widely different physical conditions. As the temperature of the flame of cyanogen probably approaches the temperature of the carbon poles of the electric arc, and as we have shown that carbon undoubtedly exists in the form of vapour in the arc discharge, from the fact of the ultra-violet line spectrum being present, the question naturally arises, is carbon present in the form of vapour in the cyanogen flame? In order to answer this question we have taken photographs of the ultra-violet spectrum of the cyanogen flame fed with oxygen, and with long exposures have had no difficulty in detecting one of the strongest carbon lines, viz., that at 2478·3, along with a trace of what may be the pair of lines at 2837, but more probably is a mercury line. No other carbon line was found in the photographs. It seems, therefore, proved that carbon vapour does exist in the flame of cyanogen, although to a much smaller extent than in the arc discharge. Observations must be made on the spectra of flames under high pressures, in order to solve many problems connected with spectroscopic enquiry, and this subject we hope to discuss in a future communication.

XIII. "Further Observations upon Liquid Jets, in continuation of those recorded in the Royal Society's 'Proceedings' for March and May, 1879." By Lord Rayleigh, F.R.S., Professor of Experimental Physics in the University of Cambridge. Received June 8, 1882.

The experiments herein described were made in the spring and summer of 1880, with the assistance of Mrs. Sidgwick. Section 2 was indeed written out as it now stands in August of that year. There were some other points which I had hoped to submit to examination, but hitherto opportunity has not been found.

On some of the Circumstances which influence the Scattering of a nearly Vertical Jet of Liquid.

§ 1. It has been already shown that the normal scattering of a nearly vertical jet is due to the rebound of the drops when they come into collision. If, by any means, the drops can be caused to amalgamate at collision, the appearance of the jet is completely transformed. This result occurs if a feebly electrified body be held near the place of resolution into drops, and it was also observed to follow the addition of a small quantity of soap to the water of which the jet was composed. In trying to repeat the latter experiment in May, 1880, at Cambridge, I was astonished to find that even large additions of soap failed to prevent the scattering. Thinking that the difference might be connected with the hardness of the Cambridge water—at home I had used rain water—I repeated the observations with distilled water, but without finding any explanation. The jet of distilled water scattered freely, both with and without soap, and could only be prevented from doing so by electricity. Eventually the anomalies were traced to differences in the character of the soap. That used at Cambridge up to this point was a clarified specimen prepared for toilet use. On substitution for it of common yellow soap, the old effects were fully reproduced.

Further experiment seemed to prove that the real agent was not soluble soap at all. If water impregnated with the yellow soap was allowed to stand, a white deposit separated, after which the supernatant liquid was found to be inactive. But after shaking up the same effects were produced as at first. The addition of caustic potash to the unclarified soapy mixture destroyed its power. On the other hand, sulphuric acid rendered the clarified soap solution active.

The natural conclusion from these facts would be that the real agent is unsaponified greasy matter distributed through the liquid; and this view is confirmed by the striking results which follow the addition of small quantities of milk. The experiment may be made conveniently by connecting a Woulf's bottle with the water tap by a rubber tube fitted to one tubulure, while the vertical nozzle is in connexion with another tubulure. If a little milk be placed in the bottle, the jet of opalescent liquid apparently coheres, and passes the summit in one unbroken stream. After a time the milk is gradually washed out, and the scattering is re-established. About one drop of skimmed milk per ounce of water is sufficient to produce the effect.

I must not omit to mention that on several occasions distinct evidence was obtained that it is possible for soap to be in excess. With a large quantity the coherence of the jet was imperfect, and was improved by dilution. The complete elucidation of the subject probably requires more chemical knowledge and experience than is at my command.

Of the various other substances which have been tried, such as glycerine, sugar, gum arabic, alcohol, sulphuric acid, none have been found active.

Vertical fountains of mercury were found not to scatter. The head was about 15 inches, and various glass nozzles were used from $\frac{1}{20}$ inch to $\frac{1}{50}$ inch in diameter. Also a nozzle terminating in an amalgamated brass plate, through which a hole of $\frac{1}{20}$ inch was pierced. In all these cases the drops of mercury coalesced at collision, behaving in the same way as drops of milky water issuing from the same nozzles. Fountains of clean water issuing from these nozzles under the same pressure scattered freely.

When the diameter of the nozzle from which a water jet issues is reduced to below $\frac{1}{100}$ inch, the scattering cannot be completely prevented by the presentation of an electrified body. One possible reason for this is evident. The mutual repulsion of the similarly electrified drops increases rapidly relatively to the masses as the size is reduced, and thus it may happen that before the *differential* electrification sufficient to rupture the separating envelope at contact is arrived at, the repulsion may be powerful enough to prevent most of the drops from coming into contact at all. In connexion with this it may be remarked that two perfectly equal and equally electrified spheres would repel one another at all distances; but that if there be the slightest difference in the size or electrification, the repulsion will be exchanged for attraction before actual contact is attained. This attraction will be local, and thus the opposed parts of the surfaces may come into contact with considerable violence, even when the relative motion of the centres of the masses is small. It is easily shown experimentally (see § 4) that violence of contact tends to promote coalescence, so that we have here a possible explanation of the action of electricity.

With respect to the persistent scattering of very fine jets, however, it would appear that the principal cause is simply that many of the fine drops fail to come into contact in any case. The capillary forces act with exaggerated power, and doubtless impress upon the minute drops irregular lateral velocities, which may easily reach a magnitude sufficient to cause them to clear one another as they pass. At any rate little difference is observable in this respect between a fine jet of clean water under feeble electrical influence, and one to which a little milk has been added, but without electrification.

With a suitable jet, say from a nozzle about $\frac{1}{20}$ inch diameter, and rising about 2 feet, the sensitiveness to electricity is wonderful, more especially when we remember that the effect is differential. I have often caused a jet to appear coherent, by holding near the place of resolution a brass ball about 1 inch in diameter, supported by a silk thread, and charged so feebly that a delicate gold-leaf electroscope would show nothing. Indeed, some care is necessary to avoid being misled by accidental electrifications. On one occasion the approach of a person, who had not purposely being doing anything electrical, invariably caused a transformation in the appearance of the jet.

