Anticipation in Geometric Calculus: from Leibniz/Grassmann to E. Kähler

José G. Vargas

July 26, 2017

josegvargas4 [at] sc [.] rr [.] com

PST Associates, LLC (USA)

ABSTRACT

We document the mathematical anticipation that started with Leibniz under the name of geometric characteristic. It was Grassmann who, one and a half centuries later, adopted the forward looking stance of Leibniz and, under the name of geometric calculus, gave rise to an avalanche of mathematical ideas not yet carried to their last consequences.

We proceed to explain how the main issues with Grassmann's main system, the progressive-regressive calculus, were solved. On the one hand, Clifford extracted his namesake algebra from the web of entangled concepts present in that system. On the other hand and independently of Clifford's work, É. Cartan enriched the calculus by basing it on another one of Grassmann's great ideas, the exterior algebra. Years later, his profound knowledge of the progressive-regressive calculus allowed Cartan to correct its limitations. Thus, after reformulating the Erlangen program, he used his exterior calculus to create modern differential geometry all the way to the famous Cartan-Finsler connection (year 1934).

We show the naiveté of what modernly goes by the name of geometric calculus and proceed with advocation of the overlooked Kähler calculus of differential forms, where the underlying exterior algebra is replaced with a superseding Clifford algebra. But Kähler's work lacked sophistication when post-scalar valued differential forms were concerned. We point out the extensions of his calculus to one for Clifford-valued differential forms and conclude with lessons of different types learned from Leibniz-Grassmann anticipation, both for mathematics and, more importantly, for the formidable problems that humanity, society and the culture face.

We wish to make potential readers aware of the fact that the term anticipation is here used from the point of view of the history of ideas (or

intellectual genealogy) and not from the acceptation of this term in papers in this notebook. It is, however, fitting to mention the emergence from the so extended Kähler calculus of a program for physical unification.

1 Introduction

In this paper, we speak of the evolution of algebra, calculus and geometry since the work by Grassmann in the mid eighteen hundreds as per the English translation by Kannenberg of his work [1]. This work was explicitly acknowledged by Grassmann himself as the realization of the Leibnizian anticipation. He is the one who was successful at launching the mathematical developments that made of Leibniz vision the beginning of an anticipation.

Mutatis mutandis, the development of this anticipation takes us through Clifford and specially E. Cartan to the Kähler calculus (KC), developed in 1960-62 [2], [3], [4]. It is significant that (to our knowledge) he never returned to his 1933 paper: *Über einer bemerkenswerte Hermitesche Metric* [5], which arguably is the work for which he is best known, even more than for his contribution to what is nowadays known as the Cartan/Kähler theory of exterior systems [6] (His metric nevertheless appears now and then in his work, but rarely if ever from 1958 on. And it is still more significant that in 1992, when he already was of very old age, he returned to the KC [7] with a more ambitious goal than in the 1960s.

He wrote the KC in German, but it would have been difficult to understand even if written in English. Several characteristics of his work go a long way towards explaining why this is so, and why it is almost forgotten in spite of its many merits. A main one is that virtually all of modern physics —- where it is most useful—- is done with techniques that were firmly established and widely used by the time when he made public the KC.

We advance that, as will become clear, any connection between the work of E. Cartan and/or Kähler with different areas of mathematics will be meant to refer to connections with the Leibniz/Grassmann anticipation. Cartan and Kähler constituting the maximum exponents of its development, their names may occasionally be used to refer to the present end point of that anticipation.

With regards to other authors that have contributed to this anticipation, we confine ourselves to significant developments directly related to Grassmann's work. Clifford falls in there, and less obviously Felix Klein, and less obviously but not less importantly Riemann. The latter's geometry is contained in the extensive work of É. Cartan on differential geometry, in turn deeply connected with Grassmann's contributions to algebra.

As intimated in the abstract, this paper started being a historical narrative of a particular line of mathematical development. But this mathematics becomes physics through the judicious choice of equations of formidable structural simplicity. That is an amazing feature of Kähler's mathematical and physical work, if one judges by the results that he himself showed. As for classical physics, which he did not deal with, the statement of zero curvature plays a similar structural role for classical physics, though less clearly so. Let us here refer to zero (affine curvature) as teleparallelism. Einstein proposed it for physical unification, and Cartan instructed him on how to extract physics out of it, but the former did not really listen [8]. The present author has taken Cartan's proposals to Einstein further, but we shall not deal with this for fear of overextending ourselves.

The issue then arises of the absence in this anticipation of what most would consider the modern mathematics used in standard theoretical physics. The Kähler calculus, which presently is the best implementation of Leibniz anticipation, is modern, but not main stream. As for mathematics that are viewed as modern, there are cases like category theory, which is virtually unrelated to physics at this time, and string theory, which is related.

Category theory can be viewed as a sector of algebra. It is so presented in the authoritative book on algebra by MacLane and Birkhoff [9]. It there constitutes the fifteenth of sixteen chapters, thus not a foundation of algebra. It may, however, be viewed as being intimately related to the foundations of other fields of other branches of mathematics.

The motivation for category theory is largely the attempt to avoid the concept of set as a corner stone in the foundations of arithmetic. The reason for such an attempt is the existence of paradoxes, the most famous of which is known as the Russell's paradox, or the paradox of the set of all sets. For a clear sophisticated description yet easily understandable statement of the paradox see for instance the book "Gödel's proof" [10]. Their sophistication is precisely what makes the authors use the term class where others would use the term set. Their definition of **class** has the flavor of the concept of **category**. In one case as in the other, the salient common feature of the definitions of those terms is the emergence and preeminence of the role of the relations that implicitly define the class or the category. The concept of category, or of class for that matter, may thus be relevant for a rigorous approach to analysis and differentiable manifold theory [11] or to differential forms in algebraic topology [12]. By the time that physics gets involved with, say, differential forms, the rigor in relating these objects to mathematical foundations has become irrelevant, provide that the use of these differential forms (meaning how we operate with them) is correct. So, category theory is presently irrelevant in the development of the Leibnizian anticipation.

There is then the modern mathematics inspired by physics, string theory being the most significant example. Half a century of work by many string theory groups has produced nothing but more string theory, without approaching its original goal of applicability to the foundations of physics, though producing other applications. Contrast that type of mathematics with the Kähler calculus and its immediate generation of new foundations for quantum mechanics. Leibniz anticipation is very old, but the application to physics of the Cartan/Kähler phase of its development beats easily anything that alternative mathematical developments have to offer. The proof is in the pudding.

The paper is organized as follows. In section two, we cite extensively from Grassmann's analysis [1] of what Leibniz did or failed to do in connection with his *geometric characteristic* [13]. In section three, we deal with the lines of evolution, respectively by Clifford and Cartan, of the work of Grassmann on that characteristic. In section four, we discuss the misappropriation by some practitioners of Clifford algebra of the term geometric calculus to refer to their work. In section five, we explain how the integration by Kähler of the Clifford and Cartan lines of evolution of Grassmann's ideas come together in his calculus. This helps to understand where the Leibniz/Grassmann anticipation appears to be going, which we explain in section six.

In section seven, we deal with considerations on anticipatory processes as judged from the Leibniz/Grassmann anticipation. We mean issues like the fog enveloping research in general and the development of an anticipation in particular, bifurcations in the process to get there, impact on paradigms (or the status quo) and the latter's' response.

In section eight, we deal with lessons on societal and historical issues that the Leibniz/Grassmann anticipation teaches us.

Section nine is a digression that points out at how much mathematics the Leibniz/Grassmann anticipation brings together.

2 Leibniz and Grassmann on the former's geometric characteristic

In general Leibnizian circles of study, in contradistinction to more mathematical circles, one usually speaks of *universal characteristic* rather than *geometric characteristic.* Comparison of the terms geometric and universal suffices to intimate that the latter was meant to be far more comprehensive than the former. It is worth being aware of the fact that Grassmann used the term *geometric analysis* to refer to the Leibnizian geometric characteristic.

Nothing much has come out from the anticipatory stance of universal characteristic. A cursory look at Wikipedia will be more than enough to get a bird's eye idea of what Leibniz might have had in mind when speaking of the *geometric characteristic*, which is the one in which we are interested here. It should be thought of as the restriction to geometry of his anticipated universal characteristic.

The cited material that follows has been extracted from the book [1]. The page numbers given below refer to this book.

In page 404, we find that Leibniz stated the following:

I was no longer content with algebra, which provides neither ... nor... This was because I believe we need another analysis properly geometric or linear, by which we express *place* directly, just as algebra expresses *magnitude*. And I believe I have seen the means, and how to represent figures and even machines and movements in characters, just as algebra represents numbers of quantities, ..." Emphasis in original.

Later in the same page, we read: "I have found some elements of a new characteristic, entirely different from algebra, ...".

And further down: "Algebra is nothing but the characteristic of indeterminate numbers or *quantities.* But it does not directly express *situation, angles* and *motion.*" Emphasis in original.

Finally: "Algebra is obliged to assume the elements of geometry, whereas this characteristic carries analysis right through to the end. If it is achieved in the manner I conceive it, one would be able to cast into symbols, that would be only letters of the alphabet, the description of ..."

Mathematical details beyond furthering this description can be found in the aforementioned essay.

We now let Grassmann speak, first of what Leibniz intended and then of his failure to achieve it. From here on, all emphasis is added.

Grassmann illustrated the exceptionality of the Leibnizian ideas by speaking of a class to which they belong (p. 316): "... all the more does such an intellect stand out in those ideas which are ahead of the times and even *anticipate* ..." Further down:

Those ideas —Grassmann goes on— "are of a type that... only a privileged few contemporaries are permitted to enter and discern the wealth of developments that will be *manifold a future time*". In the same paragraph, he stated: "Often only *after centuries* when time has pushed development to the point *anticipated* by that intellectual force, will those thoughts sow a rich harvest".

He starts a new paragraph as follows: "That Leibniz's sublime idea, ..., namely the idea of a genuine *geometric analysis*, belongs among these leading and, as it were, prophetic ideas cannot be doubted ...".

From his next paragraph, we extract the following: "For on the one hand the ideal of this *geometric analysis*, as conceived of by Leibniz for *future development*, has by no means yet been completely achieved, ...". Retrospectively, the use of "completely" speaks of how unaware Grassmann was of the fact that his own ideas would have to evolve (and the process may not yet be complete!) before it is justified to speak of any degree of completeness.

At the top of the next page (p. 317), Grassmann starts to speak of what amounts to be the two elements of anticipation. Indeed we read "Leibniz himself distinguished most clearly *on the one hand his idea* of a pure *geometric analysis*, whose formulation and completion floated before his eyes as a distant goal, whose importance however he completely understood, *and on the other his attempt* at such a new *characteristic*, ...". He concludes the paragraph with the statement: "The two must therefore be sharply distinguished if one is properly to appreciate Leibniz's contribution."

