has been employed by Lord Rayleigh * to calculate the wave-length in an oscillating water-jet, and leads, as he himself points out, to too low a result. Had he made use of the corrected value (T=·0735 gr.=72·1 C.G.S. units), the results of p. 82, *loc. cit.*, would have been in very close agreement with theory.

In conclusion, I would remark that the great value of this particular paper of Prof. Quincke's does not, as it seems to me, depend so much on the absolute accuracy of the results presented as on the emphasis with which, at the time it was written, it represented the capillary *constant* to be measured as a surface-tension, and on the justice of the general considerations which are put forward, and which remain unimpeached.

A. M. WORTHINGTON.

Clifton, Bristol, June 2, 1885.

P.S. For the benefit of readers of Maxwell's 'Theory of Heat,' in which Prof. Quincke's results are given in a synoptic table, I here reproduce the table with the corrected values. Those in brackets I have been unable to check. It must be borne in mind that the numbers in the second column of tensions are still somewhat uncertain.

The values are given in grammes weight per linear metre.

Superficial Tensions at about 20° C.

Liquid.	Specific gravity.	Tension separating the liquid from		
		Air.	Water.	Mercury.
Water	1·0	7·35	0	38·44
Mercury	13·543	50·28	38·44	0
Bisulphide of Carbon	1·2687	2·99	4·09	[37·97]
Chloroform	1·4878	[3·12]	2·63	[40·71]
Alcohol	0·7906	[2·36]	...	35·69
Olive-oil	0·9136	3·39	1·82	31·07
Turpentine	0·8867	2·82	1·13	23·60
Petroleum	0·7977	2·91	[2·83]	26·53
Hydrochloric acid	1·1	[7·15]	...	[38·41]
Solution of Hyposulphite of Soda	1·1248	6·85	...	42·08

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* "On the Capillary Phenomena of Jets," Proc. Roy. Soc. no. 196 (1879).

IX. *The Stream-lines of Moving Vortex-rings.* By OLIVER LODGE *D.Sc., Professor of Physics in University College, Liverpool**.

[Plates II., III., IV.]

THE object of the present communication is to publish drawings of vortex stream-lines, some of which I made originally for my own edification. Taking the lines of a stationary vortex, as given by Sir W. Thomson in his memoir on Vortex Motion (Trans. Roy. Soc. Edinb. vol. xxv.), or as copied into Maxwell's 'Electricity' (plate 18, vol. ii.), I merely superpose uniform motion upon them, in the shape of a series of parallel lines, and join up the corners of the quadrangles so formed.

Another way of expressing the matter is to say that you draw the lines of magnetic induction due to a circular ring conveying a current, placed in a uniform magnetic field with its lines exactly opposed to those inside the ring.

I choose two strengths of uniform field for the sake of illustration; one distinctly stronger, the other distinctly weaker, than the central intensity due to the coil alone. The relative intensities at centre of ring due to field and coil respectively are about as 1 to 5 in fig. 1, and as 64 to 5 in fig. 2 (Plate II.). Or, taking the curves as representing stream-lines: in fig. 1 the velocity of vortex-motion is equal to the translation-velocity of the whole ring at a certain circle in its plane concentric with its core and of 3·3 times the diameter of the core, and also at two points on the axis; while in fig. 2 the vortex-velocity and the translation-velocity are equal at a place 1·5 core-radii distant from the centre of the ring outside, and at another circle, say two fifths the core dimensions, inside, the ring.

In fig. 1 the ring is moving so fast that the translational flow back of fluid through its centre overpowers the forward vortex-motion there. In fig. 2 the vortex-motion predominates as far as a point on the axis which I reckon as 1·38 core-radii distant from centre of ring, a point indicated by the crossing of the partially dotted stream-line. It will be understood that though they look so different, the two Plates represent the *same* ring moving at different speeds. The size of the core or circular axis is the same in both diagrams.

It will be observed that in fig. 2 the portion of fluid permanently partitioned off from the rest by reason of its vorticity is truly ring-shaped, and would become thinner or more wiry if its forward motion were greater—the lines near the core of the ring being prolate towards the axis; while in fig. 1 the rotational portion of fluid, which is being bodily

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* Communicated by the Physical Society: read June 27, 1885.

translated through the rest, forms an ovoid mass with dimples before and behind—the dimples, however, becoming less and disappearing when the translatory motion is made still slower. The lines near the core are in this case rather displaced away from the axis. The dottedness of the portion of the line which crosses the axis of the ring is purely subjective, and only indicates uncertainty on my part as to its exact course, from want of knowledge. It is probable that the same defect exhibits itself in my terminology, which is probably incorrect, or at least unusual. Thus I cannot help calling the actual circular axis of the ring its "core," instead of the whole of the rotational portion, as is usual in dealing with rings of very small cross section in proportion to area of ring itself. The rings drawn are not of small cross section, and so one wants a name for their innermost axis or core.

We can try to apply Sir William Thomson's rule* for the velocity of translation of very thin or high-speed rings, to the case of fig. 2; though this ring is not nearly thin enough for the formula to be properly applicable.

Using the symbol λ for the ratio of radius of ring itself to its cross-section radius, the rule may be written:—

$$\frac{2\pi \times \text{velocity of translation}}{\text{vortex velocity at centre of ring}}$$

$$= \frac{2\lambda \times \text{velocity of translation}}{\text{vortex velocity at surface of rotational portion}} = \log(8\lambda - \tfrac{1}{4}).$$

In fig. 2 the value of λ is about 3; and accordingly each of the above terms is about 3 also; or the two vortex velocities specified in the formula are nearly equal, and about double that of the translational velocity.

