English Translation of Servois' 1814 "Reflections"

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Abstract

What were the philosophical foundations of the differential calculus at the turn of the nineteenth century? There were three competing notions: differentials, limits, and power series expansions. François-Joseph Servois (1768-1847), a disciple of Lagrange, supported the power series formalism and was sympathetic to a foundation based on limits. On the other hand, he claimed that the use of infinitesimals in mathematics would "one day be accused of having slowed the progress of the mathematical sciences, and with good reason." We give here an English translation of Servois' philosophical paper, "Reflections on the various systems of exposition of the principles of the differential calculus." We provide an analysis of Servois' paper in our article, "Servois' 1814 Essay on the Principles of the Differential Calculus, with an English Translation," available in the MAA online journal *Loci: Convergence* at http://mathdl.maa.org/mathDL/46/ (DOI: 10.4169/loci003487).

Reflections on the various systems of exposition of the principles of the differential calculus and, in particular, on the doctrine of the infinitely small[1]

By Mr. Servois, professor of the artillery schools

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[141][2] Among the different manners of presenting the differential calculus, I would not say that there is one that is necessary to adopt. All of those which are legitimate have at least in the eyes of these who propose them, several particular advantages. However, if it is useful to join the differential calculus solidly with ordinary algebraic analysis; if the passage from one to the other is to be easy and executed on the same level, so to speak; or if we wish to answer these questions in a clear and precise manner: What is a differential? When and how do differentials arise naturally? With which analytic functions do they maintain intimate connections, not just simple analogies? then I believe that I admit no partiality in affirming that we would incline towards that theory which I have tried to sketch an outline of in the previous article.[3]

In algebraic analysis, after having considered quantities as being determinate or constant, we are naturally led to consider them as variables. Any variation, be it constant or variable, is essentially a finite quantity, or at least this is the first judgment we bring to it. Now, [142] it is necessary to express the variation of a function composed of elementary variables by means of the variation of these same variables. This is the first problem which we may set ourselves in this matter; the first attempts at a solution lead us to series. Thus, when in arithmetic, we have not yet discovered series, rather quotients and roots, approximated by means of decimals, we are necessarily drawn there by considering the quantity to be variable. Series and the *differential calculus*, therefore ought to arise together. It is in the introduction of the latter that we encounter the first expansion of the *varied state* of an arbitrary function, z for example. In attempting to arrange this expansion in another way, we must pay attention to the very remarkable series of differences

$$\Delta z - \frac{1}{2}\Delta^2 z + \frac{1}{3}\Delta^3 z - \frac{1}{4}\Delta^4 z + \dots,$$

which one is tempted to give a name that recalls its composition: that of *differential* presents itself naturally. Already, by comparing the two different expansions

[1] *Réflexions sur les divers systèmes d'exposition des principes du calcul différentiel, et, en particulier, sur la doctrine des infiniment petits*, an article in *Annales des Mathématiques pures et appliquées* **5** (1814-1815), pp. 141-170. In some citations, the title begins with the words "*Philosophie Mathématique*," because the headline of a title page in Gergonne's *Annales* is the editorial category to which the article was assigned.

[2] Numbers in square brackets represent the original page numbers of the article in Gergonne's *Annales.*

[3] [Servois 1814a].

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to which the elementary binomial $(1 + a)^m$ is susceptible, we have discovered the series

$$a - \frac{1}{2}a^2 + \frac{1}{3}a^3 - \frac{1}{4}a^4 + \ldots,$$

to which we have given the name *logarithm* of $(1+a)$. Thus, by simple analogy, the differential is *like* the logarithm of the varied state $(z + \Delta z)$. Following this route other relations among the differential, the difference, the varied state, and numbers, are manifest. It is necessary to study the reason why. Everything is happily explained when, after stripping away the specific qualities of these functions, by severe abstraction, we simply consider the two properties that they possess in common: being *distributive* and *commutative among themselves*.

This path, although natural, was not the one followed by the inventors. [143] It is a fact that the differential calculus was born of the needs of geometry. Now the algebraic calculus, which concerns itself essentially with *discrete* quantities, that is, with *numbers*, may not be applied to *continuous* quantities, that is, to *extension*, which the numerical variations become when we suppose them to be arbitrarily or indefinitely small. Therefore, the means of unifying the calculus and geometry is necessarily the *method of limits*, which is why the inventors and the good souls that followed them took, or at least indicated, limits as the method of *exposition* and of *application* of the differential calculus.

Newton did not, as Maclaurin and some others of his compatriots did, apply his calculus of fluxions to mechanics without due consideration. His theory is founded on the theory of the final causes of quantities and, according to him, *Ultimae rationes reverà non sunt rationes QUANTITATUM ULTIMARUM, sed LIMITES ad quos rationes semper appropinquant*[4] (Book 1 of the *Principia*,[5] Scholium to Lemma XI); a very enlightened principle, one to which we have not paid sufficient attention.

Leibnitz, co-inventor of the differential calculus, professed the same doctrine. He always gave his differentials as incomparably small quantities and, in applications, he always believed that one could make the proofs rigorous by the method of Archimedes – that of limits. ... *Quod etiam Archimedes sumsit aliique post ipsum omnes, et hoc ipsum est quod dicitur differentiam esse datâ quâvis minorem; et Archimede quidem PROCESSU res semper deductione ad absurdum confirmari potest*[6] Reply to the objections of Nieuwentiit, *Oeuvres*,[7] vol. 3, p. 328). What's more, this wise man never admitted infinitely small quantities in the proper sense of the term. We know the rather long discussion which took place between him and Johann Bernoulli on this matter, a discussion

[4] "For those ultimate ratios with which quantities vanish, are not truly ratios of ultimate quantities, but limits towards which they approach nearer than by any given difference, [but never go beyond, nor in effect attain to, till the quantities are diminished to infinitum.]" English translation from [Newton 1999].

[5] [Newton 1687].

[6] "Which even Archimedes supposed, as well as others after him, and this thing which is called the differential is given to be however small as you like; and Archimedes was always able to confirm this by his process of deduction to absurdity." Translation by Prof. C. Edward Sandifer of Western Connecticut State University.

[7] [Leibniz 1765].

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in which he always held the negative (see the *Correspondence*[8] between these illustrious geometers, published by Cramer).

[144] In the beautiful preface of his *Institutiones calculi differentialis*,[9] Euler does not speak a different language. . . . *Hic autem LIMES qui quasi rationem ultimam incrementorum constituit; verum est objectum calculi differentialis.*[10] And if, in the course of his book, this great man let slip a few expressions that were a little hard to swallow, it seems to me we must interpret these benignly, following this formally recognized principle.

We know that d'Alembert distinguished himself among the geometers who applied the method of limits to the differential calculus. Thus, one ought not be surprised to count among the same ranks the good geometers who came afterwards: such as Karoten, Koestner, Holland, Tempelhof, Vincent Ricati et Saladini, Cousin, Lhuilier, Paoli, Pasquich, Gourief, etc. Furthermore, it would not be difficult to show that the particular methods, such as those of *derived functions* of the immortal Lagrange, which has many followers, and those of *indeterminates*, proposed or recommended by Boscowich, Naudenot, Arbogast, Carnot, etc., amount fundamentally to methods of limits. How then did it happen that this strange method of the *infinitely small* has acquired such celebrity, at least on the continent, and has even succeeded in placing its name among the synonyms of the *differential method*?

