On the Present State and Prospects of some Branches of Pure Mathematics. By H. J. STEPHEN SMITH, Savilian Professor of Geometry in the University of Oxford.

[*Read November 9th*, 1876.]

I have been led to believe that the Society may not be unwilling to allow a certain latitude in the scope of the remarks which they permit their Presidents to address to them upon retiring from the Chair. Relying upon this belief, I propose, on the present occasion, to invite your attention to some considerations relating to the present state of Mathematical Science, with especial reference to its cultivation in this country, and to our own position as representing a great number of those who are interested in its advancement. The subject is so extensive that I am sure you will excuse me if I endeavour to limit it in every way I can. I propose, therefore, to exclude from what I have to say all that relates to Applied Mathematics, and to ask you to confine your attention to questions of Pure Mathematics only. I am well aware how much by this exclusion I restrict the field before me; but the restriction is forced upon me, not only by the limit of time, but by the far narrower limits of my own knowledge. And I cannot help adding that I shall regard it as a fortunate circumstance, if the attention of my successor, when he in his turn is looking round him for a subject for his own Presidential Address, should be attracted by a domain, upon which I must myself decline to enter, but of which he, better perhaps than anyone among us, is fitted to take a clear and comprehensive view.

The restriction which I have mentioned is far from being the only one which I must impose upon myself. I can only presume to offer fragmentary remarks upon great subjects, in the hope that even such casual and hasty notices may not be without their use, if they serve to remind us of the vastness of our science, and yet of its unity; of its unceasing development, rapid at the present time, and promising to be no less rapid in the immediate future; of its marvellous power of assimilating to itself the accessions which each year brings to our knowledge of external nature, while yet it derives strength and vitality from roots which strike far back into the past, so that the organic continuity of its gigantic growth has been preserved throughout.

In every science there is a time and place for general contemplations, as well as for minute investigations. And it is a rule of sound philosophy that neither should be neglected in its proper season. "Itaque alternandæ sunt istæ contemplationes," says Lord Bacon, "et vicissim sumendæ, ut intellectus reddatur simul penetrans et capax."* Per-

* Novum Organon, Lib i., Aph.

haps it is the besetting sin of mathematicians to concentrate the mental vision upon as narrow and definite a field as possible. And there is much to be said in excuse for our indulgence of this tendency. If we are to find anything worth finding in the mines of mathematical research, we must dig deep; and if we want to dig deep, we must, if we are not gifted with Herculean force, confine our efforts to a narrow superficial area. But the tendency is not without its peril. The illustrious mathematician under whose auspices this Society was founded, felt it right in his opening address to warn us against the danger. "Our subject," said Augustus De Morgan, on the 15th Jan. 1865, with a characteristic irony of expression, "Our subject is really rather a wide one. But there are mathematical publications in which it is contracted; and it is often treated as a narrow subject." He cautioned us against falling into "a line which may be useful, but which is still confined and partial"; and, while exhorting us to do our part in the additions to the more rapidly developing "branches of the science," he bid us at the same time take care "not to let any one particular branch overgrow us." It would not have been Augustus de Morgan if he had not added some pointed criticisms upon examinations in general, and on Cambridge examinations in particular, and if he had not cautioned us against any excessive admiration for that part of mathematical ingenuity which devotes itself to the narrowest of all the narrow fields ever chosen by a mathematician, the invention and solution of "ten-minute conundrums."

It is now nearly twelve years since these warnings were given to the infant Society by its first President; and perhaps the time may have arrived when we might put to ourselves the question whether its subsequent history has shown that we have profited by the lessons of that eminent and large-minded teacher. It would ill become me to attempt to answer such a question. I would only venture to express, and that with great diffidence, the double opinion—that, on the one hand, the mathematical world will wholly acquit the Society of having devoted its energies to little or trivial subjects; but that, on the other hand, while it would be universally conceded that the volumes of our Proceedings contain memorable additions to mathematical knowledge, it might be alleged by an "advocatus diaboli" (if such a character should be assumed by some severe critic) that we, in this respect resembling the other mathematicians of our country, have shown, and still continue to show, a certain partiality in favour of one or two great branches of the science, to the comparative neglect and possible disparagement of others. Perhaps it would be well to begin our reply by denying the charge; but, having done so, if we should be advised to urge a second and somewhat contradictory plea, we might with great plausibility rejoin that ours is not a blameable partiality, but a well-grounded preference. So great (we might contend) have been the triumphs achieved in recent times by that combination of the newer algebra with the direct contemplation of space which constitutes the modern geometry —so large has been the portion of these triumphs which is due to the genius of a few great English mathematicians—so vast and so inviting has been the field thus thrown open to research,—that we do well to spend our time and our labour upon a country which has, we might say, been "prospected" for us, and in which we know beforehand that we cannot fail to obtain results which will repay our trouble, rather than adventure ourselves into regions where, soon after the first step, we should have no beaten tracks to guide us to the lucky spots, and where the daily earnings of the searcher for mathematical treasure are (at the best) but small, and do not always make a great show even after long years of work. Such regions, however, there are in the domain of pure mathematics, and it cannot be for the interest of science that they should be altogether neglected by the rising generation of English mathematicians.

I propose therefore, in the first instance, to direct your attention to some few of these by us comparatively neglected regions; and foremost among them I must name the Theory of Numbers. Of all branches of mathematical enquiry this is the most remote from practical applications; and yet, more perhaps than any other, it has kindled an extraordinary enthusiasm in the minds of some of the greatest mathematicians. We have the examples of Fermat, Euler, Lagrange, Legendre, and Gauss, of Cauchy, Jacobi, Lejeune Dirichlet, and Eisenstein, without mentioning the names of others who have passed away, and of a few who are still living. But, somehow, the practical genius of the English mathematician has in general given a different direction to his pursuits; and it would sometimes seem as if we in England measured the importance of the subject by what we find of it in our text-books of Algebra, or as if we regarded its enquiries as problems of mere curiosity, without a wider scope, and without direct bearing on other branches of mathematics. I might endeavour to remove this impression—if indeed it exists in the minds of any of those who hear me—by enumerating instances in which the advancement of Algebra and of the Integral Calculus appears to depend on the progress of the arithmetic of whole numbers. But, instead of wearying you with the details which would be necessary to make such an enumeration intelligible, I would rather ask you to listen to what is recorded of the most eminent master of this branch of science. "Gauss," we are told by his biographer, "held Mathematics to be the Queen of the Sciences, and Arithmetic to be the Queen of Mathematics." "She sometimes condescends"—so spoke the great Astronomer and Physicist—"to render services to astronomy and the other natural sciences, but under all circumstances the first place is her due."* In a more serious mood he wrote, "The higher arithmetic presents us with an inexhaustible storehouse of interesting truths—of truths, too, which are not isolated, but stand in the closest relation to one another, and between which, with each successive advance of the science, we continually discover new and sometimes wholly unexpected points of contact. A great part of the theories of Arithmetic derive an additional charm from the peculiarity that we easily arrive by induction at important propositions, which have the stamp of simplicity upon them, but the demonstration of which lies so deep as not to be discovered until after many fruitless efforts; and even then it is obtained by some tedious and artificial process, while the simpler methods of proof long remain hidden from us."† Or, again, let the young mathematician, who feels an instinctive liking for arithmetical enquiry, be encouraged by the observation which has been put on record by Jacobi in his brief notice of the life of Göpel, that many of those who have a natural turn for mathematical speculation find themselves in the first instance attracted by the Theory of Numbers.‡

There are three great departments of arithmetic, not, it must be admitted, wholly separable from one another, which seem to me at the present time to offer a very inviting field to the researches of the mathematician. Of these I will name first the arithmetical theory of homogeneous forms, or quantics, as we in England have now learned to call them. It is worthy of remembrance that some of the most fruitful conceptions of modern algebra had their origin in arithmetic, and not in geometry or even in the theory of equations. The characteristic properties of an invariant, and of a contravariant, appear with distinctness for the first time in the Disquisitiones Arithmeticæ; § and in that treatise also the attention of mathematicians was for the first time directed to the study of quantics of any order and of any number of indeterminates.|| Again, Eisenstein in the course of his researches on the arithmetical theory of binary cubic forms was led to the discovery of the first covariant ever considered in analysis—the Hessian of the

* "Gauss. Zum Gedächtniss." Von W. Sartorius v. Waltershausen. Leipzig, 1856.