This does not agree with what I said before, about the ratio of uniform field to ring-field at centre being as 64 : 5; hence there is something wrong, but I don't know what. The lines of uniform velocity in fig. 2 were taken 8 times as close together as in fig. 1; and this surely represents a velocity 64 times as great. I can only suppose that the ring is *much* too fat for the formula.

Plate III. is an attempt to represent a vortex-ring advancing in a very imperfect or viscous fluid, showing its gradual increase in size, and decrease in forward velocity. It is easily drawn by superposing a diverging equiangular pencil on the stationary vortex which forms the basis of all three diagrams; but that it really represents the effect of viscosity does not seem very probable. No *slip*, due to inertia of displaced fluid, is shown in any of the diagrams. This figure better represents a ring moving towards a large distant obstacle. As drawn, the vortex-velocity at centre is only a trifle greater than the translational velocity. This plate also represents the lines of magnetic force due to a circular current with a repellent pole on its axis, at a point 2·518 diameters away from the plane of the circle. The dots on the curves indicate the distribution of the crossing-points which guide the drawing.

Plate IV. shows the attempt of a ring to advance in an oblique direction, not normal to its plane. It is supposed to have been knocked out of a hole by a slant impulse. There is evidently a good deal of vibration, both of the ring as a whole and of its cross section; and it looks as though a very little would suffice to break it up altogether. The resultant velocity at the centre of the ring happens, in the particular case here chosen, to be about zero.

In cases of oblique progression a tendency to a bodily shifting of the uniform flow-lines, parallel to themselves, as they pass from before to behind the ring, is noticeable, and is exhibited in fig. 1. Perhaps this means a heaving or sinuous path of motion for the ring. The right mode of joining up the guiding-points is however in this case by no means obvious; and fig. 2 (Plate IV.) is just as likely to be correct as fig. 1. In fig. 2 no shifting of distant stream-lines occurs, but then it hardly seems a real case of vortex-motion: at least it looks only like a ring shaking itself to pieces; while fig. 1 suggests an attempt of the same ring to pull itself together.

I have a number of other diagrams drawn in the rough, indicating various features of the clash or chase of vortex-rings.

The direct clash of two equal opposite rings, or the impact of one against a looking-glass, is of course very easy. The clash of two rings of different strength is more complex—one appears to be opened out over the other.

The chase of two unequal rings, and the penetration of the front one by its pursuer, are well shown; but if the rings are of equal strength they refuse to penetrate, and seem to amalgamate or pair, no matter at what different speeds they may be going.

The deflection of one ring by another whose path is inclined to it, as calculated by Prof. J. J. Thomson in his 'Adams Essay,'* can also be illustrated, together with what I think corresponds to vibrations of the core about the circular form.

But all these diagrams I propose to publish in a more complete form later. This "experimental" method of investigation, by diagrams based on simple superposition of velocities, seems capable of great extension, because one is limited by no approximations or conditions: the only difficulty is the interpretation of results.

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* Phil. Mag. June 1867, xxxiii. p. 511.

* See also Phil. Trans. ii. 1882.

Last year I examined air vortex-rings produced in the well known manner described by Prof. Tait[*], in the light of a powerful intermittent induction-coil Leyden-jar discharge. The motion is, however, of too continuous a nature to exhibit the advantages of this mode of illumination; and though the crispations of vibrating rings are well shown, there is no obvious peculiarity noticeable which does not show itself equally well in a steady illumination.

This paper is only to be regarded as a preliminary note, and, as Prof. Carey Foster has kindly reminded me, the uniform field as I have drawn it is not quite correct. This indeed will account for the discrepancy between theory and experiment mentioned above.

The appearance of jets of water illuminated intermittently is, as is well known, very striking; and I have long imagined that a waterfall illuminated in this way would be a striking spectacle. The spark is scarcely bright enough for large-scale illumination, though there is nothing to beat it for instantaneousness. A revolving slit-disk would, however, prove a more manageable and less noisy method, and by a judicious arrangement of special lenses it can be made to give plenty of light. But the speed of the disk must be high, and its slits narrow, or the drops will be blurred and their characteristic statical beauty lost.

X. *Notices respecting New Books.*

Geschichte der Elektrizität. *Von* Dr. EDM. HOPPE.

Leipzig: J. A. Barth (pp. 620).

THE work before us may be characterized as encyclopædic. It is one of those valuable contributions to scientific literature of which we owe so many to the laborious research of our German friends, but which, for some reason, are found in English, for the most part, only in the form of translations. But Dr. Hoppe's book is not only a valuable collection and *résumé* of all that has been done in Electricity from the earliest times down to the present date, but it is also a veritable romance, in which the story of discovery in this particular science is told in a most fascinating manner. We are nowadays so familiar with the achievements of science, and so accustomed to see the giant of Electricity tamed and made to serve the purposes of every-day life, that we are apt to overlook the difficulties with which the earlier investigators had to contend; and it is just here that the charm of such a history as the one before us lies,—that, being familiar with the results of which the original discoverers were in search, we are able to survey their labours, to trace where, having hit upon the right track, they have pursued the truth, till their labours have been crowned with success, and to admire the skill with which they have overcome the difficulties in their way.

