long and half an inch wide, wetted with the solution under examination previously acidified. Allow a portion of the paper to project beyond the tube and secure it by inserting a cork. Warm the bottom of the tube over a spirit-lamp. The action of the gas on the solution is manifested by the change produced in the colour of the paper. As in Nos. 10 and 11, Table I. you have only to distinguish between black and yellow, and in Nos. 14—17, between no change, black, yellow, and orange, all of which are easily perceived upon white paper, this method of testing accomplishes the object equally as well as the more operose and disagreeable methods in common use.

Glasgow, May 1, 1838.

XXVII. *Justification of the Contact Theory of Galvanism.*

By G. TH. FECHNER.*

THE present memoir was already complete before I received Pfaff's late work "*Revision der Lehre vom Galvano-Voltaismus*," which has in general the same object in view. As I find that Pfaff's experiments refer rather to other points of the subject than to my own, I think that this paper will not have been rendered superfluous by that work, but that the one may serve as the completion of the other.

The chief arguments which can with any appearance of weight be brought to bear against the theory of contact, and in favour of the chemical theory, have lately been brought together by the zealous supporter of the latter, De la Rive, in a separate work "*Recherches sur la Cause de l'Electr. Volt.* 1836." I shall chiefly refer to this, especially since the partisans of the chemical theory rest principally on De la Rive's experiments. Faraday's late experiments militate not so much against the voltaic theory as (at least apparently so) against a theory which seeks for the origin of electricity solely in the contact of *metals* with each other, on which subject I shall at the conclusion have occasion to add a few words. Moreover they have been, together with the experiments of Karsten, which may be considered in the same light, treated of in detail by Pfaff; I can therefore pass them over at present. I have myself my own opinion on the subject, which I would rather produce at another time and with experiments, than at present with words. As to the following experiments, I have frequently repeated and varied them, in order to derive the expression from facts, not from accidents, from

* From Poggendorff's *Annalen*, vol. xlii. p. 481. Translated by Mr. W. Francis.

which, I confess, I do not consider De la Rive's experiments to be entirely free.

I. *Facts which refer to the unclosed circuit.*

1. I have given in these *Annals* (vol. xli. p. 225) the means of proving with certainty the not inconsiderable amount of electricity which two heterogeneous metals acquire by contact in an insulated state. The supporters of the chemical theory are accustomed either to deny the certainty of these experiments, a denial which will henceforth be rendered impossible by the application of the methods here stated, or to derive the electricity thus originated from friction, pressure, or the chemical influence of the air and its moisture. It is to be shown that the result cannot depend on these. I must however remark previously, that it appears to me somewhat strange to allow to friction and pressure, which after all are only particular modifications of contact, the property of exciting electricity, independent of chemical action, while it is so obstinately denied to the simple contact. Nor has it even once been tried to derive thermo-electricity from chemical action: the agency of contact is here undoubted.

That the result, in the above-mentioned experiments, cannot depend on friction, has already been shown in my memoir in these *Annals*, vol. xli. p. 235, under No. 4.*. That it does not depend on pressure, is evident partly for this reason; that wherever the contact is completely effected, which naturally cannot take place entirely without pressure, an increase of the pressure does not indicate any more evident influence on the power of the shocks; and partly, that, according to the experience of Becquerel (so very extensive on this subject) all bodies are capable of having electricity excited in them by pressure, with the single exception however of the case, in which both bodies pressed together are good conductors, in which it at least cannot be demonstrated, undoubtedly for the same reason which hinders the demonstration of the electricity produced through the friction of two good conductors. But that chemical action cannot be the cause of the result follows already indirectly for this reason; that in this case the condensation of the electricities at the surfaces of contact would be entirely inexplicable, which on the other hand is very easily explained according to the voltaic theory. In fact, it is self-evident, that, if a force exists in the contact of both plates, which can separate and transfer to the respective plates the electricities, notwithstanding their attraction for each other, it must also be capable of keeping them separate in a degree proportional to the intensity of that force.

