ON THE EXTRA TRANSMISSION OF ELECTRIC WAVES.[1]

By F. C. Blake.

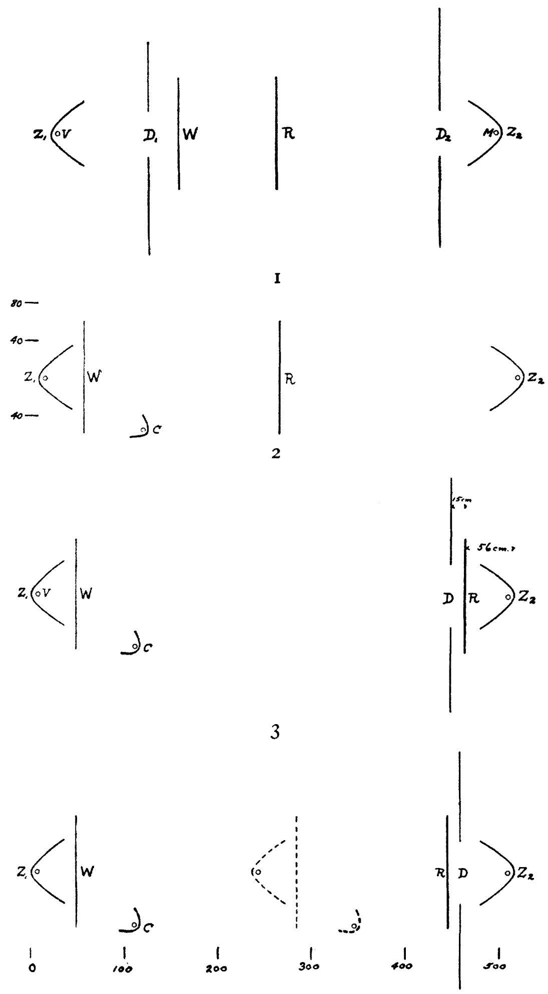
IN the May number of the Physical Review of 1907 appeared a criticism by Dr. Clemens Schaefer[2] of the work of Blake and Fountain[3] on the transmission of electric waves through resonator gratings. Dr. Schaefer takes these authors to task for overlooking an important article by him,[4] and with good right. On behalf of Dr. Fountain and myself I here express regret that this oversight occurred and cheerfully acknowledge any priority that Dr. Schaefer's article contains.[5]

Before replying to Mr. Schaefer, I wanted to perform some experiments, and it is only lately that I have been able to complete them. In one part of his criticism, he claims to have repeated some of our experiments, the only change he made being the insertion of two diaphragms. With this change he was unable to verify our results on extra transmission. I felt that the use of diaphragms was unwarranted on the ground of the enormity of the diffraction effects thereby introduced, but it plainly was a question for further experimentation to decide, hence the delay in my reply.

It seems best before taking up the various points in Schaefer's criticism, to describe the new experiments I have performed bearing upon the phenomenon of extra transmission. The apparatus used was identical with that used in the previous work,[6] with the exception of the resonator grating and its supporting framework. Columbia University kindly donated the parabolic mirrors, the vibrator

[1] Read before Section B, A. A. A. S., December, 1908.
[2] Phys. Rev., XXIV., p. 421, 1907.
[3] Phys. Rev., XXIII., p. 257, 1906.
[4] Ann. d. Phys., Vol. 16, p. 106, 1905.
[5] By letter at the time I made such acknowledgment.
[6] Blake and Fountain, loc. cit.

and the receivers and I herewith express my best thanks to Professor Hallock and the department of physics there. The arrangement of apparatus is shown in Figs. 1–4, drawn to scale. Z_1, Z_2 are the



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Figs. 1-4.

zinc mirrors, D_1 and D_2 are diaphragms of galvanized iron of variable aperture, W is a rectangular wire grid 120 cm. on a side, with the distance between the wires 2.5 mm. The wires, No. 36 B. & S., were strung vertically, at right angles to the electric force of the oncoming wave. This grid was used to make sure that the wave was plane-polarized, although I am convinced as before[1] that its use is unnecessary provided one is careful that the axes of vibrator and receiver are in the focal lines of their respective mirrors. C and M are the check and main receivers and V the vibrator. R is the resonator grating—a sheet of plate glass 122 × 96.5 cm. carrying tin-foil strips whose distribution was varied. Instead of R, a bare sheet of plate glass B could be inserted at will. The average thickness of the bare glass was 7.3 mm., that of the resonator glass 6.8 mm., a difference sufficiently great to make the ratio R/B for transmission, with both glasses bare 1.022, obtained by a long series of alternate readings. In all the curves and results of this paper this ratio has been taken into account.

One of the galvanometers used was a du Bois-Rubens type, the other a Broca instrument. They were given the same period and were interchanged in the receiver circuits whenever it was desirable. The former had a sensitiveness roughly of 2.5×10^{-9}, the latter, 10^{-8}, for a period of 4 seconds. The period was varied from time to time, according to the energy available, the time for a complete swing being varied from 4 to 6.5 seconds.

Since Schaefer had insisted upon the use of diaphragms in such work on electric waves, the first thing I did was to introduce two such diaphragms each 8 ft. 6 in. square, using a resonator system identical with Table I., Fig. 9, Blake and Fountain, except that the resonators were 5 instead of 6 cm. long. Fig. 5 shows the results obtained. It was found impossible under the working conditions at first obtainable to get sufficient energy through two apertures 32 cm. vertical by 24 cm. horizontal to make the results trustworthy, so apertures 47 × 38 were chosen. The points shown are the mean of two sets of readings taken thus: $ARBRBRA$ (A = air = free radiation, R = resonator glass, B = bare glass). These apertures fulfill the condition imposed by Schaefer, that they be smaller than the mirror apertures, 68.8 × 60.5 cm.[2] Larger aper-

[1] Blake and Fountain, loc. cit., p. 266.

[2] Apparently the figures 70 × 63 cm. given on page 259, Blake and Fountain, are slightly too large. This error in measurement is of course unimportant.

tures were then used and finally both diaphragms were removed. Extra transmission is plainly present in all cases. It is affected, though not seriously, by the size of the diaphragm apertures. It should be noted that the energy at the main receiver was greater for the apertures 70 × 60 than for the diaphragms wholly removed. This point will be discussed later.