† Preface to Eisenstein's "Mathematische Abhandlungen." Berlin 1849.

‡ Notiz über A. Göpel, Crelle's Journal, Vol. xxxv. p. 313.

§ Disq. Arith., Arts. 157, 267, 268, where binary and ternary quadratic forms are considered.

|| "Sed manifesto hoc argumentum" [the theory of binary quadratic forms] "tanquam sectionem maxime particularem disquisitionis generalissimæ de functionibus algebraicis rationalibus integris homogeneis plurium indeterminatarum et plurium dimensionum considerare . . . possumus." "Sufficiat hunc campum vastissimum geometrarum attentioni commendavisse, in quo materiem ingentem vires suas exercendi, arithmeticamque sublimiorem egregiis incrementis augendi invenient." (Disq. Arith., Art. 266.)

cubic form.* But the progress of modern algebra and of modern geometry have far outstripped the progress of arithmetic, and one great problem which arithmeticians have before them at the present time is to endeavour to turn to account for their own science the great results which have been obtained in the sister sciences. How difficult this problem may prove is perhaps best attested by the little progress that has been made towards its complete solution. One or two instances may serve to illustrate the actual position of the enquiry. The algebraical problem of the automorphics of a quadratic form, containing any number of indeterminates, may be regarded as completely solved by a formula due to M. Hermite and Professor Cayley.† The arithmetical formula which gives the automorphics of a binary quadratic form has long formed a part of the elements of the theory of numbers; and the corresponding investigation for an indefinite ternary quadratic form may be now regarded as completed by the memoirs in which M. Paul Bachmann has followed up the earlier researches of M. Hermite.‡ Again, the problem of the equivalence of two positive or definite ternary quadratic forms was completely solved by Seeber; and the problem of the arithmetical automorphics of such forms, by Eisenstein.§ The corresponding but far more difficult problem of equivalence for indefinite

* See "Crelle's Journal," Vol. xxvii. p. 89. It is remarkable that the cubic covariant does not explicitly appear in this paper (December, 1843) or in the note (p. 105) which immediately follows it. This omission is supplied in a subsequent note, dated March 3, 1844 (*ibid.* p. 319). The earliest papers of Boole, who approached the study of linear transformations from a geometrical point of view, belong to the years 1841 and 1843 (Cambridge Mathematical Journal, Vol. ii. p. 64 and Vol. iii. p. 1 and p. 106). In these papers (of which perhaps only the first should be cited here) covariants do not appear, but the first general theorem of invariance ever enunciated, the theorem of the invariance of the discriminant of any quartic, is distinctly stated and proved.

† M. Hermite, in "Crelle's Journal," Vol. xlvii. p. 309, appears to have considered forms of three indeterminates only; his solution was subsequently generalized by Professor Cayley (*ibid.* Vol. i. p. 288). See also a later memoir by Professor Cayley "On the Automorphic Linear Transformation of a Bipartite Quadric Function," in the "Philosophical Transactions" for 1858.

‡ The solution of the problem is made to depend on the solution in integral numbers of the indeterminate equation $p^2 + F(q_1, q_2, q_3) = 1$, where F is the contravariant of the given ternary form. No general method, however, of obtaining the solutions of this equation has as yet been given. (See M. Hermite in the memoir already cited, "Crelle," Vol. xlvii. p. 307 *sqq.*; M. Bachmann, "Borchardt," Vol. lxxi. p. 296, and Vol. lxxvi. p. 331, together with the note by M. Hermite completing his former solution, *ibid.* Vol. lxxviii. p. 325.)

§ See L. Seeber, "Untersuchungen ueber die Eigenschaften der positiven ternären quadratischen Formen," Freiburg, 1831; and, in connection with this work, the review of it by Gauss (in the Göttingen "Gelehrte Anzeige" for 1831; or in "Crelle," Vol. xx. p. 312; or in the collected edition of Gauss' Works, Vol. ii. p. 188), and the subsequent and simpler investigations of Dirichlet ("Crelle," Vol. xl. p. 209), and M. Hermite (*ibid.*, p. 173). The theory of the automorphics of positive ternary quadratic forms is given by Eisenstein in the Appendix to his "Table of Reduced Positive Ternary Quadratic Forms." ("Crelle," Vol. xli. p. 227.) He observes (see the note at p. 230) that Seeber, without actually solving the problem, had come extremely near to its solution.

ternary forms has received its first solution only in very recent times from M. Eduard Selling;* and perhaps it is not too much to hope that these profound researches may receive some further development from their distinguished author, and may be brought into closer relation with other parts of arithmetical and algebraical theory. So far, then, as binary and ternary quadratic forms are concerned, we have not much reason to complain of the slowness of the advances made by arithmetic. But if we pass to quadratic forms of four or more indeterminates, we shall find that the limits within which our arithmetical knowledge is confined are indeed restricted. The fundamental theorem of M. Hermite, that the number of non-equivalent classes of quadratic forms having integral coefficients and a given discriminant is finite, and the recent researches of M. Zolotareff and Korkine on the minima of positive quadratic forms, mark the extremest limit to which enquiry has been pressed in this direction.†

As a second and much simpler instance of the difficulties which remain for arithmetic after the work of algebra is done, let us consider the system of two binary quadratic forms. The first question that we naturally ask, is, what is the arithmetical meaning of the evanescence of their joint invariant? I gave myself an answer to this question some years ago in the following theorem, which for brevity I express in the proper technical language.

"If the joint invariant of two properly primitive forms vanishes, the determinant of either of them is represented primitively by the duplicate of the other." ‡

This theorem is very far from exhausting the subject to which it refers. But it may serve as a fair illustration of the class of enquiries which I wish to propose to the attention of the Society as likely to be not unfruitful. The geometrical interpretation of the invariantive character to which the theorem relates is (as we all know) that the two pairs of elements, represented by the two quantics, are harmonically conjugate; and I think it especially deserving of notice that the same invariantive character has an important meaning both in arithmetic and in geometry, but that neither of the two interpretations seems in the least likely to suggest the other. If we pass on to the case of two ternary quadratic forms, the geometrical signification of the evanescence of either of their joint invariants is now embodied in well-

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* "Borchardt's Journal," Vol. lxxvii. p. 143.

† See the Letters of M. Hermite to Jacobi ("Crelle," Vol. xl. p. 261, *sqq.*), and the papers of M. Zolotareff and Korkine ("Clebsch," Vol. v. p. 581, and Vol. vi. p. 366). I may perhaps also be allowed to refer to my own papers "On the Orders and Genera of Quadratic Forms containing more than three Indeterminates," in the Proceedings of the Royal Society, Vol. xiii. p. 199, and Vol. xv. p. 387.

‡ Report on the Theory of Numbers in the Reports of the British Association for 1863, p. 783, Art. 123.

known elementary theorems; but I do not think that any answer has been given to the corresponding arithmetical question, nor indeed do I know that anyone has occupied himself with it. I would, however, venture to hazard a conjecture that the arithmetical interpretation of these invariantive conditions may have an important bearing on the researches of M. Selling, to which I have already referred.