* The statement here alluded to will be given in the sequel of this translation. EDIT.

It would then in *this* respect occupy the place of the layer of varnish of the condenser. What however could, according to the view that the developed electricity depends on chemical action (which can least of all take place at the surfaces of contact), hinder the transference of the condensed opposed electricities into one another, and allow of the condensation even taking place? Neither can it be a layer of air or of oxide placed between the two, since this must for the same reason oppose itself to the separation of the two electricities; further, such a layer must then also appear as a hindrance to the transfer to the electrometer, where the attraction for the opposed electricity does not even support the transition. In the mean time De la Rive has mentioned several experiments, which were to prove in a direct manner, that contact, without contemporaneous chemical action, was incapable of developing electricity. We will at present follow him in these experiments.

2. Already, some time ago, De la Rive had published some experiments, which he now again brings in support of his view (*Recherch.*, p. 57), according to which the signs of electricity, produced by two plates in contact in common air, no longer appear in a vacuum and in dried air. Nevertheless it is evident from the manner in which he performed these experiments, that their negative result is rather owing to his having connected the condenser made use of with the earth, by means of perfectly dried wood: now every one may convince himself that even in performing this experiment in common air the condenser under these circumstances refuses its services. Besides, Pfaff has already previously refuted these experiments experimentally, and with regard to further observations on this point I may refer to p. 20 of his work.

3. De la Rive, previously, in another place, and lately in his *Recherch.*, p. 60, has made the following experiment of importance.

“A piece of potassium or sodium was fixed in a solid manner by one of its ends to a platinum forceps, while the other extremity was held by means of a wooden forceps, or what was better, an ivory one. If, after having well brightened it, it is surrounded by very pure oil of naphtha, [*steinöl*] and the condenser be touched with the end of the platinum forceps, no electrical sign is observable; while if the naphtha oil is taken off and none remain adhering to the metal, this is observed to oxidate rapidly by the contact of the air, and the electricity indicated by the electroscope is of the most lively kind. The condenser is scarcely necessary to render it perceptible. If sometimes some indications of electricity are obtained when the potassium

or sodium is in the oil of naphtha, then a small quantity of humidity has been introduced into the liquid, which had remained adhering to the surfaces of the two metals, and which exercises on them a chemical action which it is easy to recognise. Immersed in azote and in hydrogen the two metals still gave rise to a development of electricity proceeding from the action exerted upon them either by the gases, or by the aqueous vapour, from which it is impossible entirely to free them (?); and in proof of this chemical action, we see their surfaces lose their metallic brightness and become tarnished very much as would have taken place in the air."

The result which I would draw from a repetition of this experiment is this; that in the form adopted by De la Rive, it is altogether unfit to give a proof of the one or the other view, because potassium brought into connexion with the earth by means of *dried* wood, does not allow us to perceive either *out of or in petroleum*, [*steinöl*] any development of electricity, which, according to both views, is explicable from the property of the condenser; but brought into connexion with it, by means of *moist* wood, it immediately produces, both in and out of the petroleum, powerful action, which the supporter of the chemical view can explain from the chemical action produced through the moisture, the supporter of the voltaic theory, from the increased conducting power of the wood; of the influence of which in this case sufficient evidence is contained in the following details. The experiment, however, may be, in fact, so varied, that it becomes decisive, and in this form it speaks, as will be seen, *against* the chemical theory. The following is the detail of the experiments.

