Taylor's Theorem and Bernoulli's Theorem: A Historical Note.

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Brill and Noether in their Report, *Die Entwicklung der Theorie der Algebraischen Functionen in äerer und nlteuerer Zeit, Abschnitt* I., § 16, state in their remarks on Taylor's Theorem that "in its modern form the Theorem appears only as a Corollary (Prop. VII., Cor. II.) and is left without any application."

Again, Mr Cantor in his History (Vol. III., p. 368, 1st Edition) remarks, "to what extent Taylor in composing his *Methodus Incrementorum* may have been clear as to the possibility of the application of his Theorem to the development in a series of a function of a binomial [*i e.*, a function of $z+v$ in Taylor's notation] it is hard to say. In any case actual developments of the kind in question are not to be found there [*i.e.*, in the *Methodus*]."

A different point is raised by Voss, who seems to challenge Taylor's priority. At any rate in the *Encyklopädie der Mathematischen Wissenschaften*, Band 2, Teil 1, p. 74, note (78), Voss refers to "John Bernoulli's justifiable priority of 1694." On the other hand, Bernoulli's claim is very emphatically rejected by B. Williamson (note on page 70 of his *Differential Calculus*, 4th Edition.)

I quote these passages from recent writers to indicate the conflicting views that are held in regard to Taylor's own use of his Theorem and in respect of his claim to priority. It is my object in this paper to test these assertions by an examination of the publications of Taylor and Bernoulli so far as they bear on the matters in question. A valuable article by Pringsheim on the history of Taylor's Theorem appears in the *Bibliotheca Mathematica*, Band I. (1900), p. 433. Pringsheim's discussion of the early history is very impartial, and his main conclusion is in agreement with mine; there is, however, I think, a sufficient amount of new matter in the paper to justify its presentation to the Society. Taylor's own account of his book is too important to be overlooked, and the very early use of the Theorem by Stirling deserves greater emphasis than it has hitherto received.

Taylor's *Methodus Incrementorum Directa et Inversa* was published in 1715, and the Theorem which now bears his name is the second Corollary to Proposition VII., p. 23; the Theorem, however, had been communicated to Machin in a letter of date 26th July 1712, but without proof (*Bibliotheca Mathematica*, Band VII. (1906–7), p 367). The notation used by Taylor would make too heavy demands on the printer to justify me in reproducing it here, and I must therefore employ more modern symbols. Proposition VII. may be translated as follows:—

Let z and x be two variable quantities of which z increases uniformly by the given increments h; let

$$nh = v\,,\ v - h = v',\ v' - h = v'',\ \ldots\ldots$$

Then I say that while z increases to $z + v$ the variable x will increase to

$$x + \Delta x\,\frac{v}{1\,.\,h} + \Delta^2 x\,\frac{vv'}{1\,.\,2\,.\,h^2} + \Delta^3 x\,\frac{v\,v'v''}{1\,.\,2\,.\,3\,.\,h^3} + \text{etc.}$$

This proposition is of course a simple re-statement of Newton's Formula, Case 1 of the well-known Lemma in the Third Book of the *Principia*; Taylor's proof is on the lines now generally followed in works on Finite Differences, though the induction is rather an analogy than a strict inductive demonstration.

The Second Corollary to the Proposition is:—

If for the evanescent increments the fluxions that are proportional to them are written, the quantities $v, v', v'' \ldots$ being now made all equal to v, then while z, uniformly flowing, becomes $z + v$ the variable x will become

$$x + \dot{x}\frac{v}{1\,.\,\dot{z}} + \ddot{x}\frac{v^2}{1\,.\,2\,.\,\dot{z}^2} + \dddot{x}\frac{v^3}{1\,.\,2\,.\,3\,.\,\dot{z}^3} + \text{etc.}\,;$$

or, the sign of v being changed, while z decreases to $z - v$ the variable x will decrease to

$$x - \dot{x}\frac{v}{1\,.\,\dot{z}} + \ddot{x}\frac{v^2}{1\,.\,2\,.\,\dot{z}^2} - \dddot{x}\frac{v^3}{1\,.\,2\,.\,3\,.\,\dot{z}^3} + \text{etc.}$$

The notation of the Corollary is that of Taylor (except that the points on the third fluxion are arranged by Taylor in the form of a triangle); the Corollary gives Taylor's own presentation of the Theorem now known by his name. It may perhaps be noted that the method of obtaining it, by a passage from a formula in Finite Differences, is that adopted by Euler in his *Calculus Differentialis.* We need not worry about the validity of the proof; "existence theorems," fortunately for the progress of mathematics, did not greatly worry mathematicians of Taylor's day.