I do not wish to weary the Society with too many particular examples; but I will venture to point to one more instance from which it would appear that modern geometrical and analytical concepceptions may help us a little, if only a little, on our way in the trackless wilds of arithmetic. Let us take a question which has some relation to the familiar notions of "unicursality" and "one-to-one correspondence." It is an old theorem, that if the homogeneous indeterminate equation of the second degree containing three variables admits of one solution, it admits of an infinite number; and there is a Memoir of Cauchy* showing how from one given solution all the solutions are to be derived. But here two things deserve our notice—(1) That no geometry (so far as I am aware) helps us in any way to decide whether the given equation does or does not admit of solution. The criterion turns on the definition (first given by Eisenstein) of the generic characters of ternary quadratic forms—a definition which itself depends on a simple arithmetical inference from the algebra of such forms.† But (2), in strong contrast to what I have just stated, when once we have a single solution, the rest is a matter of intuitive geometry. Our equation represents a conic; the conic is real, because we have a single rational point on it; every rational line drawn through this point meets the conic again in a rational point; and thus the unicursality of the conic enables us to deduce from any one solution all the solutions which exist, and, what is more, to obtain them all in a natural sequence. To pass to a question of a somewhat higher order of difficulty, there is no known criterion (so far as I am aware) for deciding whether a ternary homogeneous cubic equation does or does not admit of solution in integral numbers. Such a criterion would be of great interest, and ought not, one would suppose, to lie beyond the present scope of analysis.‡ But here again geometry shows us at once, that if we have one solution, we have, in general, an infinite number. For the tangential of a rational point on a rational cubic curve is itself a

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* "Exercices de Mathématiques," vol. i. p. 233. Cauchy considers the ternary cubic as well as the ternary quadratic equation in this memoir.

† See Eisenstein, in "Crelle," Vol. xxxv. p. 117; and a note of my own in the Proceedings of the Royal Society for 1864, Vol. xiii. p. 110.

‡ Special cases of the equation here considered have attracted much attention. It will be sufficient to mention Fermat's theorem of the impossibility of the equation $x^3+y^3+z^3=0$.

rational point, and the line joining two given rational points meets the cubic in a third rational point. (There are, of course, cases of exception to this mode of derivation of one integral solution from another, but I need not advert to them here.) Advancing a little further, we have not to look very far into the connexion which modern algebra has established between ternary cubic and binary quadratic forms in order to satisfy ourselves that the Diophantine problem of rendering a biquadratic expression a perfect square (a problem which has been the subject of numerous researches ever since the time of Euler) is the same as the problem of finding rational points upon a cubic curve of which the equation is rational; and that, in particular, the *tangential* method to which I have just referred enables us in general to deduce an infinite number of solutions of the Diophantine problem from any given solution.

A second department of Arithmetic which, as it seems to me, has in quite recent times received less attention than it deserves, is the Theory of congruences. Some time before the notation of congruences had been introduced into the Theory of Numbers by Gauss, Lagrange, to whom the conception of a congruence (apart from any special notation) was perfectly familiar, had established the elementary theorem: *

"If an expression of the form $\frac{f(x)}{\phi(x)}$ [where $f(x)$ and $\phi(x)$ are rational functions of x having integral coefficients] acquires an integral value for any given integral value of x, the value of $\phi(x)$ must be a divisor of the resultant of $f(x)$ and $\phi(x)$."

This theorem naturally suggests another which was subsequently given by Cauchy: †

"If two congruences which have the same modulus admit of a common solution, the modulus is a divisor of their resultant."

These propositions suggest the possibility of transferring to the Theory of Numbers some at least of the results which have been obtained by modern researches in the theory of algebraical elimination. For example, we are led to consider the problem:

"Given two congruences, to find the number of their common roots when we take in succession for modulus each divisor of their resultant."

In all such inquiries we shall find that the considerations which suffice for the solution of the algebraical problem enter as indispensable elements into the arithmetical investigation, but that this investigation

* The "Disquisitiones Arithmeticæ" were published in 1801. The Memoir of Lagrange, entitled "Nouvelle méthode pour résoudre les Problèmes indéterminés," appeared in the Transactions of the Academy of Berlin for 1768. See also paragraph 4 (p. 528) of his Additions to the French translation of the Algebra of Euler (Lyons, 1774, and often reprinted since); the reference here is to the edition of the "an iii de l'ère républicaine."

† "Exercices de Mathématiques," vol. i. p. 164.

compels us to take notice of other elements also, with which, in algebra, we were not concerned. Thus, in the solution of the problem which I have mentioned, and which I hope at some future day to bring more fully under the notice of the Society, we should have not only to consider in a general manner the system of the divisors of the resultant itself, but we should also have to distinguish, in that system of numbers, those which are at the same time common divisors of certain systems of minors in the dialytic matrix of which the resultant is the determinant.

If I may be allowed to regard the subject of complex numbers as belonging to the theory of congruences, I must also be allowed to modify to a certain extent what I have said as to the indifference with which that theory has been looked upon in very recent times; for there is no reason to complain that complex numbers have received insufficient attention, at least from the mathematicians of the Continent. Without referring here to the results obtained by Lejeune Dirichlet, or to the splendid series of researches upon the complex numbers formed with roots of unity which we owe to M. Kummer, we may notice that the general theory has attracted the attention of M. Kronecker, whose investigations relating to it have unfortunately not as yet been published, and are only known from the application which he has made of them to the equations which present themselves in the theory of elliptic functions.* In a Supplement, added to the second edition of Lejeune Dirichlet's Lectures on the Theory of Numbers, M. Dedekind has given the outlines of a complete and very original theory of complex numbers, in which he has to a certain extent deviated from the course pursued by M. Kummer, and has avoided the introduction (at least in a formal manner) of ideal numbers.† I may remind my hearers that the inapplicability, in general, of Euclid's theory of the greatest common divisor to complex numbers formed with the roots of equations having integral coefficients renders it impossible to define the prime factors of such numbers in the same way in which we can define the prime factors of common integers, or of complex numbers of the form $a+b\sqrt{-1}$; and that, in the effort to overcome the difficulty thus arising, M. Kummer was led to introduce into arithmetic an entirely new and very important conception—that of ideal numbers. I shall ask leave to mention a very recent and very interesting application of this conception which has been made by M. Zolotareff to the solution

* See the "Monatsberichte" of the Academy of Berlin for June 26, 1862, p. 370. M. Dedekind also refers to the investigations of M. Kronecker at p. viii. of his Preface to the second edition of Dirichlet's Lectures, presently to be noticed.

