polished surface is rapidly moved, as when a wheel with a polished spoke is rapidly rotated. Another interesting example in loci of brilliant points is that of a circular saw which has been polished with emery in a lathe and thus received a great number of concentric circular scratches. The locus of the brilliant points of this family of scratches was shown in this paper to be a curve of the fourth degree. In the special case when the point source of light and the eye of the observer (the point recipient) are in a plane through the axis of the saw, the curve degenerates into a circle and two coincident straight lines. A photograph of the saw curve has been taken in which the optical center of the camera lens is the point recipient. Other interesting facts and a number of geometrical constructions were also given in this paper.

Three persons were elected to active membership in the Academy.

WILLIAM TRELEASE,
Recording Secretary.

DISCUSSION AND CORRESPONDENCE.

THE ELECTRICAL THEORY OF GRAVITATION.

IT is, perhaps, by the severe but impartial criticism of his work that the greatest of all possible obligations is laid upon the scientific investigator, for thereby his theories are purged of what may be incorrect or trivial, and that part of them which may be true is compacted and separated from what might otherwise hide its value, and cause it to be neglected.

Unfortunately I have been unable to profit as much as I felt I had a right to expect from Dr. Franklin's letter, SCIENCE, December 7th, as he has apparently been unable to find time for that careful examination and study which the subject, aside from the paper, demands. It is a matter of regret, also, in view of Dr. Franklin's admirable qualifications for dealing with the question, that he should have directed his criticism, in every single case, against theories which are the exact opposite of those which I hold, and which I have explicitly set forth in the paper referred to.

But though Dr. Franklin has with some slight lack of courtesy invited his readers to 'ignore' my remarks on the methods by which my theory was deduced, I shall not return the compliment by 'ignoring' his criticism, because it contains a number of very serious misstatements which should be promptly pointed out, as otherwise they may become sources of error.

To consider, first, his criticism of my paper, he says (par. 1):

"Professor Fessenden in a recent number of SCIENCE discusses the nature and velocity of gravitation. There is, no doubt, something of value in Professor Fessenden's suggestions and much that is new. However, the explanation of gravitation which Professor Fessenden offers is by no means so adequate as would appear from Professor Fessenden's discussion."

On careful perusal we find his reasons for making this statement to be three in number. In regard to the first he says:

"If we admit that the diminution of volume of the ether at each point is proportional to the resultant intensity of the electric field, then the part of the energy which depends upon diminution of volume cannot be separated in its effects from the part of the energy which depends upon the shearing distortion, inasmuch as both are proportional to the square of the resultant field intensity. Therefore a diminution of volume of the ether could not explain gravitation, but would only be involved in the explanation of ordinary electrical attraction and repulsion."

But, *so far from my theory implying a diminution of the density of the ether at each point proportional to the resultant field intensity, F, I have expressly stated that the change of density is proportional to F^2*, as witness the following extracts from my paper:

"Whilst the one which is a density must decrease with the *second* power of the corresponding intensity."

"And hence, as my experiments prove, the change in density is proportional to the *square* of the electric intensity."

As a matter of fact, even a cursory examination of my paper will show that the whole point of my argument rests on the fact that it is the second and not the first power which is involved. For the qualitative equation is

$$M/L^3 = T^2/L^2 \times M/LT^2,$$

i. e., density varies with the inverse square of voltivity and directly as compressibility.

Since, then, my theory calls for a change in density proportional to F^2 and since compressional energy varies as the second power of the compression, my theory makes the compressional energy vary as the fourth power of the electric intensity.

Dr. Franklin later says (last par.) :

"If, however, the compressional energy were proportional to the fourth power of the resultant field intensity, then * * * gravitation would be provisionally explained."

Out of Dr. Franklin's own mouth, therefore, we have it that my theory provisionally explains gravitation.

As regards the second point, he says :

"Professor Fessenden, in his article referred to, speaks quite in general of the compression of the ether near a charged body, or ion, without localizing the distortion."

But this is not true. I have given the precise and exact distribution, par. 40, where I state,

"This change in density varies as the fourth power of the distance from the corpuscle."

I do not wish to complain, but no one cares to be continually misrepresented, and it is much to be regretted that Dr. Franklin was not able to note that I had covered the points he has criticised.

Thirdly, Dr. Franklin says (p. 889, 1st col. bottom):

"One might therefore expect that an hypothesis as to the constitution of matter which clears up the nature of inertia, even provisionally, would throw some light upon the nature of gravitation, but it does not seem to be so, and Professor Fessenden must needs say more from his point of view before we will be convinced."