The question must now be considered whether Taylor has shown that he had any real conception of the value of his Theorem, and I think some information may he obtained from the account he gave of his book in the *Philosophical Transactions*, Vol. XXIX, pp. 339–350. Expanding a passage in the Preface to the *Methodus*, he says that the principles of the method of fluxions "may all be drawn directly as a corollary from the principles of the method of increments. ... If in any proposition relating to increments you make the increments to vanish and to become equal to nothing, and for their proportion put the fluxions you will have a proposition that will be true in the method of fluxions. This is but a corollary to Sir Isaac Newton's demonstration of the fluxions being proportional to the nascent increments. For this reason, to make the method of fluxions to be understood more thoroughly, I thought it proper to treat of these two methods together, and I have handled them promiscuously as if they were but one method." He describes the 4th and 5th Propositions as designed to explain "the method of judging of the nature and number of the conditions that may accompany an incremental or fluxional equation. This is a circumstance that I don't find to have been explained by any one before and the propositions are somewhat intricate; wherefore it will not be improper to explain this matter a little more at large." He then points out the special value of the 7th Proposition, namely, that in the solution furnished by it "you always have those indetermined coefficients which are necessary to adapt the equation that is found to the conditions of the problem proposed," and he states "this I take to be the only genuine and general solution of the inverse methods."

This last statement shows that Taylor held the 7th Proposition and its corollary to be of vital moment for the complete solution of a differential equation, and in the Scholium to Prop. VIII. of the *Methodus*, the second corollary to Prop. VII. (*i.e.* Taylor's

Theorem) is used to find the general solution of the fluxional equation

$$(z+nx)\ddot{x} = \dot{x} + \dot{x}^2$$

where $\dot{z} = 1$. From this equation he calculates the 3rd, 4th and 5th fluxions of x, and the law shown in these is so obvious that he accounts for the higher fluxions by the phrase "and so on." He then develops x in powers of v (in the notation of Prop. VII., Cor. 2) in the form

$$x = c + \dot{c}v + \frac{\ddot{c}\,v^2}{2} + \frac{\dddot{c}\,v^3}{1\,.\,2\,.\,3} + \text{etc.}$$

where $x = c$, $\dot{x} = \dot{c}$ when $z = a$, and the values of $\ddot{c}$, $\dddot{c}$, etc., are obtained from the equation

$$(a+nc)\ddot{c} = \dot{c} + \dot{c}^2$$

and from the equations for $\dddot{x}$, etc., when a, c, $\dot{c}$, $\ddot{c}$, etc., are put for z, x, $\dot{x}$, $\ddot{x}$, etc. In this form the solution satisfies the conditions $x = c$, $\dot{x} = \dot{c}$ (any two constants) when $z = a$. Taylor goes on to seek a solution in finite form, and obtains it by putting $a + nc = n - 1$.

Another explicit use of the Theorem is given on the last two pages of the *Methodus*. Further, if we bear in mind Taylor's statement that he handles the two methods of increments and fluxions "promiscuously as if they were but one method," another testimony to the value he attaches to Prop. VII. is to be found in his proof of the Binomial Theorem (p. 55), even though he applies the Proposition itself and not the Corollary.

In view of these examples I think the language of Brill and Noether and of Cantor does not do justice to Taylor, and, especially if we consider the passages quoted from the *Phil. Trans.*, I cannot see that there is any good ground for the assumption that Taylor was not well aware of the great value of his Theorem for obtaining a development of a function of $z + v$ in powers of v.

But there is another paper by Taylor in which he calls express attention to the value of his Theorem. In the *Philosophical Transactions*, Vol. XXX. (1717), pp. 610–622, he has an article with the title *An Attempt towards the Improvement of the Method of Approximating in the Extraction of the Roots of Equations in Numbers.* He there shows (compare the letter to Machin) that when an approximation, z say, has been found for a root of the equation $f(y)=v$ and $z+v$ is put for y, the correction v is obtained by solving the equation

$$0=x+\frac{\dot{x}v}{1\,.\,\dot{z}}+\frac{\ddot{x}v^2}{1\,.\,2\,.\,\dot{z}^2}+\frac{\dddot{x}\,v^3}{1\,.\,2\,.\,3\,.\,\dot{z}^3}+\text{etc.},$$

or, putting $\dot{z}=1$,

$$0=x+\frac{\dot{x}v}{1}+\frac{\ddot{x}v^2}{1\,.\,2}+\frac{\dddot{x}v^3}{1\,.\,2\,.\,3}+\text{etc.},$$

where $x=f(z)$. The two equations to which he applies his method are

$$(y^2+1)^{\sqrt{2}}+y-16=0$$

and $$\log_{10}y-0\,.\,29=0$$

of which the roots obtained are 2·31516 and 1·949 844 599 68 respectively. His process is somewhat cumbrous, but he is quite clear as to the generality of his method, and he states explicitly that the function $f(y)$ need not be a polynomial but may contain logarithms, sines, tangents, etc.; in fact, the second of the above examples requires the expansion of $\log(z+v)$.