Grassmann then writes about Leibniz's failure at the use of the anticipated outcome to guide his efforts. He thus said: "In fact, one easily convinces oneself that the *characteristic* attempted by Leibniz does not in the least realize what he generally promised of *geometric analysis*, on the contrary ... " And regarding Leibniz's awareness of his own limitations, Grassmann wrote: "... he by no means ascribed to his attempt that merit which he would ascribe in rich measure to *geometric analysis* in general".

Grassmann proceeded to rhetorically ask why Leibniz had such strong, emphatic, esteemed convictions about a subject "whose consideration he did not know". He then announced that his paper would show that Leibniz perceived correctly, and continued: "This I will do by formulating at least in outline an analysis which in general actually accomplishes what he regarded as the goal of *geometric analysis* ...” While further elaborating on how he intended to go about his proof, he mentioned that the essential merits of the analysis were already enumerated by Leibniz with a degree of completeness.

For a modern printout on Leibniz’s geometric characteristic, see the French translation of the original work under the title ”La Caracteristique Geometrique” [13].

3 Of Grassmann, Clifford and E. Cartan

Leibniz’s forward looking stance was adopted by H. Grassmann, who used it to produce a formidable amount of mathematical work. The most impressive part of this work was the so called Grassmann’s system. This is not the exterior algebra also discovered by him and that has received his name. It is far more than that, involving multiplications that go by the names of progressive, regressive and interior products (For a superb description of Grassmann’s mathematical work and of this system in particular, see the 17 pages dealing with it in Cartan’s *Complex Numbers* paper [14]).

Grassmann intended his work to be essentially geometric, not purely or predominately algebraic. This transpires clearly from his system. An important question then is: what is the connection between algebra and geometry? We are referring here to geometries where both points and lines, or both points and vectors, are involved.

Retrospectively, this question is not precise as formulated. Algebra connects directly only with so called elementary or Klein geometries [15], [16], not with the generalized ones. This distinction was not present in Klein’s original Erlangen program [17]. For him, the geometry of the sphere was like the geometry of the plane, the difference lying just in the group at work in each case (Nowadays the geometry of the sphere is viewed, in spite of its great symmetry. as a generalized geometry not as a Klein geometry which the geometry of the plane is. The difference was not understood in those times). It should then not be surprising that the existence of such different fields of mathematics as algebra, elementary geometry, calculus and differential geometry was virtually impossible to fathom at mid century. This did not impede, however, that in the foreword to *A New Branch of Mathematics*, Albert C. Lewis would state: “One outcome of Grassmann’s effort to clarify the foundations of mathematics, for example, is a degree of generality and rigor that evidently could not be fully appreciated until the twentieth century" [1].

In order to make the question precise, we concern ourselves at this point with the difference between affine and vector spaces, or, if you prefer, between Euclidean spaces and Euclidean vector spaces. The difference is that affine and Euclidean spaces do not have a zero, or a special point for that matter. Vector spaces do. There is a proper way to deal with this difference, certainly present in Cartan's work, but also present in disguised form in the work of several French mathematicians who worked with so called moving frames before him. Grassmann made significant progress in his dealing with that difference, but failed in a most important respect, as we now explain.

In applying his system to 3-D Euclidean space, Grassmann would distinguish four classes of elements, as Cartan explained [14]. They bear resemblance to the grades of a graded algebra. The first one, called the primitive class, is constituted by the points of the space and the vectors. But points do not have dimension; vectors do. This unfortunate feature of his system carries on to other classes, which is why, in dimension n, he ended up identifying n-vectors and scalars. This is only part of the larger mess that his work looks like, given the lack of ordering and structure in his myriad of new ideas.

In his paper *The Tragedy of Grassmann* [18], the notable French mathematician Dieudonné speaks eloquently of what is best and worst in Grassmann's works. In particular, he starts a paragraph with the statement "It is almost unbelievable that such an original work should have been entirely ignored by contemporary mathematicians, ..." And finishes the paragraph as follows: "... the triumph and the failure of Grassmann, namely a lot of wonderful ideas rendered ineffective by lack of the necessary technique, and ending up in a frightful mess".

How then comes that, in spite of the bad consequences of putting together points and vectors in the same class, Cartan would refer to Grassmann's calculus as being very fruitful. The reason is that he apparently understood where the flaw was and that its correction resided in connecting it with the theory of moving frames. This highly plausible influence of Grassmann on Cartan is overlooked in the literature.

We have intimated that a formal concept of geometry did not exist in Grassmann's time, except during the very last years of his life with the advent of the Erlangen program [17]. He, however, anticipated the eventual formulation of geometries in terms of groups when he said that "all concepts and laws of the new analysis can be developed completely independently of spatial intuitions, since they can be tied to the abstract concept of a continuous transformation..." (page 384 in Kannenberg's translation).

Clifford replaced progressive and regressive products with Clifford products [19]. In the case of product of two vectors, or of product of vector and arbitrary element of the algebra, this reduces to the sum of exterior and interior, which are largely similar to progressive and regressive (The exterior product was so obviously present in Grassmann's work that exterior algebra is nowadays known as Grassmann's algebra). Clifford created his algebra by cutting and pasting from Grassmann's own work, but without including points in his graded system. He treated the dot product as clearly as the exterior one, and put both of them together without being distracted by other products. Grassmann had gone through all of them since the 1840's. It was only in the 1870s that he started to put the exterior and interior products together in that special case. He had already done that in 1846, but without doing much about it in one case as in the other. Clifford formalized an algebra based on these two products, and did some work with it. The algebra got his name.

In the aforementioned paper, "Complex Numbers" [14], Cartan devotes three pages to Clifford and Lipschitz jointly. The former was luckier in that their common algebra was named after him, even if, in certain respects, Lipschitz appears to have gone further, specially with regards to rotations [20]. In footnote #280, Cartan calls attention to the fact that both of those authors, and another two that he mentions in the same footnote, only dealt with units of square minus one. This lack of generality betrays the influence of Hamilton. All these authors were thinking of the generalization of the complex numbers without a real concern for a new view of geometry. Clifford nevertheless emphatically recognized that Grassmann had done the heavy lifting in the process of reaching Clifford algebra. And, like Grassmann, his work was essentially algebraic, not geometric. If any or both of them was thinking of doing geometry, they had not understood the difference between algebra and geometry.

It was Cartan who decisively contributed to the further development of Grassmann's work, and thus to the Leibnizian anticipation. In 1899, he formulated the exterior calculus [21]. It is based on Grassmann's exterior algebra, but with elements that are differential forms, not multivectors. In addition he understood that differential geometry had to do first with groups, not with algebras. In 1910, just two years after the 140 page "Complex

Numbers" paper, Cartan was already dealing with groups of transformations within bundles of frames. But that was only the beginning.

It is easy to appreciate the relation of exterior and Clifford algebra to several of the existing calculi. It is also easy to appreciate the relation between calculus and differential geometry, as the main branch of this geometry is understood to be the calculus of vector-valued differential forms. Not as easy to appreciate is the relation between the Erlangen program and what became of Grassmann program before Cartan, not what it first intended. Geometry, and thus the Erlangen program, is first about Lie groups of transformations. Grassmann's program became one about algebras, not about groups. Algebras in geometry come in a second phase of the latter's development.

The Erlangen program was right in aim, but wrong in details. Cartan modified the Erlangen program in such a way that it entered the realm of Leibniz anticipation. He also put the original Riemannian geometry in the context of the theory of connections, enriching both in the process.

In 1908, Cartan published a second major paper (*The Subgroups of Continuous Groups,* 131 pages) of relevance for the present discussion [22] . Regarding the impact of that paper, Dieudonné [23] said that Felix Klein

> "... had envisaged groups of isometries of Riemannian spaces as a field of study of this program, but in general a Riemannian space does not admit any isometries except for the identity transformation. By an extremely original generalization, É. Cartan was able to show that there as well the idea of "operation" still plays a fundamental role; but it is necessary to replace the group with a more complex object, "called the principal fiber space"; one can roughly represent it as a family of isomorphic groups parametrized by the different points under consideration; ...".

The "more complex objects" replace groups. These thus cease to be the cornerstone of geometry, though they remain very close to it in a more general scenario. Those statements by Dieudonné are not totally fortunate. When he refers to the principal fiber space he has in mind frame bundles, but without stating the relation of bundles to groups. That is what a connection does, if one integrates it along a curve between two points on a manifold. The result is a transformation that takes us from one frame to another frame (i.e. another basis at, in general, another point). It is thus a path-dependent member of the group.

This extremely original generalization arises from clearly distinguishing between an affine space, a concept which pertains to geometry, and a vector space, which pertains to algebra. This difference appears not to have yet sank in the minds of many modern scientists and even mathematicians, which certainly know but overlook it. Let me say it for the second time. An affine space —the plane, for example— does not have a special point, as all of them are equivalent. It does not have a zero but has the defining property that, if one arbitrarily chooses a point to play the special role of a zero, one can put all its points in a one-to-one correspondence with the vectors of a vector space. But that special role brakes their equivalence. Cartan restored equivalence by considering the set of all frames, i.e. of all pairs constituted by a base and a point. This set can be viewed as a bundle of fibers, i.e. subsets constituted by all pairs that have a point in common. We thus have two relevant groups, the group of all "transformations" relating such frames to all other frames (G), and its subgroup (G_0) doing the same within each fiber. When Cartan spoke about this, he generally did so in passing. The reason was that he was usually dealing with specific groups G that had a subgroup G_0 as required.

A geometry is defined by a connection on a tangent bundle of a given space. If the connection is integrable, the geometry is then called elementary [15], or Klein according to other authors [16]. Groups G, emerge only when the connection is integrable. When it is not so, the geometry is called generalized and groups are not directly involved. Hence:

A. **The original Erlangen program was wrong in this aspects of the relation between geometries and groups.**

B. **The statement that a Lie group (G) defines a geometry is incorrect**. **A pair of a Lie group and and appropriate subgroup, (G, G_0), defines not a geometry but a type of geometry**.

C. **The early Erlangen program was also wrong in associating geometry with infinite Lie groups, i.e. pseudo groups As Cartan made clear, this point of view does not make clear —one even could say that it masks— what is geometric in geometry** [24].

To summarize, with Cartan´s work, **certain differential systems** constituted by so called equations of structure and Bianchi identities become, within the appropriate context, the essence of geometry. Cartan expressed this by simply saying that the geometric reality of a space lies in its equations of structure. Pairs (G, G_0) provide only the form of those systems, but are otherwise absent from the generalized geometries. Retrospectively, the original Erlangen program simply constitutes an area of interest for applications of the Cartan-Kähler theory.