I might, if I had the time, assign several probable causes to this usurpation. But what surprises me most is that the method of the infinitely small still persists, not only among its followers, but among enthusiastic trouble-makers. Let us listen to one of these, for a moment, and admire!

> "The care to avoid the idea of the *infinite* in mathematical research, incontestably indicates a true ignorance of the meaning of this idea as well as a blind routine. And we are not afraid to admit that we believe we anticipate the judgment of posterity in declaring that, however great the works of certain geometers may be, the care that they take in imitating the ancients in the exclusion of the idea of the infinite, proves irrefragably [145] that they are not at the heights to which the science was brought since Leibnitz, because they avoid this lofty region where the principle of the generation of quantities is found, and consequently the true source of mathematical laws, to come crawling to the domain of the senses, the only one known to the ancients, where one finds nothing but the crude mechanism of calculation." (*Réfutation de la théorie des fonctions analitiques*[11] de Lagrange, Paris, 1812, page 40).

Already in a previous work (*Introduction à la philosophie des mathémati-*

[8] [Cramer 1745].
[9] [Euler 1755].
[10] "This limit, which is, as it were, the final ratio of the increments, is the true object of the differential calculus." Translation from [Euler 2000].
[11] [Wronski 1812].

ques,[12] Paris, 1811) the same author, in announcing that "the procedures (of the differential calculus) imply an *antinomy* which makes them alternately appear as though they were endowed with, and as though they were deprived of, a rigorous exactness" (*Philosophie*, etc., page 32), has rebuked the *non-infinitary* geometers, with that trenchant tone and that dogmatic emphasis, which forms the dominant color of the writings inspired by the *Système philosophique* (that of Kant), which he professes.

Let us try for a moment, to appreciate all of this in its proper place.

First of all, I recall quite well that Kant, finding the *infinite* in *pure reason* and the *finite* in the *senses*, concluded from the coexistence of these two facilities in the cognitive being, that he ought thereby to have, relative to the *cosmological* idea for example, various *antinomies* which are fundamentally illusions that are not difficult to avoid, when one truly wishes to distinguish carefully that which each of the*forms of cognition* brings to bear on them. Let us do the same thing with respect to the supposed mathematical antinomy,[13] which the disciple congratulates himself for having discovered in the theory of the differential calculus. Let us admit, because it is true, that calculation belongs exclusively to the senses which, according to these gentlemen, is the faculty of the *individual.* It follows that there is not just paralogism, but palpable error in subjecting the *infinite* to *calculation.* The *infinite* is the domain of another faculty – that of the absolute, or what is called *pure reason.* I beg my readers pardon for the use that I have just made of an idiom [146] with which, no doubt, few people in France are familiar, but here I am making an argument which we formally called *ad hominem.*

Let us not say that this illusion is so necessary that we may not reject it...! We walk ahead of he that denies the movement. Newton, d'Alembert, Lagrange, etc. have walked there, that is to say that they have put the principles of the differential calculus into operation without any dependence on the *infinite*, or even the word *infinite*.

But is the infinite not *this lofty region where the principle of the generation of quantities is found, the true source of mathematical laws*? Certainly not, unless you have completely made up your mind to remain under the influence of the illusion which you have indicated. I will add that, with respect to the differential calculus, the introduction of the idea of the infinite is not even useful to this end.

The idea of the infinitely small does not even shorten the *exposition.* Indeed, it is impossible to establish the hierarchy of infinitely small quantities of different orders, without recourse to Taylor series, or to various other equivalents. I defy you to prove satisfactorily without this that, for example, if dz is an infinitely small quantity of the first order, then d^2z is therefore one of the second order.

[12] [Wronski 1811].

[13] In his *Critique of Pure Reason* [Kant 1902], Kant described four "antinomies," or pairs of contradictory propositions he called "thesis" and "antithesis," arising from the disagreement between the evidence of the senses and the application of reason. Both Wronski and Servois are referring to the First Antinomy, addressing the finiteness or infinity of space and time. It is not clear that either of them clearly understands Kant.

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There is the same problem with *applications*. If we do not admit the hypothesis of the polygonal curve,[14] a hypothesis which appears so strange to those who have only studied the elements of Euclidean geometry, then I defy you to prove without Taylor series, that the prolongation to the tangent from the ordinate that is infinitely close to the ordinate of the point of tangency, the difference between the infinitesimal arc and its chord, etc., are infinitely small quantities of the second order *or higher*. If we admit the gothic hypothesis that the ratio $\frac{dy}{dx}$ is *rigorously* equal to the ratio of the ordinate to the subtangent, then why do we neglect terms in differentiating the equation of the curve? Moreover, as it has very often been remarked by the author of the theory of analytic functions,[15] it is *a fact* that the results of the *infinitesimal calculus* are exact by *compensation of errors*.[16] Now, [147] I again issue the challenge of explaining this *major* fact, without having recourse to series. Having said this, because it is absolutely necessary before everything else to master expansion by series, why would we not go from there immediately to the differential calculus, by the door that is open on the same level? And why would we return by a gloomy path, that of infinitesimal considerations, to the principles of the calculus? If we wish, according to the true theory, we may form abridged methods which permit in advance us to strike out or omit the terms of a series that will disappear at the end of long calculations. I do not oppose this that at all; experienced geometers all do it. And once we are in possession of these methods we may, in geometry and in mechanics, speak in a language which is close to that of the *infinitaries*, without nevertheless attaching the same ideas to the same terms, but it is absolutely impractical to begin with this.

There is more. If we consult the history of the differential calculus, we would see their many puerile or ridiculous questions, the most animated debates, and even errors, taking as their source the obscurity spread by the infinitely small, and the difficulties of handling them. I won't engage in this discussion, but who does not recall the *incomprehensibilities* of Sturmius, the *Subtleties* of Guido Grandi; the *Bridges built* between the *finite* and the *infinite* of Fontenelle; the error of Sauveur in the problem of the *Brachystochrone*; that of Jean Bernoulli himself, in the first solution to the *Isoperimetric* problem; that of Charles in *particular solutions* to differential equations; the discussion concerning the analytic expression of the accelerative force of non-uniform motion – discussions which degenerated into disputes between Parent and Saurin, concerning the theorems of Huygens on centrifugal force, and which gave rise to that ridiculous distinction between the force in a polygonal curve and in a rigorous curve; discussions that even now have not yet ended, to [148] judge at least, by several memoirs

[14] L'Hôpital's *Analyse des infiniment petits* gave two postulates for infinitesimals, the second of which is "We suppose that a curved line may be considered as an assemblage of infinitely many straight lines, each one being infinitely small or, what amounts to the same thing, as a polygon with an infinite number of sides, each being infinitely small, which determine the curvature of the line by the angles formed amongst themselves" [L'Hôpital 1696, p. 3].

[15] This is presumably a reference to Lagrange.

[16] The doctrine of "compensation of errors," which explains how calculus can give correct answers even when based on faulty arguments, was in fact first stated by Berkeley in *The Analyst* [Berkeley 1734].

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of Trembley, (*Berlin Academy*[17], 1801, etc.) etc., etc.

In a word, I am convinced that the infinitesimal method does not nor cannot have a theory, that in practice it is a dangerous instrument in the hands of beginners, that it necessarily imprints a long-lasting character of awkwardness and pusillanimity upon their work in the course of applications. Finally, *anticipating*, for my own part, *the judgment of posterity*, I dare to predict that this matter will one day be accused of having slowed the progress of the mathematical sciences, and with good reason. But I ought to recapture the thread of my reflections.

