RESEARCH TOPICS, AND SOME REMARKS ON TEACHING MATHEMATICS

WOLFGANG BERTRAM

The aim of the following notes is to explain some of my research topics to non-specialists. I shall try to keep the text as non-technical as possible – it is not meant to be a scientific communication, but rather a personal and informal conversation with a reader supposed to be interested in mathematics and in the way mathematicians think. Thus I will allow myself to give some comments on related topics, such as teaching mathematics, and to speak about my personal experience and motivation. In chronological order, my research developed along the following three strands, which I shall explain in inverse chronological order:

(A) non-commutative harmonic analysis (section 3),
(B) the geometry of Jordan-, Lie- and associative structures (section 2),
(C) general differential calculus (section 1).

In the following, I shall give some (hopefully useful) hyperlinks, and I will refer to the list of my papers as given on my homepage: research papers are labelled [n], books [Bn], theses [Tn], and others – proceedings, overviews, lecture notes – labelled [On].

1. Differential calculus

References: [B2], [B3], main papers: [17, 19, 37, 40], others: [27, 30, 35, O5, O6]

1.1. **Introduction.** Quite early during my undergraduate studies, I became interested in the foundations of calculus: Detlef Laugwitz, who was an uncle of mine, sent me a copy of his book *Zahlen und Kontinuum – eine Einführung in die Infinitesimalmathematik (Wissenschaftliche Buchgesellschaft 1986).*[1] At that time, I did not yet understand much of the mathematics, but I retained that the form of "Analysis" is not fixed once and for all: it is a human endeavor which may change shape and color during the time. Indeed, like every other student of physics and maths, I made this experience: our physics teachers used "infinitesimal" quantities as if there were no doubt about their existence, whereas, for our maths teachers, their non-existence seemed so obvious that only supersticious people could believe in their existence. So, who is right? Although I became a mathematician, most often I continued to think like a physicist.

These questions worked on in my mind and suddenly came to light when, in 2002, Karl-Hermann Neeb was visiting professor in Nancy, and we started to work on a joint project involving infinite dimensional symmetric spaces (see Section 2.10

[1] "Numbers and continuum – an introduction to the mathematics of infinitesimals"; the book is an introduction to non-standard analysis containing a wealth of historical remarks and thoughts on pedagogical issues.

below). I had learned from Bourbaki[2] that the "correct" setting for infinite dimensional calculus is the Banach space setting; now Karl-Hermann explained to me that general locally convex vector spaces (complete or not) are equally well suited, so I wondered why to stop there: is there a "natural frontier" for generalizing calculus? I consider the quest for answering this seemingly naive question one of the most challenging intellectual adventures of my life. This work is not yet finished, but from today's perspective I would say that the answer is: *as long as you are interested in differential calculus, there is no frontier: it is, essentially, an algebraic, or formal, theory, which makes sense under most general assumptions; however, if you wish to combine it with integral calculus, then you must add more and more restrictive assumptions (of "analytic", or "topological" nature), according to the strength of results that you expect.*

Before explaining this answer in some more detail, I would like to mention two other persons to whom I owe acknowledgement of decisive hints: first, I have read with great pleasure the book "Analysis 2" by my former teacher in Göttingen, Horst Holdgrün, who gives an elegant and rigourous mathematical presentation involving some ideas to be explained below; and second, to my collegue Yannis Varouchas (whose office was next to mine): when, in 2002, I was thinking about these things, he knocked at my door, "t'as cinq minutes? – j'ai un joli truc de maths à te raconter, peut-être que cela t'interesserait..." – and what he told me was exactly what I needed for understanding the problem I was thinking about! I probably would not have told this here if Yannis had not died, suddenly and unexpectedly for all us, some months later. We have had several stimulating discussions before his death, and Yannis had been very enthousiastic about it. I always felt that the work I continue here is kind of his legacy.

1.2. **Topological differential calculus.** References: [B2], [B3], [17]. – Whereas, both for teachers and students, explaining, respectively understanding, the notion of derivative of a function of *one* variable is standard and comparatively easy, the situation is quite different for *functions of several variables*: the literature on notions such as differential, derivative, total derivative or class C^1, is both vast and often confusing.[3] Thus, paradoxally,

- on the one hand, since its historical origins from Newton and Leibniz, the success of calculus comes from the fact that it is *simple*, and that it has the form of a *calculus*, i.e., that it provides a formal, algebraic, method to find and to compute solutions to certain problems;
- on the other hand, the need for mathematical rigour and greatest generality leads to a *complicated* structure on the *conceptual* level.

I claim that the approach started by the paper [17], and christianed *Topological differential calculus* (*Calcul différential topologique)* in the book [B3], succeeds in marrying simplicity on the level of the calculus with simplicity on the – extremely general – conceptual level. To give you an idea of how simple it is: start with a function $f : U \to W$ (think of U as an open part of $V = \mathbb{R}^n$, and $W = \mathbb{R}^m$), then

[2] At that time I was not aware that Laugwitz also did pioneering work in this realm!
[3] To limit quotes, I stick here to wikipedia; every reader may do her own web search.

(1) write up the *difference quotient*, for t an invertible real number,

$$f^{[1]}(x,v,t) := \frac{1}{t}\big(f(x+tv) - f(x)\big), \tag{1.1}$$

(2) in a second step, simplify (in whatever way you like) the expression of $f^{[1]}(x,v,t)$ such that division by t disappears,

(3) the resulting expression – still denoted by the symbol $f^{[1]}(x,v,t)$ – then makes sense also for $t=0$, and this gives you the *differential of f at x applied to v*, and denoted, according to context and to local habits, by

$$df(x)v := Df(x)v := \partial_v f(x) := f^{[1]}(x,v,0). \tag{1.2}$$

On the *conceptual* side, the counterpart of the decisive step (2) is the following

Theorem 1. *The map $f : U \to W$ is differentiable of class C^1 if, and only if, the difference quotient map $f^{[1]}$ extends to a* continuous *map defined for all triples (x,v,t) for which it makes sense, i.e., for $x \in U$, $v \in V$ and $t \in \mathbb{R}$ such that $x + tv \in U$; the differential of f then is the map defined by $df(x)v := f^{[1]}(x,v,0)$.*

For convenience of the reader, I have written up here the very easy proof (see Section VII of [B3] for an expanded and commented version); it generalizes for arbitrary locally convex topological vector spaces (see [17]). What makes this theorem remarkable is that it is of purely *topological* nature: its condition is in terms of *continuous* maps, as opposed to the usual notion of (Fréchet) differential, which apparently is of *metric* nature, since its condition is in terms of *normed (Banach) vector spaces*. Now, topological spaces are much more general than metric ones. Thus we propose the following purely topological definition of "class C^1":

Definition 1. *A map $f : U \to W$ is of class C^1 if its difference quotient map admits a continuous extension to a map $f^{[1]} : U^{[1]} \to W$ defined on the set*

$$U^{[1]} := \big\{(x,v,t) \mid x \in U, v \in V, t \in \mathbb{K} : x + tv \in U\big\}. \tag{1.3}$$

The differential of f then is defined by (1.2).