The jets hitherto under discussion are such as resolve themselves naturally into drops soon after leaving the nozzle, or at any rate before approaching the summit of their path. If the diameter be increased, we may arrive at a condition of things in which the undisturbed jet passes the summit unbroken. In such a case the addition of milk, or the presentation of an electrified body, produces no special effect. One interesting observation, however, may be made. By the action of a vibrator of suitable pitch, *e.g.*, a tuning-fork, resolution on the upward path may be effected. As the vibration gradually dies down, the place of resolution moves upwards, but it cannot pass a certain point. When the point is reached, resolution into actual drops ceases, the upper part of the jet exhibiting simple undulations, when viewed intermittently. The phenomenon is in perfect harmony with theory. As it leaves the nozzle, the jet is unstable for the kind of disturbance imposed upon it by the vibrator. The subsequent loss of velocity, however, shortens the wave-lengths of disturbance, until at length they are less than the circumference of the jet, after which the disturbance changes its character from unstable to stable. The vibrator must evidently produce its effect quickly, or not at all.

Influence of Regular Vibrations of Low Pitch.

§ 2. Towards the close of my former paper on the capillary phenomena of jets, I hazarded the suggestion that the double stream obtained when an obliquely ascending jet is subjected to the influence of a vibration, an octave graver than the natural note, is due to the *compound* character of the vibration. At the time of Plateau's researches the fact that most musical notes are physically composite was much less appreciated than at present, and it is not surprising that this point escaped attention. I have lately repeated Plateau's experiments under improved conditions, with results confirmatory of the view that no adequate explanation of the phenomena can be given which does not have regard to the possible presence of overtones; and I have added some observations on the effects of the simultaneous action of two notes forming a consonant chord.

In order to make a satisfactory examination of it, it is necessary to employ some apparatus capable of affording an intermittent view of the jet in its various stages of transformation. In the experiments formerly described I used sparks from an induction coil, governed by the same tuning-fork which determined the resolution of the jet. This has latterly been replaced by a perforated disk of black card-board, driven at a uniform speed by a small water-motor. The diameter of the holes is one-fifth of an inch—about that of the pupil of the eye, and the interval between the holes is about four inches. Examined under these conditions the jet and resultant drops are sufficiently well defined, and there is abundant illumination if the apparatus is so arranged that the jet is seen projected against the sky. The speed of the motor is regulated so that there is one view through the holes in about one complete period of the phenomenon to be observed. If the power is a little in excess, the application of a slight friction to the axle carrying the disk renders the image steady, or, what is better, allows it to go forwards through its phases with moderate slowness.

Although the multiple streams are better separated when the jet is originally directed upwards at an angle of about 45°, I preferred to use a horizontal direction as giving simpler conditions. The velocity and diameter are then practically constant throughout the transformation, and may be readily calculated from observations of the head and of the total quantity of fluid discharged in a given time. The reservoir consisted of a large glass bottle, provided with a tubulure near the bottom. Into this was fitted a 1-inch brass tube, closed at the end by a flat plate, in which a circular aperture was pierced of about $\frac{1}{12}$ of an inch in diameter.

If h=head,

d=diameter of *jet*,

v=velocity of issue,

V=volume discharged in unit time,

then $$\tfrac{1}{4}\pi d^2 v = \mathrm{V}, \qquad v = \sqrt{(2gh)}.$$

Again, if N′ be the frequency of the most rapid vibration which can influence the jet, we have by Plateau's theory—

$$\mathrm{N}' = \frac{v}{\pi d} = \frac{v^{\frac{3}{2}}}{2\sqrt{(\pi \mathrm{V})}} = \frac{(2gh)^{\frac{3}{4}}}{2\sqrt{(\pi \mathrm{V})}}.$$

If N be the frequency of the principal note of the jet, then, as explained in my former paper,

$$\mathrm{N} = \frac{3 \cdot 142}{4 \cdot 508}\mathrm{N}'.$$

In the present experiment it was found that 1050 cub. centims. were discharged in four minutes, and the head was $7\frac{3}{4}$ inches, so that in C.G.S. measure—

$$\mathrm{V} = \frac{1050}{240}; \qquad h = 7\tfrac{3}{4} \times 2 \cdot 54; \qquad g = 981;$$

whence $$\mathrm{N}' = 372, \qquad \mathrm{N} = 259.$$

As sources of sound tuning-forks, provided with adjustible sliding pieces, were employed, except when it was important to eliminate the octave as far as possible; the vibration was communicated to the reservoir through the table on which it stood. The forks were either screwed to the table and vibrated with a bow, or mounted on stands (resting on the table) and maintained electrically. The former method was quite adequate when only one fork was wanted at a time.

With pitches ranging from 370 to about 180, the observed phenomena agreed perfectly with the unambiguous predictions of theory. From the point—decidedly below 370—at which a regular effect was first obtained, there was always one drop for each complete vibration of the fork, and a single stream, every drop breaking away under the same conditions as its predecessor. After passing 180 it becomes a question whether the octave of the fork's note may not produce an effect as well as the prime. If this effect be sufficient the number of drops is doubled, and unless the prime be very subordinate indeed, there is a double stream, alternate drops taking sensibly different courses. In these experiments the influence of the prime was usually sufficient to determine the number of drops, even in the neighbourhood of pitch 128. Sometimes, however, the octave became predominant, and doubled the number of drops. It must be remembered that the relative intensities with which the two vibrations reach the jet depend upon many accidental circumstances. The table has natural notes of its own, and even the moving of a weight upon it may change the conditions very materially. When the octave is not strong enough actually to double the drops, it often produces an effect which is very apparent to an observer examining the transformation through the revolving holes. On one occasion a vigorous bowing of the fork which favours the octave, gave at first a double stream, but this after a few seconds passed into a single one. Near the point of resolution those consecutive drops which ultimately coalesce as the fork dies down, are connected by a ligament. If the octave is strong enough this ligament breaks, and the drops are separated, otherwise the ligament draws the half-formed drops together, and the stream becomes single. The transition from the one state of things to the other could be watched with facility.

In order to get rid entirely of the influence of the octave a different arrangement is necessary. It was found that the desired result could be arrived at by holding a 128 fork in the hand over a resonator of the same pitch resting on the table. The transformation was now quite similar in character to that effected by a fork of frequency 256, the only differences being that the drops were bigger and twice as widely spaced, and that the *spherule*, which results from the gathering together of the ligament, was much larger. We may conclude that the cause of the doubling of a jet by the sub-octave of the note natural to it is to be found in the presence of the second component, from which scarcely any musical notes are free.

When two forks of pitches 128 and 256 were sounded together, the single or double stream could be obtained at pleasure by varying the relative intensities. Any imperfection in the tuning is rendered very evident by the behaviour of the jet, which performs evolutions synchronous with the audible beats. This observation, which does not require the aid of the revolving disk, suggests that the effect depends in some degree upon the relative phases of the two tones, as might be expected *à priori*. In some cases the influence of the sub-octave is shown more in making the alternate drops unequal in magnitude, than in projecting them into very different paths.