I have already hinted at the distinction that I established, following Euler, between the method of *exposition* and the method of *application* of the differential calculus. The latter, when it is a matter of *space* or *time*, the principal objects of applications, is necessarily the general method of series. Particularly with respect to applications, nothing, in my opinion, surpasses in elegance – I might even say majesty – the path traced out by the last two parts of the excellent *Théorie des fonctions analitiques*.[18] With regard to the first method, that of exposition, I have always found several inconveniences in deducing it from the consideration of derived functions[19] or, in general, of limits. In my opinion, one of the most serious inconveniences is not being brought to the fundamental series until after having gratuitously assigned them their form. This inconvenience, certainly sensed by the author of *derived functions*, was not happily removed by the proposed proof (*Théorie des fonctions* [*analitiques*], page 7 of the 1st edition and page 8 of the second). I explained this frankly, at the beginning of my second memoir,[20] and I cited the conforming opinion of Arbogast (*manuscript letter*) and of Burja (*Mémoires de Berlin*,[21] 1801), but no one, least of all me, could have dared to base a scandal such as the *Refutation de la théorie des functions analitiques* on this. I therefore had to cast my eye on another approach, and here is the path that I followed.

The first expansions into series that we encounter are the results of [149] successive transformations applied to an equation of identity. Let us write for example,

$$\frac{1}{1+a} = \frac{1}{1+a}.$$

Let us execute indefinitely on the right-hand side the division operation, and we will have the series

$$\frac{1}{1+a} = 1 - a + a^2 - a^3 + a^4 + \dots.$$

[17]Jean Trembley contributed a number of memoirs to the volumes of *Mémoires de l'Académie royale des sciences et belles-lettres* for the years 1801, 1802, 1803 and 1804.

[18][Lagrange 1797]. Servois used the more archaic spelling of "analytiques."

[19]I.e., derivatives.

[20]Here, Servois is making reference to the fact that his "Essay" [Servois 1814a] was based on two previously unpublished Memoirs that he had presented to the *Institut de France*.

[21][Burja 1804].

Now, write the identity

$$\frac{1}{a-b} = \frac{1}{a+x} + \frac{b+x}{(a+x)(a-b)}.$$

Letting $x = 0$, $x = c$, $x = d$, ..., successively, we have the sequence of transformed equations

$$\begin{aligned}
\frac{1}{a-b} &= \frac{1}{a} + \frac{b}{a(a-b)} \\
\frac{1}{a-b} &= \frac{1}{a+c} + \frac{b+c}{(a+c)(a-b)} \\
\frac{1}{a-b} &= \frac{1}{a+d} + \frac{b+d}{(a+d)(a-b)} \\
\cdots\cdots & \quad \cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots
\end{aligned}$$

Let us take the sum of the products of these equations by 1, by $\frac{b}{a}$, by $\frac{b(b+c)}{a(a+c)}$, by $\frac{b(b+c)(b+d)}{a(a+c)(b+d)}$, by ..., respectively and, after simplifying, we will have the series

$$\begin{aligned}
\frac{1}{a-b} &= \frac{1}{a} + \frac{b}{a(a+c)} + \frac{b(b+c)}{a(a+c)(a+d)} + \dots \\
&\quad + \frac{b(b+c)(b+d)\dots(b+p)}{a(a+c)(a+d)\dots(a+p)(a+q)} \\
&\quad + \frac{b(b+c)(b+d)\dots(b+p)(b+q)}{a(a+c)(a+d)\dots(a+p)(a+q)(a-b)}.
\end{aligned}$$

[150] It is with this formula that Nicole demonstrates how to sum an infinite series (*Mémoires de l'académie des sciences de Paris*,[22] 1727).

These series have the property of being halted at any term we wish and of having a complementary term, necessary to preserve the identity. In the first one, this complement is the remainder of the division at hand, divided by $1+a$. In the second, it is found at the end. I know that Taylor Series has, in fact, a similar complement that must also belong to all those series that are so derived, and consequently to all known series. From this I allow myself to conjecture that all series must be the result of a sequence of transformations of identical equations, and that all must enjoy the property of being halted wherever we might wish, and to preserve the identity by means of a complementary term. This conjecture is happily changed into a certainty, and the outcome is a new and very important notion of the nature of series. At the beginning of the preceding memoir, we saw how, beginning from equations of identity, I came to fundamental expansions. "The procedure that the author followed (as it is said in the report of the Commissioners) has two advantages which must be noted: the first is that he never requires that we know in advance the form of the series we seek and the second is that he permits halting this series at any term

[22] [Nicole 1727].

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whatsoever.” The form of the complement is recognized immediately. For Taylor Series, in particular, this form is the one that Ampère first noticed, in his very beautiful memoir on analysis (13th *cahier* of *Journal de l'école polytechnique*[23]).

Here, too, I find myself in direct opposition to the *Transcendental philosopher*.

> “Series taken in all of their generality, . . . have by themselves, in the indefinite number of these terms and [151] without the aid of any complementary quantity, a determined significance . . . This is the philosophical point of the important question of series. It is this point, according to us, that geometers have not yet achieved, in the state in which the science is found.” (*Réfutation*, page 58).

This time we no longer have any need to quibble in order to show up the falseness of these assertions. The equation of identity, the successive transformations, the series and its complement, are matters of *fact*. Divergent series may only be used with their complements, and in this way we have long ago happily resolved the paradox presented by the expansion of the fraction $\frac{1}{1+1}$. When convergence is recognized, we determine the successive and indefinite decrease of the complement on the basis of the comparison of consecutive expansions and by reason of identity. In practice, we only need the series itself, without having any need of this complement.

Without doubt, we might have remarked that our method of exposition offers another considerable advantage: that of preserving, in the quantities with respect to which our series are ordered, all of the generality to which they are susceptible. That is, not to require particular consideration as regards positive or negative, whole or fractional.

A second inconvenience in the application of limits to the exposition of the differential calculus - an inconvenience that it shares with the infinitesimal method - is to leave veiled in mystery these beautiful analogies of differential functions among themselves and with their factors. We have seen how I am brought to pull back this veil. With regard to this, the Commissioners have also had the goodness to say: “By showing that it is to their nature of being *distributive*, in general, and *commutative amongst themselves* and with the constant factor, that varied states, differences and differentials owe their properties, and the analogies in their expansions with those of powers, (the author) gives their *true origin*, and distinguishes this idea from *separation of* [152] *scales*, which Arbogast had conceived of, according to Lorgna, to explain the same circumstances, and that appeared somewhat dubious.” Indeed, in our formulas, we never lose sight of the *subject of the functions*, and it takes only little attention to perceive this. There is neither separation of scales, nor operations which lead exclusively to these scales. The use of the proposed notation (no. 2)[24] is not indispensible, it is simply quite useful, in so far as it saves the trouble of representing, in each instance, polynomial functions by new letters. The beautiful method of integrating equations with constant coefficients, published in

[23] [Ampère 1806].
[24] This reference is to §2 of [Servois 1814a].