One of the purposes of [B3] is to sell this definition to an undergraduate reader. The more advanced reader should appreciate that not only, V and W may be arbitrary (Hausdorff) topological vector spaces, but $\mathbb{K}$ may be any good[4] topological base field or even -ring. For fields like $\mathbb{K} = \mathbb{C}$ or $\mathbb{K} = \mathbb{Q}_p$ (the p-adic numbers), this yields indeed the correct classical notions from complex analysis and p-adic calculus (see [17] and [B3]); for rings that are not fields it leads to surprising and useful new features of our calculus: namely, *infinitesimal quantities* appear naturally (see section 1.5 below).

Topological differential calculus can now informally be defined as calculus, based solely on the topological notion of *continuity* and on the above definition. It combines simplicity of calculus with simplicity of the conceptual foundations. I shall not hide that there is a price to be paid: this approach has a high degree of abstraction and of "algebraization". Let us say some words on this. First, a short digression:

[4] By "good" I mean: the group $\mathbb{K}^\times$ shall be open-dense in $\mathbb{K}$.

1.3. **Digression on linearity of differentials.** Reference: p. 47/48 in [B3]. – It is very important for the whole structure of calculus that the first differential at a point, $df(x) : V \to W$, $v \mapsto df(x)v$, is a *linear map*. Remarkably, in our setting this a *theorem*: it is a consequence of the simple definition given above (see, e.g., [B2], or here for the easy proof). This should be compared with the usual definition of (Fréchet) differentiability, where it is part of the definition, that is, an *assumption*: we do not need this assumption, hence, by Occam's razor, this comparison is a strong argument in favor of topological differential calculus! By the way, beyond the French frontiers, many authors prefer defining the class C^1 by the property that all partial derivatives exist and are continuous: indeed, this can be seen as a purely topological condition; its drawback is that one uses a basis, so this is not conceptual. Also, Lang's generalization of Fréchet differentiability to general topological vector spaces (where linearity still is an *assumption*) may be critisized by the same argument, using Occam's razor.

1.4. **Conceptual differential calculus.** References: main [37], [38], [40], see also [39], [O6], and the "C^0-concepts" from [17] and [B3] p. 129/30. – As said above, I claim that differential (as opposed to integral) calculus is a purely algebraic theory. To prove the claim, we have to cross a last frontier: we have to get rid of topology. At a first glance, this seems to be impossible: topology and continuity are needed to "extract" the value $f^{[1]}(x, v, 0)$ out of eqn. (1.1), given the function f. However, already undergraduate students learn to distinguish "polynomials" and "polynomial mappings". The same kind of distinction can serve to define "smooth laws", which need not be uniquely determined by their underlying set-map. To define this correctly, we have to understand the algebraic nature of calculus: a first (not yet quite stable) approach to do this, called "Conceptual Differential Calculus", has been given in [37, 38], a second, more polished, in [40] . Clearly, writing up a stable version is a matter for another book; hopefully that will not take too long.

1.4.1. *Motivation.* Conceptual calculus arose from working on a couple of, apparently elementary, problems (formulated in [O6]) about *second and higher order differentials*: already on the level of topological differential calculus, it becomes necessary to look a bit closer at *higher order difference quotient maps* $f^{[2]} := (f^{[1]})^{[1]}$, $f^{[3]} := (f^{[2]})^{[1]}, \ldots, f^{[n]}$ (see [B2], [B3]). These are among the most terrifying mathematical beasts that I have ever met: the number of arguments increases exponentially, and already for $f^{[2]}$ the "explicit formula" looks quite wild (see here, or [O6]). However, there must be some pattern behind this: what is it?

1.4.2. *Algebraic shape of calculus.* Let me give (following [40]) a very short, but nevertheless complete, mathematical description of the shape of "conceptual calculus" – the reader who really tries to fill in the missing details will find that all computations are elementary, not requiring any sophisticated mathematical knowledge, but just a psychological prerequisite: you should not be afraid of *(small) groupoids*. Indeed, there is no reason to be afraid of them – they are domestic animals, like groups, rings, small cat's... (see here, or these lecture notes).

Start. Fix a (commutative) base ring $\mathbb{K}$, a $\mathbb{K}$-module V, a non-empty subset $U \subset V$, and define $U^{[1]}$, the "generalized tangent bundle of U", by (1.3).

Step 1. By direct computation, show that the following pair $U^{\{1\}}$ is a groupoid: $U^{\{1\}} := (G^1, G^0) := (U^{[1]}, U \times \mathbb{K})$, with source map $\alpha : G^1 \to G^0$, $\alpha(x,v,t) = (x,t)$, target map $\beta(x,v,t) = (x+tv,t)$, units $1_{(x,t)} = (x,0,t)$, product $(x,v,t)*(x',v',t') = (x', v+v', t)$ and inversion $(x,v,t)^{-1} = (x-tv, -v, t)$.

Step 2. For each $k \in \mathbb{N}$, let $\{k\}$ be a formal copy, called *of k-th generation*, of the symbol $\{1\}$. So, $U^{\{k\}}$ is just a copy of $U^{\{1\}}$, and so on. Define by induction

$$\forall n \in \mathbb{N}, \qquad U^{\mathsf{n}} := U^{\{1,2,\ldots,n\}} := (\ldots(U^{\{1\}})^{\{2\}}\ldots)^{\{n\}}. \tag{1.4}$$

Theorem: *U^{n} is an n-fold groupoid.* Well, to be honest, this is true almost by definition. The main problem is just to get used to the definition of such (higher) gadgets. The explanations given in [38], [40] are designed to be self-contained.

Step 3. Let $f : U \to U'$ be a map. Theorem: *under the assumptions of topological differential calculus, f is C^n iff it extends to a continuous morphism of n-fold groupoids $f^{\mathsf{n}} : U^{\mathsf{n}} \to (U')^{\mathsf{n}}$.* When there is no topology, this property may *define* the "property C^∞": a *smooth law* is given by a base map $f = f^0 : U \to U'$ together with a family $(f^{\mathsf{n}})_{n\in\mathbb{N}}$ of morphisms of n-fold groupoids (having some extra properties that are the formal analogs of certain properties from topological calculus).

Step 4 (optional). All properties and definitions carry over to the level of *manifolds* (defined in full generality in [39]). In fact, all key properties described above are *natural*, that is, essentially chart-independent, and hence valid for manifolds (and even will remain valid for "spaces" that are more general than manifolds).

This achieves our short description of the formal structure of differential calculus: the n-fold groupoids U^{n} and their morphisms f^{n} are the main actors of "conceptual calculus". They significantly generalize the n-fold iterated tangent bundles T^nU and tangent maps $T^nf : T^nU \to T^nU'$, heavily used in [19]. The only precursor of the U^{n} that I was able to find in the mathematical literature is *Alain Connes' tangent groupoid* (Section II.5 in his book "Non-Commutative Geometry"), which, for $\mathbb{K} = \mathbb{R}$, is an analog of our $U^{\{1\}}$, though given by a fairly unnatural construction.

