XV. Thoughts on the demonstration of certain formulæ

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under such a variety of circumstances, as to be enabled to deduce from experiment alone the due corrections for temperature, pressure, moisture, and the direction and velocity of the wind, independent of all theory.

XV. *Thoughts on the Demonstration of certain Formulæ.* By
T. S. DAVIES, Esq.

AS upon the truth of the binomial theorem has been supposed to rest the truth of almost every thing we know in the higher branches of analysis, it was very natural for the most profound powers of investigation to be employed in effecting a satisfactory demonstration of it. The theorem, however, in its simplest case was discovered by *induction*; its extension was also by *induction*; and the clearest, indeed the only proof we even now possess of its truth is altogether *inductive*. On this latter topic I am aware that great names are against me: but are they not also opposed to each other? Many have attempted to perform this task, but each has easily discovered some fallacy in the other's proofs: and, indeed, I doubt whether there be any writer who has not felt a latent suspicion of the logical accuracy even of that perfect one of all—*his own*. It becomes, then, an important course of inquiry which investigates the causes of these failures; and perhaps a few very simple considerations may lead us to believe that the nature of the evidence upon which this and other theorems rest, is of a character essentially different from that which is commonly supposed to appertain to mathematical truth. It will thence appear probable that the failures have not arisen from want of skill, but from aiming at a species of proof of which the subject does not admit. Nor can the perspicuity of our processes and the facility of acquisition fail to be materially improved by a reversal of our common mode of proceeding, should that procedure be founded upon an erroneous estimate of the objects to be attained and the proper steps of attainment.

The first great principle we should ever bear in mind is, that all the “laws of number” are mere deductions from a comparison instituted between a great variety of separate cases, or else deductions from the results thus obtained. Such, indeed, is the origin of our belief of the numerical fact, that m times n is equal to n times m ;

$$\text{that } (x \pm y)^m = x^m \pm m x^{m-1} y + \dots;$$

$$\text{that } \Delta^n \phi x = \phi(x + nh) - n \cdot \phi\{x + \overline{n-1}.h\} + \dots;$$

that $\phi(x + nh) = \phi x + n \cdot \Delta \phi x + \frac{n \cdot n-1}{1 \cdot 2} \cdot \Delta^2 \phi x + \dots;$

and that $\frac{d^r x^n}{dx^r} = \frac{\Delta^n 0^n}{\Delta^{n-r} 0^{n-r}} x^{n-r};^*$ and of a thousand other

formulae: indeed, of every possible formula that is true.

I do by no means intend to assert that all these propositions are equally clear and obvious to the untutored mind; but merely that they are all equally the result of a generalization of facts, which had been previously observed to be true in every one of the numerous instances which had been examined. The object of the generalization was to discover an empirical expression which should, *mutatis mutandis*, comprehend all the possible separate cases; and we felt justified in the use of the rule thus formed, so long as no exception to its generality was discovered, whilst from every instance of its accordance with separate cases that came under our notice we felt it invested with an additional degree of certainty. This was asserted in the binomial theorem; and by the analogy in integer numbers between that and some of the differential theorems, the same generalization was suggested in this class of theorems which had been successfully tried in the binomial. Such an extension was very obvious; but it required us to generalize (if, indeed, that could be done) the signification of the index of the characteristic, so as to include fractions, irrationals, and imaginaries, in order to render the resulting expressions *intelligible*. This being done, it became easy to verify the theorem by application to particular cases, and thus to place it amongst the admitted principles of the science.

My intention in the present paper, however, was principally to notice Mr. Herapath's Demonstration of the Binomial Theorem (Phil. Mag. May 1825), and his extension of it to the development of any function in terms of the finite differences of that quantity; and to point out where that gentleman has deceived himself by an appearance of greater rigour than he had actually obtained in his demonstration. I shall also notice, *en passant*, that given by the Rev. Arthur Brown in his "View of the First Principles of the Differential Calculus," p. 59, Cambr. 1824.

In the first place, upon what authority but that of induction does Mr. Herapath assume the n th line in his class of equations (B), p. 323? The induction, I grant, is easy, and the law tolerably obvious: but it is induction still, and the principle of generalization is essentially involved. The formula has been found to hold true in all the integer numbers that *have*

* Mr. Herapath, Phil. Mag. May 1825, vol. lxxv. p. 327.

been tried, and we thence infer that it will hold for all that *can* be tried.