Under the key words of projective, Finsler and Kaluza-Klein geometries, we shall later see that the preceding considerations do not exhaust what Cartan did for the Erlangen program, or the connection with Leibniz anticipation. There is also an overlooked point of contact of his work on connections with the work of Clifford on a special type of parallelism, which we proceed to discuss.

The general concept of connection did not exist until 1923 [25]. It did not, therefore, exist in Clifford's time. He, however, conceived of a concept of parallelism different from the standard one. One now understands that such spaces are thus not Euclidean. In 1924, Cartan gave the name of Clifford connection to those of a special type among those endowed with teleparallelism [26], which means existence of path-independence equality of vectors at a distance. He thus honored Clifford for his elaboration of the concept of parallelism by giving the name of Clifford connections to those endowed with (a) zero affine curvature, and (b) a contorsion (which is a bivector-valued differential $1-$form) whose components are antisymmetric with respect to all its three indices rather than just two of them. In [26], the concept of Clifford connection is explained in detail, but not what it means in terms of the contorsion. I leave for differential geometers to find out by themselves.

It should be clear by now that, except for not being sufficiently explicit, the many related contributions by Cartan saved the program of the Leibniz anticipation. Kähler would eventually fill a gap in Cartan's work by introducing the concept of interior derivative [2], [4]

4 Misuse of Clifford algebra and misappropriation of the term Geometric Calculus

The algebraic contribution of Clifford had great impact, but this was not so much because of great effort or imagination, but because the time for this idea had arrived. Several other authors (R. Lipschitz, K. Th. Vahlen and R. Becz) came with the same idea at different times before the turn of the century (See section 36 of [14]). Clifford died very young. Had he lived to an old age, his contribution might have been absolutely extraordinary.

Many practitioners of Clifford algebra have appropriated the term geometric calculus for their forays in formulating a calculus that ignores or underrates the contributions with which Cartan amazed the mathematical world. Their superficial knowledge of Cartan's work makes their efforts unnecessarily cumbersome, when not incorrect. Were it not for the confusion that their misappropriation produces, we would simply ignore them.

At the root of this situation there is the fact that Clifford algebra has become a religion for some, even a cult. Its liturgy is the rejection of mathematical computations or arguments that do not explicitly use Clifford products, or at least viewing them as inferior and thus to be avoided. Clifford algebra is certainly invaluable but sometimes misused. The exterior algebra and calculus are better suited for certain tasks.

Consider for instance a paper by Ziegler and notable mathematical physicist Hestenes [27]. They start the fourth section of their paper "Projective Geometry with Clifford Algebra" with the statement "Now we are prepared for the main business of this article, to show how the theorems of projective geometry can be formulated and proved with geometric algebra". In previous sections and for the stated purpose, they had produced a zoo of concepts with heavy use of this algebra, to which they referred as geometric. Their unnecessarily cumbersome proofs of the most representative theorems of projective geometry contrast with those by González, which require just a little bit of exterior algebra [28]. In addition to proofs of many other projective theorems, he gave two algebraic proofs of Pappus theorem. The one proof that he proposed as an exercise and that he provided at the back of his book actually is the more fundamental because it uses only concepts of projective geometry that are not affine, much less Euclidean. A more readily accessible source is [29].

One may wish to insist on using only Clifford products. One only needs to express any other product within the algebra and express it as a combination of Clifford products. But this would be artificial like artificial are the proofs of those theorems by Hestenes and Ziegler. The use of exterior products where nothing more sophisticated is needed speaks of the non-Euclidean nature of the theorems under consideration. Their paper is thus cultic because it seeks to promote Clifford algebra for Clifford algebras's sake, even though it does not fit the nature of the problem under study. It does not help the cause of Clifford algebra.

We are not against Clifford algebra, much less against its advocation and related terrific work by Prof. Hestenes, for which the scientific community owes him great gratitude. I personally consider his algebraic work to be part of the Leibniz anticipation! It is his misuse of tangent Clifford algebra in parts of calculus and geometry that the present author is against. The identification of algebra and geometry is pure nonsense. As we already said: a vector space has a special point, the zero, which a plane does not. The tangent Clifford algebra is very important but, in calculus and geometry, the Clifford algebra of differential forms is of the very essence and thus more important.

When one creates a lot, the chances are high that, in the process, one will have committed a large number of errors. For better or for worse, I personally make claim to both. It is for others to detect and warn about the errors. One can then retract and be able to present a clean sheet of accomplishments. The search for adulation by insisting too much on what one has done without recognition of errors is a disservice to one's otherwise well deserved good reputation.

For another example of the cult, consider this. Practitioners and acolytes of the geometric calculus movement profess a conceptual synergy with the Erlangen Program. But at no point of (say, for example) the representative lecture "A view of F. Klein's Erlangen Program through GA" [30] in a recent Clifford meeting is the modern concept of geometry mentioned. Instead, we find a potpourri of ideas which became passé more than a century ago. No significant issue was even mentioned, let alone resolved.

I have in mind issues of the structural importance of, for instance, what is the group on the fibers of the projective group (beyond the trivial answer that they are the projective transformations that leave a point unchanged), what is the matrix representation of that subgroup, which simple set of non-affine projective transformations complement the affine transformations to generate the full projective group. And what about the fact that, as Cartan pointed out, the projective group actually is not a group but different related groups [31] (See pages 74, 75, 104). To make matters brief let us quote from page 104: "Let us consider the group of displacements of the space. There are essentially different equations depending on whether the coordinates used are point or tangent coordinates....". It is in the same paragraph that he concluded: "The study of the group G is thus tied to the study of a considerable number of other groups" [31].

We proceed to discuss an issue in the foundations of projective geometry because it is of a nature that Clifford algebra by itself cannot even address. The issue already arises with matrix representation of Euclidean and affine transformations, but where they can be dispensed more easily than in projective geometry because they do not need to be viewed as specializations of homographies (i.e. $(n+1)\times(n+1)$ matrices up to a multiplicative factor). The matrix form of affine (i.e. "linear inhomogeneous") transformations is given by:

$$\begin{Bmatrix} \mathbf{P}_1 \\ \boldsymbol{e}_1 \\ \vdots \\ \boldsymbol{e}_n \end{Bmatrix} = \left[\begin{array}{c|c} 1 & A^1 \quad \dots \quad A^n \\ \hline 0 & \boldsymbol{a}_i^{\ j} \\ & \end{array}\right] \begin{Bmatrix} \mathbf{Q} \\ \boldsymbol{a}_1 \\ \vdots \\ \boldsymbol{a}_n \end{Bmatrix}$$

The inverse of the transformation will be given by the parameters $-A_i$ for the translation, and by the inverse matrix of the $\boldsymbol{a}_i^{\ j}$ for the linear part of the transformation. But these parameters are not what one finds when computing the inverse matrix. Representing affine and, in particular, Euclidean transformations by matrices in order to connect with the concept of projective transformations defined as homographies is a step in the wrong direction for understanding geometry.

We summarize this critique with the statement that there are occasions, like in a summer school, where one should go beyond the statement of platitudes that are one century old, certainly if they are known to be wrong, as if Klein had said all that had to be said about the foundations of geometry and its relation to algebra.

As an inkling about possible solutions of the issues that we have raised in the last paragraphs, let us mention that more faithful representations of projective transformations could lie not in thinking in terms of group theory, but in terms of direct products of groups representing elementary displacements, the translation and linear groups in the affine case. Of course, one can remove the need by a multiplicative factor by requiring that $a_1^{\ 1}$ be the unity. Thus there would be just one matrix for each projective transformation, in such a way that the matrix representation for an affine transformation is as given above. But this takes us even further away from the Erlangen program since the product of arbitrary matrices with $a_1^{\ 1}=1$ does not yield matrices with the same property and do not, therefore, form a group. This would be a problem additional to the one just mentioned about the parameters of the inverse transformations.

Another important objection to the geometric calculus program is that it is based on just one algebra, the tangent Clifford algebra, which certainly is indispensable. But to try to include in it differential forms is totally incorrect. We shall explain in the next section. For the moment and without details, let us intimate why the geometric calculus of Clifford practitioners is inappropriate ab initio even for computations with curvilinear coordinates in Euclidean space, and for the treatment of the interior derivative and the divergence (Incidentally, I am told that there is not agreement among "experts" on the geometric calculus about the definition of the interior derivative beyond the simplest cases).

A basic fact that some of those "experts" appear not to have understood is the following. We all recognize that $dx^\mu \frac{\partial}{\partial x^\mu}$ is an invariant operator and transforms like a scalar, i.e. it does not change form under change of a different coordinate system, where it becomes $dx'^\mu \frac{\partial}{\partial x'^\mu}$. But its action on the components on an element of a tangent algebra (which also is tensorial) goes beyond the partial derivatives. Without the appropriate additional terms, the result that one gets is not tensorial. The same comment applies to the action on a differential form. This manifests itself in the cumbersome form that the divergence and the Laplacian take in the vector and tensor calculi when arbitrary coordinate systems or bases of vector fields are used. In the geometric calculus, this complexity constitutes a main source of errors.

After half a century of life and work by a significant number of practitioners, nothing of mathematical or physical significance has been produced with the geometric calculus (This criticism does not apply to Clifford algebra itself). Certainly they will make claims to the contrary, but such claims only speak to the contrary of what is intended. In quantum mechanics, the geometric calculus only manages to reproduce what is known, often in a worse manner, like changing spinors into something else. In any case, a mathematical formalism that does not open the door for new physics should be retired.

5 The Kähler calculus in context

Kähler's calculus applies to tensor-valued differential forms. He emphasized the scalar-valued ones, for which he advanced very interesting applications. His choice of tensor valuedness was unfortunate. It came at the price of excluding the tangent Clifford algebra, and yielding no applications.

The KC uses many standard formulas from the work of É. Cartan. But Kähler failed to make full use of Cartan's legacy on matters of generalized geometry. He used the canonical connection of a metric (i.e. Levi-Civita's), which is not general enough since the metric does not contain all the geometric reality of a manifold. But, also in this regard, his work can easily be adapted to other connections. Hence, grossly speaking, we may say that a readily implementable modification of Kähler's work in mathematics contains the greatest realization to this day of the Leibniz-Grassmann anticipation.

By itself, the KC of scalar-valued differential forms already supersedes the vector, complex variable and Dirac calculi. This is not surprising. A vector calculus does not even exist for arbitrary dimension. It contains concepts that pertain to differential forms, but which were wrongly attributed to vector fields. As for the calculus of complex variable, it is just a disguised extension of the calculus of real variables [32], [33].

