THE ROLE OF MATHEMATICS IN PHYSICAL SCIENCES

The Role of Mathematics in Physical Sciences

Interdisciplinary and Philosophical Aspects

Edited by

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Springer

A C.I.P. Catalogue record for this book is available from the Library of Congress.

ISBN 1-4020-3106-8 (HB)
ISBN 1-4020-3107-6 (e-book)

Published by Springer,
P.O. Box 17, 3300 AA Dordrecht, The Netherlands.

Printed on acid-free paper

Printed in the Netherlands.

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Preface

What the role of mathematics in physical sciences is, is a relevant philosophical and historical question whose answer is necessary to fully understand the real status of physics, in particular of contemporary physics.

Exactly the wish to have good and plausible answers has spurred physicists, mathematicians, historians of science, and philosophers of science from many countries to join together and friendly but rigorously discuss. From that meeting, which was held in the wonderful Isle of Losinj (Croatia) in 2003, this book had its origin.

Actually, it does not simply contain the text of the lectures given. It is something different and something more. Some chapters are new and improved versions of what was presented. Some others have been added to enrich the variety of possible suggestions.

This book has been published in occasion of the 40th anniversary celebrations of the Consorzio per la Fisica of Trieste.

The editors

Acknowledgments

We would like to thank those who have made possible the meeting in Losinj and consequently this book, by sponsoring and financing them. They are: the Italian Ministery for Foreign Affair; the Consorzio per la Fisica, in the person of their president, prof. Franco Bradamante, and subsequently prof. Giancarlo Ghirardi; the Area Science Park of Trieste; the International School for Advanced Studies; the "Abdus Salam" International Centre for Theoretical Physics; the Osservatorio Astronomico of Trieste; the Università Popolare of Trieste; the Istituto Italiano di Cultura of Zagabria and the Ruder Boskovic Institute of Zagabria. And last but not least, the wonderful staff, headed by dr. Aldo Baldini, which, with unlimited, enthusiastic dedication, rendered fluent and enjoyable the meeting ands less tiring the work on the book.

The editors

THE ROLE OF MATHEMATICS IN PHYSICAL SCIENCES – INTERDISCIPLINARY AND PHILOSOPHICAL ASPECTS

Introduction

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As only a cursory examination of the subject can illustrate, mathematics and physics have been related for centuries and now it seems quite impossible to think the latter without the former. In other words, to speak about the indispensability of mathematics for physics appears to be a real platitude. However it is not at all that simple and unproblematic. In fact a lot of problems arise from this relation: is mathematics really indispensable for physics, or could we have physics without mathematics? Did physics without mathematics exist? Could physics without mathematics exist now? Which are the relations between physics and mathematics? Is mathematics just a tool, or something more? Is it the language in which is written the nature or is it the language by means of which we try to know nature? Has it only a role in the logical structuration of a physical theory or does it furnish also a good path to discover new physical entities? Should we think physically and then should we add the mathematics apt to formalise our physical intuition, or should we think mathematically and then should we interpret physically what found? Can physics generate new mathematics? Can mathematics generate new physics? How can we explain the success of mathematics in the physical sciences? Should it really be explained, or is such a question a pseudo-question? Are there any limits to the mathematical applications? Does a pure mathematical method to construct new physical theories exist? Do we get mathematical objects by abstraction from real objects, or are they a direct product of our intuition?

G. Boniolo et al. (eds.), The Role of Mathematics in Physical Sciences, 5–8.

All these questions and problems have been discussed in this book from different perspectives and by authors with different philosophical backgrounds.

We have thought of dividing the book into three parts. The first one contains four contributions on the historical role of mathematics in physics.

Giorello and **Sinigaglia** question the idea that mathematical objects are not obtained by abstraction from real ones, but rather that they are generated by mathematical practice. The authors analyse this thesis in the light of two historical cases: the evolution of complex numbers and the development of Heaviside's Operational Calculus and give arguments for supporting Lakatos's idea of quasi-empiricism in mathematics.

Gómez Pin discusses the problem of the ontological priority between continuous and discrete quantity and analyses the relationship between discrete and continuous quantity as one of the main topics in both history of philosophy and science. He explains that, while the unit of discrete quantity is a genuine (atomic) unit but ontologically is a vacuum, the unit of a continuous quantity has great ontological weight but it is in fact a false (non atomic) unit. The history the author concentrates on is the debate Aristotle-Thom/Dedekind-Cantor.