Again, Mr. Herapath finds that with a little contrivance, without the admission of principles essentially new, he can show its application to $(x + y)^{r+v}$ when $r + v = n$, and r, v and n are whole numbers. So far, viewed as a series of simple inductions, and resting on the evidence of numerous trials, the results are satisfactory.

Here, however, Mr. Herapath is obliged to pause, and to call in the aid of a new principle on which to found his inferences; viz.—*that because it was true when r and v were integers, it must be true when they are fractions, and even when they are irrationals and imaginaries too!*—the only condition to which these fractions, irrationals, and imaginaries are subjected being, that their aggregate shall be an integer. From this again, he discovers that because it is true of *two* such quantities, it must also be true of *one*; and hence that it is true universally.

Had the former steps been demonstrated in the usually strict meaning of the term, still it must be admitted that this latter step is altogether gratuitous and unwarranted; perfectly *outré* to the spirit of that pure logic of which the mathematical sciences are esteemed the very essence. The investigation lays it down as an essential datum that r and v shall be *integers*; and immediately proceeds upon the supposition of their being *fractions, irrationals, or imaginaries*, taken at pleasure! I admire the mind that can readily generalize particular results; it is always a mind of great capability: but it is asking from us too much to require our assent to a proposition which rests upon the authority of a demonstration like this. Why not at once assume n as the representative of any number whatever, even an imaginary one (if that does not involve some absurdity), rather than adopt the tedious process of considering r and v as both fractions? It was thus that Newton proceeded, and he verified the formula by numerous trials. Others since his time have involved the theorem in a maze of symbols, from which they have dexterously evolved a medley of conclusions, which they call demonstrations: but, so far as the proof of the theorem is concerned, the matter rests just where he left it, save that it has received numberless numerical verifications in its passage through the hands of successive computists.

The “*demonstration*” given by Mr. Herapath, of

$$\Delta^n x^v = (x + n)^v - n(x + n1)^v + \dots, \text{ p. 326,}$$

also fails, in consequence of his failure in the demonstration of the binomial theorem: and against that gentleman’s reasoning

ing upon periodical functions (Phil. Mag. November 1824, p. 333) where the indices are fractional, we may urge the same objections as we have done against his reasoning upon the binomial. Those assertions *may* be true—and they probably *are* true: but no proof of its truth has yet been given; and I much mistake the nature of demonstration, if any proof but that derived from successive inferences can ever be established.

The demonstration given by the Rev. Arthur Brown, to which I before referred, proceeds in the usual manner when n is a positive integer. This integer he next supposes converted into an *equivalent vulgar fraction* $\left(\frac{m}{n}\right)$, and argues that it still is true.—Granted. But he proceeds, “*Since this is true when $\left(\frac{m}{n}\right)$ is a WHOLE NUMBER, it will ALSO be true when $\left(\frac{m}{n}\right)$ is a FRACTION!* (View, p. 60.) This is the work in which, after an unsparing review of the state of academic mathematics, the author has “endeavoured to give a *rigid* demonstration of every proposition!” Pref. xxi.

Some years ago the views entertained by Mr. Herapath occurred also to myself: though of the “more general calculus” to which he refers (but of which he so carefully conceals all traces) I am unable to form an opinion. My investigations were conducted on the assumed truth of the differential formulæ when extended to any form whatever of n : and I had examined most of the functions noticed by Mr. H., together with many important ones to which he has not made any reference, though doubtless they have more or less attracted his attention. A concurrence of circumstances—amongst which the annunciation of Mr. Herapath’s discoveries more than two years ago in the Phil. Mag.; the difficulty of satisfying myself at that time concerning the fundamental principles of analytical certainty; and the force of those claims upon my attention, which I considered paramount even to the claims of science;—these prevented me from pursuing the inquiry so far as under more auspicious circumstances I probably should have done: and from knowing that the question was likely to be fully investigated by a mind of the vigorous constitution of Mr. Herapath’s, and with the aid of a new and more extensive calculus, I felt altogether unanxious concerning the state of my own investigations. It will be scarcely necessary, then, to disclaim all hostile feelings in the remarks which I have made; and to assure that gentleman, that, but for the error into which I conceive he has fallen, and the consequent loss of much valuable time in vainly attempting impossibilities,

ties, I should not have appeared before the public on this occasion.