It is no doubt the fact that Taylor did not use his Theorem in the way we do now in an elementary course on the Calculus, but I think he has shown quite clearly that it may be used to obtain the standard series for logarithms, sines, etc., and he did beyond all question apply it to obtain solutions of differential equations.

I now come to the relation between Taylor's Theorem and John Bernoulli's. In an article which appeared in the *Acta Eruditorum* for 1694, and which is reprinted in his *Opera*, Vol. I., pp. 125–128, Bernoulli gives the following Theorem:—

$$\int ndz=nz-\frac{z^2}{1\,.\,2}\frac{dn}{dz}+\frac{z^3}{1\,.\,2\,.\,3}\frac{d^2n}{dz^2}-\frac{z^4}{1\,.\,2\,.\,3\,.\,4}\frac{d^3n}{dz^3}+\text{etc},$$

which is the Theorem referred to by Voss. The method of proof is peculiar; Bernoulli writes the identity, dz being constant,

$$ndz=ndz+zdn-zdn-\frac{z^2}{1\,.\,2}\frac{d^2n}{dz}+\frac{z^2}{1\,.\,2}\frac{d^2n}{dz}+\frac{z^3}{1\,.\,2\,.\,3}\frac{d^3n}{dz^2}\text{ etc.},$$

"the series being continued to infinity so that ndz alone is left."

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Each successive pair of terms is a complete differential and integration gives the Theorem, where it should be noted there is no constant of integration. (In the original article, as in the reprint, *Integr. ndz* is put in place of the later symbol $\int ndz$.)

Bernoulli gives some examples of the use of his Theorem, the first of which is a series for log $(a+x)$. He puts y for the logarithm but writes $dy = adx/r$ where $r = a + x$; then in the general formula he puts x for z and a/r for n, and finds the series

$$y = \frac{ax}{r} + \frac{ax^2}{2r^2} + \frac{ax^3}{3r^3} + \frac{ax^4}{4r^4} \text{ etc.}$$

If we take a log $(a+x)$ instead of log $(a+x)$ for y and divide by a the series is equal to log $(a+x)$ − log a; but Bernoulli asserts that the series "though different from that of Mr Leibniz has nevertheless the same value." The mistake of course is due to the neglect of the constant of integration, a neglect which occurs very often in Bernoulli's early work. But even when the constant is inserted, the series is not a Taylor series for log $(1+x/a)$; it does not proceed by powers of x.

Another example is that of expressing the sine in terms of the arc. If, for simplicity, the radius a is taken to be unity Bernoulli's expression is, x being sin y,

$$\frac{x}{\sqrt{(1-x^2)}} = \frac{y - \dfrac{y^3}{1.2.3} + \dfrac{y^5}{1.2.3.4.5} - \text{etc.}}{1 - \dfrac{y^2}{1.2} + \dfrac{y^4}{1.2.3.4} - \text{etc.}}$$

"so that, $x/\sqrt{(1-x^2)}$ being known, x also will be known." Bernoulli adds, "it is worth noting that the series in the denominator is cos y because according to Mr Leibniz the series in the numerator is found to be sin y."

It is obvious that Bernoulli's Theorem provides the representation of a function in the form of an infinite series, but the Theorem as it stands and as it is illustrated by Bernoulli, is of a totally different character from Taylor's, and, so far as I can discover, Bernoulli himself never claimed any priority over Taylor in respect of the Theorem of Prop. VII., Cor. 2 of the ***Methodus Incrementorum***. What Bernoulli (*Opera*, Vol. II., p. 584) did claim—and this is the claim on which Williamson comments so severely—was that Taylor "after the lapse of more than twenty years thought that the series [published in 1694] was worthy of being transferred, with a mere change of notations, to the book *de Methodo Incrementorum* which he published in 1715," and adds "See his book, p. 38." Had Williamson consulted the page referred to he would have found that Bernoulli meant, not Prop. VII. but Prop. XI., namely that the fluent of $r\dot{s}$ may be expressed by either of the series

$$rs - r'\dot{s} + r''\ddot{s} - r'''\dddot{s} + \text{etc.}$$

$$\text{or} \qquad \dot{r}s' - \ddot{r}s'' + \dddot{r}s''' - \text{etc.}$$

where the accents denote fluents and the points fluxions.