A potassium ball was furnished at two opposite points with fresh surfaces of section: into one surface was inserted the end of a platinum wire, into the other the end of an air-dried bar of wood, so that the ball was situated between both; now the highly sensible condenser (at times brass, at times copper) which was connected with the electroscope with a dry pile described by me in these *Annals*, was brought into contact with the platinum, while the wood was held in the hand. I could perceive by the well-known manipulation with the condenser either no, or very inconsiderable, traces of electricity. I then substituted for the wood a cut chip of a quill, and obtained the same result. If on the contrary, I held the potassium ball in the hand, and brought the condenser into contact with the platinum, I obtained a divergence of the gold-leaf, of remarkable strength*, without comparison much more

* This was even the case when the potassium ball was not cleansed from the adherent petroleum; and I cannot in the least from my experi-

powerful than we are accustomed to obtain with zinc; this is therefore well adapted to serve as an experiment of demonstration in lectures. The same was the case if I held the bar of wood after slightly moistening it; and in the latter case, it was quite indifferent for the production of the effect, whether the ball was immersed in petroleum or not. I must therefore suppose, that De la Rive, since he obtained a result out of the petroleum, but none when the ball was in it, produced the conducting connexion with the earth through wood or ivory only within the petroleum, but when out of the petroleum by a more humid conductor; otherwise there would be a direct contradiction between his and my observations. One may easily believe, since this contradiction did not escape me, that I frequently repeated and varied the form of this experiment, in order to convince myself that on repeating it in various ways, right would remain on my side.

It remained now to observe, whether the absence of divergence by the use of air-dried wood was owing to the want of conducting power of the wood, or to the want of chemical action: for it could not indeed be denied, that, if the potassium ball was held with the hands or by means of a moist bar of wood, their moisture must act chemically upon it, which also, when the experiment is performed under petroleum, may easily be recognised by the bubbles of gas rising from the point of insertion of the slightly moistened wooden bar. The following experiment, more direct than all which De la Rive cites in support of his views, proves quite decidedly that the bad conducting power of the air-dried wood alone at least *suffices* to explain the negative result. If I moistened one half of the bar which stood in connexion with the potassium, and also the point of insertion in the potassium, just as in the former experiments in which I obtained a result, but held the wood during the contact of the condenser with the platinum by the half which remained air-dried, I obtained the former negative result; nay, this was even then the case, if while thus manipulating I moistened the potassium during the contact of the condenser with acidulated water, so that a kind of explosive, chemical action took place. By this then the insufficient conducting power of air-dried wood for such experiments is sufficiently proved, and every one may easily, and even without potassium, convince himself by substituting zinc for that metal, when the phænomena take place in quite

ments concur in the view of Ohm and Pfaff, that an isolated intermediate layer of petroleum acted a part in De la Rive's experiments.

Phil. Mag. S. 3. Vol. 13. No. 81. *Sept.* 1838. P

the same manner if the same arrangement be observed. We see therefore that De la Rive's experiment, in petroleum, according to his account of it, could not succeed, either according to the chemical theory, or according to the voltaic theory, and that when it is all taken together, he proves nothing whatever. I succeeded however in varying these experiments so that an argument may be drawn from them *against* the chemical theory.

From the extreme vivacity with which the divergence took place when the potassium attached to the condenser by means of a platinum wire or even by direct contact (for in effect the platinum is in this case quite unnecessary) stood in connection with the earth by a moist conductor, and from the excessive sensitiveness of the electrometer with a dry pile which I employed, it appeared to me not improbable, that even without a condenser, by the contact of a negative metal with potassium, a divergence would take place. If the brass point from which the gold-leaf was suspended was brought into contact either directly, or even with the interposition of an air-dried strip of paper or linen, which appeared to conduct much better than air-dried wood, with the platinum wire of the potassium ball, whilst the latter was held immediately with the hand or by a slightly moistened slip of wood, there might be perceived, when the gold-leaf had a certain stability, a very weak, but undeniable, negative electrical divergence; and also a positive electrical one, if by reversing the combination I held with my fingers the platinum wire, and brought the electrometer with the intermediation of the air-dried inter-conductor, or even only a layer of oxide which had formed on the potassium, into contact with that metal. From the numerous and varied experiments, sometimes bringing the electrometer into contact with insulated, then uninsulated platinum, *without the agency of potassium*, I became convinced, that in fact no divergence of the electrometer would be produced by such contacts; the contact of the potassium was necessary; similar counter-experiments have not been neglected in the following.