† [M. Dedekind has also commenced the publication of a *résumé* of his theory in the "Bulletin des Sciences Mathématiques et Astronomiques" for December, 1876.]

of a problem of the Integral Calculus, which was first attempted by Abel, and has since attracted the attention of MM. Tchébychef and Weierstrass. We owe to Abel the remarkable theorem that the differential expression

$$du = \frac{(x+\lambda)\,dx}{\sqrt{(x^4+ax^3+bx^2+cx+d)}}$$

can or cannot be integrated by logarithms, for some value of the parameter λ, according as the radical can or cannot be developed in a periodic continued fraction. But Abel gave no criterion for deciding whether the development is or is not periodic—a question which obviously cannot be decided by mere trial. The problem was first solved by M. Tchébychef for the case in which the coefficients a, b, c, d are rational; and the complete solution has at last been obtained by M. Zolotareff, with the help of a new theory of complex ideal numbers.*

The last part of arithmetical theory to which I would wish to direct the attention of some of the younger mathematicians of this country is the determination of the mean or asymptotic values of arithmetical functions. This is a field of inquiry which presents great difficulties of its own; and it is certainly one in which the investigator will not find himself incommoded by the number of his fellow-workers. "Vix

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* The Memoir of Abel, "Sur l'intégration de la formule différentielle $\frac{\rho\,dx}{\sqrt{R}}$, R et ρ étant des fonctions entières," will be found in "Crelle," vol. i., p. 185, or "Œuvres Complètes," vol. i. p. 33. In this memoir Abel demonstrates the general theorem that, if R be a rational and integral function of x of any order, such that $\sqrt{R}$ can be expressed by a symmetrical and periodic continued fraction of the form

$$(1)\ldots\ldots\ldots\ldots \sqrt{R} = r + \frac{1}{2\mu+}\,\frac{1}{2\mu_1+}\cdots\cdots\frac{1}{2\mu_1+}\,\frac{1}{2\mu+}\,\frac{1}{2r+}\cdots\cdots,$$

it is always possible to find a rational and integral function ρ of x such that

$$(2)\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \int\frac{\rho\,dx}{\sqrt{R}} = \log\left(\frac{y+\sqrt{R}}{y-\sqrt{R}}\right) + C,$$

y being a rational function of x; and that, conversely, if the equation (2) can be satisfied, the development of $\sqrt{R}$ is of the form (1). Abel further shows that y is always the convergent $\lfloor r, 2\mu, 2\mu_1, \ldots 2\mu_1, 2\mu\rfloor$; and he gives the application of his theorem to the case in which R is a biquadratic function. The memoir of M. Tchébychef was first published in the "Bulletin de l'Académie de St. Pétersbourg" for 1860 (tom. iii. pp. 1—12), and has been reprinted in the "Mélanges Mathématiques et Astronomiques de St. Pétersbourg" (tom. iii. pp. 182—192), and also in "Liouville's Journal" (Second Series, 1864), vol. ix. p. 225. M. Tchébychef has given his method without demonstration; this was first supplied by M. Zolotareff in a paper, "Sur la méthode d'intégration de M. Tchébychef" ("Mathematische Annalen," vol. v. p. 560). The complete solution of the problem is contained in a work by M. Zolotareff ["Théorie des Nombres entiers complexes," St. Pétersbourg,] which I have not yet seen. The result obtained by M. Weierstrass (however remarkable in itself), that the integrability by logarithms of the differential du depends on the possibility of expressing a certain constant in the form $\alpha K + i\beta K'$, where α and β are rational numbers, and K and K′ are certain complete elliptic integrals, does not supply an available criterion. (See the "Fortschritte der Mathematik," vol. vi. p. 118.)

ullus reperietur geometra," wrote Euler* in the last century, "qui non, ordinem numerorum primorum investigando, haud parum temporis inutiliter consumpserit." But I do not think that, as a rule, the mathematicians of the present day have any reason to reproach themselves on this score, or stand in any need of the apology which Euler proceeds to deliver. Nevertheless, much has been done in this direction since the days of Euler; enough, certainly, to give abundant encouragement to further inquiry. The first asymptotic results that were obtained are due to Gauss, and are given without demonstration in the "Disquisitiones Arithmeticæ"; † they relate to the average number of classes of binary quadratic forms of a positive or negative determinant. The general principles on which such inquiries depend were laid down by Lejeune Dirichlet, forty-eight years later, in a memoir entitled "Ueber die Bestimmung der mittleren Werthe in der Zahlentheorie," and inserted in the Transactions of the Berlin Academy for 1849. The subject has recently been resumed by M. Mertens of Cracow, who, in an interesting memoir (Borchardt's Journal, vol. lxxvii., p. 289), has determined the asymptotic values of several numerical functions. In particular, he has demonstrated the expression given by Gauss for the mean value of the number of quadratic forms of a negative determinant; this mean value being, in the vicinity of n, when n is a considerable number, the quotient obtained by dividing $\frac{2}{7}\sqrt{n}$ by the sum of the cubes of the reciprocals of the natural numbers.

As to our knowledge of the series of the prime numbers themselves, the advance since the time of Euler has been great, if we think of the difficulty of the problem; but very small if we compare what has been done with what still remains to do. We may mention, in the first place, the undemonstrated, and indeed conjectural, theorems of Gauss and Legendre as to the asymptotic value of the number of primes inferior to a given limit x (the former theorem assigns for this value the integral logarithm $Li(x) = \int_0^x \frac{dx}{\log x}$, the latter an expression of the form $\frac{x}{\log x - a}$). ‡ But these theorems are only approximately

* Dec. 1, 1760, "Novi Commentarii Petropolitani," vol. ix. p. 99; or, "Commentationes Arithmeticæ Collectæ" (Petropoli, 1849), vol. i. p. 356.

† See Art. 302, 304, and the Additamenta to Art. 306, X; also various passages in the posthumous fragments, "De nexu inter multitudinem classium," &c. (Gauss, "Werke," vol. ii. pp. 269—303.)

‡ See Legendre, "Théorie des Nombres" (Paris, 1830), vol. ii. p. 65; Gauss to Encke (Dec. 24, 1849), in Gauss' Collected Works, vol. ii. p. 444. Legendre assigns to a the conjectural value 1·08366. Gauss compares his own formula and that of Legendre with the results of actual enumerations of the prime numbers, and finds that, as far as the end of the third million, the comparison is in favour of the formula of Legendre, but that the error of that formula shows a tendency to increase

consistent with one another, and are perhaps still less approximately true. The memoir of Bernhard Riemann, "Ueber die Anzahl der Primzahlen unter einer gegebenen Grösse," contains (so far as I am aware) the only investigation of the asymptotic frequency of the primes which can be regarded as rigorous.* He shows that, if $F(x)$ be the number of primes inferior to x, there exists an analytical expression for the series

$$F(x)+\tfrac{1}{2}F(x^{\frac{1}{2}})+\tfrac{1}{3}F(x^{\frac{1}{3}})+\ldots\ldots,$$

which consists (1) of a term which does not increase without limit with x; (2) of a non-periodic term $Li(x)$; (3) of an infinite series of periodic terms of the type $Li(x^{\frac{1}{2}-ai})-Li(x^{\frac{1}{2}+ai})$, the constants a being the roots (infinite in number) of a certain transcendental equation. It follows that the non-periodic part of the expression for $F(x)$ is of the type

$$Li(x)-\tfrac{1}{2}Li(x^{\frac{1}{2}})-\tfrac{1}{3}Li(x^{\frac{1}{3}})+\tfrac{1}{6}Li(x^{\frac{1}{6}})-\tfrac{1}{7}Li(x^{\frac{1}{7}})+\ldots\ldots;$$

and thus the equation of Gauss, $F(x)=Li(x)$, can, roughly speaking, be correct only as far as quantities of the order $x^{\frac{1}{2}}$.

No less important than the investigation of Riemann, but approaching the problem of the asymptotic law of the series of primes from a different side, is the celebrated memoir, "Sur les Nombres Premiers," by M. Tchébychef,† in which he has established the existence of limits within which the sum of the logarithms of the primes P inferior to a given number x must be comprised. The limits assigned by M. Tchébychef are not very close, not even close enough to determine the asymptotic value of the quotient $\frac{\Sigma \log P}{x}$, to which the value 1 has been conjecturally assigned. But the theorem of M. Tchébychef may be, perhaps, said to mark the furthest point to which our knowledge of the series of prime numbers has yet been carried; and while it is truly

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more rapidly than the error of his own. He also observes that the mean value of the constant a tends to decrease, and that its true limit may possibly be 1, or may differ from 1 by a quantity of the order $\frac{1}{\log x}$. Encke had communicated to Gauss a formula of his own, $\frac{x}{\log x}\times 10^{\frac{1}{2\log x}}$, which, as Gauss observes, may, for very great values of x, be regarded as coinciding with $\frac{x}{\log x-\frac{1}{2}\log 10}$. (See the letter cited, and also the note of M. Schering, *ibid.* p. 521.)