Dr. Hoppe divides his work into six Books, which deal with different epochs in the history of Electricity, and which are, of course, of very unequal bulk. The first gives us the history of discovery from the earliest times to Franklin, and occupies 28 pages. The second embraces the times of Franklin and Coulomb, 1747 to 1789, 70 pages. Book III. gives us the history from Galvani's discovery to the year 1819, treating chiefly of galvanic electricity and the discoveries of Volta, Davy, Oersted, Zamboni, and others, occupying 73 pages. Book IV. treats of the connection between Electricity and Magnetism, and of the contributions to electrical science of Oersted, Poggendorff, Schweigger, Ampère, Faraday, and Nobili, in the years 1820–30, and covers 60 pages. Book V. extends from Ohm to the recognition of the law of conservation of energy (1827–47), 300 pages, including, amongst others, the researches of Ohm, Pouillet, Kohlrausch, Faraday, Helmholtz, Daniell, Grove, Bunsen, Poggendorff, Planté, Faure, Joule, Lenz, Peltier, Kirchhoff, Gauss, Wheatstone, Weber, and Book VI. treats of the technical applications of Electricity—of the electric light, dynamo machines, the electric telegraph, and telephone, &c.; this occupies 93 pages.

It is not practicable, within the limits of a brief notice, to do more than mention one or two points which may be of interest. Dr. Hoppe discusses the question of the discovery of the Leyden jar at some length, and, apparently, gives his verdict in favour of Kleist as the actual discoverer; at least he seems to adopt the name "Kleist's jar" instead of the usual term. Von Kleist, Bishop of Kammin, in Pomerania, on the 11th October, 1745, placed a nail in a medicine-glass and held it to the conductor of an electrical machine; on touching the nail with the other hand he received a shock, especially if the glass contained mercury; and this appears to be the first time the experiment was ever made. The result was communicated by letter to at least three people, in Berlin, Halle, and Danzig respectively, in November and December of the same year.

In January 1746 a similar observation was made, accidentally, by Cunæus, in Leyden, and repeated by Musschenbroek, Professor of Mathematics and Physics at the University of Leyden, and Gallamand, Professor of Philosophy at Leyden, who communicated the discovery to the famous Abbé Nollet, in Paris, who, knowing nothing of Von Kleist, accredited the discovery to the Leyden professors. It would appear, then, to be due to the accident of the general ignorance of the German language, in comparison to the widespread use of the French tongue, that this most important discovery has been connected with Leyden and not with the name of Von Kleist; and there is some foundation for Dr. Hoppe's complaint, that whereas every schoolboy knows what a Leyden jar means, perhaps not one ever heard the name of Von Kleist.

Some of our common appliances or observations seem to date back further than is usually supposed. We note that the date of the first lightning-rod is given by Dr. Hoppe as 1754, and that

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[*] 'Recent Advances,' p. 292.

Fig. 1.