From the distinctness of the signs obtained (which as yet do not afford any objection against the chemical theory) it appeared to me not impossible, even by the entire insulation of the potassium, to render signs of electricity perceptible, and thus entirely to do away with the influence of moisture. Under these circumstances, the condenser, for known reasons, cannot be made use of; on the other hand, it is true that this difficulty stood opposed even to the result of the application of an

electrometrical apparatus constructed on different principles, namely, that the intensity of the electricity of two heterogeneous plates in contact is in the inverse ratio of the magnitude of their surfaces or magnitude of conduction; consequently, if the superficies of the electrometer be considerably larger than that of the potassium, and moreover the former be not well insulated, no result can be expected. In effect I could no longer obtain those signs of electricity which I had previously recognised with such complete certainty, if I held the potassium in a dry insulating forceps and touched my electrometer with the platinum wire of the potassium. I completely succeeded, however, even with this fine experiment in the following manner. I had an electrometer purposely made with the smallest surface possible, consisting solely of a very thin and short brass wire, which, as the axis of a surrounding gum lac cylinder, traversed the perforated bottom of an inverted drinking-glass, and from which within the glass was suspended between the pole plates of the pile of the electrometer a very small gold-leaf, $2\frac{1}{2}$ inches long, while the electricity could be transferred to the prominent end of the brass without the glass. Into the potassium ball was inserted a thin platinum wire, as short as the convenience of transfer of the electricity allowed, and the ball itself, for the purpose of increasing its surface, was pressed between two copper plates which had been soaked in petroleum, as smooth as was possible without cutting the potassium ball with the platinum wire. Thus the entire electrometer might have been somewhat about double the size of the surfaces of the potassium*.

With this arrangement I now repeated the former experiments in the air, holding at times the potassium with the insulating forceps, at times the platinum, while the other metal was each time brought into connexion with the electroscope. If clear, opposite divergence of the gold-leaf could in this case be observed with the most complete certainty; for this arrangement, if the combination of potassium and platinum was not insulated, proved to be much more sensitive than the one previously employed, and gave without the condenser the most evident divergences, (opposite by the opposite arrangement,) which might easily be increased up to the striking of the gold-leaf against the pole plates, if these were merely separated so far from one another ($7\frac{1}{2}$ lin.) as was necessary for the stability of the gold-leaf. I now placed the potassium

* I endeavoured to unite several balls of potassium by pressure, but did not succeed, either from the petroleum which remained attached to them, or from the oxide which immediately formed at the surfaces of the fresh sections.

disc with the upwards bent wire, proceeding from it, in a small glass, covered it with petroleum up to about half an inch high, and discharged the platinum wire which projected from the petroleum (and which nowhere touched the glass) on to the electrometer, while I held the glass in my hand. *The divergence to the side, which indicates the negative electricity, followed in this case quite as constantly, evidently, and certainly as if the potassium had been insulated in the air.* I have already mentioned that the counter experiments in this case were not neglected.

The following modes appear to me still to offer themselves for the explanation of the result of this experiment according to the chemical theory:

a. Some moisture was brought together with the potassium into the petroleum, the chemical action of which caused the result.

b. The petroleum was perhaps adulterated and still capable of acting chemically on the potassium.

With respect to *a.* this objection has at first sight the appearance of some weight, since it is true that when the potassium is brought from the air into the petroleum, we observe during some time a few gas bubbles rise up from the potassium, which undoubtedly are produced by the chemical action of the adhering moisture. But this development of gas soon ceases; and long after this had *entirely* disappeared, twenty-four hours afterwards, during which time the potassium continually remained immersed in the petroleum, (I did not observe it for more than twenty-four hours afterwards) I have obtained the electrical signs in the petroleum *of quite the same force*, as during the development of gas and even in the air; so that this objection thus completely falls to the ground.