Returning now to the case of a single fork screwed to the table, it was found that as the pitch was lowered below 128, the double stream was regularly established. The action of the *twelfth* below the principal note ($85\frac{1}{3}$) demands special attention. At this pitch we might in general expect the first three components of a compound note to influence the result. If the third component were pretty strong it would determine the number of drops, and the result would be a three-fold stream. In the case of a fork screwed to the table the third component of the note must be extremely weak, if not altogether missing; but the second (octave) component is fairly strong, and in fact determines the number of drops ($190\frac{2}{3}$). At the same time the influence of the prime ($85\frac{1}{3}$) is sufficient to cause the alternate drops to pursue different paths, so that a double stream is observed.

By the addition of a 256 fork there was no difficulty in obtaining the triple stream, but it was of more interest to examine whether it were possible to reduce the double stream to a single one with only $85\frac{1}{3}$ drops per second. In order to secure as strong and as pure a fundamental tone as possible, I cause it to act in the most favourable manner upon the jet, the air space over the water in the reservoir was tuned to the note of the fork by sliding a piece of glass over the neck so as partially to cover it. When the fork was held over the resonator thus formed, the pressure which expels the jet was rendered variable with a frequency of $85\frac{1}{3}$, and overtones were excluded as far as possible. To the unaided eye, however, the jet still appeared double, though on more attentive examination one set of drops was seen to be decidedly smaller than the other. With the revolving disk, giving about eighty-five views per second, the real state of the case was made clear. The smaller drops were the *spherules*, and the stream was single in the same sense as the streams given by pure tones of frequencies 128 and 256. The increased size of the spherule is of course to be attributed to the greater length of the ligament, the principal drops being now three times as widely spaced as when the jet is under the influence of the 256 fork.

With still graver forks screwed to the table the number of drops continued to correspond to the second component of the note. The double octave of the principal note (64) gave 128 drops per second, and the influence of the prime was so feeble that the duplicity of the stream was only just recognisable. Below 64 the observations were not carried. Attempts to get a single stream of 64 drops per second were unsuccessful, but it is probably quite possible to do so with vibrations of greater power than I could command.

In the case of a compound note of pitch 64 a considerable variety of effects might ensue, according to the relative strengths of the various components. Thus, the stream might be single (though this is unlikely), double, triple, four-fold, or even five-fold, with a corresponding number of drops.

Observations were next made on the effects of chords. For the chord of the fifth the pitches taken were 256 and $\frac{2}{3}\times 256$. The two forks could be screwed to the table and bowed, or, as is preferable (especially in the case of the chords of the fourth and third to be spoken of presently), maintained in vibration electromagnetically by a periodic current from a break-fork of pitch $85\frac{1}{3}$, standing on another table. The revolving disk was driven at such a speed as to give about eighty-five views per second. As was to be expected, the number of drops was either 256 in a triple stream, or $\frac{2}{3}\times 256$ in a double stream, according to the relative intensities of the two vibrations. With the maintained forks the phenomenon is perfectly under control, and there is no difficulty in observing the transition from the one state of things to the other.

In like manner with forks 256 and $\frac{3}{4}\times 256$, driven by fork 64, and with sixty-four views per second, the stream is either triple or quadruple; and with forks 256 and $\frac{4}{5}\times 256$, we get at pleasure a four-fold or five-fold stream. To obtain a good result the intervals must be pretty accurately tuned. In the case of electrically maintained forks, the relative phase remains unchanged for any length of time, and the spectacle seen through the revolving holes is one of great beauty.

The actual results obtained experimentally by Plateau differ in some respects from mine, doubtless in virtue of the more composite character of the notes of the violoncello employed by him, but they are quite consistent with the views above expressed. The only point as to which I feel any difficulty relates to the single stream, which occasionally resulted from the action of the twelfth below the principal note. It seems improbable that this could have been a single stream of the kind that I obtained with some difficulty from a pure tone; indeed the latter would have been pronounced to be a double stream by an observer unprovided with an apparatus for intermittent views. I should rather suppose that the number of drops really corresponded to an overtone, and that from some accidental cause the divergence of what would generally be separate streams failed to be sensible.

The Length of the Continuous Part.

When a jet falls vertically downwards, the circumstances upon which its stability or instability depend are continually changing, more especially when the initial velocity is very small. The kind of disturbance to which the jet is most sensitive as it leaves the nozzle is one which impresses upon it undulations of length equal to about four and a-half times the initial diameter. But as the jet falls its velocity increases (and consequently the undulations are lengthened), and its diameter diminishes, so that the degree of instability soon becomes small. On the other hand, the kind of disturbance which will be effective in a later stage is altogether ineffective in the earlier stages. The change of conditions during fall has thus a protective influence, and the continuous part tends to become longer than would be the case were the velocity constant, the initial disturbances being unaltered.

I have made many attempts to determine the origin of the disturbances which remain in operation when the jet is protected from ordinary tremors, but with little result. By suspending the reservoir with india-rubber straps, &c., from the top of a wooden tripod, itself resting upon the stone floor of one of the lower rooms of the Cavendish Laboratory, a considerable degree of isolation was attained. A stamp of the foot upon the floor, or the sounding of a note of suitable pitch of moderate intensity in the air, had no great effect. Without feeling much confidence I rather incline to the opinion that the residual disturbances are of internal origin. As the fluid flows up to the aperture along the inner surface of the plate which forms the bottom of the reservoir, eddying motions are almost certainly impressed upon it, and these may very possibly be the origin of the ultimate disintegration. With the view of testing this point, I arranged an experiment in which the velocity of the fluid over the solid walls should be as small as possible.