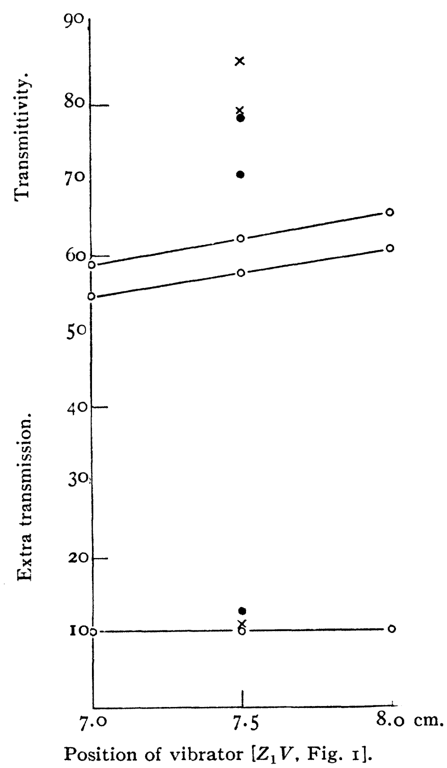

Fig. 5. Resonator grating, 13 columns of 39 each. Length of resonator, 5 cm.; width 0.2 cm. Distance between resonators, side-on, 2.8 cm., end-on, 1 cm. For the points marked by crosses the apertures D_1 and D_2 were 68.8 cm. × 60.5 cm.; circles, 47 × 38 cm.; dots, both diaphragms removed.

Schaefer explains extra transmission on the ground of the divergent properties of the waves employed by us, although he doesn't state why the waves sent out by a vibrator and collected by a receiver each of which is placed accurately within 0.5 mm. in the focal line of a parabolic mirror of 7.5 cm. focus should be divergent.

I cannot conclude by a study of his criticism that he refers to the well-known divergent property of electric waves in a direction parallel to the focal line of the parabolic mirrors. This latter property will also be discussed later. To test this explanation the vibrator was purposely moved from the focal line in both directions, the main receiver being left undisturbed at 7.5 cm. With apertures 47 × 38, the transmittivity for each glass separately (*i. e.*, the ratio between the energy at the main receiver through each glass compared to that through air) was increased in going from a divergent to a convergent wave but the extra transmission remained constant (Fig. 5).

It is well known that a vibrator formed of spherical balls shows greater irregularity in its action after a few readings than one having flat parallel surfaces at the spark gap. Accordingly a vibrator was made by boring the two $\frac{3}{8}$-inch balls and inserting steel cylinders, threaded and slightly conical, and projecting beyond the brass about one millimeter. The area of each of the cylinders at the spark gap was roughly 0.2 sq. mm. It was assumed that the length of the wave emitted by such a vibrator was not materially changed from 10 cm., but as I couldn't be sure of this it was thought better to replace the resonator system by a system consisting of strips 90 cm. long by 0.2 cm. wide, with a side-on distance of 2.8 cm.[1] Such a distribution should be independent of small variations in the wave-length. Moreover, it had been found[2] to give a larger extra transmission than that given by a system of double the resonance-length. Throughout the rest of this paper this latter system was used.

The results obtained with such a system are shown in Fig. 6. The arrangement of apparatus is shown in dotted outline in the figure, the location of the resonator system being varied between the diaphragms. The apertures 32 × 24 allowed so little energy to pass through that for them it was thought better to lay more stress on the extra transmission than on the transmittivity. Accordingly twenty-five readings were taken alternately thus: *BR*-

[1] The side-on distance used by Blake and Fountain was 2.8 cm., not 3.0 cm. as Dr. Schaefer understood. This would of course affect his arguments not at all.

[2] See Fig. 11, Blake and Fountain, page 273.

BRBRB, etc., for each of the points shown. To obtain an idea of the constancy of the readings and of the accuracy of the work the figures are given for the position of the resonator system nearest the main receiver. Ratio M/C for *B's*, 1.152, 1.168, 1.175, 1.089, 1.145, 1.147, 1.158, 1.121, 1.137, 1.112, 1.093, 1.118, 1.130. Ratio M/C for *R's*, 1.226, 1.229, 1.237, 1.187, 1.256, 1.262, 1.258, 1.274,

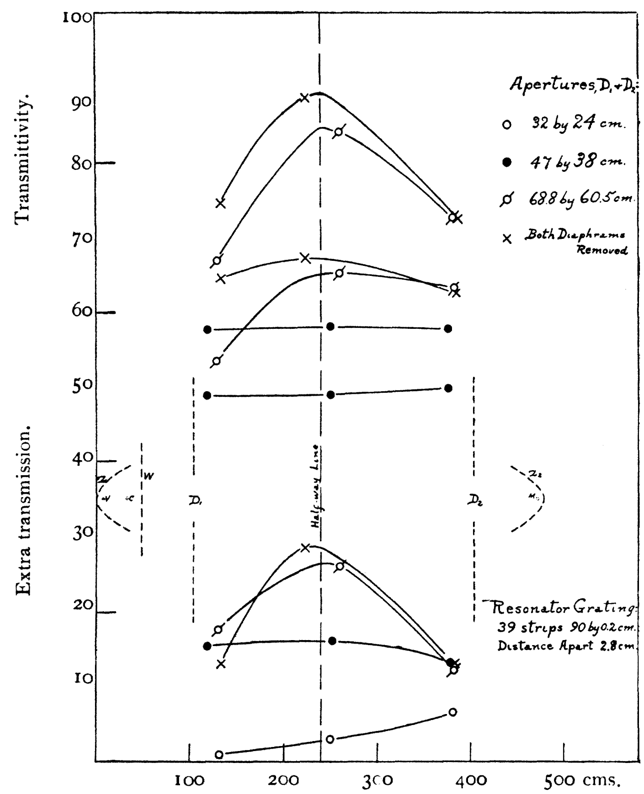

Fig. 6. In the curves in the upper part of the figure the lower curve of each pair refers to bare glass, the upper curve to the resonator glass.