Clifford algebra is present in the Dirac calculus, but in the domain of vector fields, not differential forms, and only superficially. The geometric calculus uses tangent algebra, which the Dirac calculus uses only weakly. But both of these calculi as well as the vector calculus are contra natura; the exterior differentiation of a vector field yields a vector-valued differential 1-form, in the same way as the exterior differentiation of a function yields a scalar-valued differential 1-form. So, two algebras are involved in the differentiation of elements of a tangent Clifford algebra. The Kähler calculus is the way to go, but with post-scalar valuedness.

It is quantum mechanics where the difference between using tangent Clifford algebra or Kähler algebra makes the greatest difference (See [34] and chapter 4 in [35]). A Kähler-Dirac equation plays in the KC the role of the Dirac equation in the Dirac calculus, but it is a particular case of a more general "Kähler equation" $\partial u = au$ [36], namely when a is the electromagnetic coupling and u is a column matrix.

The Kähler equation has intrinsic interest, independently of physics. It "mirrors" in the Kähler calculus the Leibnizian equation $y' = fy$. Incidentally, in reference [7], Kähler proposed his equation with the differential form a being a constant ρ, the formal similarity between $\partial u = \rho u$ and $y' = \rho y$ then becoming even more striking. So, the equation $\partial u = au$ is a very important one in mathematics independently of any role it may play in physics. All the above speaks of the fact that KC should carry the banner of Leibniz anticipation and the Leibnizian characteristic.

In KC, concepts like spinors, energy, charge, angular momentum, chirality, etc. burst into existence through specific spinors, the ones with which Kähler himself built electrons and positrons. Certainly there will be differences between different equations with the same symmetries, thus with common idempotents and thus with common ideals. They will show up in the currents built upon the different equations. But important issues of quantum mechanical flavor emerge before one even considers specific couplings.

Here is a significant difference between the Kähler and Kähler-Dirac equations, on the one hand, and the Dirac equation, on the other hand. In the first ones, the wave function belong in general to a full algebra and not specifically to any of its ideals. The spinors emerge from those wave functions through the structural or canonical solving of differential equations, of which Cartan and Kähler were the greatest masters in the twentieth century. This has important implications for even the foundations of quantum mechanics, as the already showed and the present author has emphasized.

One such important implication is the continuity equation that follows from the Kähler–Dirac equation and first Green identity. It is not a standard continuity equation. Its left hand side is the sum of the left hand sides of two ordinary continuity equations, the two densities being not only different but emerging at the same time with opposite signs. Because of the use of the electromagnetic coupling in obtaining this result, one has to interpret these densities as pertaining to the two signs of charge. The continuity equation in the paradigm corresponds to when the two terms for one of the signs of charge are zero in Kähler's continuity equation. And, when dealing with a system of particles of a given sign of the charge, the interpretation of the wave differential form as a probability amplitude ensues after normalization. But this is for special cases, which speaks of the fact that the Copenhagen interpretation is not a foundational tenet of quantum physics, but a derived or emergent concept.

6 The future of Leibniz-Grassmann anticipation

Given the just documented achievements of the KC [2], [3], [4], [34], how does one explain that it is only rarely used? A major reason is that, written in German, the work has not been translated into English. But it seems that even German speaking scholars find it difficult. This in turn may be due to the fact that his papers are not written in the prevalent modern style, which many would find more abstract and very different from Kähler's. Also, many formulas are introduced ad hoc, without explanation. Thus, those who do not know Clifford algebra and differential geometry à la Cartan (i.e. from a frame bundle perspective) are handicapped ab initio for understanding his papers on this subject. That includes most physicists and mathematicians.

This is a regrettable situation since this calculus has produced great unexpected results in relativistic quantum mechanics [3], [4], [34] (see also the above cited papers by this author from the arXiv), with implications for its very foundations. It has also yielded great results in mathematical analysis [37], [38], which have been noticed even less than the results on quantum mechanics. And it has shown promise for theoretical physics and mathematics. The KC calculus thus goes a long way in realizing the Leibnizian characteristic. We shall now show that Leibniz's anticipation still has a long course to run.

The study of Kähler's calculus of scalar-valued differential forms should be enough for being swayed by its achievements. But one should go further and consider valuedness other than scalar. Kähler actually provided mathematical formulas for calculus with tensor-valued differential forms, which is the reason why components of magnitudes in this more general calculus may have up to threes series of indices. This unusual feature is due to the fact that his differential forms are not antisymmetric multilinear functions of vector fields. They rather are functions of $r-$surfaces, their evaluation consisting on the integration on those surfaces. He did not provide a single application for them. This is not surprising. To start with, differential geometry is neither about scalar-valuedness nor tensor-valuedness. See [39]. We return to this further down in the paper.

For untapped applications of the KC, let us start with a simple example. We have shown that the calculus of complex variable actually is a succedaneum for the calculus of strict harmonic differential forms in $2D$ Euclidean space [32], [33]. There, the imaginary unit is $dxdy$. Hence the extension of the KC version of the *complex calculus* to the KC version of a *multivariable complex calculus* ($i_1 = dxdy$, $i_2 = dydz$, $i_3 = dzdx$) is all too natural [40] Since Kähler obtained the general form of strict harmonic differential forms in $3D$ [4], he automatically had an extension of the standard calculus of complex variable.

For even more sophistication consider $4D$ Euclidean space. Let r be the fourth variable. There are then the additional imaginary units $drdx$, $drdy$ and $drdz$. The interplay of $dxdy$ with $dzdr$ is going to be different from the interplay of, say, $dxdy$ with $dxdz$ since $(dxdy)(dxdz)$ returns $-dydz$, whose square is -1. On the other hand, $(dxdy)(dzdr)$ has square $+1$. All this intimates that the comparison of the standard complex multivariable calculus with the calculus of strict harmonic differential forms should be a matter of great mathematical interest.

Also of interest is that the KC has increased the contents of Hodge's theorem beyond what it is in cohomology theory. It gives the terms of the decomposition of differential forms as a sum of integrals, like in Helmholtz theorem, which we have extended to differential forms of any grade [37].

But it is at the interface of mathematics and physics that the future of Grassmann-Leibniz anticipation looks brighter. We have said interface because, as discussed in the previous section, there are topics difficult to catalog as either purely mathematical or purely physical. Indeed let us return to the equation $\partial u = au$. Choose a as $\alpha + \beta$, where α and β are respectively differential $0-$form and $1-$form. Because of its relative formal simplicity, the solving of such an equation would be one of the first one to be considered in developing the KC, regardless of physical motivation. But, if we then identify α with the mass of a particle and β with eA, where e is its charge and A the electromagnetic potential differential $1-$form, the mathematical problem becomes a physical one. Thus Kähler introduces his revolutionary version of quantum mechanics as an afterthought of the basic equation $\partial u = au$, where u need not even be a spinor. This opens new doors which do not exist when one forces u to be a spinor, as in the Dirac calculus.

Next consider symmetries. In the KC, the issue of symmetry in equations versus symmetry in their solutions is more complex than in other calculi. This is so because idempotents representative of different symmetries may not commute, which puts limits on products of idempotents to represent particles. In general, one cannot accommodate into a set of spinors as many symmetries as may be simultaneously present in both the input a and the metric. There is room in a differential form to accommodate but a very few symmetries at the same time.

There is also the additional issue of whether the idempotents that define ideals are primitive or not, which is key to the concept and properties of quarks. The papers on the subject of algebraic quarks that this author has posted in arXiv speak of the present state of the theory. High energy physics still is in the future of Leibniz anticipation, but has already appeared in the horizon.

There are other ways for expanding the KC. Consider the rotational symmetry idempotents, $(1/2)$ $(1 \pm idxdy)$. In general differentiable manifolds, there is not a so called constant idempotent to replace $dxdy$. Kähler's formulas for electrons and positrons would not be totally appropriate. The role of (1/2) $(1 \pm idxdy)$ would be played by (1/2) $(1 \pm i\omega^1\omega^2)$, where the ω^i are soldering forms, specifically those that orthonormalize the metric. Let an electron move from a region of spacetime which is practically flat into a non-flat one. Assuming that the electron is not a point particle, its structure will readjust through an exchange with its environment in the form of electromagnetic radiation. This idea remains valid in a particle accelerator. The "environment" of the electron is not only given by the metric, but also by the torsion (read the electromagnetic field in the accelerator). This is a subtle issue that we have not developed yet. Suffice to say that a charged particle being accelerated by electromagnetic means is feeling an external torsion and is not, therefore, in flat spacetime. Electromagnetic radiation then appears to be correlated with whether spacetime is flat or not.

We proceed to discuss the "metric tensor". It is a symmetric second rank tensor built upon the module of differential $1-$forms, or simply symmetric quadratic differential form. Thus ds^2 is simply shorthand for $g_{ij}dx^i \otimes dx^j$. And tensors do not have square roots.

Only after one has pulled this to a specific curve one obtains a exterior differential $1-$form for exclusively that curve. The concept of distance as a differential $1-$form actually belongs to the base space of the Finsler bundle, where it is simply (ω^0, mod ω^i), i.e. ω^0 restricted to natural liftings of, say, spacetime. For an in depth understanding of invaluable features of geometry in Finsler bundles one would have to start with its foundations [41], which is not an easy paper. Among the issues that require special attention because of their relevance for the foundations of mathematics, there is the issue of what is the elementary Finsler geometry that the theory of Finsler connections generalizes. The amount of literature on Finsler geometry is substantial, but its practitioners fail to even mention let alone discuss (Finslerian) bundles, which is the arena for connecting elementary and generalized geometry. Some authors do, but their work is not useful for physics because they fail to realize that the frames in Finsler geometry must be refibrated over the space of elements of the frames of the usual bundles. This is achieved through reduction of vectors tangent to the base space of the Finslerian fibration. One thus achieves isomorphism between the original bundle (say of spacetime) and the bundle of frames resulting from such reductions. Other important point about the Finsler bundle is that, in the last instance, trajectories are successions of the origins of a succession of frames. And that the $4-$velocity is, again in this bundle, the unit time vector $\mathbf{e}_0$ itself.

Whether Riemannian or Finslerian, differential geometers have focussed on metrics, not on connections. But, as Cartan pointed out, the metric does not contain all the geometric reality of a space [42]. So, it is not surprising that the canonical connection of a metric is not necessarily the canonical connection of a manifold. Think of the $2-$torus. Its autoparallels under the canonical connection of its metric, i.e. under the Levi-Civita connection, lack a symmetry that the autoparallels under the connection where parallels of the torus are lines of constant direction has. The focus on connections automatically calls for a KC calculus of post-scalar valued differential forms where the connection is not the Levi-Civita connection. The specific alternative of teleparallelism (i.e. equality of vectors at a distance or, grossly speaking, zero affine curvature, is of special interest).