AB (fig. 1) represents a large brass tube, to which a smaller one is soldered at B, suitable for india-rubber connexion. The bottom of

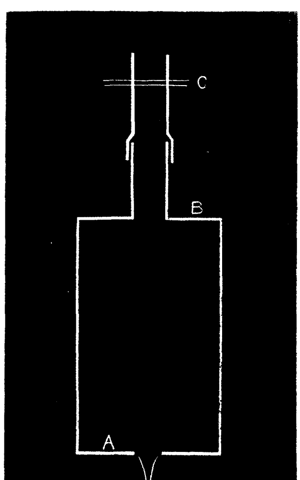


Fig. 1

the large tube consists of a carefully worked plate in which is a circular hole of $\frac{1}{2}$ inch diameter. When the rubber tube is placed in connexion with the water supply, a jet drops from A, and may be made exceedingly fine by regulaton of the pinch-cock C. By turning off the supply at C altogether, the jet at A may be stopped, without emptying the vessel. The stability, due to the capillary tension of the surface at A, preponderates over the instability due to gravity. By this device it is possible to obtain a jet whose velocity is acquired almost wholly *after* leaving the vessel from which it issues. In this form of the experiment, however, the jet is liable to disturbance depending upon the original velocity of the fluid as it passes through the comparatively narrow rubber tube, and when I attempted a remedy by suspending a closed reservoir (fig. 2), in which the water

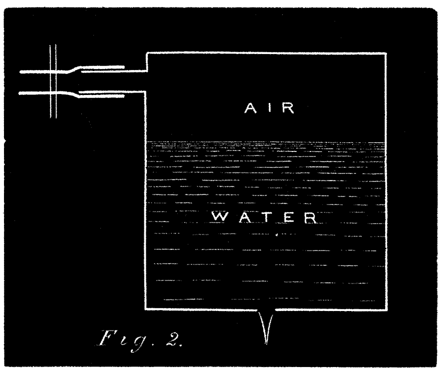


Fig. 2.

might be allowed first to come to rest, other difficulties presented themselves. The air confined over the surface of the water acts as a spring, and the flow of water below tends to become intermittent, when rendered sufficiently slow by limiting the admission of air. A definite cycle is often established, air flowing in and water flowing out alternatively at the lower aperture. The difficulty may be overcome by careful manipulation, but there is no easy means of making an adequate comparison with other jets, so that the question remains undecided whether the residual disturbances are principally of internal or of external origin.

Collision of two Resolved Streams.

§4. In the case of a simple vertical fountain, when the scattering is prevented by electricity, there is every reason to believe that the action is differential, depending on a difference of potentials of colliding drops. The principal electrification, however, of the successive drops must be the same ; and thus, sensitive as it is, this form of the phenomenon is not by any means the best calculated to render evident the smallest electrical forces. As was shown in my former paper, it is far surpassed by colliding *jets*, between which a difference of potential may be established, a subject to which we shall return in § 5. It is possible, however, to experiment upon the collision of two distinct streams of drops, which are differently,—if we please, oppositely—electrified from the first. Apart from electrical influence, the collision of such streams presents points of interest which have been made subject of examination.

Two similar brass nozzles, terminating in apertures about $\frac{3}{50}$ inch in diameter, were supplied from the same reservoir of water, and were held so that the jets rising obliquely from them were in the same plane and crossed each other at a moderate angle. The jets were resolved into regular series of drops by the action of a 256 fork screwed to the table and set in action by bowing. The periodic phenomenon thus established could be examined with facility by intermittent vision through a revolving perforated disk (§ 2), so arranged that about 256 holes passed the eye per second.

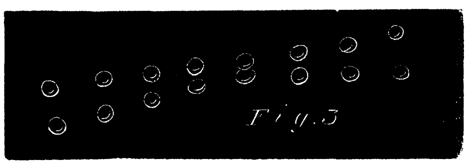


Fig. 3

When the angle of collision is small, the disposition of the files of drops may be made such that they rebound without crossing, fig. 3; more often, however, the drops shoulder their way through after one or more collisions, somewhat as in fig. 4. In both cases the

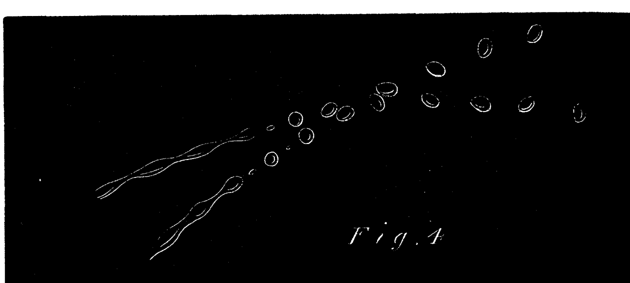


Fig. 4

presentation of an electrified body to one place of resolution will determine the amalgamation of colliding drops, with of course complete alteration of the subsequent behaviour. By judicious management a feebly electrified body may be held in an intermediate position between the two points of resolution so as not to produce the effect, confirming the view that the action is differential.

At a somewhat higher angle of collision amalgamation will usually occur without the aid of electricity, but the fact may easily escape recognition when intermittent vision is not employed. The streams do not usually join into one, as we might perhaps expect, but appear to pass through one another, much as if no union of drops had occurred. With the aid of the revolving disk the course of things is rendered evident. The separating layer is indeed ruptured at contact, and for a short time the drops move as one mass. There is, however, in general, considerable outstanding relative velocity, which is sufficient to bring about an ultimate separation, preceded by the formation of a ligament (fig. 5). In certain cases, although after

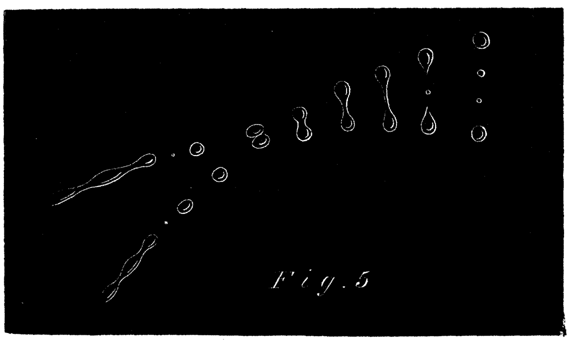
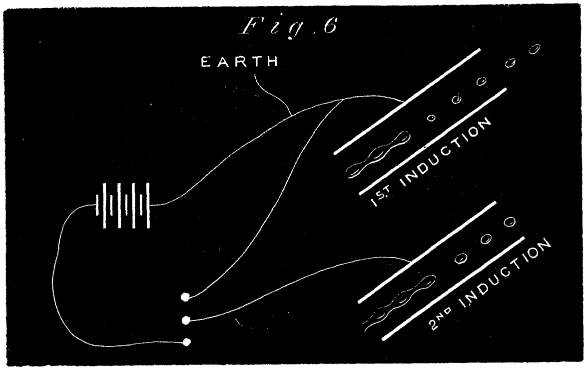


Fig. 5

contact a ligament is formed, the relative velocity is insufficient to overcome its tension, and the drops draw again together and ultimately cohere. If the impact is very direct, so that the relative velocity is almost entirely in the line of centres, the drops may flatten against one another and become united without the formation of a ligament.