But this is just what my theory *does* do. As I have pointed out elsewhere this is one of the very strongest points in favor of my theory, and in the paper criticised I have explicitly stated this, as, to quote (par. 41):

"The inertia of the atom is due to the electromagnetic inductance of the corpuscular charge, and gravity is due to the change of density of the ether surrounding the corpuscles, produced by the electrostatic stress of the corpuscular charge. *Mass and gravity thus bear a constant ratio.*"

Having thus answered all of Dr. Franklin's objections to my theory, I must now call attention to some very serious misstatements.

To take the first one. He says (p. 887, par. 2):

"Professor Fessenden claims to have derived *numerical functional relations* [the italics are Professor Franklin's] with the aid of his Qualitative Mathematics."

Now it is very wrong to say this. If I were to write an article in a scientific paper, stating that Dr. Franklin believed that the earth was flat, and after stating that this was believed by scientists to be impossible, 'on definite rational grounds,' and inviting my readers 'to ignore' his arguments, Dr. Franklin would justly consider that he had reason for complaint. But he would not have so much of reason as I have, for whilst Dr. Franklin has never, to my knowledge, published his opinions on this matter, I have, in no less than four papers, *explicitly stated views which are the exact reverse of those Dr. Franklin attributes to me.* In the very paper he is criticising I say (par. 5) :

"Qualitative Mathematics, as its name signifies, is used, *not for the exact determination of numerical values*, but for the prediction and classification of phenomena."

I really could not put it any plainer. I do not see why Dr. Franklin makes his statement. He cannot point to *any* statement or *any* work which I have ever done in which I have tried to deduce numerical relations by means of qualitative mathematics.

So far from this being the case, I have frequently stated exactly the opposite. As instance the above quotation. Also in my paper on the 'Nature of Electricity and Magnetism,' *Phys. Rev.*, Jan., 1900. Also, in my paper in the *Electrical World*, of some years ago, I point out very specifically that this same numeral coefficient, which Dr. Franklin says I have overlooked, cannot be determined by Qualitative Mathematics, and I then go on to point out that since, this coefficient being of zero quality, we can always make it equal to unity by choosing suitable units, it is a matter of no consequence in discussing the *nature* of phenomena, however important it is as regards the *quantity* of the action. *Thus, so far from overlooking it, I called attention to it, no less than four years before Lord Rayleigh did, in the article of his which Dr. Franklin quotes.*

So much for my theory. As regards my practice, one has only to read any of the papers in which I have used this method to see that I have never used other than experimental means to determine this unknown coefficient of zero dimensions. For example, in my paper on the nature of electricity, I first show that specific inductive capacity k is a density, and I then find, *by experiment*, what numeric k must be multiplied by to get the actual value of that density. Similarly, in the same paper, having shown that the magnetic coefficient a has the quality of hysteresis, I then proceeded, *by experimental means*, to find what the numerical relation between the two is.

A second misstatement is the following (p. 888, par. 4):

"Maxwell showed that the mechanical stresses in the dielectric tend to produce a diminution of volume."

There are no less than three mistakes in these lines. In the first place Maxwell never showed anything of the kind, and Dr. Franklin cannot refer to any passage in his writings where any change of volume, due to the electrically produced dielectric stresses is even hinted at. Second, the Maxwell stresses are incapable, as has been pointed out by several eminent physicists, of giving any diminution in volume, except on making assumptions not contained in Maxwell's theory or in his writings. Thirdly, the change in volume is not a diminution, but in the most general case an expansion, and only under certain conditions does it become a diminution.

Quinke had previously worked along that line, and found that some dielectrics expanded and others contracted, but did not give the law of the change. It was not until I had shown that contracting dielectrics behaved as negative uniaxial crystals in Kerr's phenomena, and did not obey the Maxwellian law, $1/\sqrt{k\mu}=$ velocity of light, whilst expanding dielectrics behaved as positive crystals and did obey that law; also that the compression depended upon the square of the electric intensity and the compressibility, and that mixtures and ionized compounds contracted whilst pure dielectrics expanded, that the phenomenon was exactly formulated, by me, as follows:

"All simple non-ionized dielectrics expand under electric stress, the change in volume being proportional to the square of the electric intensity, and inversely as the compressibility; they act as positive uniaxial crystals in Kerr's phenomenon, and obey Maxwell's law for the refractive index."

On p. 888, par. 2, he says:

"Physicists have known for many years" that attraction is to be attributed to ether energy, which decreases as the bodies approach each other."