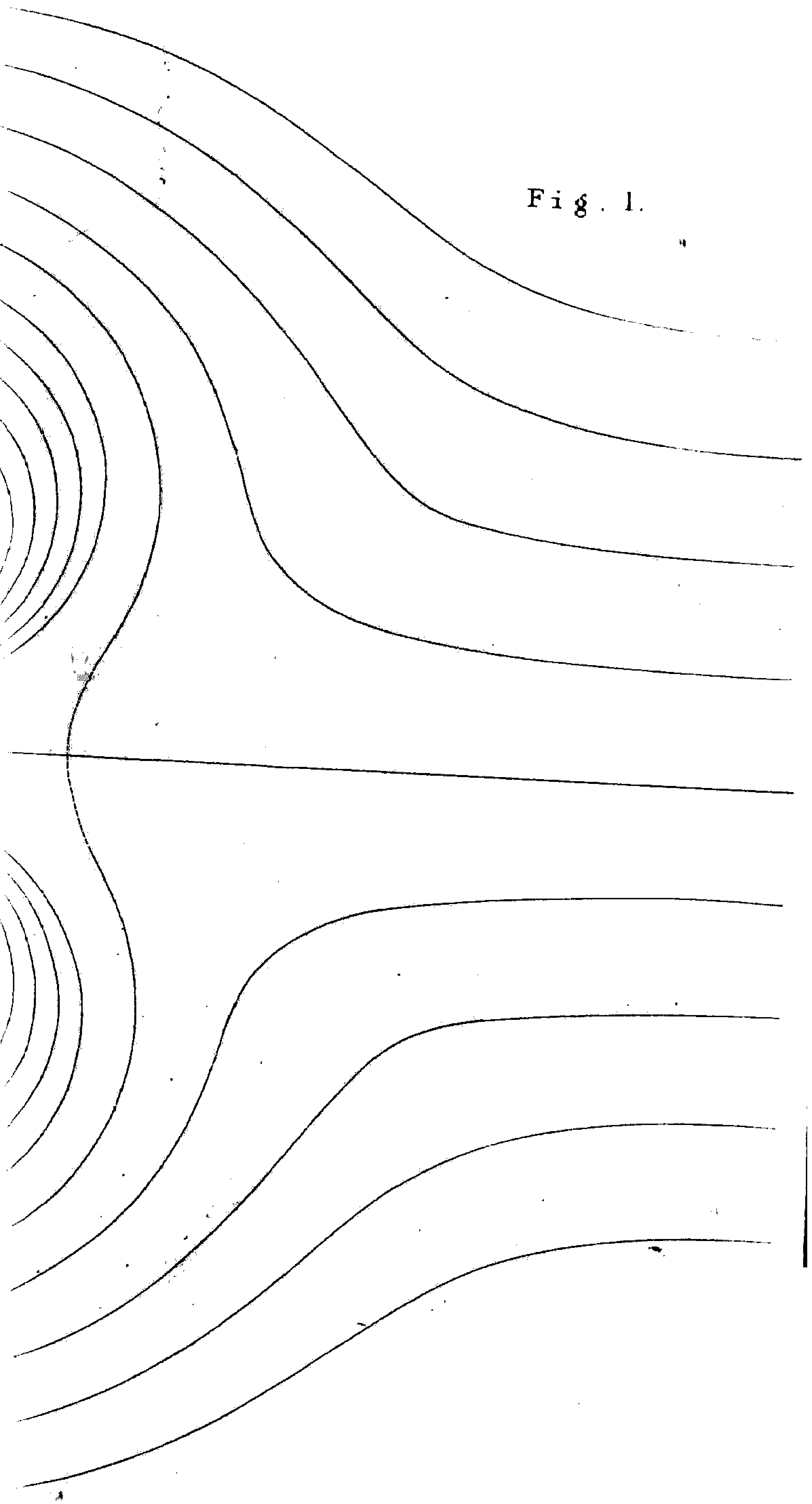

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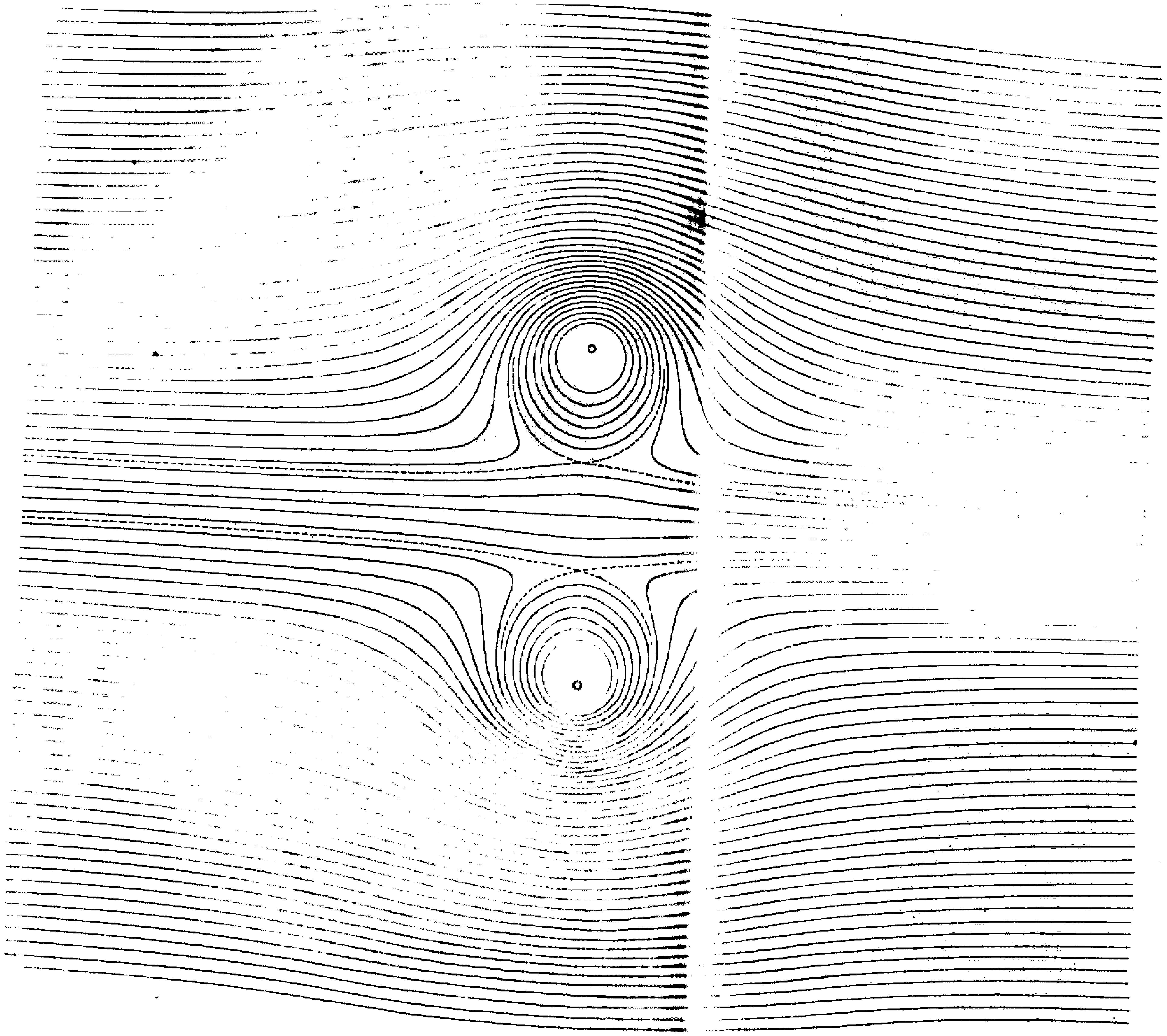
Fig. 2.