As to the objection *b.* the petroleum employed was that in which the potassium had already been preserved for many years without becoming anything more than deprived of its lustre at its surfaces; while if the petroleum had contained any oxygen, this must necessarily have produced a gradual destruction of the potassium; and even if the oil had in the beginning contained any oxygen, this would certainly have been consumed during the long preservation of the potassium in it, so that I conceive this second objection also to be completely refuted.

4. An experiment on which De la Rive appears to lay particular stress, since he refers to it in several places, is the following (*Recherch.*, p. 67.):

“ I took two zinc plates exactly similar with regard to their dimensions to the brass plates of an ordinary condenser;

I soldered to each a brass knob (*bout de laiton*) ; I covered their inner surface with a thin layer of lac varnish, so that they might perform the office of the plates of a condenser ; I further entirely covered the exterior surface of one of the zinc plates with a layer of the same varnish, so that this plate was not in immediate contact with the air at any of its points. Various experiments were made, forming the condenser sometimes with the two zinc plates, sometimes with one of them only and with a brass plate. When the zinc plate which I employed was that the entire surface of which was covered with varnish, I constantly obtained electrical signs much less strong than in employing that one of which the exterior surface was entirely uncovered and exposed to the immediate contact of the air. Presuming that the electricity, very feeble it is true, which was developed with the varnished zinc plate, arose from the circumstance that the layer with which it was covered was too thin to intercept completely all chemical action of the air and of the humidity, I successively increased the thickness of this layer, and I succeeded in rendering it such that the plate ceased to give any electrical signs. What moreover proves that it is to the action, even through the varnish, which the moist air could exercise upon the surface of the zinc, that the production of the electricity was owing, was that I observed after a short time a commencement of oxidation take place on this surface. We see, therefore, that when a zinc plate, by means of a layer of varnish, is entirely protected from the action of the air or of those agents which might exert a chemical action upon it, it does not become electric in its contact with a brass knob. Still more ; this inactive apparatus conducts itself as a homogeneous plate of brass. Thus on touching the brass knob which was soldered to the surface of the zinc, with the copper of a heterogeneous plate, of which I held the zinc in my hand, I succeeded in charging it with negative electricity. Or, according to the contact theory, the two coppers being in immediate contact, and placed between the two zincs soldered to them, viz. that of the condenser and that which I held in my hand, no result ought to have taken place. We might obtain a result of the same kind, perhaps, still more striking, by uniting for the purpose of forming the conductor the two plates of zinc and making the two brass knobs soldered to them communicate with one another. In the theory of contact, the opposition of these two pairs, perfectly similar, ought to have neutralized all kind of action ; however, the experiment showed that the zinc plate, the naked surface of which was exposed to the air, became charged with positive electricity, while that of which the

surface was covered with a thick layer of varnish became negative, just as a brass plate would have become in the same case."

These experiments struck me in a very high degree, and I have repeated them quite according to the method mentioned by De la Rive, with all the care which I considered to be due to experiments, which, accordingly as their result might turn out, would really testify for or against the chemical theory. But the issue was simply this; that the results, after the varnishing over of the zinc condenser, to which was soldered a copper knob, and after the varnish had become completely dry, were not perceptibly distinguishable from those which I obtained by over varnishing with the same condenser, although the layer of lac varnish with which I had covered by often repeated coatings the entire non-condensing surfaces of the zinc condenser (while the condensing surface could only retain their thin coating of varnish, as was the case in De la Rive's experiments according to an express statement of his in another place) was laid on excessively thick, and with the most anxious care that no point of the zinc might be left free. To go a step further still than De la Rive, I fixed, in order to lay aside the objection which perhaps might be raised respecting the chemical action of the air upon the copper, a platinum wire to the copper point, and now varnished over even the whole copper point, so that platinum alone remained free. If the platinum was now touched with the finger or with a slip of paper which had been moistened in distilled water, the zinc condensers became quite as well charged with positive electricity as if it had not been varnished. Besides we are already in possession of some experiments of Becquerel and Peltier (vide Becquerel's *Traité*, ii. p. 139) which have given a result similar to my own. However, in Becquerel's a small part of the zinc instead of being varnished was merely covered by glass, which De la Rive does not consider as being sufficient. In Peltier's experiments this circumstance was avoided; but the action of his apparatus, which was arranged in a different manner, I have not been able fully to comprehend. But also in Pfaff, I find (vide his work, p. 22.) that he had repeated De la Rive's experiment quite in accordance with his own statement and had always observed the same action of the zinc condensers with varnish as without varnish.