1.4.3. *Various versions of calculus.* Now, we have to "unfold" these abstract definitions and properties, and to study their relationship with "usual" calculus and differential geometry. The papers [37, 38, 40] are just a beginning, and a lot of work remains to be done. The structure of the n-fold groupoids U^{n} can be made completely explicit, but still is very complicated. To make life easier, I introduce a simplified version of these groupoids, called *symmetric cubic*. The symmetric cubic calculus is remarkably close to usual calculus, by keeping certain interesting features of the conceptual approach. However, the general, *full cubic*, definitions as given above, are needed when we wish to keep full advantage of the calculus in case of *positive characteristic*, and to make the link with *methods from algebraic geometry*: namely, the *simplicial differential calculus* from [27] can be imbedded into the full cubic calculus, but not always into the symmetric cubic one. Surprisingly, the most subtle features are encoded in an apparently stupid object: namely, taking $U = 0$, the zero set in the zero space, we get an n-fold groupoid 0^{n}, called the *scaloid*. The scaloid is far from trivial – I like to see it as a kind of "elementary particle", present in the whole theory at a very fundamental level.

1.4.4. *Further comments.* See the last sections of [40] : there are a lot of open problems and further topics. I hope, in the end, that the reader may agree that "differential calculus" *is* a topic of "serious" research, and not only, as many mathematicians are made to believe, a topic for historians and educationists; it is a living part of mathematics, still capable of being developed into new directions, and not the finished and unalterable tool presented in undergraduate lectures. Conceptual Calculus may be considered as an amplification of Charles Ehresmann's program: groupoids and higher categorical structures already appear *before* developing differential geometry, since they lie at the bottom of calculus.

1.5. **Weil functors.** References: [B2=19, 27, 30, 35]. – To finish this chapter, and adressed to readers who are more expert in algebraic approaches to infinitesimal calculus, I would like to discuss a series of papers that now appear to be somewhat intermediate between the topological and the conceptual approach, namely those dealing with the aspect of Weil functors and Weil bundles, named after an important paper by André Weil from 1953, "Théorie des points proches sur les variétés différentiables". My interest in this approach started with Theorem 6.2 from [19], and the reader who has followed me up to this point can safely be referred to the ample explanations given in the introduction of [19]. To make a long story short, in loc. cit. it is shown that *if M is a manifold over a good topological base ring $\mathbb{K}$, then its tangent bundle TM is a manifold over the good topological ring $T\mathbb{K}$.* The good topological ring $T\mathbb{K}$, in turn, is naturally identified with the *ring of dual numbers over* $\mathbb{K}$, $T\mathbb{K} \cong \mathbb{K} \oplus \varepsilon\mathbb{K}$ with ε an element of $T\mathbb{K}$ satisfying $\varepsilon^2 = 0$. This opens a way of implementing "infinitesimal quantities" in topological differential calculus in a completely natural and elementary way – and in this way should be compared to synthetic differential geometry. In [19], I have used this observation to give an exposition of some classical topics of differential geometry (connections, Lie groups, symmetric spaces) in a most conceptual and general way. Retrospectively, I would say that the use, in [19], of higher order tangent rings, defined inductively by $T^k\mathbb{K} := T(T^{k-1}\mathbb{K})$, has turned out to be a very efficient tool to develop "conceptual differential geometry" without having to tame the dreadful higher order difference quotient maps (see above) – the rings $T^k\mathbb{K}$, and their cousins called "jet rings", contain all the purely infinitesimal information.

But, for a deeper understanding, working with $T^k\mathbb{K}$ is not enough; and already on the infinitesimal level we have a slight loss of information, namely in the case of *positive characteristic.* As an important step for such a deeper understanding I consider the paper [27], where I show that the Weil functors corresponding to the Weil algebras $\mathbb{A} = \mathbb{K}[X]/(X^n)$ can be "imbedded" into topological differential calculus. Going even further, in his thesis, giving rise to our paper [30], Arnaud Souvay then proved an analog statement for *any* Weil algebra (thus generalizing the Covariant approach to Weil functors advocated by Ivan Kolar). It is even possible to extend these results to an abstract approach of what I call *Weil spacs* (paper [35]). This is a category of "spaces" satisfying the three requirements set out in the introduction of Moerdijk's and Ryes' book *Models for Smooth Infinitesimal Analysis*: (1) it is cartesian closed, (2) it contains singular spaces, and (3) it allows a language taking account of infinitesimal quantities. However, to ensure fullness of imbedding of the usual category of manifolds into this big category, we will need to refine it by using notions of conceptual calculus that are not yet present in [35].

2. Geometry and algebra

References: book [B1], papers [3, 4, 6, 7, 11, 13, 14, 15, 16, 18, 20, 21, 22, 23, 24, 25, 26, 28, 29, 31, 32, 33, 36] and [O1, O2, O3, O4, O7, O8, O9], book review [1].

2.1. **Jordan algebras: from physics to maths, from Göttingen via Paris to Clausthal.** References: [B1], [3,4,6,7,11]. – Following a Göttingen tradition, I started studying both mathematics and physics as "Hauptfach" (main subject). After some time, I realized that I would not understand physics by studying physics, so I did my Diplom in Mathematics, but I continued to feel attracted by mathematical structures related to foundational issues of physics. In this realm, one of the highlights certainly is Lie theory, a beautiful theory relating a class of geometric objects (Lie groups) with a class of purely algeraic objects (Lie algebras). Great minds such as Hermann Weyl and Eugene Wigner stand for the intimate relation of this theory with 20-th century physics. This was certainly the reason why I chose to write my Diplom thesis in this domain.

Already at that time, in a lecture on quantum mechanics by Gerhardt Hegerfeldt, I was somewhat puzzled when he mentioned the topic of Jordan algebras: around 1930, Pascual Jordan suggested to consider an algebra structure on the space of observables in quantum mechanics (for simplicity, think of it as a space of symmetric or Hermitian matrices) – this space is not stable under the usual associative matrix product ab, but it is an algebra with respect to the symmetrized "Jordan" product

$$a \bullet b := \frac{1}{2}(ab + ba). \tag{2.1}$$

Now, this looks very similar to defining a Lie algebra by the commutator bracket,

$$[x, y] := xy - yx. \tag{2.2}$$

So, I wondered if there were some analog of Lie theory, some kind of relation between hypothetical "Jordan groups" and Jordan algebras? Only several years later, in Paris, when I started working on my phd thesis supervised by Jacques Faraut, I discovered that such a theory was indeed developing: Faraut and Koranyi were on the way of finishing their book Analysis on Symmetric Cones, and by reading several of its drafts, I learned about the work of people who significantly contributed to the theory: Ernest Vinberg, Max Koecher, and Ottmar Loos, just to name some of them. But, in discussions with Faraut, Koranyi, Gindikin, and others, I realized that this theory was not yet finished: there was no *general* correspondence between "Jordan algebraic" and "Jordan geometric" objects. Starting with seminar talks in Paris, during the following years the problem of finding such a general correspondence became one of the Leitmotifs of my work. My Clausthal Habilitation thesis *The Geometry of Jordan and Lie Structures* (published as [B1]) summarizes a sort of complete answer to this question – at least, as far as the case of finite-dimensional real and complex structures is concerned. I still consider the introduction to [B1] a comprehensive summary of that stage of the theory, and I recommend the interested reader to have a look at it.