Rédei presents J. von Neumann's view on mathematical and axiomatic physics. The author argues that the common evaluation of von Neumann's view on the mathematical rigour in physics, according to which he considered the axioms of set theory as a purely formal system, is misleading. Namely, as the author points out, von Neumann thought that conceptual clarity and an intuitively satisfactory interpretation was more important for a physical theory than its mathematical rigour and precision.

Finally, **Singh** looks at the Indian tradition of mathematics with respect to theories of mind and matter. In particular, the author explores the reason for the absence of mathematical physics in Indian mathematical traditions, while at the same time the mathematical thought was employed by several Indian philosophical schools in order to understand the functioning of human's mind. The author enquires the reasons for this analysing the connection between mathematics and the idea of causation in Indian tradition. The relation between causation and mathematics is clarified through the causal analysis of numeric cognition.

The second group of papers deals with philosophical analyses on the interaction between mathematics and physics.

Boniolo and **Budinich** join the contemporary discussion about the relation between mathematics and physics, *via* a semiotic approach, which is useful for the many aspects it allows us to tackle. In particular, they argue that the problem of the effectiveness of mathematics in physics is actually a false problem, caused by a misunderstanding of contemporary theoretical physics, which is intrinsically mathematical. Finally, they emphasize what they call Dirac's methodological revolution according to which the contemporary physical theory should be constructed by working with pure mathematics instead of reflecting conjecturally only on physical phenomena, thus allowing the discovery of new phenomena, as it happened with the discovery of antimatter, gravitational lenses and so on.

Crivellari looks at the algorithmic representation of astrophysical structures and presents an iterative structural algorithm that is the numerical stimulation of the physical processes that occur in a stellar atmosphere. Through its analysis the author tries to show that, when the right mathematics is to be determined, it is the physics of the problem to have a bearing on what the most efficient solution is.

Dieks discusses the, so called, unreasonable effectiveness of mathematics and argues that, quite the contrary, its effectiveness is actually to be expected and its being unreasonable is unfairly attributed to it. Dieks shows that mathematics is flexible and versatile and that it is the very difference in nature between mathematics and physics that makes it applicable in the most disparate scientific domains and hence vastly effective. The author illustrates his view by offering many examples from fundamental physics.

Dorato questions the mathematical aspects of physics, by analysing the possible connection between the problem of effectiveness of mathematics in the natural sciences and the philosophical questions concerning the nature of natural laws. The author argues that the problem of the effectiveness is, contrary to what some authors endorse, a genuine one and criticises the algorithmic conception of law. The aim is to review and evaluate the available literature on that matter and suggest new possible directions of inquiry regarding the problem.

Ghirardi analyses some mathematical aspects of modern science and argues that new and inexplicable phenomena can suggest new and innovative theoretical and mathematical perspectives; those perspectives and their formal aspects might in turn yield new and innovative views about nature, and therefore all such formal aspects should be fully developed whenever they qualify themselves as successful tools, to account for some basic features of a revolutionary phenomenological framework.

Rivadulla presents some theoretical explanations in mathematical physics in the context of the analysis of the problem of the usefulness of mathematics in physics. The authors criticises the view according to which mathematics tallies with nature since it is a structural science as nature is, and because of some evolutionary reasons that make us adapted to the structured world; Rivadulla gives reasons for sustaining that such a view is incomplete because it does not take into account the overdetermination of physics.

Šikić is interested in the relationship among mathematics, physics and music. He investigates the Pythagorean law of small numbers and its relevance in order to interpret our sensory discriminations of consonance *vs*. dissonance. The author argues that the view, which is allegedly confirmed by the fact of non-western musical traditions, according to which we should take the discriminations to be acquired and subjective, is a wrong one.

Finally, **Stöltzner** looks at theoretical mathematics and points to the philosophical significance of the Jaffe-Quinn debate, which is viewed as a paradigm for problems of rigour and mathematical ontology. After going over the essential of the debate, the author concentrates on the quasi-empirical character of mathematics and the dialectics of proofs and refutations, trying to make sense of "theoretical mathematics" within the Lakatosian approach.

The third part of the book contains two interesting considerations on the relation between mathematics and physics that spur us to think about it in a wider way.