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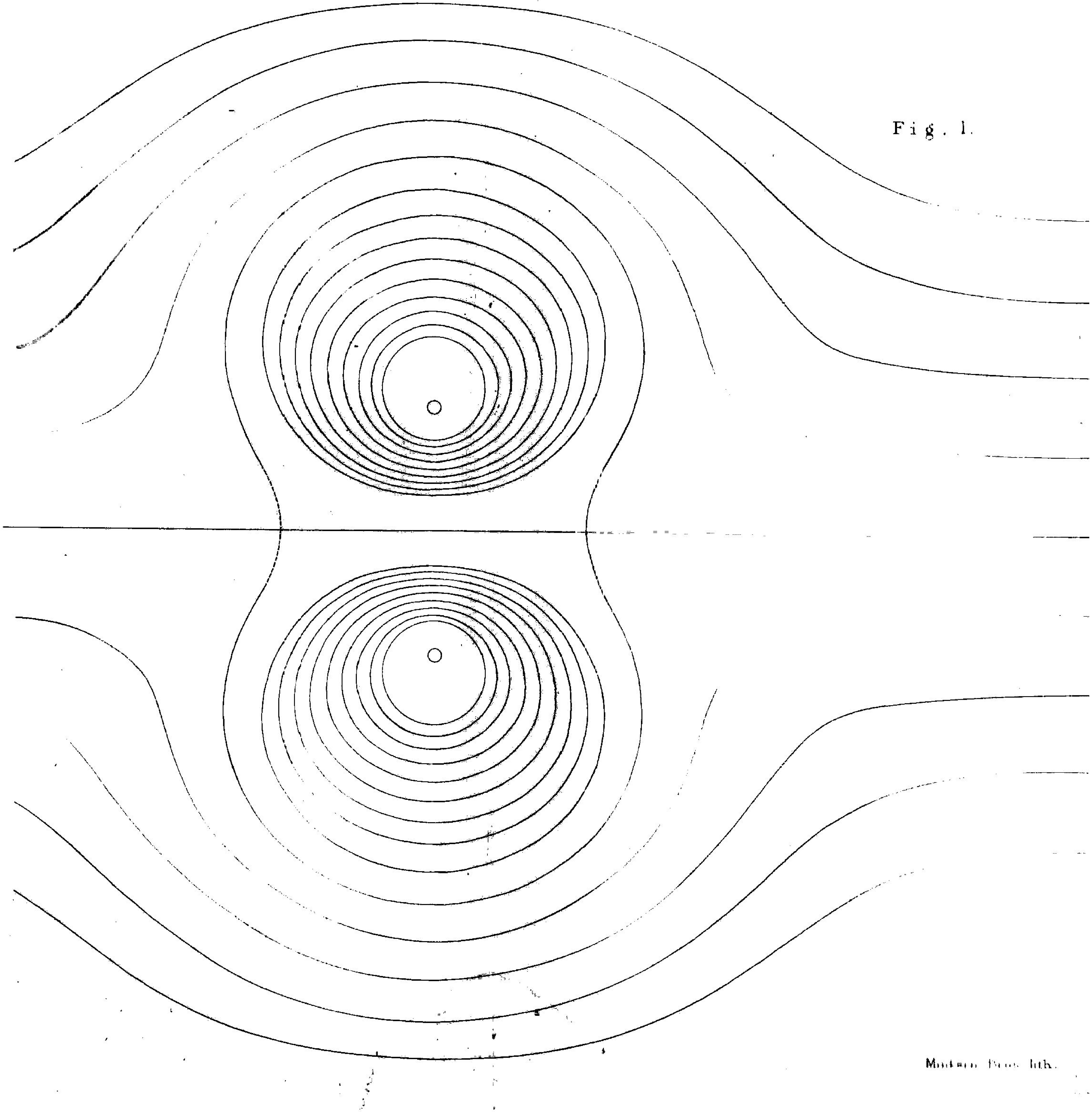

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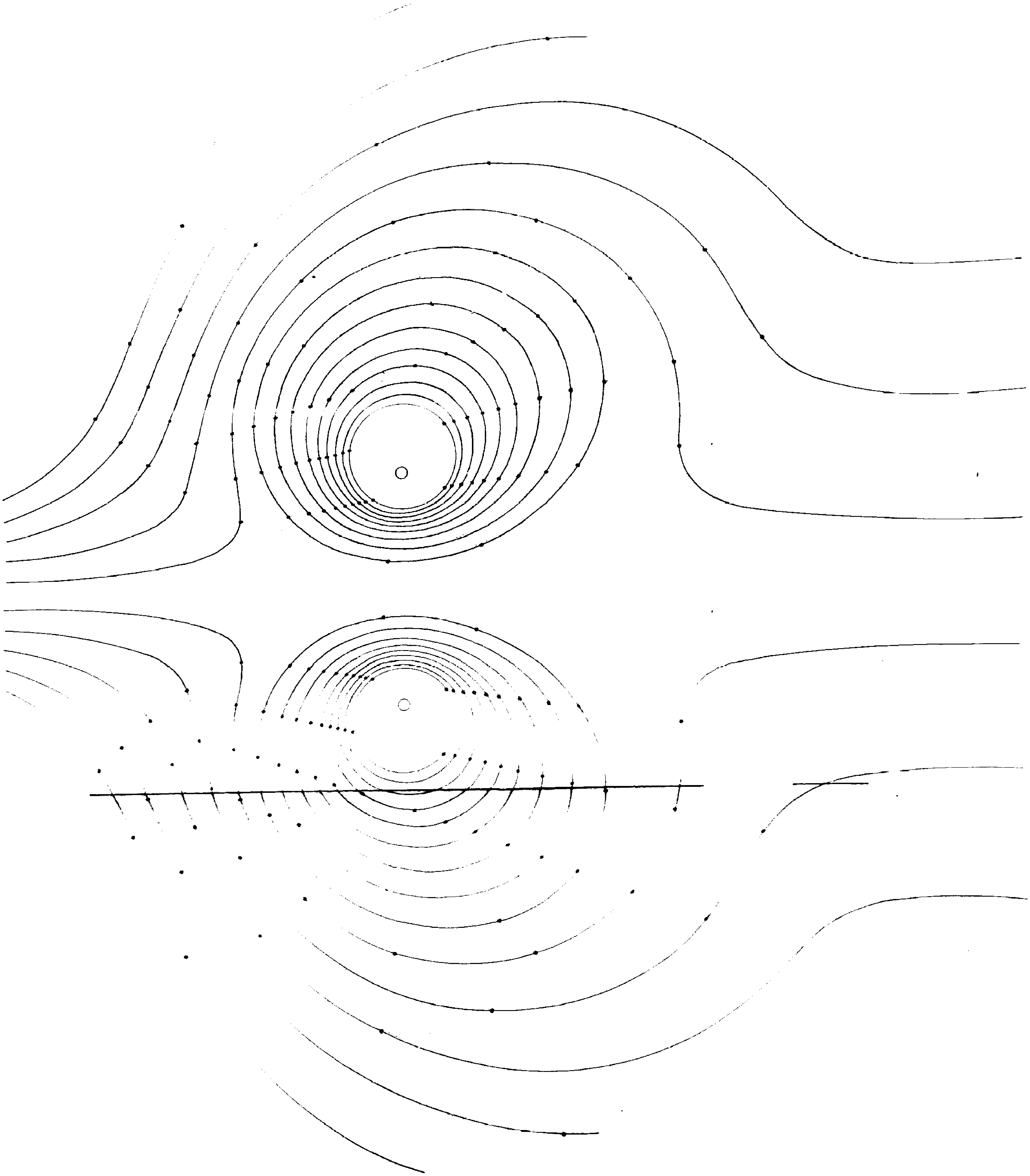