The KC does not fit well with the Finsler bundle because of the mismatch of the dimensions of the reduced tangent vectors and the dimension of the base manifold of the bundle. The additional coordinates of this base manifold are velocity coordinates. One can formulate a canonical Kaluza-Klein (KK) space where we need only an extra dimension, namely distance or propertime, depending on signature. On curves, the components of the vector dual to the fifth dimension is propertime, and its projections upon spacetime are the usual components of the four-velocity [43]. These depend on the components of the 3-velocity. Hence the fifth dimension plays the equivalent role to having the components of the 3-velocity as additional coordinates. An obvious question then is: what is then the metric of this $5D$ space? Grossly speaking, it is a reformulation of the standard $4D$ metric, as a sort of null metric in $5D$, now through the appropriate interpretation of "dot products" of differential $1-$forms in the KK space. There is then no need for reduced tangent vectors. The $5D$ space still has nevertheless its own idiosyncrasy.

One more item in further developing the Leibniz/Grassmann anticipation resides in the geometrization of the imaginary unit. There are many geometric elements whose square are -1. Through bad mathematics, one uses i where one should be using, say, $\mathbf{i} \wedge \mathbf{j}$ or $\mathbf{j} \wedge \mathbf{k}$ or $\mathbf{k} \wedge \mathbf{i}$ in dealing with the rotation of spinors around the z, y and x axes. This approaching of geometrization of physics where the imaginary unit is used is consistent with the Einstein anticipation to which we shall refer as his thesis of "logical homogeneity of geometry and theoretical physics", as intimated by the following quotation [44]:

> "... If, however, one regards Euclidean geometry as the science of the possible mutual relations of practically rigid bodies

> in space, that is to say, treats it as a physical science, without abstracting from its original empirical content, the logical homogeneity of geometry and theoretical physics becomes complete."

For a final item in further developing this anticipation, consider next the projective Klein geometry. It seems that, as we pointed out, not even a pair of a group and a subgroup defines a geometry; contrary to what is usually accepted, it makes a difference what is the set of elements on which the group acts. So the projective group in particular is different depending on what the frames are. Thus much work remains to be done on the Erlangen component of Leibniz/Grassmann anticipation.

We have pointed out several lines of research related to this anticipation and/or to Kähler mathematics. This speaks of a bright anticipated future for Cartan-Kähler mathematics, which represents Leibniz/Grassmann anticipation at its cutting edge.

7 Concomitant issues of Leibniz-Grassmann anticipation

The carrying out of an anticipation seldom is a clear cut process. While being interviewed, the notable solid state physicist P.M. Etxenique made the observation that [45]:

"... a researcher is at the border of the known and unknown and that is very confusing".

Further down, he stated:

"Almost always, researchers move at the border, where things are not clear".

The field of research of Prof. Etxenique is theoretical condensed matter physics. Quotations made here of his work should be adapted to a background of mathematics and fundamental theoretical physics (or foundations of physics if you prefer) and it is in the adapted sense that our use of the term theoretical physics should be understood. We should view the second citation as being closer than the first to what we want to imply. At the edge of mathematical research the border is not between known and unknown, since the latter is not defined. It is rather between free creations of the human mind, until one of them becomes more than that.

Why are these citations relevant here? If confusion and lack of clarity are companions of a scientific vision, an anticipation which involves much more than the forecast of something immediate will surely be highly imprecise. It is not guaranteed that each step of an anticipatory process will be a successful one, even if it provisionally looks so. Consequently, some degree of success in the development of an anticipation can lead its authors to think at some point that they are at what had been an imprecisely anticipated outcome. That happened even to Grassmann, and is now happening to many practitioners of the geometric calculus. A return to the right path in the development of an anticipation —right path of which we can never be totally sure— may require a step back in order to reach some missed, unidentified bifurcation. But what is the criterion or criteria for deciding that one had not taken the right path at a bifurcation? There might be different answers for different cases and circumstances. And even if alternative theories for comparison were identified, neither of the options might be the right one. But there could be all types of signs to the effect that something may not be quite right and impede further progress.

The birth of the elements after the big bang brings together cosmology and particle physics. Should cosmologists and particle physicists not use the same language? Is there not a missed opportunity for greater progress in the fact that they are not doing so?

There are many examples of significant missed opportunities in the foundations of physics. At times, mathematics had not caught up with theoretical physics and one had to use what was available. The missed opportunity is in that, when the right mathematics eventually becomes available, physicists take very long time to replace the retrospectively inadequate mathematics if it becomes deeply entrenched in the paradigm.

The development of a disruptive anticipation —and Kähler's quantum mechanics will certainly be seen as one, given how far away the paradigm may have come away from the right path— may be perceived as a threat. For a very significant example of resistance by paradigms to anticipated change, even when they are not threatening, is the very large amount of time that it took for Grassmann's ideas to be accepted. Even nowadays, there is much resistance by the academic establishment to the teaching of Clifford algebra, in spite of its being *the algebra* of Euclidean spaces (If only they knew!). Grassmann's work certainly was difficult to understand, largely because it was so far ahead of its time. But the time for Clifford algebra arrived almost a century ago since it is present, though in crude form, in the almost century old Dirac's theory.

An unfortunate anticipatory development may develop into delusion in extreme cases. In a different context, Etxenique has made an important point about the relation between genius and stupidity (read relation between right and delusional anticipatory processes):

> "But one has to be careful because, as Faraday said, a genius is someone who believes that what is true for him is true for all, and he therefore fights for it. But, someone who is stupid does exactly the same. Therefore, there is only a fine line between the sublime and the ridiculous."[45]

It is then important to be aware of anticipators limitations. Leibniz was not delusional since, as Grassmann explained, he knew his limitations.

If a good foresight is followed by a bad anticipatory process, the questions of when, where or to whom does anticipation happen become relevant. Anticipators like Leibniz, Grassmann and, in physics, Einstein can be said to have had a sense of architecture. Their ideas look solid and beautiful, once one has internalized them. In the case of Leibniz and Grassmann, their notational developments make clear that they had a great sense of architecture. The importance of appropriate notation is appreciated in modern mathematics. In Grassmann's case, any structure-related notational limitation was amply compensated by his conceptual revolution.

The same sense of architecture we find in Clifford. But the anticipatory element was not present in his work. It was not needed. The opportunity just was there for somebody to take the next step without extraordinary effort.

É. Cartan was a genius among geniuses, which can only be explained if he was endowed with a formidable sense of anticipation based on an as formidable sense of architecture and on unparalleled computational ability In the introduction to his book [46], Gardner provides notable mathematicians' testimonials about such an ability.

Over several decades, he authored several related major achievements, one after another in quick succession. If the works of some mathematicians may be seen as monuments in a landscape of monuments, in the case of Cartan the surrounding monuments were his own. With an analogy, his work was not like a monument in ancient Rome. It was Rome itself. It is unfortunate that most mathematicians have not seen mathematical Rome.

Consider finally Kähler from the perspective of anticipation. He obviously anticipated that a powerful calculus would result from unifying the conceptual spaces of Clifford and Cartan. A good sign of having delivered the right anticipation is that it also gives rise to unification of other calculi and unification of the mathematical languages of relativistic physics and quantum mechanics (work in progress).

8 Ideology, anticipation and the chaotic nature of history

In his magnificent book *Against Method* [47], the late philosopher Feyerabend gives a quotation from Lenin, and also incorporates into a sentence tiny quotations from Herbert's Butterfield's *The Whig Interpretation of History* [48], which says virtually the same. The sentence by Lenin, after a slight modification by Feyerabend, reads: "'History as a whole, and the history of revolutions in particular, is always richer in content, more varied, more many-sided, more lively and subtle than even' the best historian and the best methodologist can imagine". For the exact Lenin sentence, or the reference given therein. reference, see Feyerabend's book. On the other hand, the sentence by Feyerabend that he builds from mini-quotations from Butterfield is: "History is full of 'accidents and conjunctures and curious juxtapositions of events' and it demonstrates to us the 'complexity of human change and the unpredictable character of the ultimate consequences of any given act or decision of men' ". While ideas that are correlated with political party often are ideological and dogmatic, the foregoing Lenin/Butterfield thought, not being so correlated, qualifies as an idea, not as a piece of ideology.

Half a century ago, Pierre E. Trudeau, then Prime Minister of Canada, had this to say: "To ready made or second hand ideas, I prefer my own". Parties certainly provide and should provide a main forum for ideas affecting society, but we are at a point where those are fossilized ideas, not based on anticipation but on ideology (The results of elections in some of the fifteen top economies in the world speak to that, sometimes in the form of exaggerated disgust with the status quo and producing chaotic results). But, is the Lenin/Butterfield chaotic view of history, if correct, which we believe it is, not at odds with the purported value of anticipation?

In his article in a book by Abdus Salam [49], Heisenberg quotes Bohr as saying "If you have a correct statement, then the opposite of a correct statement is of course an incorrect statement, a strong statement. But when you have a deep truth, then the opposite of a deep truth may again be a deep truth." So, one may still benefit from anticipation in spite of the formidable difficulties to predict the future. Time and again, ideologies produce predictions that often are proved to be wrong, but their failure is ignored. For instance, in the USA, it was predicted, certainly based on ideology, that the "social security systems" (in the ample sense of the word) of the Scandinavian countries would go bankrupt. But the countries that have had to be rescued from virtual bankruptcy are not precisely those that have similar social systems.

Consider also the anticipated economic failure of any communist system that was extrapolated from the evolution experienced by communism in Russia. The emergence of China as an economic power in shorter time than any other country in history disproves it. This is not advocacy for communism but just an observation to the effect that one needs a degree of nuance that is the enemy of ideologies. One should certainly take into account very favorable circumstances in order not to overstate the economic success of communist ideology in modern China. But one should also take circumstances into account to judge the failure of communism in Russia. After all, communist ideology gave rise to the Soviet Union of the Sputnik from the ashes of an agrarian country. See the paper *What Communism Was* by G. Derlugian in [50].

Anticipation is compatible with the chaotic nature of history. Generalized societal circumstances should have allowed, and indeed did allow, to anticipate the eventual success of communist ideology somewhere. But the chaotic nature of history would not have allowed to foresee the details, like where it would take place.

Many had predicted WWII on the basis of the harshness of the reparations imposed on Germany at the conclusion of WWI, harshness that was largely due to the insecurity of France after it had been the dominant power of Europe from the mid 1600s to the early 1800s, when Britain emerged. That dominance extended in continental Europe until half a century later when Napoleon III was defeated by Bismarck. It is a consequence of those times that, even in the 1960's under De Gaulle, France had a political influence far in excess of its merits. Our point about to be made is that one has to go more than three centuries into the past in order to find the seeds of the cataclysmic wars of the 20th century (causing death, hunger, and even hatred of anything German, including scientists, after WWI; add genocide to that during WWII).