In order to determine how small a difference of potential would be effective in causing the coalescence of streams of drops meeting at a small angle, the two places of resolution were enclosed in inductor-tubes, between which with the aid of a battery a difference of potential could be established. The arrangement is shown in fig. 6. One of the inductors is placed in connexion with the earth, with the reservoir from which the water comes, and with one pole of the battery. By operating a key, the other inductor may be placed at pleasure in communication with the first inductor, or with the other pole of the battery. In the first case the battery is out of use, and in the second the difference of potential due to the battery is established between the two inductors.



Experiment showed that the effect depends a good deal upon the exact manner of collision. In almost all cases twenty cells of a De la Rue battery sufficed to produce amalgamation, with subsequent replacement of the original streams by a single one in a direction bisecting the angle between the original directions. With a less battery power the result may be irregular, some of the drops coalescing and others rebounding. When the collisions are very direct, even four cells will sometimes cause a marked transformation.

The complete solution of the problem of the direct collision of equal spheres of liquid, though probably within the powers of existing mathematical analysis, is not necessary for our purpose; but it may give precision to our ideas to consider for a moment the case of a row of equal spheres, or cylinders, with centres disposed upon a straight line, and so squeezed together that the distances between the centres must be less than the original diameters. By the symmetry, the common surfaces are planes, and the force between contiguous masses is found by multiplying the area of the common surface by the internal capillary pressure. When the amount of squeezing is small, the internal capillary pressure is approximately unaltered, and the force developed is simply proportional to the area of contact. In the case of the cylinder the problem admits of very simple solution, even when the squeezing is not small; for, as is easily seen, the free surfaces are necessarily semicircular, and thus the condition of unaltered volume is readily expressed. It will of course be noticed that as regards lateral displacements the equilibrium is unstable.

Collision of Streams before Resolution.

§ 5. The collision of unresolved streams was considered in my former paper. It appeared that the electromotive force of a single Grove cell, acting across the common surface, was sufficient to determine coalescence, and that the addition of a small quantity of soap made rebound impossible. Moreover, the "coalescence of the jets would sometimes occur in a capricious manner, without the action of electricity or other apparent cause."

As in many respects this form of the phenomenon is the most instructive, I was desirous of finding out the explanation of the apparent caprice, and many experiments have been made with this object in view. The observations on fountains recorded in § 1 having suggested the idea that the accidental presence of greasy matter, removable by caustic potash, might operate, this point was examined.

"*July* 8, 1880.*—*Colliding Jets.*—Two large glass bottles, with holes in the sides, close to the bottom, were fitted by means of corks with glass tubes, drawn out to nozzles of about $\frac{3}{40}$ of an inch in diameter. The bottles were well rinsed with caustic potash, to remove any possible traces of grease, and filled with tap water. The colliding jets coalesced in a manner apparently entirely capricious, the only principle observable being that they coalesced even more readily with high pressures (12 inches) than with low, and with lower pressures would stand collision at greater angles. The addition of caustic potash sufficient to give a very decided taste to the water, produced no apparent effect." Subsequently the water used was boiled with caustic potash, but without success.

"July 27, 28, 29, 30.—On the theory that when the jets collide without uniting there is between them a thin film of air, which would be very liable to be sucked up by water not saturated with air, we tried jets of water through which a stream of atmospheric air had been passed for several hours. We tried it three times. The first time the jets seemed very decidedly less liable to unite capriciously. The second time they behaved even worse than ordinary tap water usually does. The third time we thought it rather better than tap water usually is, but not materially so."

Jets of hot water, and of mixtures of alcohol and water in various proportions, were also tried at this time, but without obtaining any clue as to the origin of the difficulty.

I had begun almost to despair of success, when a determined attempt to conjecture in what possible ways one part of the stirred liquid could differ from another part suggested the idea that the anomalies were due to dust.

"Aug. 1880.—We tried dropping dust on to the colliding jets just above the point of collision, and found that union was always produced. The following powders were tried—powdered cork, sand, lycopodium, plaster of Paris, flowers of sulphur, sugar, dust that had accumulated upon a shelf, and later emery and putty powder. The lycopodium was a little more uncertain in its action than the others,

* Mrs. Sidgwick's "Note Book."

but apparently only because, owing to its lightness, it was difficult to ensure its falling upon the jets. Whenever we were sure it did so, union followed."

When mixed with the water, powders acted differently. Emery and putty powder were not effective, but sulphur caused immediate union. Much probably depends upon the extent to which the extraneous matter is wetted. A precipitate of chloride of silver, formed in the liquid itself, seemed to be without influence.

Acting upon this hint, Mrs. Sidgwick made an extended series of observations upon the behaviour of jets composed of water which had been allowed to settle thoroughly, and which were protected from atmospheric dust. For this purpose the jets were enclosed in a beaker glass, the end of which was stopped by a plug of boxwood, fitted air-tight. Through the plug passed horizontally the two inclined glass nozzles, and underneath a bent tube serving as a drain. The results, observed under these circumstances, were such as to render it almost certain that dust is the sole cause of the capricious unions. The protected jets of settled water were observed for a total period of 246 minutes, during which the unions were at the average rate of one in ten minutes. The longest intervals without unions were thirty-four minutes and twenty-nine minutes. Comparative experiments were made upon the behaviour of jets from the same nozzles under other conditions. Thus jets of unsettled water, but protected from atmospheric dust, united on an average twenty-four times in ten minutes. With unsettled water the protection from atmospheric dust is not of much use, as unprotected jets of the same water did not unite more than twenty-six times in ten minutes. On the other hand, jets of settled water, not protected from the atmosphere, united only twelve times in ten minutes. Although, no doubt, somewhat different numbers might be obtained on repetition of these experiments, they show clearly that the dust in the water is the more frequent cause of union under ordinary circumstances, but that when this is removed the atmospheric dust still exerts a powerful influence. The difficulty of getting water free from dust is well known from Tyndall's experiments, so that the residual tendency to unite under the most favourable conditions will not occasion surprise.

Although there is no reason to suppose that any other cause than dust was operative in the above experiments, it remains true that very little impurity of a greasy character will cause immediate union of colliding jets. For this purpose the addition of milk at the rate of one drop of milk to a pint of water is sufficient. It may be noticed too that the effect of milk is not readily neutralised by caustic potash.

With respect to the action of electricity, further experiments have been made to determine the minimum electromotive force competent to cause union. The current from a Daniell cell was led through a straight length of fine wire. One end of the wire was connected by platinum foil with the liquid in an insulated glass bottle, from which one of the jets was fed. The glass bottle supplying the second nozzle was similarly connected with a moveable point on the stretched wire. The electromotive force necessary to cause union, as measured by the distance between the two fine wire contacts, though definite at any one moment, was found to vary on different occasions, possibly in consequence of forces having their seat at the surfaces of the platinum oil. From one-half to three-quarters of the whole force of the Daniell was usually required.