* See the "Monatsberichte" of the Academy of Berlin for November, 1859; or Riemann's "Mathematische Werke," p. 137. In the "Annali di Matematica," tom. iii. p. 52 (1860), M. Genocchi has given a very interesting account of the method of Riemann, and has arrived at a result differing in one respect (see p. 58) from that of Riemann.

† "Liouville," First Series, vol. xvii. p. 366. The memoir was presented to the Academy of St. Petersburg in 1850.

remarkable that, in a matter of so much difficulty, a process so apparently simple as that which he has employed should be capable of leading to a result of so much interest and importance, it is somewhat disappointing to find that this method, even in the hands of its eminent inventor, should seem incapable of being pursued further, and unlikely to furnish any nearer approximation to the truth. The method of M. Tchébychef, profound and inimitable as it is, is in point of fact of a very elementary character, and in this respect contrasts strongly with that of Riemann, which depends throughout on very abstruse theorems of the Integral Calculus.

I do not know that the great achievements of such men as Tchébychef and Riemann can fairly be cited to encourage less highly gifted investigators; but at least they may serve to show two things—first, that nature has placed no insuperable barrier against the further advance of mathematical science in this direction; and, secondly, that the boundaries of our present knowledge lie so close at hand that the inquirer has no very long journey to take before he finds himself in the unknown land. It is this peculiarity, perhaps, which gives such perpetual freshness to the higher arithmetic. It is one of the oldest branches, perhaps the very oldest branch, of human knowledge; and yet some of its most abstruse secrets lie close to its tritest truths. I do not know that a more striking example of this could be found than that which is furnished by the theorem of M. Tchébychef. To understand his demonstration requires only such algebra and arithmetic as are at the command of many a schoolboy; and the method itself might have been invented by a schoolboy, if there were again a schoolboy with such an early maturity of genius as characterised Pascal, Gauss, or Evariste Galois.*

* In addition to the memoirs to which reference has been made in the text, we may also mention the following:—(1) M. Tchébychef, "Sur la totalité des nombres premiers inférieurs à une limite donnée" ("Liouville," 1st Series, vol. xvii. p. 341). In this paper (which was presented to the Academy of St. Petersburg in 1848) M. Tchebychef proves (among other things) that, if the expression $\frac{x}{F(x)} - \log x$ has a limit at all, the value of that limit must be -1. This result shows that in the approximate formula of Legendre, $F(x) = \frac{x}{\log x - a}$, we ought to take $a = +1$. (2) A paper by the late Judge Hargreave, in the "Philosophical Magazine" for 1849 (vol. xxxv. p. 36), in which it is shown (but not by any very rigorous demonstration) that the average interval between two consecutive primes in the vicinity of any very great number x is $\log x$; a result at which Gauss had arrived while still a boy, as may be inferred from his letter to Encke, quoted above. (3) A paper in the "Mathematische Annalen," vol. ii. p. 636, in which the author, M. Meissel, by the aid of a method suggested by those employed by Legendre ("Théorie des Nombres," vol. ii. p. 86), obtains a formula which greatly facilitates the determination of the number of prime numbers contained between given limits; he has thus found that the number of primes in the first ten millions is 664579; and in a later note ("Mathematische Annalen," vol. iii. p. 523) that the number of primes in the

I pass on to speak of some other branches of analysis which appear to me at the present moment to promise much in the immediate future.

I will first refer to one or two points to which the transition from the arithmetic of whole numbers is easy and natural.

We owe to Jacobi the first suggestion of a method of approximation which forms a natural extension of the theory of continued fractions, but which still remains in an incomplete condition. In the memoir "De functionibus duorum variabilium quadrupliciter periodicis," ("Crelle," Vol. xiii. p. 55,) Jacobi demonstrated the theorem that an uniform function of a single variable can at most be doubly periodic; and that, if it be doubly periodic, the ratio of the two periods is necessarily imaginary. He effected this by proving that, if $aa'a''$, $bb'b''$ are independent irrational quantities, it is always possible to find integral numbers $mm'm''$ such that the value of each of the two expressions

$$ma+m'a'+m''a'',$$
$$mb+m'b'+m''b'',$$

shall be less than any quantity that can be assigned.

This idea of Jacobi was subsequently further developed by M. Hermite, who showed its connexion with the theory of the reduction of quadratic forms (see his letters to Jacobi in "Crelle's Journal," Vol. xl. p. 261 *sqq.*) The same conception lies at the basis of Lejeune Dirichlet's researches on complex units, and led him to his celebrated generalization of the theory of the Pellian Equation.*

Since the death of Jacobi, a memoir of his (apparently left incomplete) has been published in "Borchardt's Journal" (Vol. lxix. p. 29), in which he examines the relations between the successive sets of integral numbers x_0, x_1, x_2, which render an expression such as

$$x_0+x_1\omega_1+x_2\omega_2$$

(where ω_1 and ω_2 are irrational quantities) approximately equal to zero. He applies the theory to the examples $\omega_1 = 2^{\frac{1}{3}}, = 3^{\frac{1}{3}}, = 5^{\frac{1}{3}}$; $\omega_2 = 2^{\frac{2}{3}}, = 3^{\frac{2}{3}}, = 5^{\frac{2}{3}}$, and finds that in each case the development is

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first hundred millions is 5,761,460. (4) A note by Mr. J. W. L. Glaisher in the Report of the British Association for 1872 ("Transactions of the Sections," p. 19), in which the results of some enumerations of the primes are given, and are compared with Mr. Hargreave's theorem as to their average frequency. (5) A paper by M. Mertens ("Borchardt's Journal," vol. lxxviii. p. 46), in which he determines the asymptotic values of the functions $\Sigma\frac{1}{P}$, $\Pi\left(1-\frac{1}{P}\right)$. These functions had been already considered by Legendre ("Théorie des Nombres," vol. ii. p. 67), and by M. Tchébychef in the memoir already cited in this note; but M. Mertens obtains a more precise result by more rigorous reasoning. [(6) A preliminary note on an enumeration of primes, by Mr. Glaisher, in the Proceedings of the Cambridge Philosophical Society, Doc. 4, 1876.]

* See the "Monatsberichte" of the Academy of Berlin for 1842, p. 95, and for 1846, p. 105.

periodic; but he appears not to have obtained any demonstration of the general theorem that the corresponding development in the case of the root of any cubic equation having integral coefficients is always periodic.* These unfinished researches of Jacobi, to which M. Borchardt has called the special attention of mathematicians (in the preface to the 68th volume of his Journal) have been resumed by M. Bachmann,† and still more recently, though from a somewhat different point of view, by M. Fürstenau. The latter of these mathematicians defines a continued fraction of the second order to be a continued fraction in which each element is itself a continued fraction; and, availing himself of this definition, he has succeeded in showing that we can always approximate to the real root of an equation of the order n, having integral coefficients, by means of a periodic continued fraction of the order $n-1$.‡ It is evident that the discovery of such a mode of approximation to the root of an equation may lead to theoretical considerations of great interest, though it is hardly likely that the method itself will be found practically useful.