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Annales de mathématiques (volume 3, p. 244ff),[25] which adds much interest to the formulas of the analogy, does not require separation of scales, as it will be easy to show. I can say nothing else here about another method of application that these formulas provide to the author of the cited memoir (*ibid* no. 9 & 10); this would take me too far afield. I will simply make the observation that, if one fears stumbling into a risky and little traveled path, one need take, as the initial formulas, only these one has derived oneself, and which, beginning with identities, were transformed only according to the twin properties of numbers, that of being distributive and commutative amongst themselves. Thus, for example, I would at very least commit to a revision of the initial formula if, among the results that it had provided me, I found a series like the following one (*ibid*, page 252, formula 23).

$$\frac{\pi}{4}\cdot\frac{1}{x^2}=\frac{1}{x^2-1}-\frac{1}{3}\frac{1}{x^2-3^2}+\frac{1}{5}\frac{1}{x^2-5^2}-\dots.$$

Indeed, because

$$\frac{x^2}{x^2-1}=1+\frac{1}{x^2-1},\quad \frac{x^2}{x^2-3^2}=1+\frac{3^2}{x^2-3^2},\quad \frac{x^2}{x^2-5^2}=1+\frac{5^2}{x^2-5^2},\quad \dots$$

it changes into [153]

$$\frac{\pi}{4}=\left(1-\frac{1}{3}+\frac{1}{5}-\frac{1}{7}+\dots\right)+\left\{\frac{1}{x^2-1}-\frac{3}{x^2-3^2}+\frac{5}{x^2-5^2}-\dots\right\}.$$

From this, because

$$\frac{\pi}{4}=1-\frac{1}{3}+\frac{1}{5}-\frac{1}{7}+\dots,$$

we conclude that[26]

$$0=\frac{1}{x^2-1}-\frac{3}{x^2-3^2}+\frac{5}{x^2-5^2}-\dots=\left\{\begin{array}{l}\frac{1}{x-1}-\frac{1}{x-3}+\frac{1}{x-5}-\dots\\-\frac{1}{x+1}+\frac{1}{x+3}-\frac{1}{x+5}+\dots.\end{array}\right.$$

In this, I make $x=0$ and I have, dividing[27] by 2

$$0=-\frac{1}{1}+\frac{1}{3}-\frac{1}{5}+\frac{1}{7}-\dots=-\frac{\pi}{4},$$

a result which is not true. If I also let $x=1$, I have

$$0=\frac{1}{0}+\frac{1}{2}-\frac{1}{4}+\frac{1}{6}-\dots-\frac{1}{2}+\frac{1}{4}-\frac{1}{6}+\dots=\frac{1}{0}$$

[25] [Français 1812].

[26] The equation below is as Servois wrote it. Clearly, the rightmost expression should have been divided by 2.

[27] Because of the error noted in the previous footnote, Servois does not actually need to divide by 2 here.

a result even more strange than the first.[28]

I think I may be permitted to draw a consequence of another kind from my theory of [154] *distributive* and *commutative* functions: that the Leibnizian notation for the differential calculus ought to be preserved. Leave the dotted letters to the English; let us preserve accents for the useful work of extending our alphabets. In applying powers to that notation which, according to all analysts, is the most perfect, let us reserve numerical exponents exclusively for representing the different orders of repeated functions. As for my notation for partial differentials,[29] one may think what one wishes; it has no other advantage than to be in harmony with that notation which I believed should be adopted for *partial functions* in general, which could hardly be more simple and more meaningful. Furthermore, it is remarkable that Euler had proposed something entirely similar in a memoir that appears in the *Nova Acta of Petersberg* (1786, page 17)[30].

I could have dispensed with giving (no. 19) an idea of the extensions to which the fundamental series (no. 15)[31] are susceptible, had I believed I should have restricted myself to establishing precisely what is necessary for differentiating functions. However, in my opinion, the *pure* differential calculus extends further than we commonly think. In particular, the expansion of functions into series belongs more to the substance of this calculus than to its application. Furthermore, I wished to show how, from fundamental series, we may arrive at something more general, in a very natural way. Here again, I am in opposition to the *Philosophe*, at least in the method. We know with how much fuss he communicated a certain general formula to the highest scientific body of Europe, and then to the public, *from which he draws all of those formulas that we know for the expansion of functions*; that is, he *descends* where I am endeavoring to *ascend*.

The general formula of the *Criticiste* gives Fx expanded according to the successive products of the varied state of φx, namely

$$\varphi x,\ \varphi x \cdot \varphi(x+\xi),\ \varphi x \cdot (x+\xi) \cdot \varphi(x+2\xi),\ \ldots;$$

[155] where ξ is the constant difference in the variable x. The coefficients of the various terms are very complicated functions of the differences of the same functions, in which one must, after expanding, substitute one of the values of x given by the solution of the equation $\varphi x = 0$. No doubt, one has noticed

[28]Servois inserted the following footnote here: "This is formula (21) of the cited memoir, borrowed from Euler, and from which the author deduces his formula (23), which contains the germ of the error I am noting here. This formula of Euler's, true in several particular cases, is in general nothing more than a manifest falsity, because by supposing $\alpha = n\pi$, n being a positive or negative whole number, we get $\frac{n}{4}\pi^2 = 0$."

[29]In his "Essay," Servois used the notation $\frac{\mathrm{d}}{x}z$ and $\frac{\mathrm{d}}{y}z$ for the partial differentials of a function z of two variables. In general, when the function f (which we would call an operator) applied only to one variable of z, Servois called it a *partial function* and denoted it by $\frac{f}{x}z$ or $\frac{f}{y}z$.

[30][Euler 1789].

[31]These references are to §19 and §15 of [Servois 1814a].

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that this formula is itself but a *particular case* of our formula (23, no. 13)[32]. Actually, it is sufficient to let

$$\varphi x = \varphi x,\ \varphi' x = \varphi(x+\xi),\ \varphi'' x = \varphi(x+2\xi),\ \ldots,$$

and then

$$\alpha = \alpha,\ \beta = \alpha + \xi,\ \gamma = \alpha + 2\xi,\ \ldots,$$

to have, by our equations (23) and (27), the series and the coefficients of the *Philosophe*.

To go from here to the series ordered according to the powers of φx, he supposes ξ to be infinitely small and, under this pretext, he quite simply changes the Δ's into d's. This might seem quite good to eyes afflicted with the infinitesimal squint, but it is not just a matter of this; I was waiting for the details of this transition, pushed all the way to one or another of the forms recognized in the previous memoir (no. 19[33]). However, on this score he is marvelously discreet. Indeed, see the tables of equivalent expressions (*Refutation*, etc. pages 18, 19, 33) which are bound together by these laconic sentences: "furthermore, we will see that these simplified expressions may be put in the form . . . we may easily transform those expressions into these . . . ." And, if you don't wish to take his word for it, have the courage to undertake his transformations . . . ! We add to this that his lists of analytic expressions do not always present a well articulated general law: in particular, such as the expressions denoted by the letter N (p. 19). I have hinted at it (no. 13),[34] and I positively affirm it here: these difficulties in the details are a major flaw in the *descending* method (which I would call *synthetic*, were I not [156] discussing it with a *Criticiste*), and the absence of these in the *ascending* method gives this method a great advantage over its rival.[35]

[157] Before finishing, I hope I may be permitted to present several reflections here on the application of transcendental philosophy and, in general, on the application of [158] metaphysical systems to mathematics, reflections which could only be given elsewhere with great difficulty, and which the subject that occupies me seems to lead to in a very natural way.

[159] In reading Kant, I foresaw that geometers would, sooner or later, become the object of petty criticisms by his sect. [160] In the prolegomenas of his *Critique of Pure Reason*,[36] one finds this very significant passage: (I'm following the Latin translation of Born) *Cum enim vix unquam de mathesi suâ*

[32] This is a reference to §13 of [Servois 1814a].