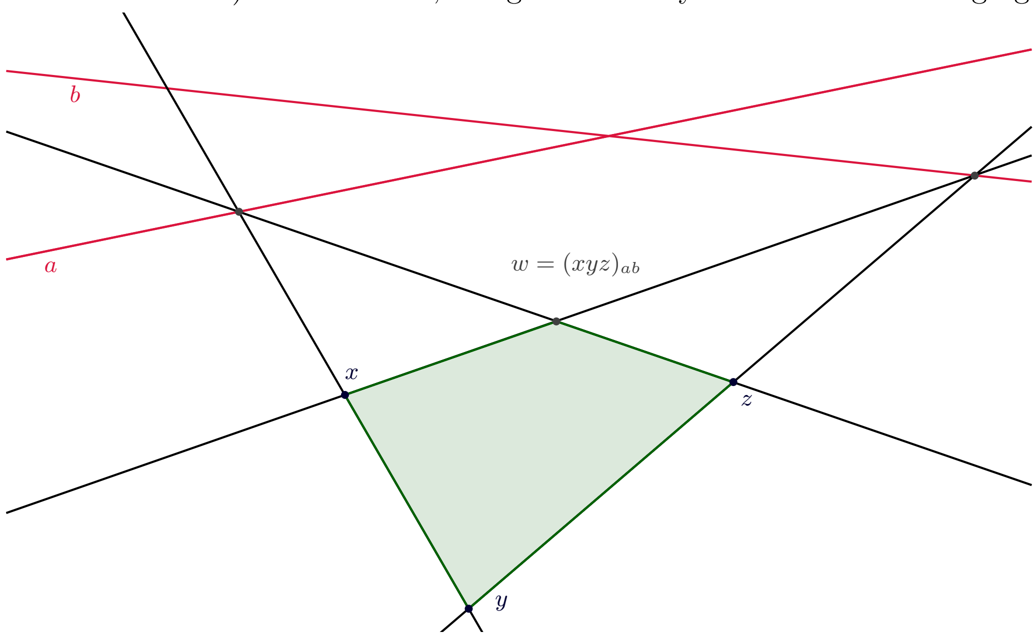
2.2. **The general coquecigrue problem.** Going a bit ahead, let us state the following general "coquecigrue problem": *given some class of algebras, defined in the sense of universal algebra by "identities" – such as Jordan or Lie algebras –, is there a class of geometric objects "integrating" it, in such a way that conversely the algebra can be recovered by "differentiating" the geometric object at some base point?* I employ the word "coquecigrue" here in hommage à Jean-Louis Loday, whose too early death has been a great loss to the mathematical community: Loday had raised such a question in relation to a special class of algebras called Leibniz algebras, *cf.* Section 14 in Some problems in operad theory. As far as I know, at present there is not even the slightest beginning of a theory solving the general problem, and probably most mathematicians would qualify it as ill-posed. However, for my research it turned out to be a very fruitful guideline, leading to interesting mathematics in all special cases where I tried to understand examples. I believe that, in the long run, this question, suitably formalized, might help to organize some large fields of mathematics.

2.3. **Associative algebras and associative geometries.** References: [25, 26, 32], [O9]. – If one takes the "general coquecigrue problem" seriously, then one cannot avoid looking at the most prominent class of algebras, besides Jordan- and Lie algebras, namely at associative algebras. Thus we ask: *is there a geometric object corresponding to an associative algebra, in a similar way as a Lie group corresponds to a Lie algebra?* In joint work with Michael Kinyon (who before had worked on Loday's original coquecigrue problem) we show that the answer is "yes": we call *associative geometry* this "associative coquecigrue". I will try below to explain a little bit more what kind of geometry this is; but let me just add here that, of course, we not only want to define "objects", but really categories, and all correspondences should be functorial, and, at best, equivalences of categories. Now, since the procedures of "symmetrization", respectively "skew-symmetrization" obviously define functors "associative algebras $\to$ Jordan algebras" and "associative algebras $\to$ Lie algebras", there shall also be functors pointing from the category of associative geometries to those of Jordan-, respectively Lie-geometries. And, indeed, this is the case: hence an associative geometry is a very rich structure, it contains as "underlying structure" a lot of Lie groups, as well as a Jordan geometry.

2.4. **Digression on non-commutative geometry.** Reference: introduction to [O9]. – At this point it is probably necessary to say some words on the relation between our associative geometries and Non-Commutative Geometry. Indeed, I have often had occasion to listen to talks by mathematicians working in this domain – especially, coming from the school of Alain Connes –, and, usually, they stress that, as soon as an associative algebra becomes non-commutative, the corresponding object, a "non-commutative space", is no longer a "space in the usual sense" (i.e., not a point set with some structure), and that the non-commuative, or "quantum", world cannot be described by the language of usual geometry. This seems to be in contradiction with what I have said above, since associative geometries *are* "usual" geometries.

My reply to this apparent contradiction is that both theories are mathematically correct, and, to me, they seem to shed light on aspects of associative algebras that are *complementary*, in the philosophical way the term is sometimes used in quantum physics. Namely, there are two different paradigms: Kinyon and I follow the paradigm of the Lie group-Lie algebra correspondence, and Connes and his school follow the paradigm of the manifold-function algebra correspondence (cf. the overview by Pierre Cartier). Both paradigms are very important in mathematics, and applied to non-commutative associative algebras they lead to very different theories.

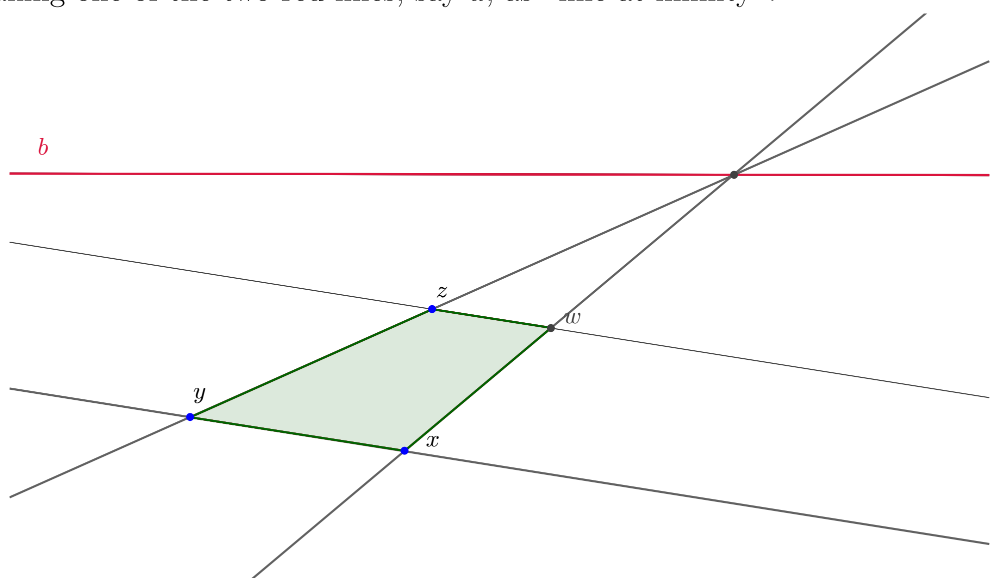
2.5. **Geometry at last!** References: [32, 33]. – When doing geometry, in "usual" spaces, I draw figures – and reading great classics like Hilbert-Cohn Vossen or Courant-Robbins, I use to somewhat regret that much of modern mathematics seems to be too abstract to be rendered by figures... I was surprised and satisfied when I realized that it is possible to visualize, at least low-dimensional, associative geometries. Even better, by using dynamical software such as GeoGebra, one can create *dynamical* figures. This is very easy – undergraduate, and even high-school students can do it (here you may find a "travaux pratiques" session I did with high-school students). For instance, using GeoGebra you draw the following figure:



The dynamic version allows to illustrate (at least) three basic mathematical ideas: first, the idea of *function of several variables*: an object is "variable" if you can freely move it in your GeoGebra file – there are 5 of such free objects: the points x, y, z and the lines a, b may freely move, and the point w follows: it is a *function of the variables* (x, a, y, b, z). To describe it by a mathematical formula, let us write $u \vee v$ for the line joining two points u, v, and $e \wedge c$ for the intersection of two lines e and c; then the formula describing w as a function of (x, a, y, b, z) is

$$w := (xyz)_{ab} := \Big(\big((x \vee y) \wedge a \big) \vee z \Big) \wedge \Big(\big((z \vee y) \wedge b \big) \vee x \Big) \; ; \tag{2.3}$$

second, the idea of a *continuous* such function: there are no "jumps", and – surprisingly – GeoGebra gives you a reasonable value (by *continuous extension*) even when there should be an error message (e.g., when x, y, z are collinear); and third, the idea of *symmetry*. This last item makes the link with associative geometries – indeed, what you see here is an image of the simplest non-commutative associative geometry.[5] The underlying point set of the geometry is the real projective plane $X = \mathbb{P}^2\mathbb{R}$, and we have drawn an "affine picture" of it. As usual in projective geometry, you may consider other affine pictures. A particularly useful one is obtained by taking one of the two red lines, say a, as "line at infinity":



When $a = b$, both lines are at infinity, and then w is nothing but the fourth point in the parallelogram spanned by x, y, z, that is, $w = x - y + z$ in the plane $\mathbb{R}^2$. Now, what is truly remarkable here (and what characterizes associative geometries) is that for $a \neq b$ we get something similar, a sort of "non-commutative plane": when y is fixed, the law $(x, z) \mapsto w$ is *associative*, and defines a *group law*, but– much better indeed! – the whole ternary law $(x, y, z) \mapsto w$ is what we call a *torsor*.[6] This algebraic statement can be proved by geometric arguments – see [32, 33], or (best) make your own GeoGebra-files. To geometers, it will not come as a surprise that, behind such geometric proofs, you find Desargues' theorem. Indeed, the relation between Desargues' theorem and associativity is an important theme in foundations of geometry. This leads us to the question: where does the associative law come from? Some more words on this.

2.6. **Exotic planes.** Reference: [32]. – To understand the associative law, one may compare it with laws that are non-associative but "nearly associative". In geometry, there are certain projective planes that are non-Desarguesian, that is non-associative, but close to it – the best known examples are the Moufang planes. Now, our formula (2.3) makes sense in *any* projective plane (and even in any lattice) – for Desarguesian planes it defines a torsor; but what does it define for other planes or lattices? In [32] we give an answer for the case of Moufang planes: the object

[5] To be more specific, this geometry corresponds to the "associative pair" $(\mathbb{R}^2, (\mathbb{R}^2)^*)$ of column and row matrices, and not to an algebra of square matrices; it is degenerate – solvable rather than semi-simple –, and thus still resembles much the usual commutative and flat geometry.

[6] A torsor is for a group what an affine space is for a vector space – see here for some explanations. The main problem is terminology: there are (too) many other names for this child.

defined by (2.3) is what we call a *Moufang torsor*. But the general case remains wide open – and also in the Moufang case a more systematic theory of "alternative geometries" is still missing (work with Kinyon in progress).

2.7. **Classical spaces, Grassmannians and "relational mathematics".** References: [31, 36]. – On the less exotic, classical side, the main source of associativity in mathematics is *associativity of composition of mappings*, or, more generally, of composition of binary relations. Indeed, all associative geometries are "classical spaces", that is, can be constructed from generalized Grassmannians, and their geometric law is in general not defined by a lattice-theoretic formula of the kind of (2.3), but by a linear algebra formula which is closely related to the ternary composition $R \circ S^{-1} \circ T$ of linear relations (see [25]). Since this construction seems to be of some rather fundamental nature, I tried to understand what kind of general mathematics might be behind it: first of all, one may replace the usual linear Grassmannian by a non-commutative version that I call the *projective geometry of a group* [31], and this then leads to looking at a completely general version dealing with arbitrary binary relations [36]. By its generality, this belongs to the topic of universal algebra: I call it *universal associative geometry*. In spite of this generality, I do think of this geometry in terms of the two figures given above: the geometry lives on a point set X, which carries as geometric structure a function of 5 variables, $(x, a, y, b, z) \mapsto w = (xyz)_{ab}$, the *structure map*, denoted by

$$\Gamma : X^5 \to X, \quad (x, a, y, b, z) \mapsto w = \Gamma(x, a, y, b, z) := (xyz)_{ab}. \tag{2.4}$$

This structure has nice features, just as for the associative geometries discussed above: for $a = b$ fixed, we get an analog of the "commutative spaces" (to be more precise: a space with some "pointwise defined product", like in usual function algebras); in the opposite case (called "transversal"), we get an associative product of the pure type "composition of mappings"; and between these two extremal cases, we have various degrees of "deformation" or "contraction" of one kind towards the other. I have the impression that this is only the beginning of some more general story which so far remained hidden since mathematicians are too much used to think in terms of *mappings*, and not in terms of general *(binary) relations*. (I got the same impression also via conceptual differential calculus, cf. last section of [40].)

2.8. **Jordan geometries.** References: [B1], [13, 14, 15, 21, 34], [O1, O3, O4, O7, O9]. – It's time to come back to Jordan algebras and "Jordan coquecigrues" – as promised above, I shall now try to give some ideas about what this is. During the time, my point of view changed, so there are several answers:

(a) in [B1], in terms of usual (finite-dimensional real) differential geometry, the object is a *symmetric space with some additional structure*, called a *twist*,
(b) in [14], working in arbitrary dimension over (commutative) base fields or rings containing $\frac{1}{2}$, the object is called a *generalized projective geometry*,
(c) in [34], for completely arbitrary (commutative) base fields or rings, the object is called a *Jordan geometry*, defined by a family of *inversions*.