1.208, 1.211, 1.226, 1.229. Mean R, 1.234; mean B, 1.134. R/B = 1.088; extra transmission reduced, 6.5 per cent. For apertures 47 × 38, the extra transmission as well as the transmittivity seemed more or less independent of the location of the resonator system. For apertures the size of those of the parabolic mirrors the extra transmission was larger when the resonator system was near the position half-way between vibrator and main receiver than for either of the two other positions. This increase in the extra transmission in the mean position was augmented when the diaphragms were removed, and so a more careful study was made of it. It should be noted from Fig. 6 how, for apertures 47 × 38 or larger, the extra transmission was practically constant at 13 per cent. for

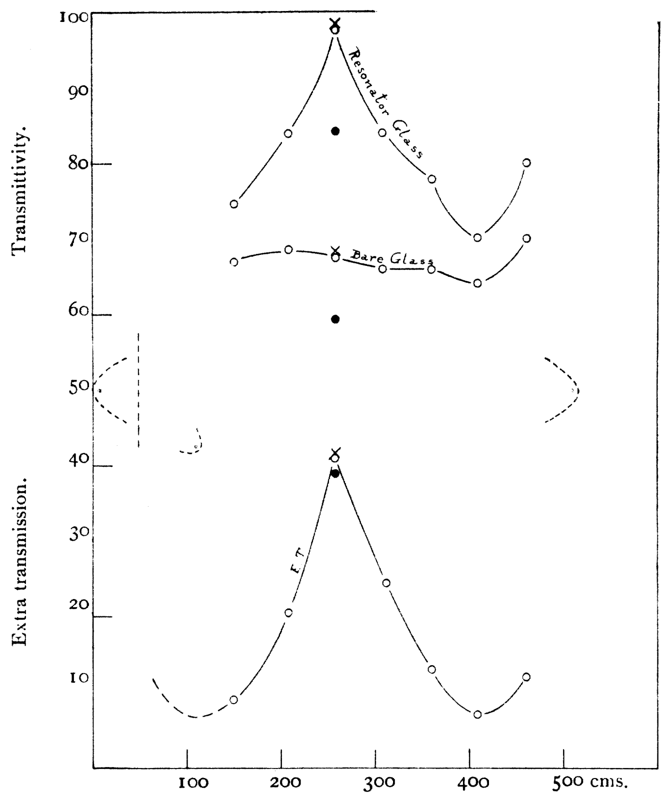

Fig. 7. Both diaphragms removed.

the positions of the resonator system nearest the main receiver. The fact that for apertures 68.8 × 60.5 cm. the transmittivity for the bare glass was a function of the location of the resonator system with respect to the diaphragms points to diffraction effects around the glass. I think that these diffraction effects, as will be seen later, occur around those edges of the glass which are at right angles to the electric force.

After changing the location of the check receiver to the position shown in Fig. 2 and in dotted outline in Fig. 7, the curves of Fig. 7 were taken with both diaphragms removed. These curves were taken before the great importance of *extremely* accurate adjustment was realized for this position of the resonator system half-way between the two parabolic reflectors. Each point of the curves was determined from the following series, *AARBRBRBRAA*, and so could not be much in error. However, the extra transmission curve of Fig. 7 shows the half-way position to be a critical one, one where it is very necessary when diaphragms are not used to have the straight line joining the vibrator and main receiver accurately divide the 90 cm. of the strip lengths into two equal parts. To illustrate, the points (circles o) of Fig. 7 having been taken with the resonator grating of 39 strips the top eight and the lower eight strips were removed, leaving 23 strips extending over 66.2 cm. space in a vertical direction, so that in a direction at right angles to the electric force the resonator grating was now smaller than the aperture (68.8 cm.) of the parabolic mirrors. With no great care being used in pushing the resonator plate into the path of the wave the black circles of Fig. 7 were obtained. A sideward adjustment of 2.3 cm. (determined by the use of plumb-lines at the centers of the vibrator, resonator-plate and main-receiver) gave the crosses shown in the figure. Repetition of the adjustment and non-adjustment confirmed this. To show clearly the need for this adjustment the actual figures are given in Table I. Fig. 7 shows clearly that when the proper care was used to have the straight line connecting the vibrator and main receiver pass through the center of the resonator system, the transmittivity was unaffected by the removal of the eight strips that extended above and below the apertures of the parabolic mirrors. Indeed, although without this adjustment the transmittivity for both the resonator and the bare glass is affected, the extra transmission is but slightly changed.

I can not ascertain from Schaefer's discussion of our experimental arrangements whether in his criticism of the resonator system's having a larger area than the aperture of the parabolic reflectors his objection refers so much (if at all) to the direction parallel to the focal line as to the direction at right angles to such line. If one

TABLE I.

Imperfect Adjustment (Displaced 2.3 cm.).					Plumb-line Adjustment.				
Medium Inserted.	Ratio M/C.	Means.	Ratios (Per Cent.).	Extra Transmission (Reduced).	Medium Inserted.	Ratio M/C.	Means.	Ratios (Per Cent.).	Extra Transmission (Reduced).
A	1.243	*A* 1.238			*A*	1.234	*A* 1.248		
A	1.213		*R*/*A*		*A*	1.261		*R*/*A*	
R	1.062		84.2		*R*	1.214		98.4	
B	0.748	*R*1.042			*B*	0.853	R1.227		
R	1.030		*B*/*A*		*R*	1.237		*B*/*A*	
B	0.719		59.4		*B*	0.846		68.2	
R	1.030	*B*0.735			*R*	1.229	*B*0.850		
B	0.738		*R*/*B*		*B*	0.850		*R*/*B*	
R	1.045		141.8	38.8 per	*R*	1.227		144.4	41.3 per
A	1.246			cent.	*A*	1.230			cent.
A	1.248				*A*	1.267			

is to limit the resonator system in the former direction to dimensions less than the mirror apertures in this direction it becomes at once important to ask oneself to what extent this dimension of the parabolic mirrors plays a part in his experiments. In other words, if the focal line be thought of as limited in its length by the two parallel bounding planes of the parabolic mirrors, what, expressed in wave-lengths, should be its length. The question is a fair one and has, so far as I know, never been investigated in electric-wave work. Although I think it is of some importance, yet I do not believe Schaefer had it in mind in expressing his objections. If he did, he might well have asked himself ought not he and Aschkinass and all the other investigators in this field to have employed paraboloidal rather than cylindrically parabolic mirrors?