Let us go back to 1520, when Charles V of the House of Hapsburg was crowned Emperor of the Holy Roman Empire. His religious zeal while king of Spain in years prior went into a higher gear when in 1621 he started the crushing of the Lutheran reformers. Within a few years, his brother, later Emperor Ferdinard I of the House of Hapsburg, became his delegate for administering the German sector of the Empire. He is credited with giving rise, in the 1550's, to what is formally known as the Counter Reformation, which reflects an increase that then took place in religious intolerance. He died in 1564. Ferdinard II became emperor in 1619 and sent the Counter Reformation to overdrive. The thirty year war (1618-1648) had already started in 1619, but no major war action took place until 1620. He did not start the war but was the one who, under his clock, became the cruellest war in history to that day (at least in Europe).

By the end of the sixteenth century, France had successfully dealt with the dislocations caused by the reformation, though at the price of fifty years of war after war. This was not the case in the German speaking world, which, except for Austria, was adamantly opposed to the domination of Christianity by the church of Rome. So, early in the thirty year war and under the stewardship of Richelieu, France took advantage of the lack of anticipation of the emperor. First it was economic support to the many little German states and Sweden. Eventually it entered the war. Through his actions, France emerged as the dominant power in Europe. That is not the problem, which is that, as Kissinger pointed out, these events were a major contributing factor to the unification of Germany happening two centuries later than might have been the case otherwise [51].

This much could have been anticipated: France and its different allies at different times should not have impeded the emergence of Germany as a nation state. But one cannot blame Richelieu for what he did for France. At that time, he did what others would have wanted to do: dominate in order not to be dominated. But the actions of the House of Hapsburg decisively contributed to this unfortunate state of affairs. The big culprit was its religious fanaticism. Without it, the emergence of Germany as a nation state might have resulted in the 17th century, soon followed by the unity of the non-Germanic part of continental Europe as a counter balance in power. This balance would have been achieved a century earlier than it was (It had not been perfect but it nevertheless helped). It might later have evolved into a union that would have included the German nation. One cannot anticipate that this would have been the case (recall the chaos of history), but is plausible. The French revolution in an exhausted France trying to maintain its predominance might have not occurred. And, certainly, it seems too natural that a unification of Germany would have had a much lower human cost. The relations among European countries in the twentieth century would have been very different if decisions had been made in the early seventieth century with a long term anticipatory perspective. Of course, the anticipation would have been of a nebulous nature.

We are now in a situation where one major cause of societal instabilities —perhaps comparable to the power of the financial world and the emergence of China as far and away the major economic power in the world— has to do again with religious ideology. The lack of anticipation (meaning foresight and preventive actions) and actual disdain of it by the major powers portends that history will continue to be the report of the failure of societies to anticipate.

What is the relation of all this to Leibniz/Grassmann anticipation? The chaotic nature of history applies to the history of Leibnizian anticipation like to all history. The longevity of this mathematical anticipation makes it very interesting because it reinforces by corroboration the view of history from a long term anticipatory perspective.

Like history itself, the development of Leibniz/Grassmann anticipation was affected by chaotic events. Progress in mathematics largely depends on understanding this chaotic nature, specially that which is deeply intertwined with theoretical physics. A few examples follow. General relativity was born almost two years before there was an affine connection in Riemannian geometry.

When, for the first time, the Levi-Civita connection (LCC) was formulated in 1917 [52], it was automatically adopted by general relativity. At the time, there was no alternative connection and only the likes of Cartan, Eddington, Weitzenbock and Weyl had the knowledge to conceive of any such alternative. In hindsight, only Cartan had the background to formulate a general theory of connections, which he did in the early 1920s. If the work by Cartan had preceded the LCC connection, modern general relativity would have been based not on Riemannian geometry but on teleparallelism. This would have contained and exceeded its contents of 1915, when it was founded.

For another example, consider vector algebra. It was formulated before spaces of dimension larger than three entered physics. This was unfortunate because vector products only exist in dimensions 3 and 7. Clifford algebra is the real algebra of Euclidean space of arbitrary dimension. But the level of algebraic knowledge in the nineteenth century was very low. There was a confusing interplay of algebra (exterior, Clifford and vector) with geometry, calculus, physics and engineering. Key names in this regard are Grassmann, Clifford, Hamilton, Gibbs, Heaviside, Ricci, Levi-Civita and, only in the last year of the century, Cartan. Vector algebra emerged as the winner, not because of intrinsic merit, but because anything can happen in a chaotic situation. Exterior algebra, created by Grassmann and developed mainly by Cartan 1899 [21], would be recognized and triumph among the cognoscenti only after 1930. And awareness of Clifford algebra would only make significant strides only with the work and example of Hestenes, starting in the 1960's.

The vector calculus —which is based on vector algebra and to which Dieudonné referred as "horrible Vector analysis" and of which he said that it represents "a complete perversion of Grassmann's best ideas" [18]— took the place that the calculus of exterior differential forms should have taken. This was a consequence of the dominance of vector algebra, which underlies vector analysis or calculus. The aforementioned restriction to dimensions 3 and 7 propagates to the calculus, where the curl is defined in terms of vector products.

The tensor calculus was a later response to the problem of dealing with arbitrary dimension. It unfortunately became and continues to be the calculus for general relativity, but not for differential topology, modern differential geometry and Lie group theory. Little by little (too little by little), the tensor calculus is becoming less important and other mathematics closer to the Leibniz/Grassmann anticipation are taking its place. The chaos in algebra propagated to the vector and tensor calculi, which dominate the mathematics for physicists to this day. This would not be the case if one were more focussed on where is Leibniz/Grassmann's anticipation taking us.

The spacetime of special relativity was considered as pseudo-Euclidean, at a time when physicists knew about the foundations of geometry even less than they know now, which is appallingly little. There are different structures consistent with the Lorentz transformations, thus with the metric of special relativity. One of them is Finslerian. Physics would be far more unified and its foundations better understood if the concept of Finsler bundles had been developed in the nineteenth century. The canonical connections in Finsler geometry are based on the Lorentzian signature. We cannot extend ourselves here on this topic. Suffice to say that Finslerian structures are custom made for special relativity and electrodynamics. In the study of autoparallels of Finsler spaces on Lorentzian metrics, the form of the Lorentz force is inescapable. It appears that nobody has noticed that. This is a very strong view of chaos.

Although Clifford algebra underlies the Dirac calculus, this was very poorly understood (Ironically, like Dirac, Clifford had been faculty at Cambridge university). Theoretical physics would nowadays be much better if the KC had preceded the Dirac calculus. This is certainly correct, but other things that went wrong should have gone right before this observation becomes relevant. The experimental technique of a century ago was such that one was led to believe that quantum physics and microphysics were one and the same. This led to other misconceptions that persist to this day. One of them is the concept of point particles and the preeminence of the particle over the field concepts. Fermions are said to interact through the exchange of other particles called bosons, which are even more mysterious than the fermions themselves. The chaotic emergence of new ideas and experiments is unavoidable. But more awareness of this chaos would prompt theoretical physicists to more often revisit all decisions made when mathematical tools were less sophisticated.

In the first part of this section, we have made the case that history looks like *the report of the failure by societies to anticipate.* In the twentieth century and helped by ominous events (WWII, global warming), governments and even societies at large (though not in the form of individuals acting collectively) are becoming increasingly anticipatory. This is not so in theoretical physics and much of mathematics. This raises the issue of whether the educating of professionals in these field is doing what it is supposed to do.

9 The physics paradigm, anticipators and societal issues

In a previous section, we have intimated that those who benefit from a status quo may feel threaten by disruptive anticipation. Suffice to mention Machiavelli's *Il Principe* to understand what we are talking about. But the threat may not be just Machiavellic, in the negative sense of the term. It has to do with the lack of preparation of the citizenship to understand the effects on society of financialization, globalization and the indiscriminating abuse of technology.

Assume a well established physics paradigm (We are justified in using physics for our discourse in this section because the Kähler calculus touches its foundations). Scientifically ambitious and very capable youth in, say, the United States will try to be educated where the winners of the Nobel Prize in their chosen discipline are. The faculties of those institutions are the ones which, directly or by proxy, determine what is or is not funded in fields like physics. But the reason for that choice is idealistic: "I am trying to become my best by staying close to the best". This influence of the top American institutions is also felt in other countries. A reliable source told me a few decades ago that German scientists in certain areas of physics had to spend time in American institutions in order to have a reasonable chance of getting a professorship in the top physics institutions of their country. This may no longer be the case. However, again in Germany, certain departments in at least one area of theoretical physics became string theory departments. None of this happened by regulation, but a consequence of what is thought to be good and relevant science. Either you conform with the groupthink or you are not funded.

Anticipators in non-mainstream lines of research may not need and care much about the extra income that research funding provides them. But, without it, their positions in an increasing number of institutions is in danger because these are then not getting the overhead that funded projects provide. Anticipators veering too far from the party line are not seen as competitive. This is an aspect of the financialization of foundational theoretical physics and, to a lesser extent, of mathematics.

Let me give a couple of examples from my personal experience. A good differential geometer once told me that he was passionate about doing research in Finslerian differential geometry, which he said he was doing in his spare time. The bulk of his research time, however, was devoted to Riemannian geometry in order to obtain research funding. He might have been far more productive at advancing the field of differential geometry if he had been funded for what was his passion, not for what lesser brains have decided should be funded.

The same philosophy I have heard from other quarters in the following form: "I do research on so and so in order to get tenure and then be able to do research in what I am really interested". The problem with this approach is that, in the process, one has become too embedded in the paradigm to be effective at paradigm changing. In addition, becoming an expert at something too specialized risks of knowing too much about irrelevant things that are useless for carrying out an anticipation. It results in lack of experience with situations where great new ideas may serendipitously emerge.

Although one should stop short of seeing here a cause-effect relationship, it would be foolish not to accept that how the funding of research may have a discipline specific effect on the health of the science. Some authoritative physicists are increasingly concerned about it. The opening statement of the Introduction of his book *Collective Electrodynamics: Quantum Foundations of Electromagnetism* by the eminent microelectronics pioneer Carved Mead (Emeritus Professor at Caltech) reads: “It is my firm belief that the last seven decades of the twentieth century will be characterized in history as the dark ages of theoretical physics” [53]. The book is from the year 2000, but remains valid at the time of this writing, in 2017. There may be hyperbole in the use of the term dark ages, and certainly in when the problem started. But the statement still reflects how clearly Mead’s experience in the laboratory allows him to see flaws in much of theoretical physics.