Closely allied to the investigation of new methods of approximation is the problem of determining the arithmetical or transcendental character of irrational quantities. It was first shown by M. Liouville § that irrational quantities exist which cannot be the roots of any equation whatever having integral coefficients; a proposition which certainly required the proof which it has received from him, although it might easily seem incredible *à priori* that such irrational quantities should not exist. We may, perhaps, be allowed to designate by the terms arithmetical and transcendental the two classes of irrational quantities between which the theorem of M. Liouville has taught us to distinguish; and it becomes a problem of great interest to decide to which of these two classes we are to assign the irrational numbers, such as ϵ and π, which have acquired a fundamental importance in analysis. To Lambert, the eminent Berlin mathematician of the last century, the first great step in this direction is due. He showed that neither π nor π^2 is rational; with regard to ϵ he was even more successful, for he was able to prove that no power of ϵ, of which the exponent is rational, can itself be rational.|| There (with one slight exception) the question remained for more than a century; and it was reserved for M. Hermite,

* This theorem had been already obtained by M. Hermite. See the Letters already cited, "Crelle," Vol. xl. pp. 286—289.

† "Borchardt's Journal," Vol. lxxv. p. 25.

‡ E. Fürstenau, "Ueber Kettenbrücke höherer Ordnung," Wiesbaden. I regret to say that I only know this work from the notice in the "Jahrbuch über die Fortschritte der Mathematik" for 1874.

§ "Comptes Rendus," Vol. xviii. (1844), p. 883, and p. 910; reproduced with additions in "Liouville's Journal" (1st series), Vol. xvi. p. 133.

|| "Mémoire sur quelques propriétés remarquables des quantités transcendantes circulaires et logarithmiques," in the "Mémoires de l'Académie des Sciences de Berlin"

in the year 1873, to complete by a singularly profound and beautiful analysis, the exponential theorem of Lambert, and to prove that the base of the Napierian logarithms is a transcendental irrational.* But, in the letter to M. Borchardt already cited, M. Hermite declines to enter on a similar research with regard to the number π. "Je ne me hasarderai point," he says, "à la recherche d'une démonstration de la transcendance du nombre π. Que d'autres tentent l'entreprise; nul ne sera plus heureux que moi de leur succès; mais croyez m'en, mon cher ami, il ne laissera pas que de leur en coûter quelques efforts." It is a little mortifying to the pride which mathematicians naturally feel in the advance of their science to find that no progress should have been made for one hundred years and more toward answering the last question which still remains to be answered with regard to the quadrature and rectification of the circle. But mathematical discovery is like electricity; it follows the lines of least resistance; and an adherence to the rule which this analogy suggests is certainly conducive to the comfort of the individual mathematician, and is probably also, in the long run, conducive to the progress of mathematics themselves. It has often happened in mathematical history that a difficulty, which had for ages

for 1761, p. 265; the demonstration depends on the continued fraction

$$\frac{\epsilon^x-\epsilon^{-x}}{\epsilon^x+\epsilon^{-x}}=\frac{x}{1+}\,\frac{x^2}{3+}\,\frac{x^2}{5+}\,\frac{x^2}{7+\ldots}.$$

A different method of proving the incommensurability of ϵ (depending on the exponential series) has found its way into many elementary treatises; it would seem that this simple method cannot be applied to prove the more general proposition that $\epsilon^{\frac{m}{n}}$ is incommensurable; but it has been successfully employed by M. Liouville ("Liouville's Journal," 1st series, Vol. v. p. 192) to show that neither ϵ nor ϵ^2 can be the root of a rational quadratic equation. This result forms the only extension which the theorem of Lambert had received up to the date of the memoir of M. Hermite, "Sur la fonction exponentielle," to which we shall presently refer. The only elementary work in which (so far as I know) the incommensurability of $\epsilon^{\frac{m}{n}}$ is demonstrated, is Mr. Todhunter's "Algebra," ed. 5, p. 530. The theorems as to the incommensurability of π and π^2 are excluded from English text-books; the only exception that occurs to me being Sir David Brewster's English edition (Edinburgh, 1824) of the Geometry of Legendre, where Legendre's reproduction of the demonstration of Lambert is given in Note iv. p. 239. The exclusion of these theorems is a matter of regret; for they constitute the only "short method with the circle-squarers"; and perhaps the extraordinary prevalence within the United Kingdom of the form of delusion known as circle-squaring may partly arise from the appearance of an "ipsi dixerunt" on the part of the mathematicians, which is certainly suggested by the omission in elementary works of any rigorous demonstration of the irrationality of π. M. Hermite has given a demonstration of the irrationality of π and π^2, which is very beautiful and entirely different from that of Lambert (Letter to M. Borchardt in "Borchardt's Journal," Vol. lxxvi. p. 342); with this single exception, the theory of the quadrature of the circle rests to-day where Lambert left it in the year 1761.

* See the Memoir "Sur la fonction exponentielle," already cited in the preceding note, "Comptes Rendus," Vol. lxxvii., pp. 18 etc.; and also published separately by Gauthier-Villars, 1874.

resisted all direct attempts to overcome it, has yielded at last to the gradual advance of science; just as in the operations of strategy a strong position, which cannot be carried by a front attack, may nevertheless be turned and taken in the rear by an enemy who has possessed himself of the country round it.*

I may, perhaps, mention yet one more class of questions lying on the border land of arithmetic and algebraic analysis; I mean the questions which relate to the transcendental or algebraic character of developments in the form of infinite series, infinite products, or infinite continued fractions. The theorems of Eisenstein and M. Heine, of which a simple and beautiful demonstration has lately been laid before us by our colleague M. Hermite, are amply sufficient to awaken the expectation of great future discoveries in this almost unexplored field of enquiry.†

I have detained you so long over arithmetical and quasi-arithmetical subjects that I can only venture to glance hastily at some topics on which I could have wished to have dwelt much longer. I am afraid that I have only given you an additional instance of that one-sidedness against which, as I have reminded you, we were cautioned by our first President. In the hope of convincing you that I have not wholly forgotten the claims of other parts of our science, I will now hazard the assertion, that (after all) the advancement of the Integral Calculus is at once the most arduous and the most important task to which a mathematician can address himself. In the applications of mathematics to physics the integral calculus is confessedly of ever increasing importance; and it is especially interesting to observe that some of the most recent developments which it has received have had their origin in considerations of pure analysis, and yet have come just in time to furnish us with the most appropriate instruments for dealing with the problems which at the present moment are the most prominent in physical enquiries. But I must not dwell on the prospects of great future extension which are thus opened up for the various branches of mathematical physics; I can only advert (and that very hastily) to some of those parts of the integral calculus which, even from the point of view of the pure mathematician, seem to promise an abundant and immediate harvest.

* I find that I am here closely following (haud passibus æquis) some observations of the late Dr. Hermann Hankel in his inaugural address, "Die Entwickelung der Mathematik in der letzten Jahrhunderten," Tübingen, 1869. This discourse, by one who was at once a learned scholar and an original investigator, contains much which deserves the attention of all who are interested in the progress of mathematical science, or who wish to see a higher spirit infused into the mathematical teaching given in the schools and universities of this country.

† Eisenstein, "Monatsberichte" of the Berlin Academy for 1852, p. 441; M. Heine in "Crelle's Journal," Vol. xlv. p. 285, and Vol. xlviii. p. 267. The note of Eisenstein is reproduced in the first of these memoirs.