[33] [Servois 1814a, §19].

[34] This is a reference to §13 of [Servois 1814a].

[35] Here Servois begins a lengthy footnote, regarding material found within his "Essay on the Principles of the Differential Calculus." This footnote can be found in the appendix that begins on page 19.

[36] Kant's *Critique of Pure Reason* [Kant 1902] and *Prolegomena to Any Future Metaphysics* [Kant 1912] are actually different books. Servois used the plural, lower case and unitalicized "les prolégomènes," as though there were more than one *Prolegomena*. Perhaps there was something in the nature of F. G. Born's 1796 four-volume Latin translation of the works of Kant that confused him.

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philosophati sint (arduum sanè negotium) .... tritae regulae atque empiricè usurpatae .... iis sunt instar axiomatum.[37] However, I was far from imagining up to [161] what point they would be mistreated. In this magnificent conclusion to the *Philosophie des mathématiques* (p. 256ff), see with what superb disdain he responds to the question: *What was the state of mathematics, and above all of algorithmie*[38]*, before this philosophy of mathematics*? He repeats twenty times: "We did not know it ... We did not even suspect ... We had no idea ...."

But are we really as impoverished as he says? And doesn't the *Philosophe critique* strut about somewhat at the expense of our plumage?

"Purely algebraic theories of logarithms and sines were not known ...." Someone has already spoken against this allegation, citing, among others, the work of Suremain-de-Missery (*Theorie purement algébrique des quantitiés imaginaries*; Paris 1801).

"The fundamental law of the theory of differences was not known ...." In this way he describes the expression of the difference Δ^μ of the product $Fx \cdot fx$, in terms of the differences of Fx and fx, a formula which Taylor published long ago in the *Philosophical Transactions*[39] (vol. 30, page 676ff). It is certainly true that we had not "recognized [it] as the fundamental law of the entire theory of differences and differentials," because it is not true that it enjoys this property. The truly fundamental laws of these two theories are in the definitions of the difference and the differential. From these definitions we deduce several general facts, very useful in practice; the alleged law is numbered among them. In addition, the *Philosophe* is well aware of the insufficiency of his law, when it is a matter of differentiating a function of several variables, because he did not go so far as to give the form of the expansions in differences and partial differentials. But one must admire the subterfuge that he uses to preserve the universality of this law. He asserts that the *form* in question "needs no artifice in order to be deduced or proven ...." But if so, you are [162] all the more culpable of having presented this *form* in a false formula. (*Philosophe*, etc., formula (bh), page 116). We may compare this to the true formula that I gave in the previous memoir (75), and which contains the philosophical law as a *very particular case.*

"The theory of *grades* and *gradules*[40] was not even known ..." that is, that we had not yet thought of creating new notation to represent expressions as simple as[41]

$$\frac{\Delta^m \ln \varphi(x+\mu\xi)}{\ln \varphi x}, \quad \frac{d^m \ln \varphi x}{\ln \varphi x}.$$

I can grant him this much, *but nothing more.* The new calculations of the

[37] "Indeed, whenever anything of mathesis is philosophisized upon (a truly lofty activity) ... trite rules and empirical things are seized upon ... and these are the images of axioms." Translation by Prof. C. Edward Sandifer of Western Connecticut State University.

[38] Wroński uses the term *algorithmie* where we would use the term analysis.

[39] [Taylor 1717].

[40] Wroński defined *grades* and *gradules* analogously to differences and differentials, where the increments in the variables are given exponentially instead of additively [Montferrier 1856, pp. 96-103].

[41] We are using the modern ln notation, where Servois used L to denote the natural logarithm.

philosopher are too close to those of differences and those of differentials to constitute a particular branch of analysis. Certainly, it would not be worth the trouble of making the differential calculus itself an algorithm separate from that of differences, if the differential were expressed as a function of differences as simply as the *gradule* is expressed as a function of differentials. This is a very common philosophical consideration, which suggested to analysts - Euler in particular - the triple generation of numbers according to the formulas $N = P + Q$, $N = P \cdot Q$, and $N = P^Q$. From the same considerations it has escaped none of them that we may let x vary, as $z = \varphi x$, in three ways. That is, by supposing that x becomes $x + \xi$, $x \cdot \xi$, and x^ξ, and that as a consequence of these hypotheses, the function z may also vary in three ways by becoming $z+\xi$, $z \cdot \xi$, and z^ξ. Because of this, in order to determine what becomes of z when the increment ξ is repeated a certain number of times, there are in general nine problems to solve. The calculus of differences and that of differentials were born of the consideration of the first of these problems, that is, of the correspondence between the varied states $x + \xi$ and $z + \xi$. If the other problems were as fecund, there would still remain [163] many new algorithms to create, so that the enumeration presented in the transcendental philosophy, the branches of what is called the *Theory of the algorithmic constitution*, would be far from complete. However, analysts have noticed that the other problems reduce easily to the first. However, the calculus of *gradules* seems immediately to suggest itself in an important application, that which the philosopher used in his research on the form of the roots of a determinate equation, expressed as a function of its coefficients. This, at least, is what he would like us to conclude from a discussion that occupies fourteen mortal pages in quarto (*Philosophie*, etc., pages 83-96), bristling with the wildest algorithmic symbols. But when, unphased by all this machinery, we take the trouble to discuss the arguments, to simplify the calculations, and to translate the formulas into the common analytic language, we cannot completely refuse to agree with the assertions of the author.

After having proposed the equation of identity

$$(a' + x)(a'' + x) \ldots = A + Bx + \ldots = \Xi, \tag{1}$$

we are told that we should use the differential calculus to find expressions of $A, B, \ldots$ as functions of $a', a'', \ldots$ and that reciprocally it is by the calculus of *gradules* that we ought to arrive at expressions of $a', a'', \ldots$ as functions of $A, B, \ldots$. "Indeed, the product $(a' + x)(a'' + x) \ldots$ could not be decomposed into parts of the summation except by the differential calculus, and the sum $A+Bx+\ldots$ cannot be composed into factors except by the calculus of *gradules*" (*ibid*, page 83). The first proposition is false: we knew how to express coefficients as functions of roots long before the discovery of the differential calculus. The second proposition, which is nothing but a consequence of the first, at least if we do not wish to introduce vague reasoning into analysis that undermines the correctness of the science, is not even proved. [164] Using common analysis, I will quite easily derive the result to which the philosopher, armed with his *gradules*, is brought.

