest increase due to incoming signals causes the valve to oscillate, and immediately ceases as soon as the impressed potentials are removed. The patent appears to be of rather a limited nature, and seems to be confined to the adjustment rather than the circuital arrangement, the specification containing very specific instructions as to the best manner of adjusting the first valve.



A STAMPED GRID.

(*Convention date* (*U.S.A.*), *23rd February*, 1924. *No.* 229,704.)

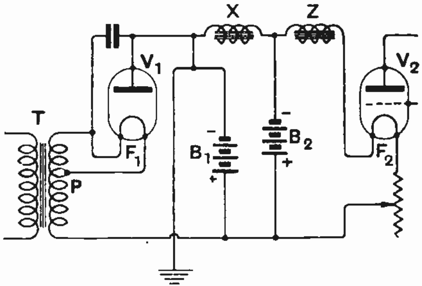
A stamped grid is described in the above British Patent, No. 229,704, which has been granted to the Westinghouse Lamp Company. The arrangement will be seen from the accompanying illustration, in which it will be noticed that a rectangular shaped anode A is employed (which is half cut away), H a hairpin filament, and the special grid G. This is stamped out from a sheet of rectangular metal, after which the bars B, which are thus formed are displaced first on one side and then on the other as shown at D. The sides of the metal are made of channel section X so that they can be attached to upright supports. The object of the invention is, no doubt, to provide a very rigid structure for the grid instead of the more usual helix or spiral of some description. We should imagine that, providing the other electrodes were suitably supported, the valve would be very non-microphonic.



AN A.C. FILAMENT SUPPLY.

(*Application date*, *19th June*, 1924. *No.* 239,939.)

Many schemes have recently been devised for utilising alternating current supply for filament heating in a valve receiver. The arrangement shown in the accompanying illustration is described in British Patent No. 239,939 by E. L. W. Bryne, which, in addition to details of the fundamental arrangement, describes several specific details and modifications of the idea. The power is obtained from a transformer T which is provided with a tapping on the secondary at P and heats the filament F_1 of a rectifying valve V_1. This rectifying valve charges a battery B_1, which is connected through a choke X to a battery B_2, which in turn is connected to the filament F_2 of a valve V_2 in the receiver, another reactance Z being connected between the end of the second battery and the filament. The battery B_1 is charged by pulsating currents derived from the valve V, and in turn is said to charge the battery

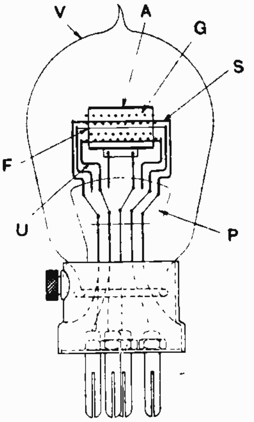


B_2, the object of the chokes *X* and *Z* being to eliminate any pulsating voltages from the valve filament F_2 which might be obtained from the source of supply. Floating batteries of this nature are well known, and we should imagine that the particular arrangement should prove quite successful in practice, provided that the chokes are suitably designed and constructed.

A SPACE CHARGE VALVE.

(*Convention date* (*Germany*), 27*th November*, 1923. *No.* 225,541.)

A special type of valve designed to work with a very low filament consumption and also to overcome the detrimental effect of space charge is described by N. V. Philip's Gloeilampenfabrieken in British Patent No. 225,541). One form of construction is shown in the accompanying illustration, in which it will be seen that the valve comprises the usual glass envelope *V* and the foot *P*, which supports an anode *A* of cylindrical formation and an ordinary helical grid or control electrode *G*, while the filament *F* is of the usual straight wire variety fixed to two upright supports *U*. The filament *F* is coated with some thorium or similar compound so that it works at an exceedingly low temperature. The necessary power required to operate the valve is further reduced by providing a space charge grid *S* arranged near the filament and intended, of course, to suppress the space charge, which thereby allows a far greater filament emission to reach the anode. The novelty of the invention seems to lie in the provision of the method of neutralising the space charge by an additional grid or other electrode, in combination with a very low temperature filament. This, of course, gives a valve which requires the very minimum amount of power in order to operate it.



DETECTION WITH A FOUR-ELECTRODE VALVE.

(*Convention date, France,* 15*th October*, 1923. *No.* 223,580.)

A system of detection employing a four-electrode valve is claimed in the above British Patent by Compagnie Générale de Télégraphie sans Fil. The accompanying diagram illustrates the idea, and it will be seen that an ordinary aerial system *ALCE* is employed which is inductively coupled to a tuned circuit *L*1 *C*1. This is connected between the outer grid *O* and some point on the filament heating circuit, such as at *F*. The anode *P* is connected directly to the high tension battery. The inner grid *I* is also given a positive potential so as to reduce the effect of the space charge by means of the same high tension battery, but the inner grid circuit includes an inductance *L*3 and telephone receivers *T*. The inductance *L*3 is coupled directly to the aerial inductance *L*1, and is arranged so as to provide a regenerative effect when taken to the point of oscillation. The patent, to us, appears to be rather of a limited nature, as there does not appear to be anything very fundamental and novel in the circuit shown.