Let me first mention the theory of ordinary differential equations. This is a subject which ought to have a special interest for ourselves, as one of the latest advances that have been made in it—the introduction of symbolical methods—is due in great measure to English mathematicians, and above all others to George Boole. Nor have Englishmen been behindhand in the cultivation of another branch of the subject (intimately connected with the use of symbolical methods) —the representation of the solutions of differential equations by means of definite integrals. But, simultaneously with these investigations, a line of research has been pursued on the Continent to which we in England have not paid equal attention. I refer to the endeavours which have been made to determine the nature of the function defined by a differential equation, from the differential equation itself, and not from any analytical expression of the function, obtained by first "solving" the differential equation. The generality and importance of such an inquiry (whatever be its difficulty) cannot be overrated; for, just as we long since learned to regard integration in a finite form (or, more properly, integration by means of algebraic or exponential and logarithmic functions) as only a very small part of the problem which the Integral Calculus has to solve with regard to differential expressions containing a single variable, so also, when we come to differential equations, we are forced to remember that the variety and complexity of the functional relations expressed by them may altogether transcend any other means of expression at our disposal. Perhaps we may regard as the fundamental theorem in the whole subject the proposition of Cauchy, that every differential equation admits (in the vicinity of any non-singular point) of an integral which is synectic within a certain circle of convergence, and which is consequently (within that circle) developable by the series of Taylor. Various applications of this theorem (together with a demonstration somewhat simpler than that given by Cauchy) will be found in the classical treatise of MM. Briot and Bouquet.* Closely allied to the point of view indicated by the theorem of Cauchy is that adopted by Riemann, who regards a function of a single variable as defined by the position and nature of its singularities, and who has applied this conception to the linear differential equation of the second order which is satisfied by the hypergeometrical series. In the memoir, "Beiträge zur Theorie der durch die Gauss'sche Reihe $F(\alpha, \beta, \gamma, x)$ darstellbaren Functionen," † Rie-

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* "Théorie des Fonctions Elliptiques," ed. 2, Paris, 1875, p. 325. See also a memoir by the same authors in the "Journal de l'Ecole Polytechnique," cahier 36, p. 137. For an enumeration of Cauchy's Memoirs on Differential Equations, see his Life by M. Valson, Paris, 1868, vol. ii., cap. 9, pp. 104—117.

† "Transactions of the Academy of Göttingen" for 1875, vol. vii., or in Riemann's Collected Works (Leipzig, 1876).

mann sets out with the conception of a function which possesses three discriminantal points (I venture to propose this word as the most natural English rendering of "Verzweigungs-punkte"), and which is further characterised by the property that any three of the values which it admits at any point are connected by a linear and homogeneous equation with constant coefficients. Such a function Riemann shows, by reasoning of great beauty and originality, necessarily satisfies the linear differential equation of the hypergeometric series; and thus the nature and mode of existence of the functions defined by that equation are put before us with a precision and clearness which could not, perhaps, have been attained by any application of the ordinary methods of analysis to the discussion or integration of the equation.

The collected works of Riemann include another, but unfortunately unfinished memoir ["Zwei allgemeine Lehrsätze ueber lineäre Differential-gleichungen mit algebraischen Coefficienten"], relating to the case in which the number of independent functional values is any whatever instead of only three. And the fertility of the conceptions of Cauchy and of Riemann is further attested by the researches to which they have given rise, and are still giving rise, in Germany—researches among which I must especially mention those of L. Fuchs, whose papers on linear differential equations, in the 66th and subsequent volumes of "Borchardt's Journal," must form, it seems to me, the basis of all future inquiries on this part of the subject.

There is one celebrated problem connected with differential equations which, after all that has been written and said about it, remains a problem still; I mean the problem of Singular Solutions. If it were not for the papers of M. Darboux ("Bulletin des Sciences Mathématiques et Astronomiques," Tom. iv., p. 158), and of Professor Cayley ("Messenger of Mathematics," Vol. ii. p. 6., and Vol. vi. p. 23), I do not know where I should advise a student to turn to acquire any distinctness of insight into this important question. These papers have at any rate rendered one great service; they clearly show that there is a difficulty, and a difficulty not yet surmounted. The point of the difficulty I presume to be, that whereas a singular solution, from the point of view of the integrated equation, ought to be a phenomenon of universal, or at least of general occurrence, it is, on the other hand, a very special and exceptional phenomenon from the point of view of the differential equation. The explanation suggested by M. Darboux is (to say the least) deserving of very careful consideration. He says, at p. 167 of the memoir just cited, "Since differential equations are formed by the elimination of constants from an equation in finite terms and its derived equations, writers have supposed (and it would seem erroneously) that, when we are given a differential equation of the first order (for example), it always possesses an integral of the first order expressible in the form $f(xyc) = 0$, where f is a function having in the whole extent of the plane the properties generally recognised in analytical functions. This function f might be more or less difficult to find, but it was conceived of in every case as existing. Now this is just the disputable point, and we think that recent researches on the theory of functions ought to lead us to adopt a different view." It is evident that, if the observations of M. Darboux are well founded, an important series of questions arises as to the nature of the integral equation answering to a given differential equation; and, further, that some of the elementary considerations with which it is usual to introduce the subject of differential equations must be abandoned as untenable. The rules that can be given in aid of mathematical discovery are, I suppose, very few, and I have already ventured to call your attention to one of them—the rule that bids us follow up any opening that may present itself, rather than try to force a way against obstacles which may prove insurmountable. In the case before us, I think we come upon an illustration of another rule, which is of less general application, but nevertheless often useful. The rule is, that an apparent contradiction (as distinct from a mere misunderstanding) is always to be regarded as an indication of some undiscovered truth. Yet it is remarkable what a tendency there is in the minds of men to ignore or soften down such apparent contradictions, instead of looking for the reality which lies at the bottom of them. The equation $\int_1^x \frac{dx}{x} = \log x$ must have been familiar to mathematicians for a century at least before they set themselves seriously to examine the apparent contradiction presented by the equation of a single-valued to a multiple-valued expression. And yet what a flood of light was thrown on the whole theory of functions by the researches to which Cauchy and others were led when the endeavour was at last made to account for this and similar apparently inexplicable phenomena. If the clue offered by such familiar instances as the equations

$$\int_1^x \frac{dx}{x} = \log x, \quad \int_0^x \frac{dx}{\sqrt{1-x^2}} = \sin^{-1} x,$$

had been seized and followed up, it is difficult to believe that the main outlines of the theory of elliptic functions would not have been discovered much sooner than they actually were. If we look back on the history of the past, the discovery of the "principle of double periodicity," and, with it, of the essential characteristic of an elliptic function, cannot but appear to us as one of the most extraordinary efforts of mathematical genius. The integration of the differential equation of elliptic integrals by Euler—an integration obtained by a sort of divination, which has deservedly remained celebrated in the history of science; the systematization of the calculus of elliptic integrals by Legendre; the simultaneous discovery by Jacobi and Abel of the double periodicity latent in the equation of Euler,—these were the successive steps—and each one a gigantic step—by which those great mathematicians arrived at the theory of elliptic functions in the form in which we now possess it. But if we compare the actual history of the discovery with the outlines of the theory, as we find them, for example, in the work of MM. Briot and Bouquet, it is impossible not to be struck with the contrast. Each step in the theory, as exhibited in that work, appears to follow from those that precede it in such a natural and necessary order that we are inclined to wonder why those who discovered the great results themselves should have failed to find the easiest path of access to them.