What the reader may retain, without being burdened with technical details: in all three approaches, the "Jordan coquecigrue" is an animal that stems from the marriage of two respectable mathematical beings: *projective geometries* and *symmetric spaces*. Today, I believe that (c) is the most elegant presentation, and it certainly is the most general – see introduction to [34]. It is axiomatic in nature, and strongly motivated by the associative geometries discussed above: namely, just as a "special" Jordan algebra can be obtained from an associative algebra with product $(x, y) \mapsto xy$ by retaining only the *squaring map* $x \mapsto x^2$, a "special" Jordan geometry is obtained from an associative geometry by retaining from the map Γ given by (2.4) a kind of "squaring map" by restricting to the "diagonal $x = z$":

$$J : X^4 \to X, \quad (x, a, y, b) \mapsto J_x^{ab}(y) := (xyx)_{ab} = \Gamma(x, a, y, b, x). \tag{2.5}$$

Now, for a fixed couple (a, b), the law $(x, y) \mapsto (xyx)_{ab}$ is a *symmetric space* law, and thus we see that a special Jordan geometry contains a large family of symmetric spaces, parametrized by $(a, b) \in X \times X$. As above, when $a = b$, the spaces are commutative; for $a \neq b$, we get non-commutative deformations. But we have more: a quite amazing *symmetry relation* for associative geometries from [25] implies that in this case the map J has two "dual" aspects: as said above, it describes families of symmetric spaces, but it also describes *families of abelian torsors*. Axiomatizing these features leads to our definition of "Jordan geometries". This class not only includes special geometries, but also exceptional ones, closely related to the above mentioned Moufang planes, as well as the semi-exceptional case of projective quadrics.

I'm well aware that all this may sound rather abstract to readers who are used to the finite dimensional theory as presented in [B1], or in books on the particular cases of symmetric cones and bounded symmetric domains. However, one should not forget that such books either use the full apparatus of Lie theory, or of algebraic groups or of general algebraic geometry, whereas the abstract approach is both self-contained and much more general, which *in fine* makes it simpler. The particular cases mentioned above, with their wealth of particular and detailed results, remain, of course, important instances of the general theory – just as the theory of compact Lie groups remains a central chapter in general Lie theory.

2.9. **A "Jordan dictionary".** References: [15, 21, O4, O7]. – Given that there is an equivalence of categories between Jordan algebraic structures and Jordan geometries, one may start to construct a "dictionary" – in analogy with the famous "Lie group-Lie algebra dictionary" – translating algebraic properties from Jordan theory into geometric properties which "globalize" them. Writing such a dictionary is by no means an automatic translation procedure: in most cases one needs genuinely new ideas and concepts; for reasons of time, this work is only in its very beginnings:

2.9.1. *Inner ideals and intrinsic subspaces.* Inner ideals play an important rôle in Jordan theory, and in [21], with Harald Löwe, we define their geometric counterpart, which we call *intrinsic subspaces*. Whereas ordinary subspaces "look linear from inside", the intrinsic ones "look linear both from the inside and the outside".

2.9.2. *Duality and self-duality.* Duality is a fundamental meta-concept in mathematics. One of its main incarnations is duality of vector spaces and another, closely related, is duality in projective geometry. There is a profound link beween these concepts and Jordan-theory: vector spaces in duality (V, V') are generalized by Jordan pairs (V^+, V^-). Likewise, in geometry, the duality of projective spaces is generalized by duality of Grassmannians. Inside the category of Jordan pairs, we find again the one of usual (unital) Jordan algebras: they correspond to Jordan pairs having *invertible elements*. Now, to complete our dictionary, we ask: *among the geometries corresponding to general Jordan pairs, what is the geometric characterization of the geometries corresponding to unital Jordan algebras*? Answer [15]: they are the *self-dual geometries* among the general Jordan geometries. The concept of self-duality is quite subtle, so we shall just give an example: every Grassmannian $\mathrm{Gras}_{p,q}(\mathbb{K})$ of p-spaces in $\mathbb{K}^{p+q}$ is isomorphic to its dual Grassmannian $\mathrm{Gras}_{q,p}(\mathbb{K})$; but only for $p = q$ there is a *canonical* isomorphism (since then they are "the same"; for $p = 1$ it may be realized by the canonical symplectic form on $\mathbb{K}^2$). Correspondingly, only for $p = q$, the Jordan pair of rectangular matrices $(M_{p,q}(\mathbb{K}), M_{q,p}(\mathbb{K}))$ comes from a Jordan algebra of square matrices. Geometrically, the self-dual spaces are sort of *generalized projective lines*.

2.10. **Models of Jordan geometries.** References: [18], [O7, O9]. – Let's now make a junction with the topics from Chapter 1: with Karl-Hermann Neeb, we have shown in [18, 20] that, under suitable assumptions, Jordan geometries are infinite dimensional manifolds with smooth structure maps; in particular, all symmetric spaces arising in this context are "smooth symmetric spaces". On the analytic side, at this occasion the calculus from [17] was developed (see Chapter 1), and on the geometric level, we needed to construct a *"universal" model* for the abstractly defined Jordan geometries. This construction is of independent interest.

2.10.1. *The projective geometry of a Lie algebra.* This is the title of our paper [18] – its core is the study of the interplay between graded and filtered Lie algebras: the *space of all* 3*-filtrations* of a Lie algebra is a model for a Jordan geometry, such that the important *transversality relation* is modelled by a natural *transversality of filtrations* – two 3-filtrations are transversal if, and only if, they fit together to a grading. This leads to a very nice "universal model of Jordan geometries", giving explicit formulae for many of the abstractly defined objects and mappings. In the subsequent part [20] this is used to show that structure maps are smooth and their domains of definition are open.

2.10.2. *Higher graded analogs.* References: [16], thesis of Julien Chenal. – In his phd-thesis, *Géométries liées aux algèbres de Lie graduées* (arxiv), Julien Chenal has shown that the construction of a Jordan geometry out of a Lie algebra generalizes to higher $\mathbb{Z}$-graded Lie algebras: one works with filtrations and grading of length n, and again it can be used to construct smooth geometries associated to such algebras. However, the geometric structure living on these spaces is much more complicated, and, for the time being, we are quite far from understanding them fully. The only case – apart from Jordan geometries – where a lot of work has been done, is the 5*-graded case*, which is related to Freudenthal's magic square – see [16] for an overview.

2.11. **Symmetric spaces.** References: [B1, B2], [11, 22, 28, 29], thesis of Manon Didry. – I have used above, at several occasions, the term "symmetric space", without really saying what I mean by this. In the tradition of Helgason's important book, most mathematicians think here of Riemannian symmetric spaces. However, I speak of general symmetric spaces; and in my general expositions of this subject in [B1, B2], I strongly advertise the beautiful approach given by Ottmar Loos in his book *Symmetric Spaces I* (Benjamin, 1969) – it turned out to be best suited for generalizing symmetric spaces both into the direction of infinite dimension and of general base fields and -rings: see [B2], Chapters I.5 and V.