Here are hypothesis which are clearly permissible:

When the factors	$a'+x, a''+x, \ldots$	the function	Ξ
become	$\begin{cases} 1.\ (a'+x)^{t'-a}, (a''+x)^{t''-a}, \ldots, \\ 2.\ (a'+x)^{t'-b}, (a''+x)^{t''-b}, \ldots, \\ 3.\ (a'+x)^{t'-c}, (a''+x)^{t''-c}, \ldots, \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots, \end{cases}$	becomes	$\begin{cases} \Xi^{n-a}, \\ \Xi^{n'-b}, \\ \Xi^{n''-c}, \\ \ldots \end{cases}$

For greater simplicity, we take only three factors. The first hypothesis gives

$$(a'+x)^{t'-a} \cdot (a''+x)^{t''-a} \cdot (a'''+x)^{t'''-a} = \Xi^{n-a}.$$

Applying the second hypothesis to this result, we have

$$(a'+x)^{(t'-a)(t'-b)} \cdot (a''+x)^{(t''-a)(t''-b)} \cdot (a'''+x)^{(t'''-a)(t'''-b)} = \Xi^{(n-a)(n'-b)}. \quad (2)$$

If we had considered four factors, we would apply the third hypothesis to this result. In general, when there are m factors we apply $m-1$ successive hypotheses. In the present case, let $t'=a, t''=b, t'''=c$ in (2), and it becomes[42]

$$a''' = \Xi^{\frac{(n-a)(n'-b)}{(c-a)(c-b)}} - x. \quad (3)$$

If we make a, b, c infinitely small, then n and n' will also be infinitely small. Because in general $a^x = 1 + x \ln a$, when x is infinitely small, equation (3) becomes [165]

$$a''' = \{1 + (n-a)(n'-b) \ln \Xi\}^{\frac{1}{(c-a)(c-b)}} - x. \quad (4)$$

When we suppose "the arbitrary quantity x is equal to zero, for greater simplicity" (*ibid*, page 90) this expression takes the form

$$a''' = \left\{1 + N\frac{1}{\infty_1 \cdot \infty_2}\right\}^{M\infty' \cdot \infty''}. \quad (5)$$

This, quite seriously, is the unique result of the role that we entrusted to the calculus of *gradules*, to ensure it a brilliant entry into the world. I dare to ask: is it worth the trouble?

I remark that in (4) that we may dispose of the arbitrary value of x by giving it the value of zero, but this hypothesis reduces Ξ to A, and consequently nothing but the coefficient A enters into the right hand side of (5), and the root a''' is no longer expressed except by a single coefficient of the equation. What's more, this hypothesis clearly contradicts the one he is obliged to make further on (page 95), according to which the successive differentials of x, that is dx, d^2x, ..., must satisfy certain conditions, which as it is said in passing,

[42] In the original there was a typographical error in (3), where the power of Ξ was given as $\frac{(n-a)(n'-b)}{(c-a)(c'-b)}$.

would have a great need to be reconciled among themselves. In any case, even admitting that the right hand side of (5) is a function of the coefficients A, B, $\ldots$, what consequences do we intend to draw from it? That "the quantity a''' is an irrational quantity or radical of the order 3 - 1" (page 90), or of the form

$$a''' = \varphi\left\{\sqrt[\beta]{\sqrt[\alpha]{n} + n'} + n''\right\}, \tag{6}$$

where n, n' and n''' are functions of the coefficients A, B, $\ldots$.

[166] Here the philosopher has completely wrapped himself in the transcendental mystery. We perceive nothing less than that his arguments reduce to the following: the expression on the right hand side of (6) may be put into the right hand side of (5), thus this expression represents the form of a'''. I deny this conclusion. In order for two things to be pronounced equal to each other when they are equal to a third thing it is necessary that these be determinate. Now the expression in the right hand side of (5) is completely indeterminate, and it reduces to the form $N^{\frac{\infty}{\infty}}$ or $N^{\frac{0}{0}}$. What would we say about the logic of the analyst who, having discovered at the end of his calculations the two expressions $A = \frac{0}{0}$ and $B = \frac{0}{0}$, would conclude from them that $A = B$?

"The fundamental law of the theory of numbers was unknown ...." For this he gives us an algebraic theorem (*ibid*, equation (D), page 67) which is not the fundamental law of this theory. Rather it is the well-known theorem

$$\frac{x^n - a^n}{x - a} = x^{n-1} + ax^{n-2} + \ldots + a^{n-2}x + a^{n-1},$$

of which the first result is a somewhat distant consequence. The whole numbers are the terms of an indefinite series of numbers which has zero as its origin and 1 as the difference between any two consecutive terms. This is their definition and consequently the true fundamental law of their theory. The *Philosophe* hastens to conclude from his theorem the *impossibility of submitting the prime numbers to a law* (*ibid*, page 68). However, I would be very curious to see how he reconciles this consequence with a singular remark that Lambert had recorded in his *Essai d'architectonique* (Riga, 1771, page 507), the substance of which is the following. In the right-hand side of the equation

$$\frac{x}{1-x} + \frac{x^2}{1-x^2} + \ldots + \frac{x^m}{1-x^m} + \ldots = x + 2x^2 + 2x^3 + 3x^4 + 2x^5 + 4x^6 + 2x^7 + \ldots,$$

each coefficient is equal to the number of divisors of the exponent,[167] so that *all the terms and only those terms* with coefficient 2 have a prime exponent.

"The theoretical resolution of equations of equivalence was entirely problematical ..." Despite the promises of philosophy, it is still at the same point. The formulas assigned to roots (*ibid*, page 94) are neither more or less problematical than they had been, and *the general resolution of (literal) equations of all degrees*, given by the philosopher (Paris, 1812), is certainly quite far from having

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relieved all doubts. See, among others, those of my estimable friend Professor Gergonne, in this journal[43] (vol. III, page 51, 137, 206).

"The solution of differential equations was still quite imperfect ...." Philosophy has greatly advanced it! I am not at all persuaded. Rather, I would have desired that he made a little mention of the general methods proposed by Fontaine, Condorcet, Pezzi, etc., even had it been for no other reason than to oppose them.

"The law of the general form of series (the expansion of Fx, following the powers of φx), and even less the law of the most general form of these technical functions (with an expansion following the products of the varied states), were not at all known ...." The first of these, however, was but a particular case of Burman's formula that I have given (112).[44] It is found in the *Calcul des derivations*[45] of Arbogast (no. 287). The other, as I have said, is a particular case of my formula (23), known at least for the most general cases; it is as follows

$$Fx = A + Bx\varphi x + Cx^2\varphi x \cdot \varphi' x + Dx^3\varphi x \cdot \varphi' x \cdot \varphi'' x + \dots$$

because the solution of the problem of article 348 of *Calcul des derivations* reduces to this. Let us add that Euler was brought to something even more general in a very original memoir[46] (*Nova Acta Petrop.* 1786) on the famous series of Lambert, when he began with this expression [168]

$$z^n = 1 + \varphi n + \varphi' n + \varphi'' n + \dots.$$

"Taylor's law extends only to functions given immediately, and not to those that are given by equations ..." (*Refutation*, etc., page 30). We have demonstrated the contrary in the previous memoir (no. 19).

"To deduce the expansion of x (from the given equation $0 = \varphi(x, a)$), following the powers of φa, it is already much more than what has been done up till now in *algorithmie*" (*ibid*, page 32). This pretension should be appreciated after having read articles 318 through 326, inclusive, of *Calcul des derivations.*

I shall be even more brief on the other question of the *Criticiste*: *What will the state of* algorithmie *be, after this philosophy of mathematics*? I see the promises. The miser himself is not stingy. And the announcements of results ... well, listen, that is another thing all together. (*Refutation*, etc. page 38).

"If the philosophy had already given the laws of mathematics ...." No doubt, these laws belong to philosophy in general, and not to a particular system. The Peripatetics Hertinus, Dasypodius and Comp. had based geometry on syllogisms. The philosophers of Port-Royal, the new Procustes, had tortured this same geometry to reduce it to the proportions of their narrow logic. A German philosopher, who began as a disciple of Kant, but defected to the opposite

[43] These are three articles written by Gergonne in the *Annales de mathématiques pures et appliquées.*

[44] [Servois 1814a].

[45] [Arbogast 1800].

[46] [Euler 1789].

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ranks, came to persuade the mathematician Langsdorf that he should re-found the principles of this science by admitting *spatial points* into geometry. This is just a cross-section of the services that these systems render to mathematics.