With a view to further speculation upon this subject, an important question suggests itself as to whether or not there is electrical contact between colliding and rebounding jets. To solve this question it was only necessary to introduce a fine wire reflecting galvanometer into the arrangement just described, taking care that the electromotive forces employed fell short of what would be required to cause the union of the jets. Suitable keys were introduced for more convenient manipulation, and sulphuric acid was added to the water, in order to make sure that absence of strong galvanometer deflection could not be due merely to the high resistance of the thin columns of water composing the jets. Repeated trials under these conditions proved that so long as the jets rebounded their electrical insulation from one another was practically perfect.

As to the explanation of the action of electricity in promoting union, it would be possible to ascribe it to the additional pressure called into play by electrical attraction of the opposed water-surfaces, acting as plates of a condenser. But it appears much more natural to regard it as due rather to actual disruptive discharge, by which the separating skin is perforated, and the equilibrium of the capillary forces is upset. A small electromotive force, incapable of overcoming the insulation of the thin separating layer, is without effect.

XIV. "On a Collection of Rock Specimens from Socotra." By Professor T. G. Bonney, M.A., F.R.S., F.G.S. Received June 12, 1882.

(Abstract.)

In the spring of 1879 the island of Socotra, which lies off the north-east corner of Africa, about 140 miles from Cape Gardafui, was visited by Professor Bayley Balfour. Landing at the north-west extremity, he traversed the northern side of the island up to the eastern end, then returning by a more central course to the sea, he

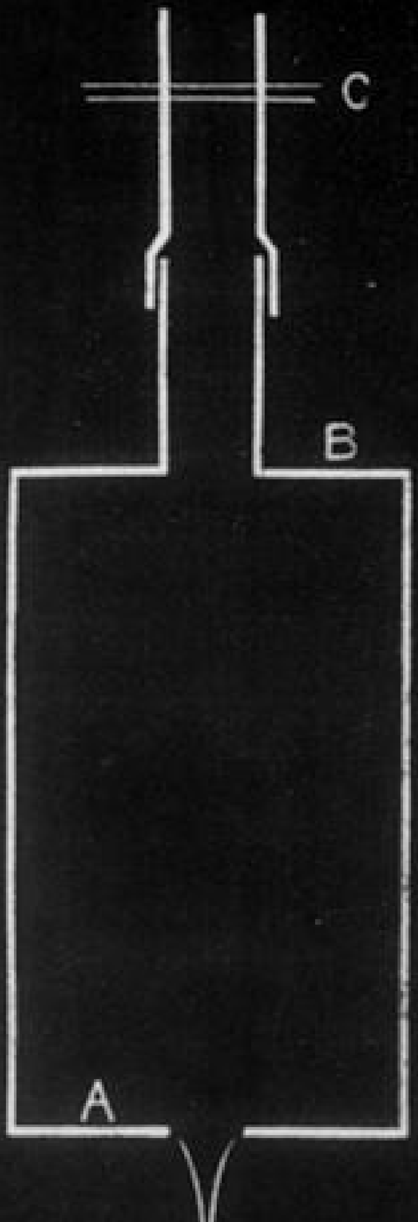


Fig. 1

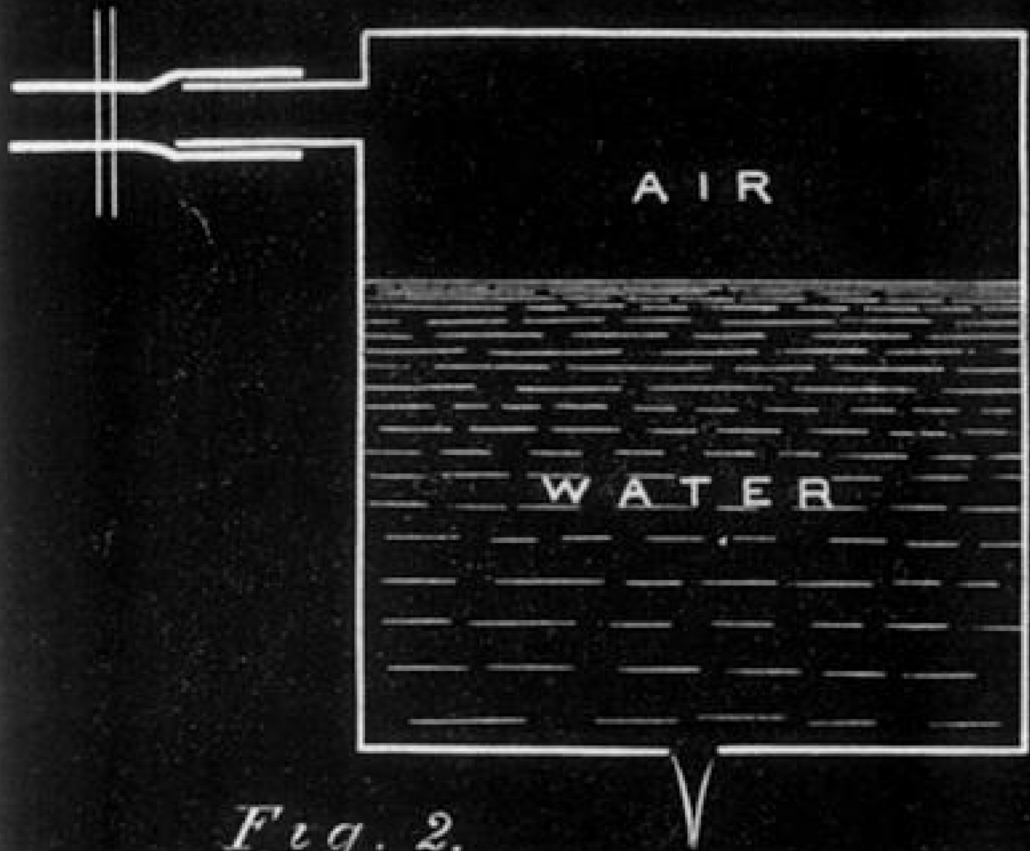


Fig. 2.