Has this statement any basis of fact? Can Dr. Franklin adduce a particle of evidence to show that Hick's bubble theory, as developed by McAulay, or Newton's, or that theory of Bjerknes's which Larmor seems to regard with some favor, is more probable than the old corpuscular one (of le Sage? I write away from my books). As a matter of fact, can Dr. Franklin refer to the slightest evidence that ether is necessary to transmit gravitational force? And if Kelvin's value for the ether constants were correct, would this not be very improbable? And had it ever been shown that the ether has the properties requisite to do it, before I showed it, a couple of years ago?

Still one more point, and I mention this because I think Dr. Franklin has been a little unfair. In writing of the electrical theories of matter, he does it in such a way, no doubt unintentionally, as to convey the impression that the theories I have advanced are not original with me, but form a part of the common scientific stock of knowledge. For instance, in speaking of the 'electrical hypothesis of the constitution of matter.'

But it was the writer who first introduced the idea of the universal association of the electrical charge with matter. I believe that it is a fact that Dr. Franklin cannot refer to a single sentence in all scientific literature in which this theory was put forward, still less any proof given, prior to my papers of 1891 and 1892 in the *Electrical World* and SCIENCE, with their contained proofs. Prior to that date the ionic charge had never been considered in connection with the atom save in relation to chemical and molecular effects.

The last statement I shall criticise is the following: He says (p. 888, 3d par.):

"It is now pretty well established that the ether energy having to do with electrical attraction and repulsion is dependent upon a sort of *shearing distortion* of the ether unaccompanied by any sensible diminution of volume, that this ether distortion is what is known as *electric field*, that the propagation of this energy constitutes *electrical waves*, and that the movement of the ether which comes into play during the establishment of this shearing distortion, or which comes into play while distortion at one place is relieved and distortion at a contiguous place is built up, is what is known as *magnetic field*."

Surely not!! So far from being established, Dr. Franklin cannot adduce the slightest particle of evidence for it. Though Maxwell and Lodge have used this theory, yet both Lord Kelvin and Professor J. J. Thomson have suggested exactly the opposite theory, and Heaviside has pointed out (Electromagnetic Theory, Vol. 1), that the theory which Dr. Franklin states is 'pretty well established' is at present as hard to reconcile with the facts as the other theory, so that the weight of authority would appear to be fairly evenly divided. And one of our greatest living physicists, J. J. Thomson, uses the opposite theory, of late exclusively. Moreover I have elsewhere pointed out that the variation of μ with the first power and of k with the second power is conclusive proof that the opposite theory is true.* If we chose to be unconvinced by this, then there is not the slightest evidence one way or another, and Dr. Franklin can add considerably to his already brilliant reputation by producing some evidence in favor of his statement.

REGINALD A. FESSENDEN.

* Those who are acquainted with my work on the nature of electricity and magnetism may remember that the proof that magnetism was a shear was based upon the following:

(a) The determination of the fact that either k or μ must be a density, thus confirming Williams's result.

(b) The demonstration of the fact that whichever one of the two k or μ is a density, must depend upon the first power of the corresponding force, whilst the other must depend upon the second power of the corresponding force.

(c) The experimental determination of the fact that μ varies with H whilst k varies with F^2.

A second proof was then indicated, depending upon the nature of the Lagrangian terms involved in the change of k and μ in elastic phenomena.

I have now to add a third. Briefly stated it is as follows: Since either k or μ is a density, then either H must be a shearing stress and F a velocity or *vice versa*. It is next shown that in the electric current we have a non-conservative system, and from quite general principles it is shown that it is the non-conservative system which must involve the velocities. And it is shown that under no circumstances could the equation expressing the amount of the I^2R loss be of the form it is if F were a shear, since in that case an operator which experiment shows is attached to an electric term would be connected with a magnetic term instead.

A BIBLIOGRAPHIC CATCH TITLE FOR THE YEARS 1900 to 1999.

IN a note published in SCIENCE, May 11th, I called attention to a bibliographic matter which I wish to return to again.

Some twenty years ago I adopted the plan of placing all bibliographic titles at the end of an article in a single list with authors' names arranged alphabetically and each author's papers arranged chronologically. As an essential part of the plan, the citation in the text consisted simply in giving the author's name and the last two figures of the year of publication preceded by an apostrophe. To avoid ambiguity, in case two or more cited papers were published by an author in one year, the abbreviated dates were followed by a lower-case letter used as an exponent. This plan has been kept up since then in the 'Contributions from the Zoological Laboratory at Harvard College.' Owing to its simplicity and the evident advantage which it gives the reader by acquainting him at once with the date of the paper cited, this plan has come into rather common use.

The apostrophe used to mark the omission of the first two figures of the year-date could not be used without ambiguity for dates subsequent to 1899, and I have consequently urged in the note