In particular **Arnold** joins the discussion about the relationship between mathematics and physics. He presents, through examples, the problem of the mathematical rigour of the bases of physics and explains what the utility of a precise mathematical perspective of the real world is. The author also offers some arguments for the existing difference in the approach to the truth as understood by mathematicians and physicists.

In the last paper, **Zovko** questions the notions of value and meaning in quantum universe. The author suggests that the mental universe is subject to the same mechanism as the physical universe and that human thoughts are just actual quantum events over the entire brain or over a large part of it. He points out that both the mental and material universe can be unified as a physical reality on a deeper level, beyond our direct experience; such a realm could also accommodate ethical concepts of choice, meaning and value.

PART 1

MATHEMATICS AND PHYSICS: REFLECTING ON THE HISTORICAL ROLE OF MATHEMATICS

OLIVER HEAVISIDE'S "DINNER"

*Algebraic Imagination and Geometrical Rigour**

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Abstract: In the following pages we begin, in the first chapter, with a reappraisal of some ideas of Edouard Le Roy about mathematical experience, mainly in relation with the history of complex numbers. In the second chapter we discuss in some detail the *i*-story, and we draw a comparison between "Imaginary Quantity" and Operational Calculus from the perspective of Heaviside's conceptions of the growth of mathematics. In the third chapter we reconstruct the δ-story, i.e. the Heaviside calculus leading to the *constitution* of a new mathematical object, the so-called Dirac's δ-function. Finally, in the last chapter, we bring together methodological and historical considerations in order to support Lakatos' idea of *quasi-empiricism* in mathematics.

Key words: complex numbers; operational calculus; δ-function; abstraction; quasi-empiricism in mathematics; mixed mathematics; applications to physics.

1. "MATHEMATICAL FACTS" AS CONSTRAINTS

> Le progrès [de la Mathématique] consiste moins en une application de formes intelligibles données d'avance rigides et toutes faites qu'en une création incessante de formes intelligibles nouvelles, en un élargissement graduel des conditions de l'intelligibilité. Elle suppose une transformation de l'esprit lui-même. (Le Roy, 1960, p. 304).

* We wish to thank G. Bertolotti, G. Boniolo, P. Budinich, V. Fano, N. Guicciardini, and B. Sassoli for suggestions and comments.

G. Boniolo et al. (eds.), The Role of Mathematics in Physical Sciences, 11–32.

The quotation is from Le Roy's lectures at the Collège de France (Paris) in the years 1914-1915 and 1918-1919. More or less in the same years, Le Roy's key idea is echoed in Pierre Boutroux's search for the objective character of mathematical knowledge, based on

1. the so called "résistance" (resistance) of the mathematical matters to our will (we have really some "mathematical facts") and
2. the "contingence" (contingency) of mathematical findings or discoveries (see e.g. (Boutroux, 1920)).

Le Roy's version, as we shall see, helps to clarify crucial epistemological notions concerning "discovery/invention" in mathematics, mainly in connection with Lakatos' quasi-empiricism (Lakatos, 1976a); see also (Crowe, 1975; Gillies, 2000; Cellucci, 2000). Moreover, even if the title of Le Roy's lectures sounds *Pure Mathematical Thought*, some of his remarks contribute powerful insights into the standard dichotomy pure/applied mathematics, and throw important light on the controversial matter of the status of "mathematical objects". Indeed, in Le Roy's own words (Le Roy, 1960): "Même en Analyse pure, l'expérience joue un rôle, et un rôle capital. L'invention y est souvent découverte" (p. 298); see also (Hadamard, 1949).

According to Le Roy (see Boutroux point (1)), the working mathematician receives some inputs from the constellation of established ideas; however this constellation is not sufficient for generating outputs. The case of complex numbers will be exemplar. Le Roy observes (Le Roy, 1960):

> Les [quantités] imaginaires ne se *déduisent* pas de la science antérieure. Mais elles sont *réclamées* par celle-ci comme une condition de sa vie et de son progrès (p. 298).