"And what is guaranteed to them by the rigorous explanation of the difficulties . . . ." Yes, the imaginary difficulties of the differential calculus explained by a *critical antinomy*! The paradoxes of [169] Kramp resolved by zeros, or by *even* or *odd* infinitesimals, etc.!

"And above all, by the discovery of the fundamental laws of this science . . . ." I will repeat, there are no fundamental laws other than the definitions, which need no discovery.

"Laws that ought to bring us to the solution of great problems that we could not resolve before today . . . " *Fiat! Fiat!*

"What is left for geometers to do? Two things: the first . . . , to receive the principles of mathematics from philosophy . . . " This would be my position, if philosophy were a body of revealed doctrine.

"The second is to study transcendental philosophy, which is the foundation of this latter . . . ." However, what if the result of this study were too no longer to believe in transcendentalism, or at least to doubt it? Because, after all, it is a human opinion. Furthermore, it is a system shrouded in darkness that few people are able to pierce. Ch. Villers accuses the academics of Berlin of having seen nothing; others pay them the same compliment. In the midst of this brouhaha of philosophical discussions across the Rhine, we hear only this refrain . . . . "I am not being understood . . . !" And they intend to establish the clearest and most certain of the sciences on a basis of this kind! . . .

As for me, I declare, in closing, that I hold provisionally to the philosophy of mathematics that d'Alembert, as worthy as any other, and as philosopher and as mathematician, set forth these principles. "Because the certainty of mathematics," he says, (*Encyclopédie*[47], Art. *APPLICATION*) "comes from the simplicity of its object, its metaphysics should [not] be too simple and too illuminating. It should always be reducible to clear and precise notions, without obscurity. Indeed, how could the consequences be certain and self-evident, if the principles were not also." Also, he adds that this metaphysics (*Ibid*, art. *ÉLÉMENS*) "is simple and easy and, so to speak, [170] popular, and it is also precious. We might even say that the ease and simplicity are its touchstone."

Moreover, quite convinced that I am right to oppose the *Philosophie critique*, I only wish to disagree with his philosophy. I hasten, therefore, to declare that it pleases me to recognize, in the author of *Philosophie des mathématiques*, a very able and learned geometer whose works may become extremely useful to the science, if he manages to escape the influence of a philosophical system to which, according to me, he has rather unphilosophically subjected himself.

La Fère, August 10, 1814.

[47][Diderot 1751].

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Appendix: Servois' Footnote Beginning on Page 156

[156] I have said (no. 15)[48] that, by a simple change in the method of arrangement, we may change from an expansion following the products $\left(\frac{x-p}{\alpha}\right)$, $\left(\frac{x-p}{\alpha}\right)\left(\frac{x-p-\alpha}{\alpha}\right), \left(\frac{x-p}{\alpha}\right)\left(\frac{x-p-\alpha}{\alpha}\right)\left(\frac{x-p-2\alpha}{\alpha}\right), \ldots,$ to an expansion following the powers $\left(\frac{x-p}{\alpha}\right), \left(\frac{x-p}{\alpha}\right)^2, \left(\frac{x-p}{\alpha}\right)^3, \ldots$. We will see, perhaps with some interest, how I can justify this assertion.

I will take as the simplest case, the expansion of $F(x+n\alpha)$. It takes only a little attention, after the first attempts at expansion, to recognize that we have[49]

$$F(x+n\alpha) = Fx \quad \left.\begin{array}{l} +\frac{n}{1}\left\{\Delta Fx - \frac{1}{2}\Delta^2 Fx + \frac{1}{3}\Delta^3 Fx - \ldots\right\} \\ +\frac{n^2}{1\cdot 2}\left\{\Delta^2 Fx - \frac{3}{3}\Delta^3 Fx + \frac{11\Delta^4 Fx}{3\cdot 4} - \ldots\right\} \\ +\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ +\frac{n^m}{1\cdot 2\cdots m}\left\{\Delta^m Fx - \frac{A\Delta^{m+1}Fx}{m+1} + \frac{B\Delta^{m+2}Fx}{(m+1)(m+2)} - \ldots\right\} \\ +\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \end{array}\right\} \quad (1)$$

where A, B, ... are the coefficients in the series that multiplies $\frac{n^m}{1\cdot 2\cdots m}$, a series that I will denoted by Π in what follows, for simplicity. According to the general theory of equations, $A = S_1(1,2,\ldots,m), B = S_2(1,2,\ldots,m+1), C = S_3(1,2,\ldots,m+2),\ldots, M = S_\mu(1,2,\ldots,m+\mu-1)$, where $S_1, S_2, S_3, \ldots$, S_μ is the series of products taken one-by-one, two-by-two, three-by-three, ..., μ-by-μ, where μ is the rank of the letter M, when A is assumed to be the first. I will denote by P the series that multiplies $\frac{n^{m+1}}{1\cdot 2\cdots m+1}$. Its coefficients are $\frac{A'}{m+2}, \frac{B'}{(m+2)(m+3)}, \ldots$; where [157] A', B', ..., are what becomes of A, B, ..., respectively, when we change m to $m+1$. Now, it is clear that we have the relations

$$A' = A+m+1, B' = B + A'(m+2), C' = C + B'(m+3), \ldots, \\ M' = M + L'(m+\mu),$$

from which we immediately conclude

$$\begin{array}{rl} M' = & M + L(m+\mu) + K(m+\mu-1)(m+\mu) + \ldots \\ & +A(m+2)\cdots(m+\mu) + (m+1)\cdots(m+\mu). \end{array} \quad (2)$$

To abbreviate, I let[50]

$$\overline{A} = \frac{A}{m+1}, \ \overline{B} = \frac{B}{(m+1)(m+2)}, \ldots; \ \overline{A}' = \frac{A'}{m+2}, \overline{B}' = \frac{B'}{(m+2)(m+3)}, \ldots,$$

[48] [Servois 1814a, §15].

[49] In the original, Servois had $m-1$ in place of $m+1$ in the denominator of the last fraction below.

[50] For clarity we are using overlines where Servois simply used Roman capitals.

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which gives

$$\begin{aligned}\Delta^m Fx - \overline{A}\Delta^{m+1}Fx + \overline{B}\Delta^{m+2}Fx - \ldots &= \Pi && (3)\\ \Delta^{m+1} Fx - \overline{A}'\Delta^{m+2}Fx + \overline{B}'\Delta^{m+3}Fx - \ldots &= P && (4)\end{aligned}$$

and the general relation (2) becomes

$$M' = \frac{m+1}{m+1+\mu}\left\{\overline{M} + \overline{L} + \overline{K} + \ldots + \overline{B} + \overline{A} + 1\right\}. \qquad (5)$$

Here I let $m = 0$. Thus, (1) $\overline{A}, \overline{B}, \overline{C}, \ldots,$ are zero and I have

$$\overline{A}' = \frac{1}{2},\ \overline{B}' = \frac{1}{3},\ \overline{C}' = \frac{1}{4}, \ldots,\ \overline{M}' = \frac{1}{1+\mu},$$

as we already knew (1). Next, I let $m = 1$ in (5) and using the values of $\overline{A}, \overline{B}, \overline{C}, \ldots,$ corresponding to $m = 0$, I have [158]

$$\begin{aligned}\overline{M}' &= \frac{2}{2+\mu}\left\{\overline{M} + \overline{L} + \ldots + \overline{A} + 1\right\}\\ &= \frac{2}{2+\mu}\left\{\frac{1}{1+\mu} + \frac{1}{\mu} + \frac{1}{\mu-1} + \ldots + \frac{1}{2} + 1\right\}.\end{aligned} \qquad (6)$$