Mr. Herschel was aware that to deduce the differential formulæ in the usual way had very much the appearance of "an inductive process:" and to evade the difficulty he resorts to a method equally inductive—"the calculus of generating functions *." In the proof by this method, were no other inductive principle involved, there is yet the binomial theorem. But there are other inductions mingled up in the investigation: for instance, the assumption of a *series* to represent the development of ϕt ; which series, especially as following a regular and successively dependent law, we do not know to be always possible. It is true Mr. Herschel attempts to parry this objection, by assuming the series as the "*definition*" of the function ϕt : but even in this view, except considered as induction, it is inadmissible; for how can we define any function, except by stating some constantly observed quality or circumstance belonging to it, and which may serve as a distinguishing character of that function? In any other case than this it is not a definition, but an assumed property; and our assumption is therefore merely a fiction, to be subjected to investigation and analysis,—not less so than any theorem in the ancient geometry; and by comparing its results with previously established principles, we learn its consistency with those principles. If correspondent, we admit the fiction; if not, we reject it. Here, however, since this series is considered a general representation of every numerical case of every possible series, we can only compare it with each separately, and admit its *partial* truth by the successive probabilities which arise out of those comparisons. Upon what then, but induction, does the principle of investigation itself depend? We have found *in all the cases we have yet tried*, that ϕt may be developed in the form of the fundamental equation of the calculus of generating functions: and we cannot question the *probability*, though we must ever make a reserve of the term "*absolute certainty*," that every possible form of ϕ is such as would admit of such an evolution. Indeed, Mr. Herschel's view of $\phi t = \dots$ series $\dots$ being a definition, is at once resolvable into the principle which La Grange calls the "*theory of series*:" and this principle is merely an observation that certain properties belonged to every separate function which had been examined, and an induction of the universality of a law, in consequence of no deviation from it having yet been noticed.

Taking this view of the subject, there appears no reason for Mr. Herapath's considering the expression for the develop-

* *App. Transl. Lacroix Diff. et Int. Calc.* p. 472.

ment of $\Delta^n \sqrt{-1}$ as *demonstrated*; nor, on the other hand, for Mr. Herschel's denominating the asserted equality of the function and its development "*a definition*.*" That the theorem is true, provided the expression can be considered *intelligible*, there is a high degree of probability; but that probability can only arise from the identity of its general form of expansion with the general form of expansion of the binomial theorem. Indeed, I cannot doubt, on the faith of the general principle, the truth of

$$\left\{ \epsilon^{\frac{d}{dx}} - 1 \right\}^n = \Delta^n,$$

in their developments, whatever be the form assigned to n : against the affected rigour and consequent inconclusiveness of its demonstrations rest the only objections I have to make.

To conclude, and in reference to notation, I shall just remark that Mr. Babbage's views† on this subject do not appear to be quite correct, though the phraseology which he generally uses indicates a near approximation to such correctness; perhaps a nearer approximation than can be found in the writings of any other mathematician whatever. However, concerning new notations, "not to agree with, but to *include* the former," we ought to remark that this including notation is merely a statement of facts (or in truth, a more general theorem), so contrived as to embrace in one expression the facts of two or more of the old notations; just in the same manner that n is used to include 2, 3, 4, &c. as indices in any power of ϕx . It is true, that from this new statement, or notation, new relations might be observed, or new theorems suggested, or the relations so expressed might excite some collateral inquiries arising out of mere analogy, as in the differential theorems to which I have so often referred, and which were suggested by the binomial. The formulæ were not intended originally to "*include*" negative, fractional, irrational, or imaginary values of n ; the signification of the formulæ was so interpreted *after* experiments had shown that the formulæ held good in every tried case of all those classes of values. Improvements in notation are in truth identical with the discovery of new theorems, or with a more extended application of the old.—But I have far exceeded the limits which I had prescribed to myself (notwithstanding several important considerations on these subjects remain untouched, and not even hinted at): I shall, therefore, defer the further inquiries to which the considerations already stated so obviously lead, till a future and more convenient period.

Bristol, July 19, 1825.

T. S. D.

* I infer that it is to Mr. Herschel that Mr. Herapath alludes, Phil. Mag. May 1825, p. 326.

† Cambridge Phil. Trans. vol. i. pt. i.; and Edinb. Encycl. art. "Notation."
XVI. *Che-*