He goes on:

> [Les quantités imaginaires] marquent pour l'analyste je ne sais quelle *obligation* de synthèse créatrice. Et leur apparition au bout d'une foule de voies dialectiques diverses, comme point de concours ou centre de convergence, comme élément simple ou invariant méthodique, leur confère une réelle *objectivité*, c'est-à-dire une existence indépendante de nos procédés d'étude. Mais une véritable *expérience* en a été nécessaire pour en arriver là. […] On […] saisira mieux encore [ça] en songeant aux deux problèmes que soulève encore de nos jours – au moins en quelque mesure – la conception des imaginaires. Comment, inventées qu'elles furent pour la résolution de l'équation du second ou du troisième degré, sont-elles non seulement nécessaires, mais encore suffisantes, pour la démonstration générale du théorème de D'Alembert qui domine toute l'algèbre? Comment ne faut-il pas des imaginaires nouvelles pour chaque

> degré nouveau d'équation? Pourquoi d'autre part, couples numériques représentables par des vecteurs dans un plan, ne se prêtent-elles à aucune extension, complexes à *n* éléments, vecteurs de l'espace à trois dimensions ou même de l'hyperespace, qui respecte la permanence des formes opératoires? (p. 298).

In these two passages, Le Roy emphasizes the *need* (this is the meaning of the French "*réclamées*") of resorting to a sort of experience in connection with the genesis of objectivity: in his own example, such is the research on factorisation of extensions of **Q** or **R** via some particular complex numbers (e.g. see (Ellison, 1978), as well the research on extensions of **C** violating some relevant formal properties (as in the case of William Rowan Hamilton's quaternions; see (Kline, 1972; van der Waerden, 1985).

So far, so good. However, it is not so easy to find any "counterpart in nature" for complex numbers (Giusti, 1999). This is not tantamount to claiming that complex numbers have no applications to the physical world. Of course, they do; indeed, applications in Electromagnetism and in Quantum Mechanics are well known. The point is rather this: the *genesis* of complex numbers theory, and in the building of the complex functions theory, "abstraction from physical objects" does not seem to be working (Giusti, 1999).

Yet, even here, we are dealing with what Le Roy calls "experience" (Le Roy, 1960):

> les imaginaires ne sont pas [...] le résultat d'une création factice. Elles ont été suggérées, amenées, appelées par toutes sortes d'exigences préalables. De bien des manières, avant même qu'on en eût élucidé la théorie, elles voulaient être, elles s'imposaient. Puis elles se sont montrées infiniment fécondes et, de plus en plus à mesure qu'on les expérimentait davantage, elles ont heureusement réagi sur le système entier de la mathématique. Aurait-on pu prévoir *a priori* qu'elles permettraient de résoudre les équations de tous les degrés, qu'elles engendreraient la théorie générale des fonctions par où l'Analyse a été plus que doublée ? Qui aurait pu deviner avant toute expérience le line merveilleux qui devait s'établir entre les nombres e et π et l'unité imaginaire i ? Remarque sur l'imprévisibilité du fait que les imaginaires seraient suffisants pour les équations de tous les degrés, alors qu'on avait démontré l'impossibilité d'une résolution algébrique. De même, qui aurait pu deviner avant toute expérience tant de liens merveilleux entre des éléments réels, établis par l'intermédiaire des nombres complexes? Remarque sur l'étonnement qu'on éprouve à trouver la dépendance foncière de certaines intégrations par rapport aux fonctions de variable imaginaire, jusqu'en physique mathématique. Cauchy a eu profondément

ce sens du réel dont je parlais tout à l'heure, et le travail par lequel s'est constituée peu à peu la doctrine des imaginaires nous présente vraiment l'aspect d'une élaboration expérimentale. (pp. 301-302)