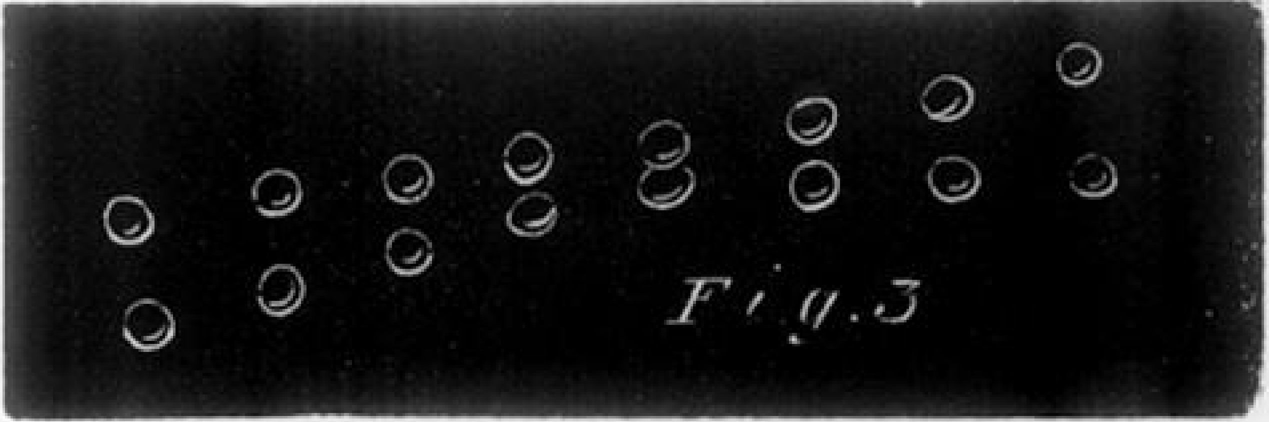


Fig. 3

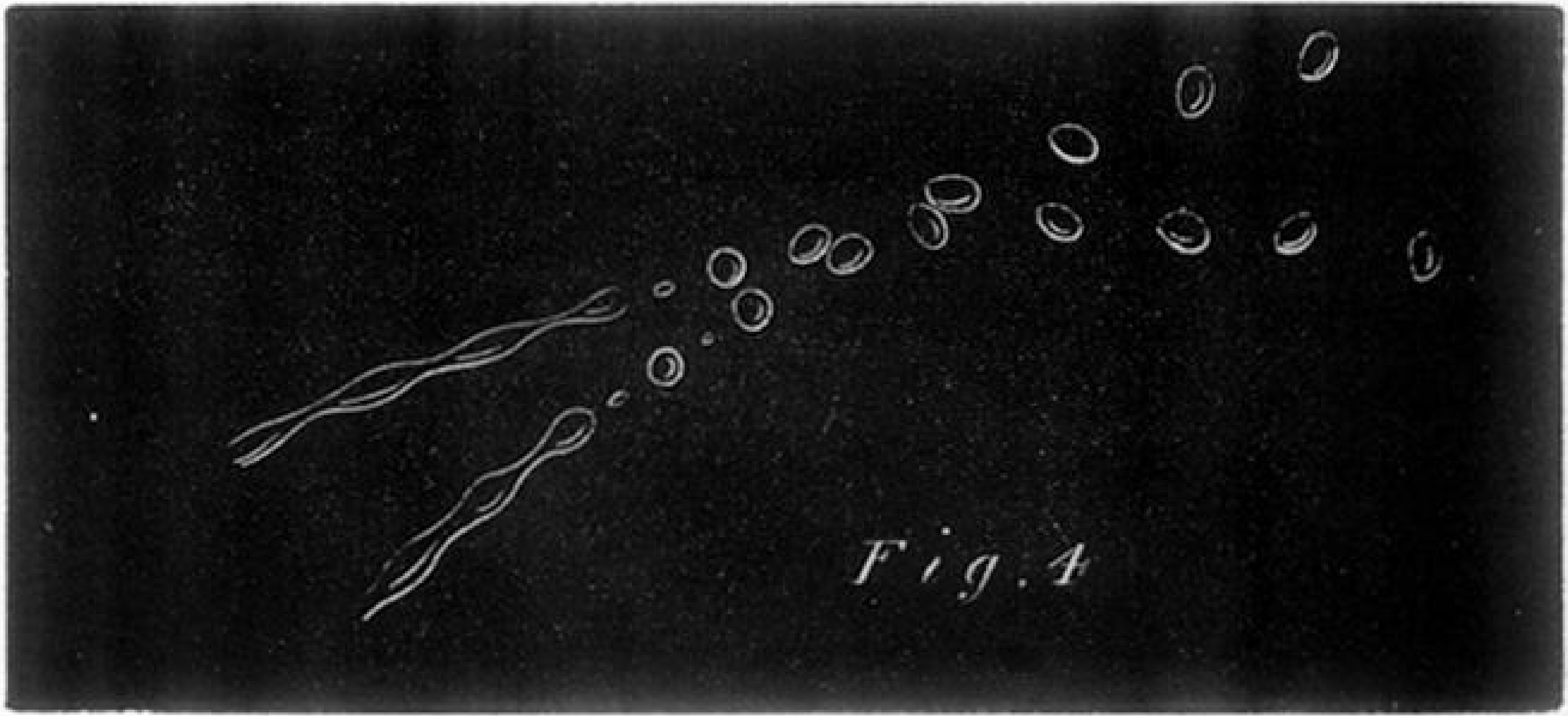


Fig. 4