Since my phd-thesis (see Chapter 3), much of my work is related to "symmetric spaces with additional structures". This is the point of view explained in [B1], [11], [O3]. Naturally, this leads to study general symmetric spaces, with the question in mind: *which symmetric spaces do carry a "Jordan structure"? in other words, which are in the image of the Jordan-Lie functor [B1]?* Here are two themes that arise in this context:

2.11.1. *Symmetric bundles.* Reference: [24], thesis Manon Didry. – In her phd-thesis *Structures algébriques sur les espaces symétriques* (see paper [24]), Manon Didry studies *symmetric bundles*: these are "vector bundles in the category of symmetric spaces", the geometric analog of the algebraic concept of "representation". Whereas every symmetric space admits an "adjoint symmetric bundle", the Jordan symmetric spaces also admit another, "twisted" symmetric bundle structure on the tangent bundle.

2.11.2. *Homotopy, contractions and deformations.* References: [22, 28, 29]. – A feature that clearly distinguishes "Jordan symmetric spaces" is that *they come in large families*, which I call *homotopes*. The most conceptual and abstract version of this has been explained above: Equation (2.5) defines families of products $(xyx)_{ab}$, parametrized by pairs (a, b). My first approach to this, in [22], was more complicated, but also more explicit. In a series of two papers [28, 29] with Pierre Bieliavsky, we classify such homotopes for the classical Jordan-symmetric spaces. The list is very long, and it can be seen as a sort of "analytic continuation" of Berger's famous list of simple symmetric symmetric spaces: namely, it shows that Berger's discrete list really is part of some "continuous list" of spaces. The first part [28] gives an elementary and computationally very efficient construction of spaces by commuting pairs of involutions, and in the second part [29] we show that the list thus obtained is complete, by reducing the classification problem to an equivalent one on *classification of inner ideals* (see above).

2.12. **Liouville type theorems.** References: [T2], [B1], [3, 4, 7, 15], [O1, O3]. – Starting with my phd-thesis, my first work on the geometry of Jordan algebras was concerned with a local differential geometric characterization of the "conformal group" of a Jordan algebra. There is a classical theorem of Liouville on conformal mappings which can be seen as a particular instance of my general theorem from [3], saying that *the conformal group of a (simple) Jordan algebra (of dimension bigger than* 2*) is generated by the translations, the structure group, and the Jordan inverse map.* Liouville's classical theorem concerns the case $V = \mathbb{R}^n$ with positive definite scalar product, where Jordan inversion essentially is inversion with respect to the unit sphere. When $\mathbb{R}^n$ carries a Lorentzian metric, the theorem serves to determine the *causal group* of Lorentz space – a topic developed in [4] in greater generality.

In [7] and [B2], I present this theorem in a version that unifies it with another classical theorem: the (second) fundamental theorem of projective geometry, in its differential geometric version, saying that a local diffeomorphism preserving segments of straight lines must come from the (real) projective group (if the dimension is bigger than 1). Whereas the Liouville theorem deals with unital Jordan algebras, the fundamental theorem deals with Jordan triple systems: it arises for the triple system $\mathbb{R}^n$ of column matrices.

Jordan geometries, and in particular, projective spaces and conformal spheres, are prime examples of parabolic geometries, and the differential geometric methods used in the theorems quoted above fit in the approach of considering them as "conformally flat models" of general Cartan geometries. Certainly, using Jordan geometric methods, the analysis of Cartan geometries having such model spaces could be developped in great generality.

2.13. **Back to physics ?** References: [20, 23], [O8]. – At the end of this chapter, be it permitted to add some words that surely can be qualified as "speculation": a more or less daring guesswork as an indispensable source of inspiration. As I told at the beginning of this chapter, Jordan algebras owe their existence to physics; and also the Lie group-Lie algebra correspondence has become a fundamental tool in theoretical physics. Therefore, I wonder what rôle the Jordan geometry-Jordan algebra correspondence, or other "coquecigrue-algebra correspondences", might play in foundations of physics? I have been thinking about such questions for quite a while: maybe these things play no rôle whatsoever, but maybe they do – I have written up some arguments supporting the latter option, in [23] and [O8]. For a mathematician, the most convincing argument may be that, once you have realized that certain structures are *natural* and belong to *fundamental* mathematics, it is hard to believe that Nature did not "use" them.[7] For other people, probably this is no argument at all, but I have no other one, and all I do in loc. cit. is to unfold it: mathematically, if usual quantum mechanics can be formulated as a *linear theory* (an aspect stressed by Penrose in loc. cit.) *in the linear Jordan algebra of observables*, and if you have an equivalence of categories of these Jordan algebras with some non-linear and geometric category, then you can "translate" linear quantum mechanics into a "non-linear" and geometric theory (as I said above, writing such a "dictionary" is not a simple and automatic translation). Doing so might fill a certain "gap" (also pointed out by Penrose, and by many others): it is unsatisfactory that of the two theories that revolutionized physics in the beginning of the 20-th century, general relativity and quantum mechanics, the first is geometric and non-linear, whereas the second is linear and ungeometric. This seems to be the main reason why they appear to be incompatible in their present form. Of course I know that it sounds utopian to propose a "coquecigrue-algebra" correspondence as a tool of attack to this problem; but, since Non-commutative Geometry has

[7] See Roger Penrose, Road to Reality, Section 34.2, for a thorough development of this phrase.

been proposed as such a tool, shouldn't it be possible that, taken together, these two "complementary paradigms" (see Section 2.4 above) may help to draw a more complete picture?

3. Harmonic Analysis

References: theses [T1, T2], papers [1, 2, 5, 8, 9, 10, 12]. — This work is more or less directly related to my phd-thesis [T2], and I have not been actively working in this direction for more than a decade. For this reason, I will only give some short comments, complemented by (very uncomplete) links to newer developments. If time is permitting, I would certainly like to return to work on these topics – the domain of representation theory and harmonic analysis on (semisimple or reductive) Lie groups and their homogeneous spaces contains so much beautiful and deep mathematics; however, personally, I feel that I would like to have more profound answers to the general questions outlined in the preceding two chapters, before turning to more specialized topics.

3.1. **Göttingen.** I started mathematical research in Göttingen by reading Helgason's books and by writing a Diplom thesis on a paper by Gestur Olafsson and Bent Ørsted, *The holomorphic discrete series for affine symmetric spaces, I*.

3.2. **Paris.** The principal topic of my phd-thesis dealt with a problem of "analytic continuation of spherical harmonic analysis" between a compact symmetric space and its non-compact dual, both seen as certain real forms of their common complexification. I obtained results for the rank-one case (published in [2, 5]) and for the case of symmetric cones [1]. In particular, the result in the latter case can be seen as a non-commutative generalization of Ramanujan's master theorem. Quite recently, a result for the general case (confirming conjectures from my thesis) has been obtained in the paper Ramanujan's Master Theorem for Riemannian symmetric spaces by Angela Pasquale and Gestur Olafsson, who subsequently further generalize their result in the framework of the hypergeometric Fourier transform. The result for symmetric cones has, simultaneously, also been obtained by Ding, Gross and Richards.