Let us take an example. Remember that in the ring of the whole numbers **Z** we have the fundamental theorem of arithmetic (a generalization of *Euclid's Elements*, IX, 14: "If a number be the least that is measured by prime numbers, it will not be measured by any other prime number except those originating measuring it" (Euclid, 1956); see also (Heath, 1981)) stating that (except for + 1 and – 1) a number can only be resolved into prime factors in one way. After Pierre de Fermat and mainly thanks to Leonhard Euler, it was an interesting new mathematical practice to study "numbers" of the form $a+\sqrt{D}$, with $a, b \in \mathbf{Z}$, where D is a given integer (positive or negative) which is not a perfect square. The idea was to build a kind of arithmetic of *numeri surdi*; indeed, for $D < 0$, "numbers" $a + b\sqrt{D}$ are complex numbers, as it happens in Euler's procedure for Fermat's equation $x^3 + y^3 = z^3$, where $D = -3$. Moreover, rings $\mathbf{Z}[\sqrt{D}]$ proved to be very useful tools in dealing with many mathematical problems in 19th Century; the same is true for rings $\mathbf{Z}[\zeta]$, where ζ is a complex nth-root of the unity (i.e. $\zeta^n = 1$). Yet, the initial approach to problems like higher forms of Fermat's Last Theorem was guided by the idea that, for $\mathbf{Z}[\sqrt{D}]$ or $\mathbf{Z}[\zeta]$,we have "natural" analogues of Euclid fundamental theorem of arithmetic. Now, this is obviously true for $\mathbf{Z}[\sqrt{-3}]$, but it is false in general. For instance, assume $D = -5$, and try with "numbers" $a + b\sqrt{-5}$, with $a, b \in \mathbf{Z}$. Check that $6 = 2 \times 3 = (1 + \sqrt{-5})(1- \sqrt{-5})$. It proves that in $\mathbf{Z}[\sqrt{-5}]$ it is impossible to get a unique prime factors decomposition. Likewise, it is possible to find counterexamples to the unique decomposition also in $\mathbf{Z}[\zeta]$. (For the question see (Ellison, 1978, pp. 172-193); see also (Ribenboim, 1979; Giorello and Sinigaglia, 2001)

The *proof* that for some rings unique decomposition does not hold amounts to a *refutation* of this initial conjecture, which seemed so useful within Euler's approach. It is precisely a conjecture like this that for Le Roy (Le Roy, 1960) constitutes a kind of guiding ideas, a sort of preconceived hypotheses, something similar in the realm of mathematics to the empirical hypotheses "qui, selon Claude Bernard, constituent le premier moment d'une expérience" (p. 299). As it is the case of $\mathbf{Z}[\sqrt{-5}]$, we ignore *a priori* wheter or not this conjecture might be incorporated into the body of formal mathematics. The only way to settle the question is (Le Roy, 1960):

mettre en pratique, en service, mettre à l'essai, faire fonctionner le concept et voir comment il se comporte dans le calcul, bref éprouver l'idée par ses fruits (p. 299).

And Le Roy rhetorically asks (Le Roy, 1960):

> Nous ne savons aucunement d'avance quelle sera la réponse, ni quel remaniement l'épreuve nous forcera de faire subir au système antérieur at au concept nouveau, quel aspect final ils prendront l'un et l'autre (p. 299).

(Note that in this case one interesting "remaniement" led to Kummer's theory of ideal numbers; on this point see (Ellison, 1978, pp. 195-200.))

Considerations like these support Mach's well-known idea of a structural analogy between experiments in physics and demonstrations in mathematics (e.g. see (Mach, 1976). Indeed, this seems to explain why in general complex numbers offer a typical example of circumstances where "the body of mathematical tools anticipated the physicist's needs" (Thom, 1982).

2. THE I-STORY

Keeping this in mind, let us come back to the crucial object studied by mathematicians who were building an arithmetic for various **Z**[*D*] or directly for **C**: the quantity *i*, where $i^2 = -1$. To begin with, consider the following quotation from Heaviside's *Theory of Electromagnetism* (*ETM*) (Heaviside, 1899):

> It is not so long ago since mathematicians of the highest repute could not see the validity of investigations based upon the use of the algebraic imaginary. The results reached were, according to them, to be regarded as suggestive merely, and required proof by methods not involving the imaginary. (p. 459)

Heaviside remarks that in a research of this kind, strict Euclideanism represents an obstacle.[1] To those critics who note that "the rigorous logic of

[1] "The reader who may think that mathematics is all found out, and can be put in a cut-and-dried from like Euclid, in proposition and corollaries, is very much mistaken; and if he expects a similar systematic exposition here he will be disappointed. The virtues of the academical system of rigorous mathematical training are well known. But it has its faults. As very serious one (perhaps a necessary one) is that it checks instead of stimulating any originality student may possess, by keeping him in regular grooves. Outsiders may find that there are other grooves just as good, and perhaps a great deal better, for their purposes. Now, as my grooves are not the conventional ones, there is no need for any formal treatment. Such would be quite improper for our purpose, and would not be favourable to rapid acquisition and comprehension. For it is in mathematics just as in the real world; you must observe and experiment to find out the go of it. All experimentation is deductive work in a sense, only it is done by trial and error, followed by new deductions

the matter is not plain", Heaviside replies (Heaviside, 1899): "Well, what of that? Shall I refuse my dinner because I do not fully understand of the process of digestion?" (p. 9).