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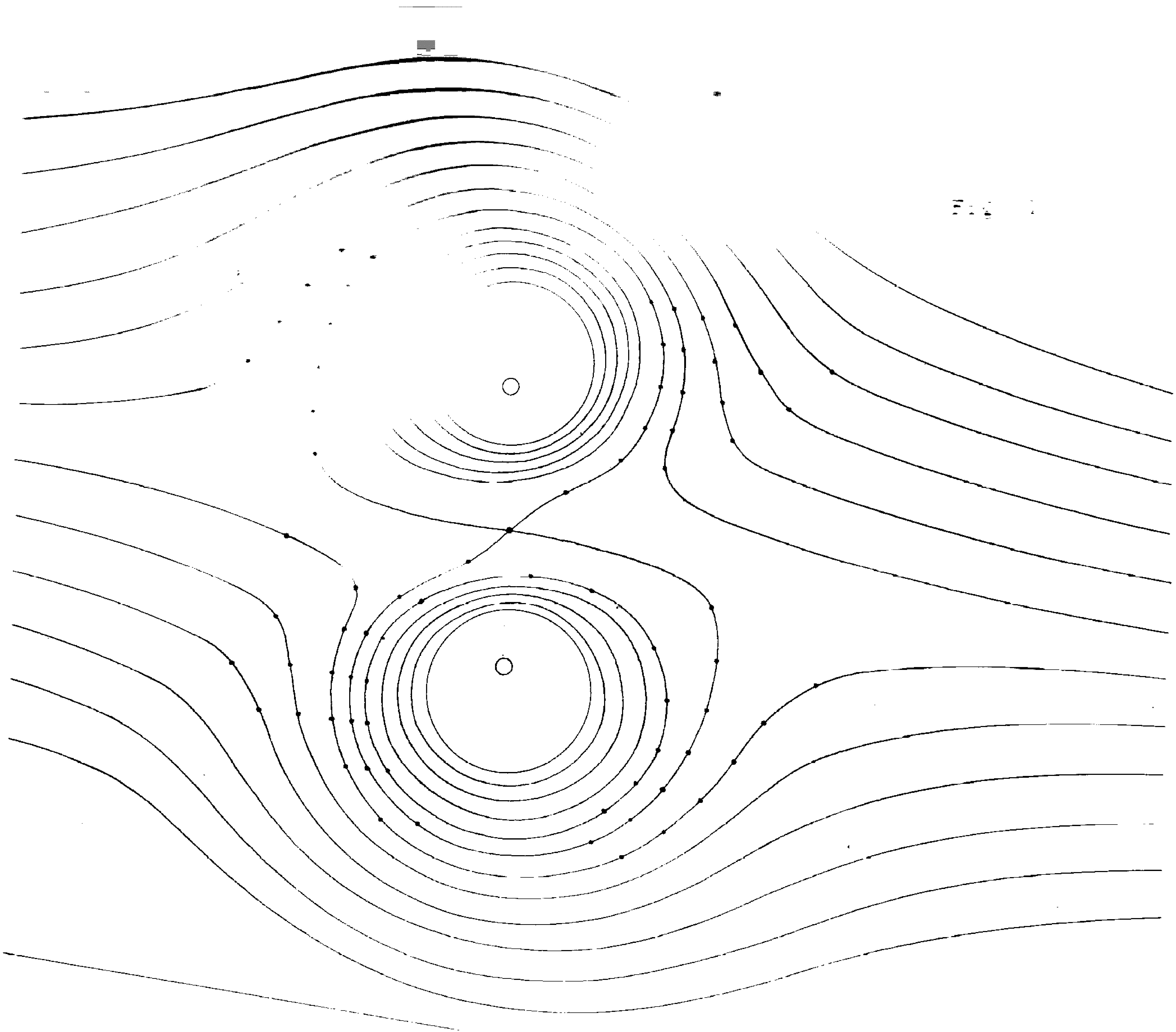