The importance of side-adjustment was investigated more fully in Fig. 8. With the same distances and apparatus as in Fig. 7 (except that the bare glass alone was used) the central position was carefully determined by the use of plumb-lines in the manner already described. To eliminate vibrator-deterioration this central position was used as a check position after determining any two successive points of curve *B*, Fig. 8. The energy at the main receiver through the bare glass in the central position was chosen as 100. When the bare glass plate was 27 cm. out of center enough energy got around it by free radiation to give 100 at the main receiver again. The slope of curve B at the central position toward either side shows the need for accurate adjustment. Then the apparatus was changed to an exact duplication of the arrangement employed by Blake and Fountain for transmission (*l. c.*, Fig. 4). Employing the resonator glass alone a similar curve, R, Fig. 8, was obtained. However, it has far less slope than B, showing the smaller need for accurate side-adjustment as the resonator system is moved from the position half-way between the parabolic mirrors. Then although the vi-

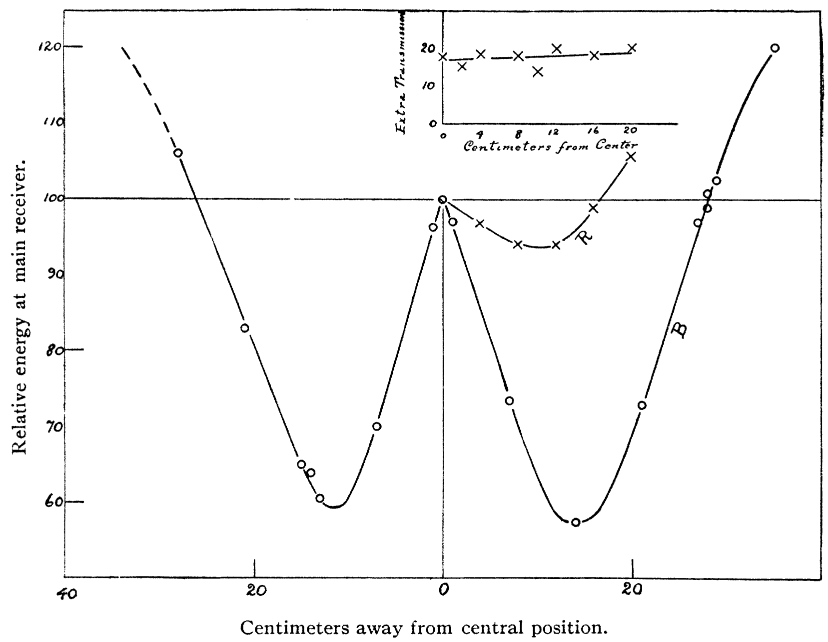

Fig. 8.

brator was pretty well worn and only a few readings were taken it was thought worth while to see to what extent the extra transmission was affected in the latter position by any lack of side-adjustment. Curve ET, Fig. 8, shows that it is practically unaffected, just as curve ET, Fig. 7, does for the half-way position. The transmittivity curves of Fig. 7 are subject to some correction due to lack of plumb-line side-adjustment. However, as I have just shown, curve ET of the same figure is free from this objection largely, since care was always used to have the bare glass plate duplicate *exactly* the position of the resonator plate in taking a set of alternate readings. The minimum point at 408 cm. in curve ET is undoubtedly present. At the half-way position the extra transmission is a maximum and is seen to be a very large effect, so large that for 23 strips 90 cm. long and 2.8 cm. apart the resonator glass comes within 2 per cent. of giving the full free radiation energy at the main receiver! Undoubtedly diffraction effects play some part at the main receiver and it is very probable that the amount of diffracted energy as well as its phase is a function of the index of refraction of the intervening obstacle. If this is so it is not easy if at all possible to separate diffraction and extra transmission phenomena.

Since, as Fig. 7 shows, the extra transmission is so large in the half-way position, the cause for this is naturally to be sought. The phenomenon may be said to be a sort of *lens action* of the resonator system. The front of the wave as it strikes the resonator system may be taken as cylindrical approximately, with the generating line of the cylinder at right angles to the tin-foil strips. Thus the plane system of long strips takes these cylindrical waves and converges them, still kept cylindrical, upon the main receiver mirror just as a cylindrical lens converges the waves from a line source of light. As I have said I cannot determine from Dr. Schaefer's criticism that it is *this* divergent property of electric waves from cylindrically parabolic reflectors that he refers to. Moreover, if this property be an error it is an error that is common to the work of all the previous investigators. For I doubt if Dr. Schaefer is able to tell us what the width of an aperture in a screen should be in a direction parallel to the vibrator axis to eliminate this property. For instance, had the focal line of each of my parabolic mirrors been 100 cm. in length, and thereby for the arrangement represented in Fig. 7 the condition imposed by Schaefer being fulfilled that the resonator system be smaller in area than the mirror apertures, I believe the curves of Fig. 7 would not have been materially changed.

If the *lens-action* of the resonator system is the proper explanation for this very large increase in the extra transmission when vibrator and receiver are equally distant from the system, I see no reason why this lens-action should not be present if one were to employ resonator strips in air instead of on glass. I mean to test this point as soon as may be feasible and to see how, starting with long strips, the lens-action if present is affected by successive shortenings of the strips. Probably there is a certain angle formed by the line joining vibrator to receiver and the line joining vibrator to either end of that particular strip in the same horizontal plane as the vibrator, where the effect is a maximum.

On account of the wave-front being cylindrical in form and the lens action of the resonator grating being thus for certain positions of the grating superimposed upon the true effect of extra transmission it seemed best to place the resonator grating as near as possible to the main receiver and by means of a single diaphragm of aperture 68.5 × 16 cm. to allow so small a portion of the wave-front through (except for diffraction effects due to the presence of the diaphragm) that the transmitted portion could be said to be plane in both directions of the aperture. This being done as shown in Fig. 3, the effect of changing the vibrator from its normal position was tried. In this work the usual series *AARBRBRBRAA* was taken for each position of the vibrator. Fig. 9 (circles o) shows the results. The extra transmission was about twelve per cent. (the vibrator was made of spherical balls without points) and was slightly larger for a convergent wave than for a divergent one. Then the effect of changing the width of the aperture was tried. Fig. 9 (crosses ×) gives the results. The extra transmission was constant at 11 per cent. though the transmittivity of both glasses varied. The relative energy at the main receiver for this last case as the width of the aperture was varied is plotted in curve *B*, Fig. 10. This curve is a free radiation curve through air, the energy at the main receiver for a width of aperture 32 cm. being taken as 100. The interposition of either the bare or resonator glass changed the character of the curve only for the larger apertures (see curve B' — here of course the energy through the bare or resonator glass for diaphragm aperture 32 was taken as 100). This curve was plotted incidentally from the results obtained from Fig. 9 (crosses ×) and as only four points were taken its character was not wholly determined. The fact that for an aperture 61 cm. the energy at the main receiver was only 77 per cent. of that for an aperture 32 cm. showed the presence of diffraction effects, as was to be expected. Other diffraction curves are shown in curves *A* and *C*, Fig. 10, *A* being taken with a diaphragm placed 15 cm. in front of the resonator position, Fig. 2. *C* was taken as shown in Fig. 4 in dotted outline. For all three curves of Fig. 10 the length of aperture was