Quite correctly, Heaviside (1899) insists on the need for algebra to reach "a certain stage of development" before the imaginary "turns up":

> It was exceptional, however, and unintelligible, and therefore to be evaded, if possible. But it would not submit to be ignored. It demanded consideration, and has since received it. The algebra of real quantity is now a specialisation of the algebra of the complex quantity, say $a + bi$, and great extensions of mathematical knowledge have arisen out of the investigation of this once impossible and non-existent quantity. It may be questioned whether it is entitled to be called a quantity, but there is no question as to its usefulness, and the algebra of real quantity would be imperfect without it. (pp. 457-458)

As has recently been suggested (Stillwell, 1989), the quantity *i* seemed unintelligible because "a square of negative area did not exist in geometry" (p. 189). Appeal to history is here fundamental. The same historian pinpoints (Stillwell, 1989):

> The usual way to introduce complex numbers in a mathematical course is to point out that they are needed to solve certain quadratic equations, such as equation $x^2 + 1 = 0$. However, this did not happen when quadratic equations first appeared, since at that time there was no *need* for all quadratic equations to have solutions. Many quadratic equations are implicit in Greek geometry, as one would expect when circles, parabolas, and the like, are being investigated, but one does not demand that every geometric problem have a solution. If one ask whether a particular circle and line intersect, say, then the answer can be yes or no. If yes, the quadratic equation for the intersection has a solution; if no, it has no solution. An "imaginary solution" is uncalled in this context. (p. 189)

Indeed, the origin of *i* as a "solution" of the equation $x^2 + 1 = 0$ is a myth (Giusti, 1999). The context for the imaginary quantity was the solution of the

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and changes of direction to suit circumstances. Only afterwards, when the go of it is known, is any formal exposition possible. Nothing could be more fatal to progress than to make fixed rules and conventions at the beginning, and then go by mere deduction. You would be fettered by your own conventions, and be in the same fix as the House of Commons with respect to the despatch of business, stopped by its own rules" (Heaviside, 1899, pp. 32-33). On the limits of the Euclidean approach see also (Lakatos, 1976a, pp. 205-207).

cubic equation in the heroic age of the Italian algebra. In fact, the del Ferro-Tartaglia-Cardano solution of the cubic equation $y^3 = py + q$ is

$$y = \sqrt[3]{\frac{q}{2} + \sqrt{\left(\frac{q}{2}\right)^2 - \left(\frac{p}{3}\right)^3}} + \sqrt[3]{\frac{q}{2} - \sqrt{\left(\frac{q}{2}\right)^2 - \left(\frac{p}{3}\right)^3}} \quad [\ldots].$$

The formula involves complex numbers when $\left(\frac{q}{2}\right)^2 - \left(\frac{p}{3}\right)^3 < 0$.

However, it is not possible to dismiss this as a case with no solution, *because a cubic always has at least one real root* (since $y^3 - py - q$ is positive for sufficiently large positive y and negative for sufficiently large negative y).

Thus the Cardano formula raises the problem of reconciling a real value, found by inspection, say, with an expression of the form (Stillwell, 1989, p. 189):

$$\sqrt[3]{a + b\sqrt{-1}} + \sqrt[3]{a - b\sqrt{-1}} \ .$$

The first work to take complex numbers seriously was not Cardano's *Ars Magna* (1545) (in spite of the phrase "Cardano's formula"), but Rafael Bombelli's *Algebra* (1572). We will not attempt a detailed historical discussion of the solutions to this particular paradox of the cubic equation. For us, obviously, the solution is connected with the nature of *i* and the geometrical explanation of the meaning of this symbol in the Wessel-Argand-Gauss geometrical interpretation (Kline; 1972; van der Waerden, 1985, 178). But this interpretation came centuries after Cardano's formula and the algebraic approach sketched in Bombelli's work! Moreover, the turning point occurred when Descartes, in his *Geometry*, merged the problem of the nature of square root of –1 with the more general problem of "demonstrating" the so-called fundamental theorem of algebra. As he wrote, every algebraic equation has many solutions as his degree, but these solutions "ne sont pas toujours reelles, mais quelquefois seulement imaginaires" (Descartes, 1637). Aptly, Giusti comments that (Giusti, 1999) "Descartes does not explain what these imaginary roots are, and we have to intend literaliter this adjective *imaginary*" (p. 90); see also (van der Waerden 1985, pp. 72-75).