Moreover I have succeeded in deciding affirmatively the question, whether varnished zinc in connection with electro-negative metals developes electricity in a way by far more simple than by means of the condenser.

By the application of an electrometer, quite similar to the one described under No. 3, except that it has a longer gold-

leaf ($4\frac{1}{2}$ Parisian inches), and instead of a brass point passing through the lac, one of gold, I obtain with a zinc plate to which a platinum wire is fastened, a constant divergence of the gold-leaf to the right or left, very small, it is true, but yet subject to no deception, accordingly as I connected the one or the other metal with the gold wire, while I held the other in the hand; only by the connexion of the zinc with the gold wire the interposition of a moist conductor is, for reasons easily understood, necessary; but not so, if I connected the platinum with it. For the latter case only I have employed unvarnished zinc, as also coated with a thick layer of varnish (so that even the point of connexion of the zinc with the platinum was carefully covered with it), and in both cases I obtained results not differing in any perceptible degree (namely, a remarkable equally strong negative divergence). The size of the zinc plate was such that the surface of the electrometer was inconsiderable in comparison with it. For the success of this experiment the pole plates must be placed as close as is compatible with the stability of the gold-leaf.

5. De la Rive makes of importance (*Recherch.*, p. 66) that Becquerel by means of sensitive apparatus, according to the method which I have called the *second* in my paper in the *Annals*, vol. xli. p. 226, with the application of a gilt condenser could, however, not demonstrate the slightest trace of electricity between gold and platinum (*Ann. de Chim. et Phys.*, xlv. p. 292, or *Traité de l'Electr.*, t. ii. p. 137), and attributes this to the want of the chemical action of the air upon both metals; while in fact, experiments with the multiplier, by the application of a fluid which would attack these metals, show that their combination is capable of exciting electricity. To this we may answer, that a multiplier only moderately sensitive is without comparison a more sensitive instrument for the demonstration of the weakest traces of electricity than the most sensitive condenser; therefore, by the undeniable only very weak electromotive oppositions of the gold and platinum, for which their chemical state itself speaks, it is very evident that the one instrument still indicates an action where the other would appear to show that none whatever takes place. Besides, the experiment of Becquerel completely loses all weight, for this reason; that he found in the same experimental series, that graphite and some other bodies, on which no chemical influence of the air can be proved, are capable of a development of electricity with gold, plainly recognisable with the condenser, even after they had previously been washed in distilled water; and I myself have already formerly men-

tioned the development of electricity between gold and silver, which may be demonstrated in a weak degree even without a condenser. If, indeed, De la Rive even in all these cases *supposes* (supponit) a chemical action, I have only to observe, that it must also be allowed me to suppose in all cases an electrical action, where it cannot be demonstrated, where further it agrees better with the contact theory.

6. Biot long ago convinced himself that piles constructed of an equal number of pairs of plates, with fluids of very different chemical action, as water, solution of common salt, of sal ammonia, of chloride of potassium, of sulphate of iron, indicated at the electrometer the same intensity of the poles. If other fluids, such as solution of soda, showed exceptions (which become explicable according to the theory of contact by a change of the metallic surface), these are certainly not of the kind that can speak in favour of the chemical theory. De la Rive himself performed similar experiments (*Recherch.*, p. 142) and found that piles constructed of an equal number of plates with river water, solution of Glauber's salts and greatly diluted nitric acid, indicated an equal power of electricity at the insulated pole, where (as was also the case in Biot's experiments) the other pole stood in connexion with the ground; if on the other hand both poles are insulated, a difference occurs, and the diluted nitric acid gives the weakest, often quite imperceptible electrical signs.