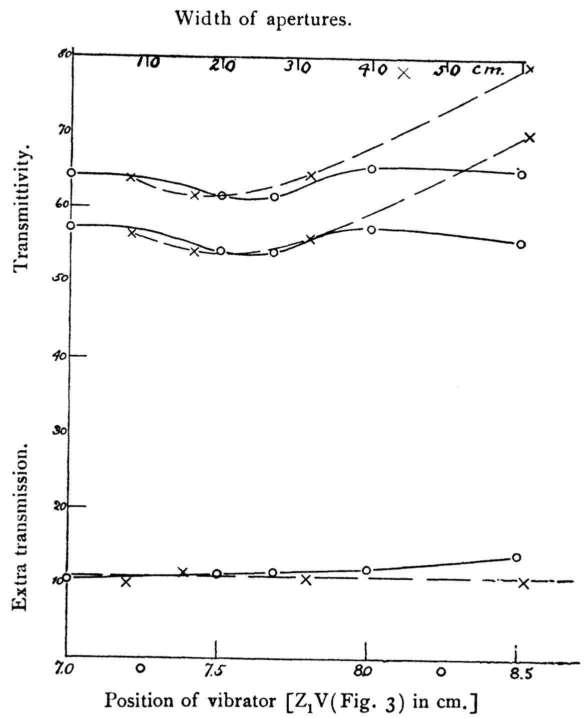

Fig. 9. For arrangement of apparatus see Fig. 3. Length (vertical) of aperture, 68.5 cm.

68.5 cm., the width alone being varied. *C* shows a diffraction maximum at 39 cm. and *A* at 63 cm.

Now theory shows[1] that if a is the distance of the vibrator and b that of the main receiver from the screen and δ the width of aperture then maxima occur when

$$\frac{\delta^2}{2\lambda}\frac{a+b}{ab} = \frac{3}{2} + 4h$$

[1] See, for example, Winkelmann's Handbuch der Physik, 2d edition, Vol. 6, p. 1060.

and minima when

$$\frac{\delta^2}{2\lambda}\frac{a+b}{ab} = \tfrac{7}{2} + 4h\,,$$

where h is a whole number. Of course both a and b are to have 15 cm. (twice the focal distance) added to them. Doing this and making $h = 0$, the figures for curve A are $a = 250$ cm., $b = 280$ cm., $\lambda = 10$ cm., whence $\delta_{\text{max.}} = 62.9$ cm., $\delta_{\text{min.}} = 96.1$ cm. For curve

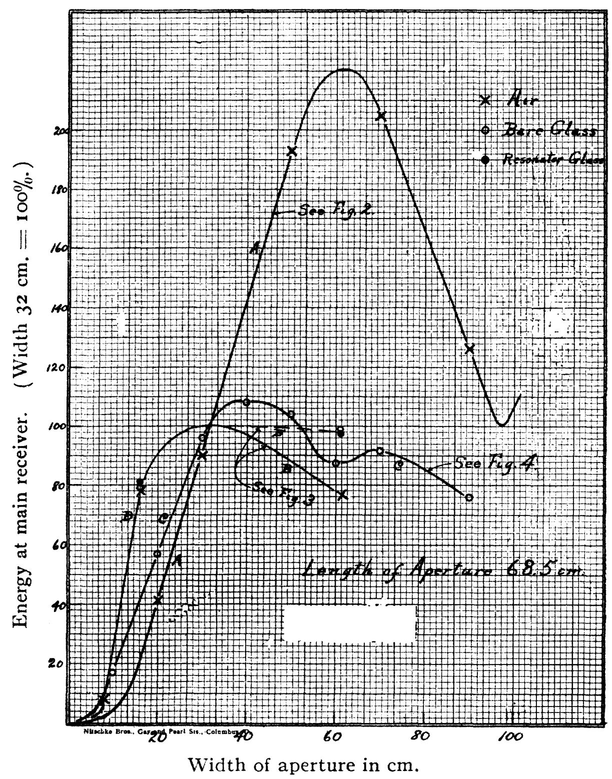

Fig. 10.

C, $a = 231$ cm., $b = 64$ cm. and $\delta_{\text{max.}} = 38.8$ cm., $\delta_{\text{min.}} = 59.2$ cm. There is thus for the maxima entire agreement between theory and experiment. Making the same calculation for curve B with $a = 451$, $b = 79$, $\delta_{\text{max.}}$ comes out 44.9 and $\delta_{\text{min.}} = 68.6$ cm. The maximum for B could easily be at 44.9 instead of at 32 as shown, since no points between 32 and 61 were determined. For *A* and *B* the minima are not yet reached in the curves. For *C* the theoretical minimum at 59.2 cm., though small, plainly shows itself. For *A* I have extrapolated the minimum at 96 cm.

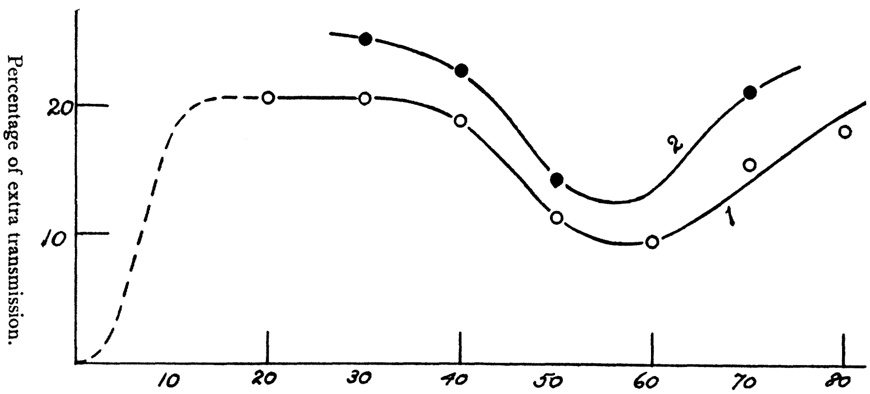

Fig. 11.