Now, we have the equation of identity

$$\begin{aligned}&\frac{(2+\mu-1)}{1+\mu}\cdot 1 + \frac{(2+\mu-2)}{\mu}\cdot\frac{1}{2} + \frac{(2+\mu-3)}{\mu-1}\cdot\frac{1}{3} + \ldots\\ &+\frac{(2+\mu-\mu-1)}{1}\cdot\frac{1}{\mu+1} = 1 + \frac{1}{2} + \frac{1}{3} + \ldots + \frac{1}{1+\mu},\end{aligned}$$

or rather

$$\begin{aligned}&(2+\mu)\left\{\frac{1}{1+\mu} + \frac{1}{\mu\cdot 2} + \frac{1}{(\mu-1)\cdot 3} + \ldots + \frac{1}{2\mu} + \frac{1}{1+\mu}\right\}\\ &-\left\{\frac{1}{1+\mu} + \frac{1}{\mu} + \ldots + \frac{1}{2} + 1\right\} = 1 + \frac{1}{2} + \frac{1}{3} + \ldots + \frac{1}{1+\mu},\end{aligned}$$

from which we conclude

$$\begin{aligned}&\frac{1}{1+\mu} + \frac{1}{\mu\cdot 2} + \frac{1}{(\mu-1)\cdot 3} + \ldots + \frac{1}{2\mu} + \frac{1}{1+\mu}\\ &= \frac{2}{2+\mu}\left\{\frac{1}{1+\mu} + \frac{1}{\mu} + \ldots + \frac{1}{2} + 1\right\}.\end{aligned}$$

In other words, by (6) we have, when $m = 1$,

$$\overline{M}' = \overline{M} + \frac{1}{2}\overline{L} + \frac{1}{3}\overline{K} + \ldots + \frac{1}{\mu-1}\overline{B} + \frac{1}{\mu}\overline{A} + \frac{1}{\mu+1} \qquad (7)$$

In general, if we have relation (7) in the case of m, I say that, for the case $m+1$, we have

$$\overline{M}'' = \overline{M}' + \frac{1}{2}\overline{L}' + \frac{1}{3}\overline{K}' + \ldots + \frac{1}{\mu-1}\overline{B}' + \frac{1}{\mu}\overline{A}' + \frac{1}{\mu+1}. \tag{8}$$

Indeed, according to hypothesis (7) and the general relation (5), we have [159]

$$\begin{array}{lll} \overline{M}' & = \overline{M} + \frac{1}{2}\overline{L} + \frac{1}{3}\overline{K} + \ldots & = \frac{m+1}{m+1+\mu}\left(\overline{M} + \overline{L} + \overline{K} + \ldots\right), \\ \overline{L}' & = \overline{L} + \frac{1}{2}\overline{K} + \frac{1}{3}\overline{I} + \ldots & = \frac{m+1}{m+\mu}\left(\overline{L} + \overline{K} + \overline{I} + \ldots\right), \\ \overline{K}' & = \overline{K} + \frac{1}{2}\overline{I} + \frac{1}{3}\overline{H} + \ldots & = \frac{m+1}{m-1+\mu}\left(\overline{K} + \overline{I} + \overline{H} + \ldots\right), \\ \ldots\ldots & \ldots\ldots\ldots\ldots & \ldots\ldots\ldots\ldots \end{array}$$

From this, I draw these two results

$$\begin{array}{rcl} 1. \quad \overline{M} + \overline{L} + \overline{K} + \ldots & = & \frac{m+1+\mu}{m+1} \cdot \overline{M}', \\ \overline{L} + \overline{K} + \overline{I} + \ldots & = & \frac{m+\mu}{m+1}\overline{L}', \\ \overline{K} + \overline{I} + \overline{H} + \ldots & = & \frac{m+\mu-1}{m+1}\overline{K}', \ \ldots, \end{array}$$

2. $\overline{M}'+\overline{L}'+\overline{K}'+\ldots = \overline{M}+\overline{L}+\overline{K}+\ldots+\frac{1}{2}(\overline{L}+\overline{K}+\overline{I}+\ldots)+\frac{1}{3}(\overline{K}+\overline{I}+\overline{H}+\ldots)+\ldots,$

Thus, substituting the first into the second, we have

$$\overline{M}' + \overline{L}' + \overline{K}' + \ldots = \frac{(m+1+\mu)}{m+1} \cdot \frac{\overline{M}'}{1} + \frac{(m+\mu)}{m+1} \cdot \frac{\overline{L}'}{2} + \frac{(m+\mu-1)}{m+1} \cdot \frac{\overline{K}'}{3} + \ldots.$$

Consequently,

$$(m+1)\left(\overline{M}' + \overline{L}' + \overline{K}' + \ldots\right) = (m+2+\mu)\left(\overline{M}' + \frac{\overline{L}'}{2} + \frac{\overline{K}'}{3} + \ldots\right)$$
$$- \left(\overline{M}' + \overline{L}' + \overline{K}' + \ldots\right),$$

which gives,

$$\frac{m+2}{m+2+\mu}\left\{\overline{M}' + \overline{L}' + \overline{K}' + \ldots\right\} = \overline{M}' + \frac{1}{2}\overline{L}' + \frac{1}{3}\overline{K}' + \ldots.$$

According to (5), the left hand side of this equation is $\overline{M}''$. Therefore, [159] relation (8) is true whenever relation (7) holds. However, this latter is proven for $m = 0$ and $m = 1$, thus it is true in general. Applying this to equation (4), we have

$$\begin{aligned}P = \Delta^{m+1}Fx - \overline{A}\Delta^{m+2}Fx + \overline{B}\Delta^{m+3}Fx - \overline{C}\Delta^{m+4}Fx + &\quad \dots\\ -\frac{1}{2}\Delta^{m+2}Fx + \frac{1}{2}\overline{A}\Delta^{m+3}Fx - \frac{1}{2}\overline{B}\Delta^{m+4}Fx + &\quad \dots\\ +\frac{1}{3}\Delta^{m+3}Fx - \frac{1}{3}\overline{A}\Delta^{m+4}Fx + &\quad \dots\\ -\frac{1}{4}\Delta^{m+4}Fx + &\quad \dots .\end{aligned}$$

The first horizontal line is the same (3) as $\Delta\Pi$, the second is the same as $-\frac{1}{2}\Delta^2\Pi$, the third is the same as $\frac{1}{3}\Delta^3\Pi$, ... Therefore

$$P = \Delta\Pi - \frac{1}{2}\Delta^2\Pi + \frac{1}{3}\Delta^3\Pi - \frac{1}{4}\Delta^4\Pi + \dots .$$

This is the relation that holds between two consecutive series, the coefficients of n, in the expansion of $F(x + n\alpha)$, following the powers of n, a relation which we have established in another manner (no. 15). From this it follows that, if we let

$$\Delta Fx - \frac{1}{2}\Delta^2 Fx + \frac{1}{3}\Delta^3 Fx - \dots = dFx,$$

as in (no. 15), we have

$$\Pi = d^m Fx, \quad P = d^{m+1}Fx.$$

In absolutely the same way, we go from an expansion of $(1 + b)^n$, given by the binomial formula, to the expansion following the powers of n. From this we see that it is sheer laziness on the part of analysts to introduce the infinite in order to make this change.

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