I now proceed to tie opinions like that by Professor Mead with my own experience, which is made relevant by the various non-main stream areas in physics and mathematics in which I have been involved. It illustrates how great questions posed and tried (but failed) by great minds like Schrödinger’s and Einstein are solved “by paradigm outsiders”. The late Professor Yeaton H. Clifton (to whom I owe the name I made for myself in Finsler geometry two decades ago) was a differential topologist highly praised by the great S.S. Chern, but whose personality and research interests were not consistent with long holding of a professorship position. He eventually became interested in Finslerian geometry. Clifton once confessed to me that he studied all sorts of mathematics “in order to address the big problems of physics”, and he certainly addressed them, but without success. His approach may have been the wrong one to enact anticipation. The following may justify what I just stated.

I had just made a discovery that Cliffton greatly and immediately appreciated, but which was totally disregarded by those in the theoretical physics paradigm. It dealt with the issue of geometrizing a simple but important equation of classical electrodynamics [54]. He told me: “Jose, I would not have obtained in one thousand years the result you obtained. I tried this, and that and that, etc. Nothing worked.” I have replaced “this and that and that, etc." for whatever he actually said, which I did not try to remember. He had gone very deeply into several fields where he had expected his anticipation to lie, but without the sense of architecture that was required.

My knowledge of mathematics was just a tiny fraction of Clifton's. But my physical training (from reading the masters, not from formal courses) allowed me to solve that problem in the only scenario that I knew about [54]. It was more an issue of guessing right where to look for an answer, since, knowing it, somebody with his knowledge would have found in no time what took me more than half a year to get. Another physicist, Harry Ringermacher got an equivalent result at about the same time as I did [55]. He had found an error in a proof by Schrödinger [56], who also was looking in the right place, and corrected it (I learned from Schrödinger's attempt later, from Ringermacher's paper). All three of us worked with post-Riemannian pre-Finslerian differential geometry. Retrospectively, this geometry was not sophisticated enough for the result obtained to be considered rigorous. I knew enough to realize that the treatment should be Finslerian. As I was notifying this to Clifton on the phone, he immediately saw the result as pertaining to a connection in a Finsler bundle. Serendipity (my having met Clifton, who was not a top one and where he was barely surviving) largely contributed to the solving of the problem.

Serendipity does not occur in a vacuum. One must have been exposed to the right experiences, the ones that will lead one to ask the right questions. These experiences are not obtained in institutions where the paradigm is most developed. Incidentally, it was Cartan who, by correcting the flaw in Grassmann's system, developed the theory of connections on bundles, but in a very difficult way to understand it in the case of Finsler bundles. And it was Clifton who, subsequently and prodded by my asking him the right questions, formulated Finsler geometry in very elegant, efficient and clean form [41].

It is just a matter of time for virtually any anticipatory process to start going in the wrong direction. We have a very clear case of this in theoretical physics. Judging from the laboratory experiences of Professor Mead [57], some basic tenets of theoretical physics have become ideology, as they continue to be maintained in spite of having been disproved in the laboratory.

A small, initially innocuous deviation from a natural course of development of an anticipation —often resulting from the lack of the appropriate mathematical knowledge or from insufficiently precise experimental equipment at a crucial point in the history of a discipline— can bring us to a state of knowledge where deep foundational progress seems ever less likely. All sorts of new epicycles are created. This certainly brings us close to the dark ages.

Built upon large amounts of phenomenology and disregarding its future survival value, the growth of theoretical physics has nevertheless been so phenomenal that it takes several decades for a physicist to know enough of the different branches of the discipline (and of related mathematics) to be in a position to anticipate where a breakthrough may lie. Whereas in the first three decades of the 20th century it was possible to become an expert in cutting edge theoretical physics and related mathematics by one's early twenties, this is no longer the case. Paradigmatic changes based on an anticipation may thus become increasingly rare, if not outright impossible, since one has to be knowledgeable enough in different areas for not only create but also rebut all sorts of objections coming from the paradigm, not always made in good faith.

So, what is the remedy for this inherent difficulty with the effecting of necessary change if and when it may be needed? The solution cannot come from academia as a whole, given its inner workings already discussed. And no institution prepares physicists and mathematicians for acquiring a sense of architecture and the instincts of a forensic investigator. A renewal of the institutions does not work under metrics like "publish or perish". The number of publications is often reason for suspicion that an author is simply writing papers, and no longer an indication of true scholarship. As for citations, a large number of them may simply mean that an author is deeply mired in groupthink. How many citations (specially deep ones) is the Kähler calculus receiving? Virtually nil at present. Does it mean that it is not important?

Here is an example of the nefarious influence of the indiscriminate application of the dictum "publish or perish". Scientists will participate in scientific events largely as a function of whether such participation will facilitate their having papers published in journals that scientific administrators in charge of disbursing funding or approving permanence in research institutions will consider relevant (If one fails to produce enough of often teaser papers in a period of a few years in a row, one may find oneself without a job). So, a corrupting dynamics is going on that involves the administration of research institutions, participation in conferences and financial interest of editors. Most researchers who attend conferences understand that.

The self-regulation of a theoretical physics or mathematics paradigm that will allow for deep change might not happen without a cataclysmic event in society. The world is presently further afar from equilibrium than it was just two decades ago. Changes even more relevant than those required by theoretical physics or mathematics are needed for a return to equilibrium. We shall later argue that the educational systems of even some rich countries are having funding the education of citizens for the job market, let alone citizens who can understand and collectively find solutions to cataclysmic events that threatens our culture.

10 Historical anticipation and political crises

The political discourse and/or results in recent elections of past but still powerful including the dominant country shows that there is disconnect between the political class and much of the citizenship, and/or that there is increasing political polarization (USA) and/or that old political parties to which progress owes so much are becoming irrelevant. This portends a certain cataclysmic event of which we speak below and which can be averted only with a type of education that no longer exists.

Several potential cataclysmic events are well known to everyone who reads a good newspaper (global warming, environmental degradations, global nuclear war, overpopulation, depletion of wild fish stocks, pandemics, clashes of civilizations, replacement of the dominant power at a global scale). But there is one that has not received attention in the mass media. It has been anticipated by very few people, the two political scientists that predicted the fall of the Soviet Union, R. Collins and I. Wallerstein, among them. Their cataclysmic event would be the fall of capitalism as we know it, unless something unexpected brings it closer to an equilibrium situation [50]. The culprit is the disruption that is being produced by the financialization of everything, including education and health, without regard for the common well being. There is only a little bit of foresight about the negative future it portends, less foresight of a solution and even less about a course of action following the anticipation.

At the individual level or in small groups, people try to protect themselves, but without a collective action responding to an anticipation. Consider the small to medium investor in the market. There is a shift by investors (certainly not the greedy members in the billionaire club) about replacing investing in exchange traded funds with investing in index funds. If this type of investing became dominant, it would give rise to, say, a third of the population *collectively* owning a very large part of the means of production, rather than each investor owning shares from selected or groups of companies. Another positive development to fight financialization could be a major development of cooperatives, i.e. employee owned companies. Both developments would lessen the power of the financial sector over all aspects of life, the buying of politicians among them. But the financial world has become so powerful that not even major corruption gets punished. During the great crisis of a few years ago, no financial officer of any importance went to jail.

Reactions to the system at the individual level are the decisions by young people to live with parents, or to get married if at all at a later age than traditionally was the case, or of women not to have children. Educated populations, the Swedish one probably being the standard bearer, try to confront these problems head on. Sweden is not the only small country that is doing well. The Netherlands has a very positive trade balance with the rest of the world. But the Dutch solution cannot be the recipe for all countries. A positive trade balance of somebody is a negative balance of somebody else. So, a return to stability, while possible, looks ever more unlikely.

Legislatures are the venues through which changes in society are supposed to usually happen in normal times. But are politicians sufficiently educated, and even intelligent enough, to understand these issues? This complicates enormously a situation which, by its very nature, is very complicated. We have learned from our history of anticipation in mathematics that progress does not follow a straight line.

Globalization represents a march towards an equalization of economic, scientific and technical standards of an increasing number of countries in the world. This in turn implies that the most populous countries, being so by large margins, will emerge as the dominant economic powers, though there will not be and need not be an overwhelming superiority in military power (Nuclear weapons are a great qualitative equalizer). There will be enormous friction and feeling of insecurity among the power(s) that are and those to be, like happened with France and Germany after Bismarck. The issue of emerging powers comes to roost together with the issue of the potential end of capitalism, since the present emerging power is China, where financialization is subordinated to state planning. This is not an advocation of the Chinese system simply because of this subordination. The whole issue is more complicated than that, but more specificity does not belong here.

Recent unexpected political events in numerous advanced countries should be looked upon with a long term anticipatory perspective, rather than as standard actions of return to an equilibrium that has lasted three quarters of a century. Those who are in power (governments and big money) do not think with long term anticipatory perspective. But the situation is still worse because, when a party does, its analysis is not accepted by the electorate of but a few highly educated populations. Acceptance would presuppose a degree of analytic, non-nationalistic education that only a few countries provide. Even some rich countries struggle to make education affordable for the masses. We are not talking here about educating with the goal of generating an enlightened citizenship, but simply educating for the job market, which is much cheaper.

Educational systems, specially in inhomogeneous societies even if rich or relatively rich, will try to show high graduation rates by whatever means. A negative effect of that philosophy, the devaluation of academic standards, is compensated through the introduction of an increasing number of levels of educational graduation and retraining courses. That goes totally against the anticipatory philosophy. At each step, one is told what to think, since the time for how to think was left behind. It should have taken place at an earlier age, when the economic responsibility lies with the previous generation. Technology is making this effect ever worse in several fields of mathematics and in theoretical physics. Financial advisers can use their computer programs to tell you how much you will pay to amortize your house at a given rate of interest in a given amount of time. But can they compute it from first principles? And can they provide long term advice without understanding where the world is going?

Without the replacement of ideologies with ideas, specially those arising from long term anticipation, the world may be doomed. The replacement of effective two party systems with a multiparticle system amounts in the worse of cases with a replacement of two ideologies with several of them, where new ideas have a greater opportunity to flourish and send the political dynamics in new directions.

Let us briefly return to the "Leibniz/Grassmann to Cartan/Kähler anticipation". We have learned from it that, once a paradigm has been established, changing it becomes a virtually impossible task. It is sometimes said that Poincaré was the last universal mathematician, since mathematics has become so large that there is no possibility for another universal mathematician. That makes changing the mathematical paradigm more difficult, if and when it will become really needed. Theoretical physics possibly is at such a stage, but there are only isolated voices denouncing this situation, not a program for change.