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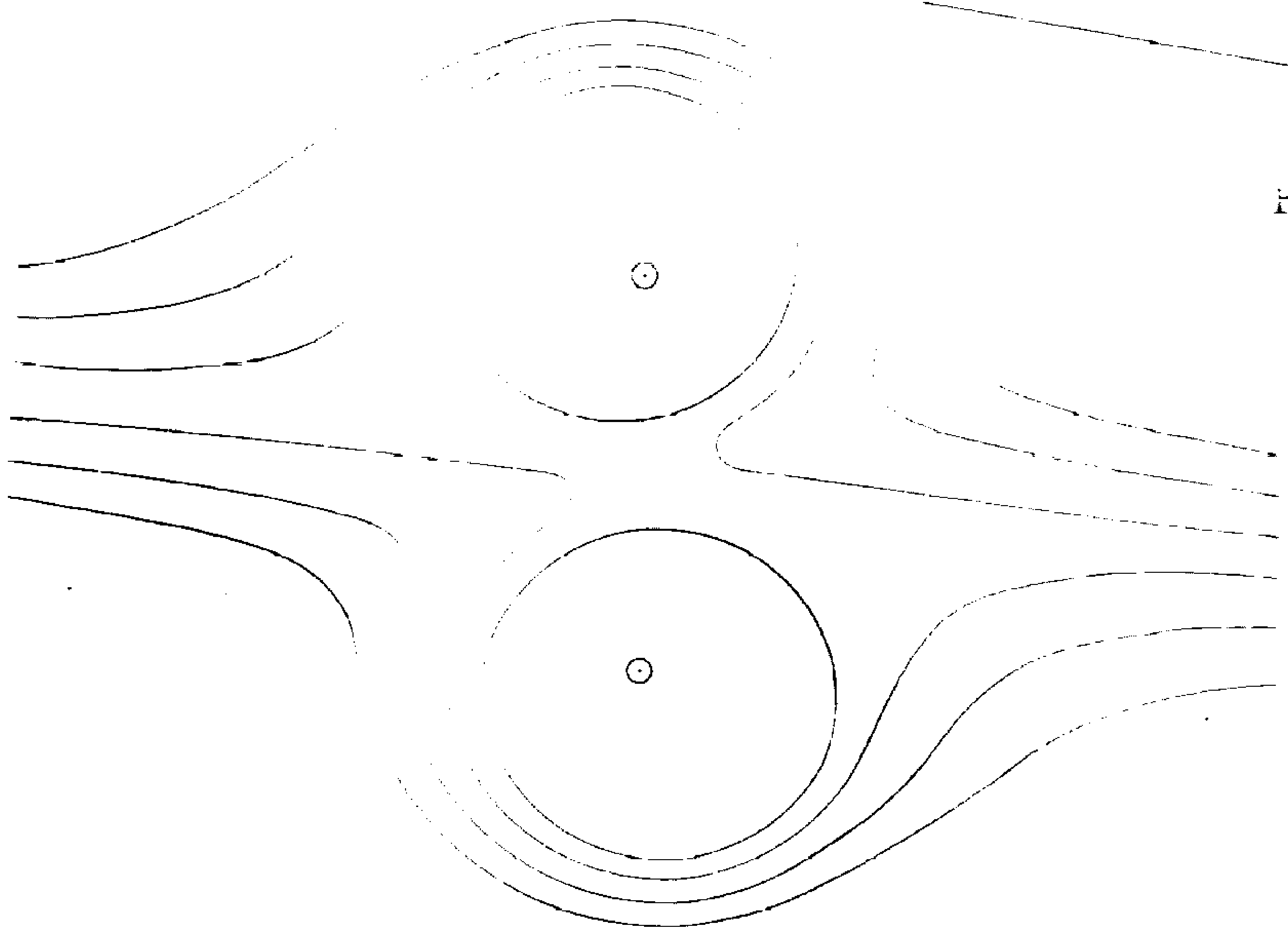

Fig. 2.

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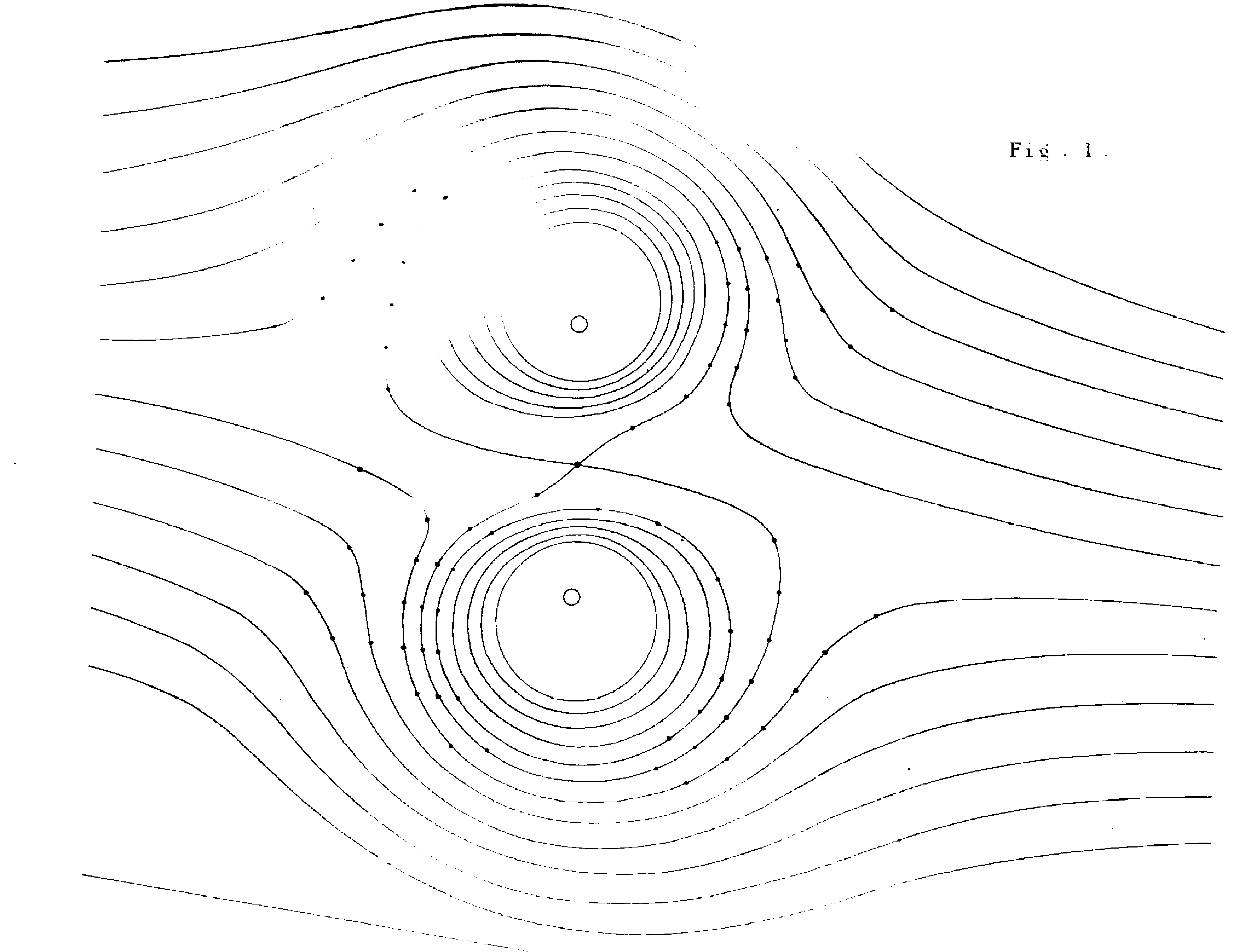