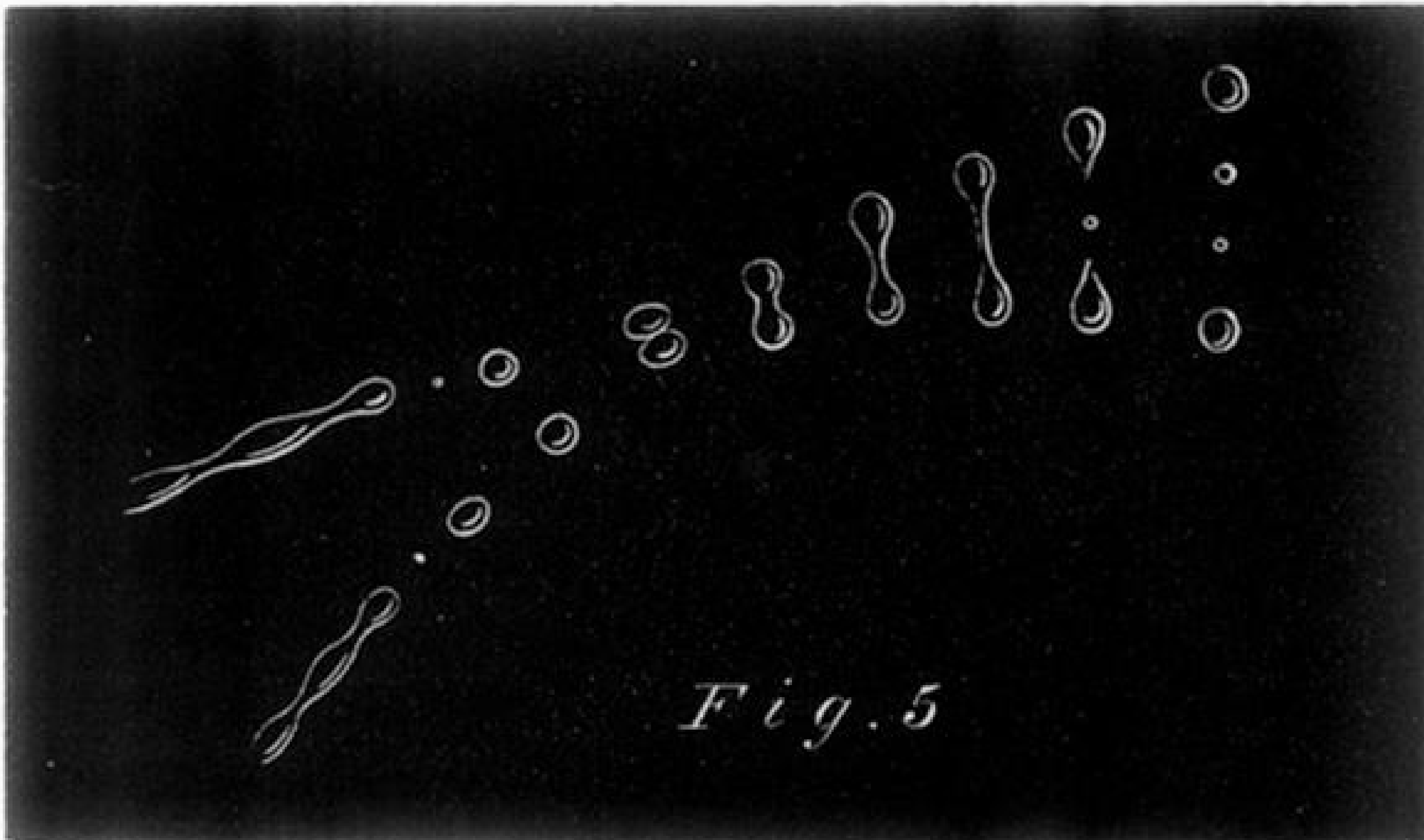


Fig. 5

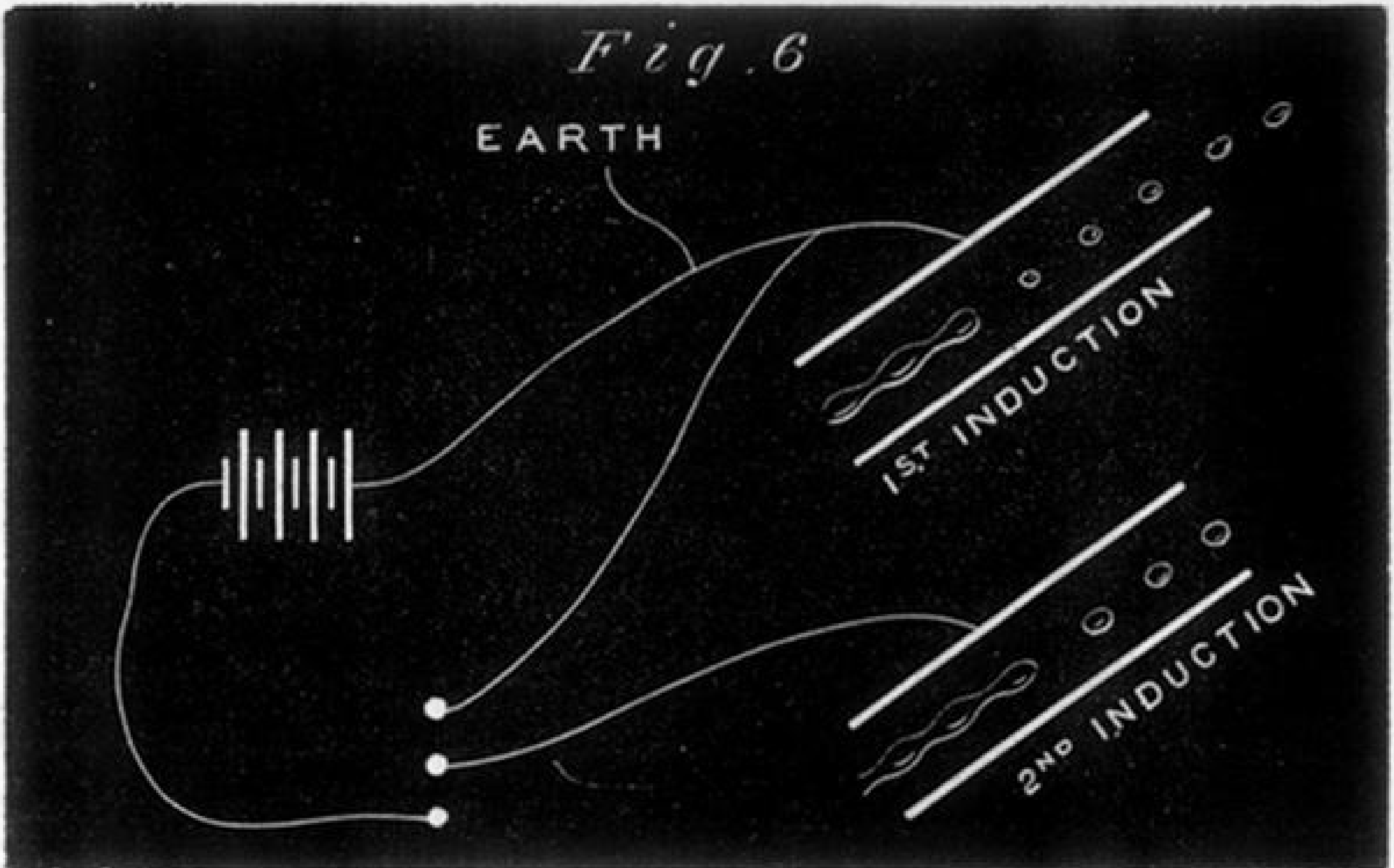


Fig. 6
EARTH
1ST INDUCTION
2ND INDUCTION