In order now to explain the equality of power, (for the first appearance is evidently opposed to the chemical theory) by construction of the pile with different fluids (in the case of non-insulation) De la Rive supposes that there is, it is true, more electricity developed by the liquids which attack more strongly, but that always one part of the developed opposite electricities reunites even through the members of the pile. But since fluids of stronger chemical action possess in general also a better conducting power, they would allow of a quicker reunion, and with this would be explained how the free portion of electricity is not stronger with them than with fluids which act less powerfully. Disregarding however other objections which might easily be raised against this view, it were yet very curious, if this compensation in the various fluids should amount exactly to the equality of the actions, and the more so, since the conducting power of the fluid members of the pile also depends in some degree on their dimensions; consequently the compensation could only be exact with one single thickness of the fluid layers. With the theory of contact the equality of the intensity of non-insulated piles in the construction with various fluids (in so far as they do not charge

their metallic surfaces) proceeds as a natural consequence without the aid of any hypotheses. The various action of the insulated pile is still enigmatic, but might possibly depend on this, that with a bad conducting fluid the amount of conduction of the pile becomes greater, since then the electricity probably penetrates even between fluid and metallic surface, which might be less the case if the conducting power of the fluid approached more to that of the metal; a subject moreover on which special experiments are still desirable.

[To be continued.]

XXVIII. *A New and General Solution of Cubic Equations.*

By JOHN T. GRAVES, of the Inner Temple, Esq., M.A.*

IN the ordinary books of algebra, (so far, at least, as my limited acquaintance with them extends,) where cubic equations are discussed, the cases of *real* coefficients only are considered, and different methods of solution are given in order to effect the separation between the constituents† of the roots in different cases. I have obtained a symmetrical solution of the equation

$$x^3 + (x + \sqrt{-1}\lambda)x + \mu + \sqrt{-1}\nu = 0 \quad (1.)$$

which presents the constituents of x in an explicit form in all cases. This is all that is wanted, for the solution of a cubic equation of the general form.

$$\begin{aligned} (\alpha_1 + \sqrt{-1}\beta_1)y^3 + (\alpha_2 + \sqrt{-1}\beta_2)y^2 + (\alpha_3 + \sqrt{-1}\beta_3)y \\ + \alpha_4 + \sqrt{-1}\beta_4 = 0 \end{aligned} \quad (2.)$$

may easily be made to depend on the solution of a cubic equation of the form (1.); and, from the nature of the relation between the transformed equation (1.) and the original equation (2.), the constituents (x, λ, μ, ν) of the transformed coefficients $(x + \sqrt{-1}\lambda, \mu + \sqrt{-1}\nu)$ are easily determinable, supposing the constituents of the original coefficients to be explicitly given: and if the constituents of x be determinable, those of y can easily be determined.

The limits of this Magazine do not permit an exposition of my *process*, which I intend hereafter to communicate at length through some more appropriate medium. It consists in an analysis of the following formula for x .

$$x = -\sqrt{-1} \frac{2}{\sqrt{3}} p^{\frac{1}{2}} \cos \left\{ \frac{1}{3} \cos^{-1} \left(\sqrt{-1} \frac{3\sqrt{3}}{2} \frac{q}{p p^{\frac{1}{2}}} \right) \right\} \quad (3.)$$

where $p = x + \sqrt{-1}\lambda$ and $q = \mu + \sqrt{-1}\nu$, and where the

* Communicated by the Author.

† I call α and β the "constituents" of the expression $\alpha + \sqrt{-1}\beta$.