3.3. **Clausthal.** Parallel to geometric topics related to my habilation thesis, I investigated, with Joachim Hilgert who worked at that time at Institut für Mathematik TU Clausthal, questions in representation theory and harmonic analysis on symmetric spaces. Throughout, our point of view is as geometric as possible (e.g., explaining coincidences of function spaces by looking at them as spaces of sections of vector bundles, [8], [10]). The topics of *duality* (compact vs. non-compact, compactly causal vs. non-compactly causal,...), and of *analytic continuation*, already present in my thesis, re-appear in various settings [9]. Especially in [9], we develop a fairly complete Jordan-theoretic approach to harmonic analysis on certain symmetric spaces that can be obtained by *involutions of Jordan algebras*. By now, Jordan theory is a frequently used tool in representation theory and harmonic analysis – the literature is far too rich for even trying to give here some sample links. The interested reader may do her own web-search by typing keywords such as "(generalized) Maslov index", "minimal representations", "analysis on symmetric domains", to realize that research in this realm is ongoing and active.

4. On teaching and research

In an official research report, I would not talk about *teaching* mathematics – especially in France, where we have a strict organizational separation between research business ("laboratoire") and teaching business ("département d'enseignement"). In the French system, investing your time in teaching has no payoff whatsoever for your career, and mentioning teaching questions in research work is considered as being not serious. In Germany, there used to be a tradition of marrying teaching and research – e.g., the already mentioned books by Hilbert-Cohn Vossen, Laugwitz or Courant-Robbins, which influenced very much my view on these things, stand for this tradition. I don't know if the German system still allows for such luxury goods; the French certainly does not, but nevertheless I would not like to seperate my own research from the question how its results can be communicated to non-specialists, that is "taught". After all, mathematics is produced and receipted by human brains, and therefore the question "how do human brains digest mathematics?" should be of considerable interest, both practically and theoretically.

Be it a pure coincidence or not, the research topics outlined in Chapters 1 and 2 correspond precisely to the two main courses of mathematics as taught in the German (or at least: Göttingen) undergraduate curriculum: *Differential and Integral Calculus* and *Linear Algebra and Analytic Geometry.*

4.1. **Linear and affine algebra and geometry.** References : [13, 33], [O2, O9].

– Geometry is the big looser in all recent reforms of mathematical curriculae in French high-schools and universities. No surprise, our students find linear algebra a hard and difficult matter, as they no longer have the slightest geometric intuition for understanding it. I don't know if ever the tide will change, but here are three remarks or proposals that, maybe, could be useful.

4.1.1. *Use of dynamical software.* I think that the *use of dynamical software* such as GeoGebra offers very interesting possibilites to do "linear geometry" – see [33] and Section 2.5 above; as mentioned there, I did some "travaux pratiques" with high school students based on this. It would be worth trying out to combine systematically the first course in linear algebra with "travaux pratiques" using dynamical geometry software. In this first step, one should only use the "linear" or "lattice" operations (intersection, line through two points), and only later, when introducing metric notions, add other operations of Euclidean geometry.

4.1.2. *Affine spaces and "affine algebra".* On the theoretical level, the usual approach to "affine spaces" makes things unneccessarily complicated and theoretical – in France, the topic is considered to be difficult and hence is taught (in Nancy) not before the 4th semester of studies! It is quite instructive to compare the English and the French wikipedia pages on "affine spaces": the French approach is very formal and notationally confusing by its heavy use of the traditional arrow-notation. I propose in a short note that you can find here to turn the informal description from the English page into a rigourous mathematical definition, and to do affine geometry by what I call "affine algebra". In [13], I explain that this idea can be continued further, to do projective geometry by "projective algebra". This has many advantages, all related to its excellent categorical properties (see below). Moreover, it leads to a nice and transparent construction of the "universal space", and, preparing for more advanced topics, it naturally opens a conceptual view on torsors, principal bundles and related topics.

4.1.3. *"Reasonable" use of categories.* I think that a "moderate use" of categorical notions is beneficial for teaching maths: it seems to me that a "categorical" view of mathematics is one of the ways the humain brain digests mathematics – you need not anew re-state and re-write a lot of definitions, once the general organizational pattern is sufficiently clear. In this respect, a big advantage of the approach on affine spaces mentioned above is that it automatically leads to a nice category of affine spaces: in this respect, the usual, "hybrid", definition of affine spaces is rather bad, and students do not really see what lesson it tells you. Similar things can be said about topological differential calculus, compared to the usual "metric" one (see below). At present, the tide is very much against abstract concepts and in favor of applications and techniques; to some extent, this is a reaction to certain exaggerations of the past; but, as often, people tend to throw out the baby with the bathwater.

4.2. **Differential and integral calculus.** References: [B2, B3]. – Much has already been said above and in [B3] – especially referring to teaching in France –, and I shall not repeat this here. Instead, two side remarks:

4.2.1. *A cultural remark.* It is quite amazing how stable certain "cultural traditions" are in our globalized world, where everything seems to change so quickly. For instance, in France the topic of "développement limité" is considered very important when teaching analysis – but it is not even translated into any other language! In Germany (and elsewhere, I guess) it is merged into the topic of Taylor series. Perhaps the reader may find illuminating the discussion I give in [B3], of the relation of these concepts with each other (and with the topic of analytic functions, Chapter XVI in [B3]). Another French (and certainly Bourbaki) tradition is to teach several variable analysis in general Banach spaces, since this is conceptual and base-free. Although I appreciate thinking in concepts, the example illustrates very well that the choice of concept is never totally innocent: here, it clearly is a choice for a category of "metric" differential calculus, and hence closes the door for explaining to students the idea of "topological" differential calculus.

4.2.2. *Categories: bis.* The categories defined by topological differential calculus are much nicer and more natural than those obtained by "metric" calculus. Without turning lectures into category courses, one can explain that this has numerous advantages: the biggest is certainly to have a single concept covering real, complex, p-adic, and many other more "exotic" instances of differential calculus, manifolds, Lie groups, and so on. I also hope that "conceptual" differential calculus may help to "tame" n-fold categories, which are considered even by specialists to be a difficult topic. At the end, a conceptual and categorical approach to differential calculus should give new strength to the vision of Charles Ehresmann of a conceptual and algebraic approach to the whole of differential geometry.

5. On mathematics in France

Without falling into paralyzing pessimism, one may say that the present situation of mathematics in France is quite bad and becomes worse every year. There are not enough students, except for some elite-schools, and both research and teaching at universities suffer under hypercentralized bureaucracy and desastrous political decisions. National and European politicians, who have no idea what science is, put big money on certain domains they consider to be socially and economically useful, by cutting off everywhere else. This is, of course, a far too big topic – I should come back to this elsewhere.

In spite of this bad situation, it is important to encourage young people to learn mathematics. Maths still is one of the best ways of keeping your mind fit and of protecting it against pollution by the huge clouds of spam floating around everywhere. If, by chance, the reader were in the situation wanting to start a phd thesis on one of the domains described above, s-he may write me an e-mail.

W.B., Nancy, march 2017

Institut Élie Cartan de Lorraine, Université de Lorraine at Nancy, CNRS, INRIA, Boulevard des Aiguillettes, B.P. 239, F-54506 Vandœuvre-lès-Nancy, France

E-mail address: wolfgang.bertram@univ-lorraine.fr