11 Concluding remarks

We have tied to the Leibnizian anticipation the mathematical contributions by H. Grassmann, Clifford, F. Klein, É. Cartan and Kähler. Because of its importance, we now briefly indicate how "everything Riemannian" fits this anticipation. We shall distinguish two epochs in Riemannian geometry, namely before and after the LC connection (year 1917).

The first epoch is dominated by the view of this geometry from a perspective of problems of equivalence. The great master on this problems was É. Cartan. The port of entry for this is an already mentioned Cartan paper of 1908 [22]. But that is a tall order. A book by Gardner on the method of equivalence [46] is a good replacement.

The second epoch starts in 1917, when a connection among tangent vector spaces was cooked unearthed from the Christoffel symbols independently but almost simultaneously by a few authors. Levi-Civita was the one to get credit for it [52]. Riemannian geometry became ready for its incorporation, a few years later, into Cartan's theory of connections [25], thus into the Leibniz/Grassmann anticipation.

We could add similar integrations of theory from Frobenius, Lie, Pfaff, etc. into the same anticipation. We cannot think of any other development that unifies so much mathematics, much less one which was anticipated.

A related anticipation concerns physics, its authors being Einstein and Cartan. The first anticipated the unification of classical physics through teleparallelism [8]. But he did not deliver anything that is physically worth considering. Retrospectively, Cartan did, but his motivation was simply to help Einstein and he did not pursue the matter beyond trying to help Einstein, to little avail. This author has concluded that Cartan was in the right path in his development of Einstein's anticipation (A paper by the present author should follow in a not distant future, updating with new results research already published).

Extrapolated from his analysis of the relation between Euclidean geometry and the mutual relations of rigid bodies, Einstein concluded (a little bit naively if we take him too literally) that "the logical homogeneity of geometry and theoretical physics becomes complete [58]. But teleparallelism is not enough. Kähler's work coming from another angle is also needed, specifically for quantum physics. The language of classical physics is the language of differential forms. Kähler has shown that this calculus is even better than the Dirac calculus for relativistic physics. The using of the same mathematical language is a necessary condition for achieving the unification of the interactions and of those two branches of physics.

Leibniz, H. Grassmann, Riemann, Clifford, F. Klein, Levi-Civita, Einstein, É. Cartan, Finsler, Kaluza-Klein and Kähler. Is anybody listening?

References

[1] Grassmann, H. (1995): The Ausdehnungslehre of 1844 and Other Works of Grassmann, English translation by L. C. Kannenberg titled "A New Branch of Mathematics", Open Court, Chicago.

[2] Kähler, E. (1960): Innerer und äusserer Differentialkalkül, Abh. Dtsch. Akad. Wiss. Berlin, Kl. Math., Phy. Tech., **4**, 1-32.

[3] Kähler, E. (1961): Die Dirac-Gleichung, Abh. Dtsch. Akad. Wiss. Berlin, Kl. Math., Phy. Tech **1**, 1-38.

[4] Kähler, E. (1962): Der innere Differentialkalkül, Rendiconti di Matematica **21**, 425-523.

[5] Kähler, E. (1932): Über eine bemerkenswerte Hermitesche Metrik, Hamburg Abh **9**, 173.

[6] Kähler, E. (1934): Einführung in die Theorie der Systeme von Differentialgleichungen, Teubner. Reprinted: Chelsea Publishing Company, New York, 1949.

[7] Kähler, E. (1992): Raum-Zeit Individuum, Rend. Accad. Naz. Sci. XL Mem. Mat. Appl. (5) **16**, 115-177.

[8] Debever, R. Editor (1979): Elie Cartan-Albert Einstein, Letters on Absolute Parallelism: 1929-1932, Princeton University Press, Princeton.

[9] MacLane, S. and Birkhoff, S. (1967): Algebra, MacMillan Company, London.

[10] Nagel, E. and Newman, J.R. (1958): Gödel's Proof, New York University Press, New York.

[11] Choquet-Bruhat, Y., DeWitt-Morette, C.and Dillard-Bleick, M. (1982): Analysis, Manifolds and Physics, Part I: Basics, Elsevier Science Publishers, Amsterdam.

[12] Bott, R. and Tu, L. W. (1982): Differential Forms in Algebraic Topology, Springer-Verlag, New York.

[13] Leibniz, W. G. (1996): La caractéristique géométrique, J. Vrin, Paris.

[14] Cartan, É. J. (1908): Nombres complexes, French edition of Encyclop. Sc. math., **I5**, 329-468. This is a very extended version of an article originally by E. Study.

[15] Clifton, Y. H. (1966): On the Completeness of Cartan Connections, J. of Math. and Mechanics, **16**, 569-576.

[16] Sharpe, R.W. (1996): Differential Geometry, Springer, New York.

[17] Klein, F. (1872): Verglichende Betrachtungen über neuere geometrische Forschungen, Reprinted in Math. Annalen **43**, 63-100 (1893).

[18] Dieudonné, J. (1979): The Tragedy of Grassmann, Linear and Multilinear Algebra, **8**, 1-14.

[19] Clifford, W. K. (1882): Mathematical Papers by William Kingdon Clifford (R. Tucker, Ed.), Macmillan, London. Reprinted by Chelsea (1968), New York.

[20] Lipschitz, R. (1880): C. R. Acad. sc. Paris **91**, 619, 660.

[21] Cartan, É. J. (1899): Sur certains expressions diffèrentielles et le problème de Pfaff, Ann. École Norm, **16**, 239-332.

[22] Cartan, É. J. (1908): Les sous-groupes des groups continus de transformations, Ann. École Norm. **25**, 57-194.

[23] Dieudonné, J. (1970-1990): Biography in Dictionary of Scientific Biography, New York.

[24] Cartan, É. J. (1936): Le rôle de la théorie des groupes de Lie dans l'évolution de la géometrie moderne, C. R. Congrés intern. Oslo, **I**, 92-103.

[25] Cartan, É. J. (1923): Sur les variétés à connexion affine et la théorie de la relativité généralisé, Ann. École Norm. **40**, 325-412.

[26] Cartan, É. J. (1924): Les récentes généralizations de la notion d'espace, Bull. Sc. Math. **48**, 294-320.

[27] Hestenes, D and Ziegler, R. (1991): Projective Geometry with Clifford Algebra, Acta applicandae mathematicae **23**, 25-63.

[28] González Calvet, R. (2007): Treatise of Plane Geometry through Geometric Algebra, Cerdanyola del Vallès.

[29] González Calvet, R. (2016): The Affine and Projective Geometries from Grassmann's Point of View, http://cs.unitbv.ro/~acami/early_proceedings_alterman_conference.pdf

[30] Xambo, S. (2017): A view of F. Klein's Erlangen Program through GA, https://mat-web.upc.edu/people/sebastia.xambo/santalo2016.

[31] Cartan, É. J. (1937): La théorie des groupes finis et continus et la géométrie différentielle traitées par la méthode du repère mobile, Gauthier Villars, Paris. Reprinted (1992) Éditions Jacques Gabay, Sceaux.

[32] Vargas, J. G. (2012): Real Calculus of Complex Variable: Weierstrass Point of View, arXiv 1205.4657.

[33] Vargas, J. G. (2012): Real Calculus of Complex Variable: Cauchy's Point of View, arXiv 1205.4256.

[34] Vargas, J. G. (2008): The Foundations of Quantum Mechanics and the Evolution of the Cartan-Kähler Calculus, Found Phys **38**, 610-647.

[35] Vargas, J. G. (2016): Notes on the Kähler Calculus, http://altermanschool2017.uacg.bg/noteskc.htm.

[36] Vargas, J. G. (2016): Kähler's Quantum Mechanics, http://altermanschool2017.uacg.bg/Kähler_qm.pdf.

[37] Vargas, J. G. (2014): "Helmholtz Theorem for Differential Forms in 3-D Euclidean Spaces: Unification of Decomposition and Integration Perspectives, arXiv 1404.3679.

[38] Vargas, J. G. (2014): "Helmholtz-Hodge Theorems: Unification of Decomposition and Integration Perspectives, arXiv 1405.2375v.

[39] Vargas, J. G. (2014): Differential Geometry for Physicists and Mathematicians, World Scientific, Singapore.

[40] Vargas, J. G. (2012): Real Units Imaginary in Kähler's Quantum Mechanics, arXiv 1207.5718v1.

[41] Vargas, J. G. and Torr, D. G. (1993): Finslerian Structures: The Cartan-Clifton Method of the Moving Frame", J. Math. Phys. **34**, pp. 4898-4913.

[42] Cartan, É. J. (1922): Sur les equations de structure des espaces généralisés et l'expression analytique du tenseur d'Einstein, Comptes Rendus, **174**, 1104.

[43] Vargas, J. G. and Torr, D. G. (1997): The Emergence of a Kaluza-Klein Microgeometry from the Invariants of Optimally Euclidean Lorentzian Spaces, Found. Phys. **27**, 533-558.

[44] Einstein, A. (1933): On the Method of Theoretical Physics (The Herbert Spencer Lecture, delivered at Oxford, June 10). Published in Mein Weltbild (1934), Querido Verlag, Amsterdam. Also collected in Ideas and Opinions (1954), Bonanza Books, New York.

[45] Etxenique, P. M.: www.euskonews.com/0452zbk/elkar_en.html

[46] Gardner, R. (1989): The Method of Equivalence and its Applications, SIAM, Philadelphia.

[47] Feyerabend, P.E. (1993): Against Method, Verso, New York.

[48] Butterfield, H. (1965): The Whig interpretation of History, W. W Norton and Company, New York.

[49] Salam, A. (1990): Unification of Fundamental Forces, Cambridge University Press, Cambridge.

[50] Wallerstein, D., Collins, R., Mann, M., Derluguian, G. and Calhoun, C. (2013): Does Capitalism Have a Future?, Oxford University Press, New York.

[51] Kissinger, H. A. (1994): Diplomacy, Simon and Schuster, New York.

[52] Levi-Civita, T. (1917): Nozione di parallelismo, Rend. Circ. Mat. Di Palermo **42**, 173-205.

[53] Mead, C. A. (2000): Collective Electrodynamics: Quantum Foundations of Electromagnetism, The MIT Press, Cambridge, Massachusetts.

[54] Vargas, J. G. (1991): On the Geometrization of Electrodynamics**,** Found. Physics **21**, 610-647.

[55] Ringermacher, H. I. (1994): An Electrodynamic Connection, Class. Quantum Grav. **11**, 2383-2394.

[56] Schrödinger, E. (1950): Space-Time Structure, Cambridge University Press, Cambridge (Reprinted 1986).

[57] Mead, C. A. (2001): The Spectator Interview, in The American Spectator, September/October, 68-75.

[58] Einstein, A. (1930): Théorie unitaire du chanp physique, Ann. Ins. Henri Poincaré, **1**, 1-24.