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Rewriting Points

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Abstract

A few episodes from the history of mathematics of the 19^{th} and 20^{th} century are presented in a loose sequence in order to illustrate problems and approaches of the history of mathematics. Most of the examples discussed have to do with some version of the mathematical notion of point. The Dedekind-Weber theory of points on a Riemann surface is discussed as well as Hermann Weyl's successive constructions of the continuum, and the rewriting of Algebraic Geometry between 1925 and 1950. A recurring theme is the rewriting of traditional mathematics, where 'rewriting' is used in a colloquial, non-terminological sense the meaning of which is illustrated by examples.

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1. Rewriting History

History is all about change and difference; philosophy rethinks things; and mathematics moves on by constantly reinventing its own history.

In a series of lectures on the classification of algebraic threefolds delivered 15 years ago at an instructional meeting in Ankara, Miles Reid announced a new method he was about to present by saying: "In order to go further, we have to rewrite history." Such "rewritings of history" occur time and again, and on varying scales, whenever mathematicians get down to work. The astronomical treatises of the *Siddhantas* for example do not focus on the chord associated to a central angle in the circle, but work with relations between the half chord and its associated angle. In this way, they introduced and computed the sine and other trigonometric quantities, and also coined the terminology of *jya, kojya,* etc. which, via confused rewritings in Arabic, produced our

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pseudo-Latin expressions *sine* and *cosine*. This seemingly small step of renormalization brought about a corresponding rewriting, a reorganization of the traditional tables handed down from the Babylonians and from Ptolemy. Another well-known rewriting in the same domain occurred in 18^{th} century Europe when the complex exponential function was brought in to reorder the plethora of trigonometric formulae.

Once adopted for further work, such rewritings tend to stick. If you are an algebraic geometer used to working with cohomology theories, you will find it difficult to imagine how your predecessors have dealt with things without seeing cup products, vanishing $H^i(X, ..)$, etc. mapping out the geometry. One might think that this is purely psychological or superficial, like asking silly questions such as: How could former generations survive without cellular phones? After all, at least *in principle*, all information carried by a cohomology group could be spelled out in non-cohomological, geometric terms. But in the development of science in general, and particularly in mathematics, no technological advance ever leaves the world intact. Every rewriting of a mathematical theory recreates both its objects and the ways to handle them.

In the example of cohomological methods in algebraic geometry this is highlighted by the very beginning of W.V.D. Hodge's international career with his 1930 paper [46], where he proved that a nonzero holomorphic n-form—an "n-ple integral of the first kind" in Hodge's terminology—on a complex n-dimensional algebraic variety cannot have all its periods equal to zero, thus answering a question posed by Francesco Severi for algebraic surfaces ($n = 2$). Atiyah in [2], p. 104, tells the story how Solomon Lefschetz, whose very methods Hodge was generalizing in his proof, would not get the point, asked Hodge to withdraw the paper, and took months to be convinced. Lacking independent sources for the details of this affair, which earned Hodge a first invitation to Princeton, let me just point out an interesting twist in Atiyah's account of it: On the one hand, after sketching the proof in modern cohomological terms, he justly points out that back in 1930, "complex manifolds (other than Riemann surfaces) were not conceived of in the modern sense, and the simplicity of the proof indicated above owes much to Hodge's work in later years which made complex manifolds familiar to the present generation of geometers." Yet on the other hand, Atiyah also expresses his surprise that Lefschetz did not grasp Hodge's argument immediately. In my view, that this happened to somebody like Lefschetz provides additional, first hand historical evidence for how different the situation in 1930 actually was from what our rewritten account suggests. Once a line of thought—like the argument developed in [46]—has been recast in a universally practised technique, its originality at the time it was conceived becomes very hard for us to appreciate.

Lovers of Western classical music may have encountered this problem. Being familiar, say, with Robert Schumann's piano works that are part of today's standard repertoire (*Carnaval, Davidsbündlertänze, Kinderszenen, Kreisleriana, . . .*), one cannot listen to his more rarely performed little *opus* 1, the Abegg-Variations in F-major, without being *reminded* of what Schumann actually composed *later*. This then makes it genuinely difficult to understand the helpless critiques which Schumann's *opus* 1 received in 1832, when the best available stylistic comparison was probably with the time-honoured composer Johann Nepomuk Hummel.

It is the job of the historian of mathematics to recognize such *remembrance of things to come* in the reading of old documents which the ongoing rewriting of mathematics offers us time and again, and to set the historical record straight as far as the available documentary evidence permits.

2. Rewriting Historiography

Rewritings on all scales make up the very fabric of mathematical activity through the centuries. But the notion of rewriting—which I will use here in a loose, non-terminological fashion—works best on a relatively local, micro-historical level which makes specific comparisons of an original document with rewritten versions of it possible. The objective is then to describe explicit *transformations of epistemic objects and techniques*, where these latter terms are to be taken in the sense that Moritz Epple ([23], pp. 14–17) has extracted from Hans-Jörg Rheinberger's approach to the history of laboratory science [63]. Since the mathematical research we will be looking at works essentially without physical machinery, its epistemic evolution, even if a lot of it may happen orally, will finally be documented almost exclusively by textual documents, published or unpublished papers, notes, correspondence, etc. This may justify speaking of rewriting.