These curves of Fig. 10 show how strongly diffraction effects are present at the main receiver when a diaphragm is employed. Moreover, curve *B′* shows that the amount of diffracted energy is influenced by the presence of the resonator or bare glass, and in general in different degrees by the two glasses. Vice versa, the presence of diffracted energy influences in different degrees the energy that reaches the main receiver through the two glasses, as the curves of Fig. 11 clearly show. Curve 1 was taken with a vibrator having conical points about one mm. long, the parallel faces of the points having a cross-section of about 0.2 sq. mm. Each point on the curve was determined as the mean of the following series, *RBRBRBRBR*, and vibrator irregularity was eliminated by increasing the width of aperture by successive steps to the maximum width and then returning successively by steps to the minimum width. Relative distances for the apparatus are shown in Fig. 4 (vibrator mirror dotted), and plumb-line adjustment was used throughout. Curve 2 was taken under identical conditions except that the vibrator had 3 mm. points. The extra transmission is greater for the 3 mm. vibrator than for the 1 mm. vibrator, to be explained probably by the change in the length of the wave emitted. Fig. 11 shows plainly the influence of diffraction on extra transmission. Curve *C*, Fig. 10, is the diffraction curve obtained from the measurements taken for curve 1, Fig. 11. It is very significant that the minimum extra transmission occurs at 59 cm., the exact location of the diffraction band in curve *C*, Fig. 10. Weak though this band is, it has a very large effect on extra transmission.

If, then, diaphragms are to be employed what should be their aperture width? A study of the curves of Fig. 10 shows that, in order to be as free as possible from the influence of diffraction, an aperture width should be chosen which falls on the first straight line part of that diffraction curve which corresponds to the momentary arrangement of apparatus. Naturally, one would choose if possible the width such that the energy would be equal to that corresponding to the entire absence of diffraction bands. For instance in Fig. 10, curve *A*, the ordinate for minimum is 100, for maximum 220. This makes the ordinate corresponding to the asymptotic point of Cornu's spiral 151, thus approximately giving for the diffractionless width of aperture 42 cm. But this width for another arrangement of apparatus may not be justifiable at all (*e. g.*, that for curve *C*). On such reasoning one is permitted from curve *C*, Fig. 10, and hence in Fig. 11 to choose width 30 cm. Thus for a vibrator having 1 mm. points the true extra transmission is 20 per cent., for a 3 mm. vibrator 25 per cent. For a spherical ball vibrator without points the true extra transmission was not obtained in the way indicated above, but judging from the results of Fig. 9 it is distinctly smaller, say 12 per cent. Now the curves of Fig. 10 were taken with a 1 mm. point vibrator and the diffraction bands were calculated for $\lambda = 10$ cm., the wave-length determined by Blake and Fountain for a spherical ball vibrator; moreover, the observed and calculated diffraction bands were found to agree exactly. And yet the point vibrator gives greater extra transmission than the spherical ball vibrator of the same wave-length. Possibly the point vibrator throws a larger per cent. of its radiated energy into the equatorial region than does the spherical ball vibrator and hence the greater extra transmission. It is to be borne in mind, of course, that these values for the true extra transmission are given as correct only for the particular resonator system used in this paper. Fig. 11, curve 1, shows the same extra transmission for width 20 as for width 30 cm. It would seem that for smaller widths the extra transmission remains constant at 20 per cent. so long as one remains on the straight line part of the diffraction curve, *i. e.*, down to 13 cm. width. Below that it probably bends rapidly toward zero. I have thus extrapolated the curve in Fig. 11.

Keeping the width of aperture constant at 40 cm. I then tried to see the effect on extra transmission of moving the vibrator away from the resonator system. In doing this the relative distance between vibrator and check receiver was kept constant. Here to eliminate vibrator irregularity the vibrator was moved away and then nearer again by successive steps. The usual series of nine alternate readings were taken for each position. Fig. 12, curve 1, shows the extra transmission curve, the energy curve being curve 2. The sinusoidal nature of curve 1 is very marked, and I thought it could be explained by giving successive values to h in the diffraction formulæ above. But I haven't been able to make the figures fit the formulæ. However, it should be remembered that the diffraction system is a compound system consisting of the resonator or bare glass and the diaphragm.

From the first it was thought that in general the use of diaphragms was not justifiable and so with the arrangement of apparatus shown in Fig. 3, the following variation was tried. Diaphragm aperture 47×38 cm., extra transmission 17 per cent.; aperture 32×24 cm., extra transmission 9 per cent.; relative energy at main receiver, 3 to 1. Then a second screen was inserted 120 cm. in front of vibrator, aperture 32×24 cm. for both screens. Extra transmission 5 per cent.; relative energy at main receiver for one screen (the one near the receiver) as compared with that for two screens, 3 to 1. Thus one ninth as much energy gets to the main receiver with two screens of aperture 32×24 as with one of aperture 47×38. Moreover, the second screen cuts the extra transmission almost in two. All this shows, it seems to me, that the relative energy diffracted out into regions other than the main direction of the wave is much larger for the smaller aperture than for the large one, and hence the extra transmission is cut down. Each new aperture radically changes the character of the wave-front, diverting more and more of the energy from the equatorial path and the amount diverted would be affected differently by the resonator