Be that as it may, the general development of algebra needed the consideration of numbers like $a + b\sqrt{-1}$, as Heaviside pointed out. Today, we can say that (Stillwell, 1989)

> at the beginning of their history, complex numbers $a + b\sqrt{-1}$ were considered to be “impossible numbers”, tolerated only in a limited algebraic domain because they seemed useful in the solutions of cubic equations. But their significance turned out to be geometric and ultimately led to the unification of algebraic functions with conformal mapping, potential theory, and another “impossible” field, non Euclidean geometry. This resolution of the paradox of $\sqrt{-1}$ was so powerful, unexpected, and beautiful that only the word “miracle” seems adequate to describe it. (p. 188)

This “miracle” is more astounding than the description of the *i*-story offered by Heaviside would suggest. However, Heaviside’s account discloses an interesting pattern in the growth of mathematics: namely, the transition from intuition to geometrical rigour *via* a process guided by the reliance on the power of algebra, tested by some kind of “mathematical experiments”. Even more significantly, he draws a comparison between Imaginary Quantity and his Operational Calculus, in particular with the so-called fractional differentiation (Heaviside, 1899):

> Now just as the imaginary first presented itself in algebra as unintelligible anomaly, so does fractional differentiation turn up in physical mathematics. It seems meaningless, and that suggests its avoidance in favour of more roundabout but understandable methods. But it refuses to be ignored. Starting from the ideas associated with complete differentiations, we come in practice quite naturally to fractional ones and combinations. This occurs when we known unique solutions to exist, and asserts the necessity of a proper development of the subject. Besides, as the imaginary was the source of a large branch of mathematics, so I think must be with generalised analysis and series. Ordinary analysis is a specialised form of it. There is a universe of mathematics lying in between the complete differentiations and integrations. The bulk of it may not be useful, when found, to a physical mathematician. The same can be said of the imaginary lore. (pp. 459-460)

We claim that an analogous pattern can be found in the Operational Calculus or in what we call the δ-story.

3. THE DRIVING FORCE OF "ALGEBRAICAL" IMAGINATION. THE δ-STORY

It is well known that Heaviside's main contribution to science was his development and reformulation of Maxwell's Electrodynamics.[2] It was in this context that his mathematical ideas concerning Vector Analysis and Operational Calculus arose. In both fields, Heaviside was a great *dissenter* with respect to the scientific community of his time. In what follows, we shall focus just on the Operational Calculus. In his classic article on Heaviside, sir Edmund Witthaker writes (Whittaker, 1928/1929):

> We should now (1928) place the Operational Calculus with Poincaré's discovery of automorphic functions and Ricci's discovery of the Tensor Calculus as the three most important mathematical advances of the last quarter of the nineteenth century. Applications, extensions and justifications of it constitute a considerable part of the mathematical activity of to day. (p. 216)

The same source emphasizes Heaviside's discomfort caused by criticism from Cambridge mathematicians (Witthaker, 1928/1929, pp. 211-216). In hindsight, however, we can say that it was precisely his experimental conception of mathematics, so despised by his purist critics, to lead him to the definition of operational methods and to the intuition of what would later be known as Dirac's δ-function.

In the rest of this section, we are going to offer a reconstruction of Heaviside's procedure with respect to some physical issues discussed in his *EMT*. Along the lines of (Lützen, 1979) and (Petrova, 1987) (see also (Struppa, 1983; Guicciardini, 1993)), though in a somewhat different way, we shall distinguish four steps in Heaviside's procedure:

a) operational solution
b) algebrization
c) fractional differentiation
d) impulsive function

(a)Operational solution

In *EMT* §§ 238-242, Heaviside considers a semi-infinite cable and a network with resistance operator Z in sequence, operated upon by an electromotive force E. Putting aside the self-induction in the cable, he finds that the potential $V(x, t)$ and the current $C(x, t)$ are connected by the equations:

[2] On Heaviside's life and work see (Süsskind, 1972; Nahin, 1988; Lynch 1991).