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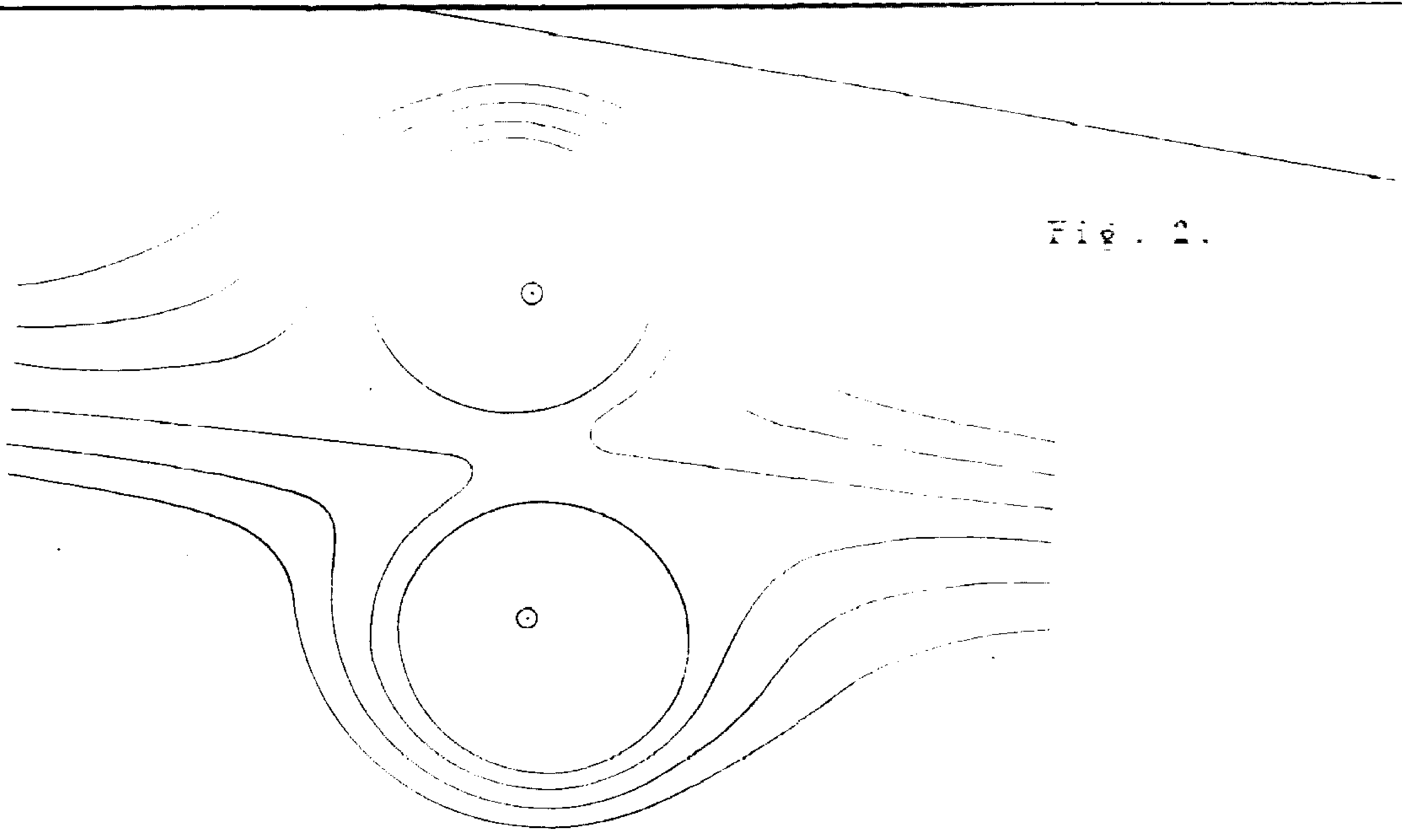

Fig. 2.

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