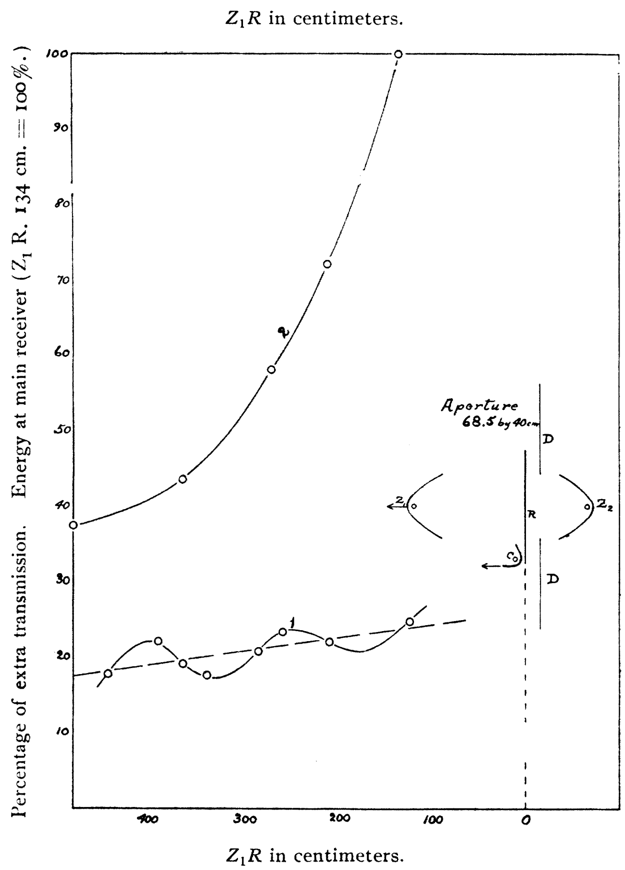

Fig. 12.

and bare glasses, hence the lowering of the extra transmission by using a second screen. Moreover, changing the length of the gap ought to introduce diffraction bands just as changing the width has been seen to do.

I contend, after these experiments, that in general in electric wave work of the sort here described, the use of diaphragms is *not justifiable*. The employment of two diaphragms is *never* justified, and if a single one is used, it should be placed as near to the main receiver mirror as possible and the width of its aperture should be such that diffraction bands are entirely absent at the receiver. If one uses cylindrically parabolic mirrors, the aperture *length*, should in my opinion, be at least equal to the height of the mirror aperture, although a small change of the length either way is probably immaterial. I have already explained how, in the most crucial position of the resonator system (the half-way position, Fig. 7) it was immaterial whether one employed a resonator system whose *length* was greater than or equal to the height of the mirror apertures. Doubtless the effect measured at the main receiver is dependent upon the *width* of the resonator system and its relative distance from the receiver. But so long as cylindrically parabolic mirrors are employed I see no criterion by which one may determine what the proper width of the resonator system is to be. To my mind it is meaningless to say that the width should be less than the width (dimension parallel to the focal line) of the parabolic mirrors.

In my opinion, for transmission work in electric waves it is far safer to employ no diaphragms but to have the transmission system as near as possible to the main receiver mirror and of such a *width* that it may properly be said to be infinite, thus preventing diffraction past the edges affecting the receiver. This could best be done by having the system practically touching the mirror and yet having the parabolic mirrors shallow.

Dr. Schaefer's objections consisted of two. He denied the existence of the phenomenon of extra transmission because he could not find it when he used diaphragms. And for good reasons. The apertures he employed were too small. The effect was there to the extent of 5 per cent. even with his arrangement, though it was not surprising he didn't find it. Had he studied the effect of the size of aperture he might have seen where the trouble lay. Nor is it surprising that Aschkinass and Schaefer[1] didn't find the phenomenon, although they just missed it in their curve 4, Fig. 2,

[1] Aschkinass and Schaefer, Ann. d. Phys., V., p. 489, 1901.

for a length 2 cm., twice the resonance length. Had they used an aperture in their diaphragm somewhat larger than 20 cm. on a side they doubtless would have found it. For the other curves the number of resonators used of double the resonance length was entirely too small and the side-on distance between them too great to show the effect. Nor does the fact that they didn't find it presuppose, as Schaefer argues, "large errors of observation" on their part.

Dr. Schaefer's second objection to our work lay in this, that when we changed resonator gratings, holding vibrator and receiver constant, we didn't change all distances and dimensions proportionately. He maintains, following the analogies[1] of optics, that in work with a resonator grating one can obtain correct results only when all dimensions of the grating are changed by the same relative amounts, using a constant vibrator and receiver. Before I can answer this objection I shall have to discuss the results that Schaefer obtained in his work[2] with resonator systems. With a constant resonator grating by means of a variable vibrator and receiver Schaefer determined the maximum absorption of a single column grating. By repeating this with a grating differing from the first only in that the resonators were closer together, he observed that the absorption maximum was displaced toward the smaller wave-lengths. By considering two adjacent resonators Schaefer explains this result theoretically as follows. Since the two resonators are entirely identical, if we represent the resistance, self-induction, mutual induction and capacity of each of the resonators by w, L_{11}, L_{12}, C respectively, these differential equations hold:

$$\frac{d^2 i_1}{dt^2} + \frac{w}{L_{11}} \frac{di_1}{dt} + \frac{i_1}{L_{11}C} = - \frac{L_{12}}{L_{11}} \frac{d^2 i_2}{dt^2},$$

$$\frac{d^2 i_2}{dt^2} + \frac{w}{L_{11}} \frac{di_2}{dt} + \frac{i_2}{L_{11}C} = - \frac{L_{12}}{L_{11}} \frac{d^2 i_1}{dt^2},$$

where i_1 and i_2 are the currents in the two resonators at the time t. Neglecting the resistance term the general integral is of the form

[1] For a view upon the closeness of these analogies see Webb and Woodman, PHYS. REV., Vol. XXIX., p. 90, 1909.