Bernoulli's Theorem is in fact most simply represented as the result of successive "integration by parts"; Taylor's Prop. XI. is at most a generalisation of Bernoulli's and was certainly suggested by it. (Mr Cantor's account of Taylor's Prop. XI. [History Vol. III., p. 368, 1st Edn.] can not be considered a fair reproduction). The direct proof of Taylor's dependence on Bernoulli is to be found in his method of investigation. The method is identical with that of De Moivre in his *Animadversiones in D. Georgii Cheynaei Tractatum de Fluxionum Methodo Inversa* (1704). Cheyne had given Bernoulli's Theorem, but his method of presentation was severely criticised by De Moivre, who took the opportunity (p. 69) of giving a proof that was quite different from Bernoulli's and free from the eccentricities that characterised Cheyne's. It is inconceivable that Taylor was not aware of the fact that his Prop. XI. was substantially the same as Bernoulli's Theorem, and one can not be surprised that Bernoulli felt aggrieved at Taylor's omission of any reference to him, especially in view of the frequent references to Newton.

It is of course the fact that Taylor's Theorem may be established by integration by parts, but that fact is by no means sufficient to justify the suggestion that Bernoulli did so establish it, and I do not think he ever claimed to have done so. Even so late as the publication of L'Huilier's *Principiorum Calculi... Expositio* (1795) we find separate investigations of the two Theorems without any indication of their relations; it is quite unhistorical to import into the writings of one age ideas and inferences that were only made clear at a much later date.

Stirling in his *Lineae Tertii Ordinis* (1717) establishes (p. 32) the form of Taylor's Theorem now usually called Maclaurin's Theorem; he employs the method of undetermined coefficients, as Maclaurin also did (*Fluxions* § 751), and gives various examples of its use, among them the expansion of $(a+x)^n$ and $\cos x$. *In fact he uses the Theorem in the way now common in elementary work*, and there is little doubt that he has no thought of his series as being anything else than Taylor's. He does not expressly mention Taylor in this connection in the *Lineae*, though his establishment of the series follows immediately a discussion of another Theorem of Taylor's; but in his *Methodus Differentialis* (1730) where he again deduces the same form of the Theorem (but now by the passage from a formula in increments in the manner of Taylor) he has the words (p. 102) "the first to discover this Theorem was Mr Taylor in his *Methodus Incrementorum*, and afterwards Hermann in the Appendix to his *Phoronomia*." The fact that Stirling in 1717 so freely used Taylor's Theorem in the expansion of functions seems to me to be weighty evidence in favour of the view that in the circles influenced by Taylor the use of the Theorem for the expansion of the familiar functions, $(a+x)^n$, $\log\,(a+x)$, $\sin x$, etc., as well as for much more complicated cases was quite familiar.

Stirling's reference to Hermann puzzles me. In the Appendix to his *Phoronomia* (1716) Hermann presents a proof of Newton's 5th Lemma. The proof is very cumbrous, and it is no easy matter to ascertain from Hermann's work what the final form of the coefficients is. But the values, when extracted, are correct. The difficulty begins with the Corollaries (p. 393). Cor. 2 gives Newton's Case 1, in which the ordinates are equidistant, but Hermann is not careful in stating whether a difference is $y-y_1$ or y_1-y, and when we come to Cor. 3 this ambiguity is vital. He there makes the interval between consecutive ordinates infinitesimal, equal to dz, but he takes the differences of the ordinates y in the wrong order, as I think. The odd differences dy, $d^3y, \ldots$ should have their signs changed, and if this change were made Taylor's Theorem would follow. However that may be, Hermann's conclusion is quite different. He does not write down the formula that would be deduced directly from Newton's by making the interval infinitesimal, *i.e.*, Taylor's Theorem; he only puts down the formula *obtained by integration* and thus arrives at *Bernoulli's*

Theorem. I do not think Bernoulli's Theorem is a legitimate deduction from his work (Stirling's reasoning (*Methodus Differentialis*, p. 103) does not seem to apply here), but the puzzle is "did Stirling think that Hermann had established Taylor's Theorem"? Of course had Hermann carried out the work correctly he *might have deduced the Theorem*, it was Taylor's own method; but he did not, and instead he arrived at Bernoulli's Theorem. It is possible that Stirling is merely referring to the *method* of making the distance between consecutive ordinates infinitesimal, but his statement is nevertheless somewhat puzzling.

The Appendix to the *Phoronomia* was written after the MSS. of the treatise had been sent to the printer. Had Hermann gone more fully into the Newtonian Lemma he might have come a very close second to Taylor, but the fact that it is Bernoulli's Theorem he reaches shows how easy it is to be near a discovery and yet miss it. It is, from our present standpoint, strange to see how near Newton in particular came to Taylor's Theorem and yet did not actually attain to it; the reason for this is not quite easy to understand but we should at least learn the lesson that it is not safe to credit a writer with the possession of recondite theorems unless on plain evidence. The tendency to read our own ideas into the work of previous writers is just as bad as the opposite tendency of crediting to ourselves what was the possession of our predecessors. Above all, the detestable vice of "nationalism" in science must be studiously shunned; it would be hard to overstate the loss that British mathematics suffered from the baleful controversy on the invention of the Calculus. At the present moment we should take warning from the experience of the past.