If I had had the honour of addressing the Mathematical Society ten years ago, I think I should have had to complain of the neglect in England of the study of elliptic functions. But I cannot do so now. The University of Cambridge has given this subject a place in its Mathematical Tripos; the University of London in its examination for the Doctorate of Science. The British Association has supplied the funds requisite to defray the cost of printing Tables of the Theta function—Tables of which the mathematicians of this country may justly be proud, and which will form an enduring memorial of the great ability and indefatigable industry of our colleague, Mr. Glaisher. We further owe to Professor Cayley an introductory treatise on elliptic functions, the first which has appeared in our language. I consider that the service which he has thus rendered to students is an important one, and one for which we ought to be very grateful. I am convinced that nothing so hinders the progress of mathematical science in England as the want of advanced treatises on mathematical subjects. We yield the palm to no European nation for the number and excellence of our text-books of the second grade—I mean, such text-books as are intended to guide the studies of the undergraduate within the courses prescribed by our University examinations in honours. But we want works adapted to the requirements of the student when his examinations are over—works which will carry him to the frontiers of knowledge in various directions, which will direct him to the problems which he ought to select as the objects of his own researches, and which will free his mind from the narrow views he is too apt to contract while "getting up" subjects with a view to passing an examination, or, a little later in his life, while preparing others for examination. Can we doubt that much of the preference for geometrical and algebraical speculation which we notice among our younger mathematicians is due to the admirable works of Dr. Salmon; and can we also doubt that, if other parts of mathematical science had been equally fortunate in finding an expositor, we should observe a wider interest in, and a juster appreciation of, the progress which has been achieved? There are, of course, other treatises besides those of Professor Cayley and Dr. Salmon to which I might refer; there is, for example, the work of Boole on Differential Equations; and there are the great historical treatises of Mr. Todhunter, so suggestive of research and so full of its spirit; we have also a recent work by the same author on the functions of Laplace, Lamé, and Bessel. But the field is not nearly covered, though, indeed, my enumeration is not complete; and, even without leaving the domain of the Integral Calculus, I might point out that there are at least three treatises which we greatly need—one on Definite Integrals, one on the Theory of Functions in the sense in which that phrase is understood by the school of Cauchy and of Riemann, and one (though he should be a bold man who would undertake the task) on the Hyperelliptic and Abelian Integrals. I fear that our colleague, Professor Clifford, would hardly listen to us if we were to appeal to him to undertake this task; but at least we may express the hope that he may be able to continue the profound researches which he has commenced on this great branch of analysis.

I feel I must now bring these somewhat desultory remarks to a conclusion; though, if your time and patience were unlimited, there are many things I could wish to say. Among other matters I should have adverted to the great efforts which have been made in very recent times, in Germany, in Russia, and in Norway, to advance the theory of Partial Differential equations; and I should have noted with pleasure that our own Society has received important communications on this subject from Professor Tanner. And again, leaving the field of the Integral Calculus, I think I should have hazarded some references to the Theory of Substitutions, and its applications to the Theory of Equations; and though I should have been relapsing into a region dangerously near to the Theory of Numbers, I should have exhorted the younger mathematicians of our time not to turn away from a subject which, if forbidding at the first aspect, contains so much promise of future development, and lies so near the very centre and fountain of much that is important in Algebra. Lastly, I should have endeavoured to make my peace with Geometry, which all this time I have been treating with such marked neglect, and would have invited your attention, though it were but for a few moments, to some of those questions of geometry which the natural advance of science has brought to the forefront at the present time. Even here, I might have found one example more of a study which we in England too much neglect; and I might perhaps have reminded you of the great hopes which Gauss entertained of the Geometry of Situation, containing, according to him, vast and as yet quite uncultivated regions over which our present analytical methods can pretend to no dominion. I certainly should not have forgotten to congratulate the Society on the part which its members, under the guidance and inspiration of Professor Sylvester, have taken in the development of the great geometrical theory of link-work movements.

> "Verum hæc ipse equidem spatiis exclusus iniquis
> Prætereo, atque aliis post commemoranda relinquo."

I will not sit down without again offering my excuses for the fragmentary and disconnected nature of the reflexions I have laid before you this evening. But, indeed, over so wide a field I could only take a wandering course. My object has been to impress upon those who have been indulgent enough to listen to me that the vast increase which this century has witnessed in the extent of the ground already covered by mathematical science has been accompanied by a proportionate increase in the number and variety of the objects of interest to which the mathematician may turn his attention, and by an even more than proportionate increase in the opportunities of discovering new truths which have been brought within his reach. Our border on every side is the unknown; and the further our boundary line is extended, the more multitudinous become the points at which we may hope to penetrate beyond it. In these days when so much is said of original research and of the advancement of scientific knowledge, I feel that it is the business of our Society to see that, so far as our own country is concerned, mathematical science should still be in the vanguard of progress. I should not wish to use words which may seem to reach too far, but I often find the conviction forced upon me that the increase of mathematical knowledge is a necessary condition for the advancement of science, and, if so, a no less necessary condition for the improvement of mankind. I could not augur well for the enduring intellectual strength of any nation of men, whose education was not based on a solid foundation of mathematical learning, and whose scientific conceptions, or in other words whose notions of the world and of the things in it, were not braced and girt together with a strong framework of mathematical reasoning. It is something for men to learn what proof is, and what it is not; and I do not know where this lesson can be better learned than in the schools of a science which has never had to take one footstep backward, which has never asserted without proof, or retracted a proved assertion; a science which, while ever advancing with human civilization, is as unchangeable in its principles as human reason; the same at all times and in all places; so that the work done at Alexandria or Syracuse two thousand years ago (whatever may have been added to it since) is as perfect in its kind, and as direct and unerring in its appeal to our intelligence, as if it had been done yesterday at Berlin or Göttingen by one of our own contemporaries. Perhaps also it might not be impossible to show, and even from instances within our own time, that a decline in the mathematical productiveness of a people implies a decline in intellectual force along the whole line; and it might not be absurd to contend that on this ground the maintenance of a high standard of mathematical attainment among the scientific men of a country is an object of almost national concern. But I need not ask your assent to such wide assertions; I shall be more than satisfied if anything that may have fallen from me may induce any one of us to think more highly than he has hitherto done of the first and greatest of the sciences, and more hopefully of the part which he himself may bear in its advancement.

On Curves having Four-point Contact with a Triply-infinite Pencil of Curves. *By* W. SPOTTISWOODE, Esq., F.R.S.

[*Read November 9th*, 1876.]

IN a paper published in the "Mathematische Annalen" (Vol. iii., p. 459) Brill has investigated the case of curves having three-point contact with a doubly-infinite pencil of curves; and in the same journal (Vol. x., p. 221) H. Krey of Kiel has applied a method, similar to that of Brill, to the next step, viz., the problem now proposed. He does not, however, appear to have succeeded in completely eliminating the differentials which occur in the process; and in that respect his solution is incomplete. Some formulæ, however, used in my papers on the contact of curves and of surfaces, and in particular in that "On the Sextactic Points of a Plane Curve" (Phil. Trans., 1865, p. 657), prove to be directly applicable to the question. An application of them to Brill's problem will be found in a paper in the "Comptes Rendus," 1876 (2[e] sémestre, p. 627).

In fact, if we put H for the Hessian of U; and if

$$\Delta = \begin{vmatrix} u_1, & w', & v', & d_x \\ w', & v_1, & u', & d_y \\ v', & u', & w_1, & d_z \\ d_x, & d_y, & d_z, & . \end{vmatrix} = (\mathfrak{A}, \mathfrak{B}, \mathfrak{C}, \mathfrak{F}, \mathfrak{G}, \mathfrak{H})(d_x, d_y, d_z)^2,$$

where u, v, w are the first, and $u_1, v_1, w_1, u', v', w'$ the second differential coefficients of U; or writing, as is sometimes convenient,

$$\begin{aligned} A &= \mathfrak{A}d_x + \mathfrak{H}d_y + \mathfrak{G}d_z \qquad \Delta = Ad_x + Bd_y + Cd_z \\ B &= \mathfrak{H}d_x + \mathfrak{B}d_y + \mathfrak{F}d_z \\ C &= \mathfrak{G}d_x + \mathfrak{F}d_y + \mathfrak{C}d_z; \end{aligned}$$