[2] Clemens Schaefer, loc. cit.

$$i_1 = A \cos 2\pi \left(\frac{t}{T_1} + \delta_1 \right) + B \cos 2\pi \left(\frac{t}{T_2} + \delta_2 \right),$$

$$i_2 = A \cos 2\pi \left(\frac{t}{T_1} + \delta_1 \right) - B \cos 2\pi \left(\frac{t}{T_2} + \delta_2 \right),$$

where A, B, δ_1, δ_2 are constants of integration and

$$T_1 = 2\pi \sqrt{(L_{11} + L_{12})C}, \quad T_2 = 2\pi \sqrt{(L_{11} - L_{12})C}.$$

Schaefer then says that since the two resonators are wholly identical and since, for both, the initial conditions are the same it follows that $i_1 = i_2$, that is, that $B = 0$ and hence that

$$i_1 = i_2 = A \cos 2\pi \left(\frac{t}{T_1} + \delta_1 \right).$$

Thus the free period of vibration of a resonator

$$T_0 = 2\pi \sqrt{L_{11} C_0},$$

is altered by the presence of the second resonator to the period T_1 above. Schaefer says further, "In distinguishing between T_1 and T_2 it must not be overlooked that the capacity C_0 is changed by the approach of the second resonator." By writing $i_1 = i_2$ he thus arbitrarily drops T_2, the second possible[1] period of vibration, and so *is forced* to explain the displacement of the absorption maximum upon the approach of the second resonator by the lowering of the capacity, that is, "as if $L_{12} = 0$." No wonder Schaefer found that in each of the twenty gratings that he worked with the influence of the capacity factor far exceeded that of the mutual induction! Surely, in considering the effect upon the absorption maximum of the approach of a second resonator, one cannot rightly do else than say that i_2 in the differential equations above is the current in the second resonator due to the impressed electromotive force in the first resonator. It is true, of course, that the total current in each resonator is the same, for their mutual effects are the same and both are subject to the same external force of the wave. But

[1] See A. Oberbeck. Ann. d. Phys., Vol. 55, p. 624, 1895, or Fleming, "Principles of Electric Wave Telegraphy," 1st edition, p. 209.

it is the mutual effects *alone* (in general *both* of capacity and induction) that enter into the question of the displacing of the absorption maximum.

Now it is well known[1] that the effective inductance of a circuit is *decreased* by the presence of a second circuit, and for currents of high frequency the amount of the effective inductance is $L_{11}-L_{12}^2/L_{11}$. That is, instead of Dr. Schaefer's value for T_1 above, the correct value should be

$$T_1 = 2\pi\sqrt{\left(L_{11} - \frac{L_{12}^2}{L_{11}}\right)C},$$

which shows that for two resonators approaching each other side-on, *both* the inductance and capacity tend to decrease the period. Now the fact that Curve A, Fig. 11, for Blake and Fountain was a straight line showed that the capacity of a resonator was practically unchanged by the *end-on* approach of a second resonator up to 5 mm. from the first. Accordingly we argued that for *side-on* approach of two resonators, since in our experiments they were never closer than 1 cm., any change in period must be attributed to inductance and not to capacity changes. By false theoretical considerations Schaefer was forced to explain the facts of experiment (which I do not at all deny) by capacity changes alone. I agree with him that for end-on approach one may safely take $L_{12} = 0$ and hence any change of period must be due to increase of capacity. If this is so I see no better way of trying to determine to what extent change of period is due to inductance and to what extent to capacity for side-on approach of resonators than by first determining within what limits changes of distance in end-on approach do not affect the capacity. This is what we did in curve A, Fig. 11, although I am free to say that we did not try this out in any thorough manner.

Only one other point in Schaefer's criticism needs to be mentioned. Is it necessary, as he insists, for satisfactory conclusions to be drawn, that in changing from one grating to another all dimensions and distances be changed relatively the same amount?

[1] See Maxwell, Phil. Trans., 1865. See also Lord Rayleigh, Phil. Mag., Vol. 21, p. 375, 1886.

I do not believe it is. Certainly one cannot stick too close to optical analogies in determining what are proper methods of experimentation. For, so long as optical resonators are in general very small compared with the wave-lengths they emit, while electrical resonators are comparable in size to the wave emitted, the analogies are certain to break down in many vital points just as Schaefer himself found to be the case in comparing the theory developed by Planck[1] with experiment.

In this connection Schaefer invites comparison between the curves of Blake and Fountain with those of Aschkinass and Schaefer as well as with his own. He insists that by keeping the same relative distances for all the gratings he and Aschkinass obtained relatively simple relations where we obtained complicated ones. Remembering that Blake and Fountain did not change the dielectric, what could be more simple than to expect minimum absorption for twice the resonance length? And yet for only two of the four curves does Fig. 2 of the work of Aschkinass and Schaefer show this minimum correctly. One has only to compare the character of their curve 2 with that of curve 3 for resonator lengths greater than 4 cm. to see how simple (?) the relation between them is. I venture the assertion that if Aschkinass and Schaefer had for all their gratings kept their resonators constant at 2 mm. width and the side-on distance between the resonators constant at 6 or 8 cm., then, starting with an end-on distance great enough so that capacity changes would not enter (say 2 cm.), had they proceeded to change the length only of their resonators they would have obtained the same ratio L_1 that their Table II. shows and far more satisfactory and comparable curves than their Fig. 2 shows.

Late as is the date, I believe that I have now satisfactorily refuted Schaefer's objections. It seems certain that the work of Blake and Fountain must stand practically as they left it. It remains only to say a word about Cartmel's paper.[2] Cartmel has endeavored to explain extra transmission on the ground that the phase relation between the energy reflected from the front and

[1] Planck, Sitzungsber. d. k. Akad. d. Wissensch. zu Berlin, Vol. I., p. 470, 1902; loc. cit., p. 480, 1903.

[2] Cartmel, PHYS. REV., Vol. XXV., p. 64, 1907.

back faces of the glass is affected by the presence of the resonators. I am unable at the present time to throw any light on the question as to which is the better explanation, Cartmel's or ours. Certainly it couldn't account for the *lens-action* of the resonator system. At most it could account for what I (as well as Cartmel) have called *true extra transmission*. But it seems no simpler than ours.[1]

I cannot close this paper without seconding the appeal made by Webb and Woodman[2] for a more systematic study of apparatus and conditions in the field of short electric waves. Certainly there have been great confusion and contradiction of experimental data and results. May we hope that in the future experimenters in this field will not take so much for granted as they have in the past.

To several of the teaching staff of the department I owe my best thanks for their help in various ways. Especially do I wish to thank Professor Kester, now of the University of Kansas, for much aid in taking the observations and for many valuable suggestions and criticisms.

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[1] Possibly Woodman and Webb (see Physical Review, Vol. XXX., page 561, 1910) are right in saying that phase change and change in velocity necessarily accompany each other, and hence that the two explanations are the same. Perhaps further work will give us clearer notions on the *modus operandi* of what we term "change of phase."

[2] Webb and Woodman, loc. cit.