In [30], Catherine Goldstein has not only conducted such analyses of various rewritings—she speaks of different readings (*lectures*) instead—of one of Fermat's marginal notes—the one that proves in particular that there is no right-angled triangle with rational sides whose area is a rational square—but she has in fact reconstructed the history of this note over 350 years as a collection of such readings. If one likes mathematical metaphors, the structure that Goldstein winds up with may remind one of a complicated covering space that history has superimposed on that marginal note written in the first half of the 17$^{\text{th}}$ century; each reading is a reading of Fermat's note and thereby related to it, and certain readings are also related among each other. But for the historian all rewritings are created equal; each lives on its own respective sheet.

When it comes to major, general upheavals in the history of mathematics, the analysis of rewritings can easily run into the famous warning sign that every French child reads and learns: *un train peut en cacher un autre*. For instance, Herbert Mehrtens [55] has tried to describe the overall history of mathematics around the turn from the 19$^{\text{th}}$ to the 20$^{\text{th}}$ centuries as the finally victorious incursion of modernism into mathematics. Understanding this modernist transformation continues to be a currently active research topic in the history of mathematics.[1] Already certain strands of this macrohistorical phenomenon, such as "the entrance of non-Euclidean geometries" during the 19$^{\text{th}}$ century, may appear so cataclysmic that "exceptionally one uses the term 'revolution' even for mathematics" to describe them ([55], p. 44). Yet, a closer look usually discovers subtle webs of intertwined rewritings which in any event deserve to be disentangled.

An example from the history of non-Euclidean geometries is provided by Eugenio Beltrami who would insist on building (the planimetry) of his non-Euclidean pseudosphere on the *substrato reale* of C.F. Gauss's differential geometry of curves (see [29], pp. 48–51; letter to Hoüel of 2 Jan. 1870, p. 117), thereby integrating it into an established part of mathematical analysis, i.e., the differential geometry available to him. This is still a far cry from David Hilbert's reworking of the Foundations of Geometry at the end of the century (see [43]) which transformed the axioms of geometry from specific claims about known objects, such as points, into implicit definitions of these very (potential) objects, turned geometry into an autonomous mathematical discipline—*which* geometry fit nature was no longer for the geometer to decide—and would in due course give birth to mathematical logic and model theory as a new, equally autonomous sub-discipline of mathematics. Obviously, qualifications like 'modern' or 'abstract' apply to Hilbert's approach much better than to Beltrami's attitude. Yet, Beltrami was a pivotal author of the non-Euclidean 'revolution', rendering Riemann's vision of intrinsic geometry concrete in a crucial case.

There are thus different stories—or histories—to be told here, and a good history of non-Euclidean geometry will of course mention both of them in turn, plus several others, like the ones alluded to in [39], pp. 690–699. The question remains whether one wants the formalist re-interpretation of axioms as implicit definitions, and the logical inspection of various axiom systems, to be, or not to be, a core feature of the rewriting of geometry after the dawning of non-Euclidean geometries. Rouse Ball, for example, included at the end of the fourth, 1908 edition of his *History of Mathematics*, in the discussion of non-Euclidean geometries ([3], pp. 485–489), references to recent works published as late as 1903; but he would not mention Hilbert's 1899 book on the Foundations of Geometry ([43], chap. 5) at all. Did he maybe think that the problem about the nature of space raised by the new geometries was such a burning issue for our natural philosophy that Hilbert's purely "logical analysis of our intuition of space" ([43], p. 436) was less relevant in comparison? After all, "there are indeed reasons . . . for suggesting that to see the search for non-Euclidean geometry in this axiom-based way is an artifact of mathematical modernism that distorts the historical record." ([33], p. 51)

The scattered episodes I am about to present are loosely held together by the mathematical concept of a point and they deal with rewritings. They date

[1]See for instance [33], [81], and http://web.uni-frankfurt.de/fb08/HS/wg/gif.html.

from before, during, and after World War I. I present these episodes in order to illustrate various historical regards.

3. Arithmetic Points

Just as his teachers Peter G. Lejeune-Dirichlet and Bernhard Riemann, whose conceptual approach to mathematics he consciously emulated and carried further, Richard Dedekind (1831–1916) has been seen by many as a pioneer of modern mathematics, particularly by Emmy Noether who participated in the edition of his collected papers. Her forward-looking comments on many of them, and her own further development of the theory of modules and ideals, make it plausible that she sometimes felt as if "all she had done was to develop Dedekind's ideas" [1]. But Emmy Noether's close reading and rewriting of Dedekind's is not our subject here. I will look at Dedekind's two famous contributions to the question of what a point is: in his little 1872 brochure [17], he proposed what we all know as Dedekind cuts to define real numbers arithmetically; and in his seminal joint paper with Heinrich Weber ten years later [20], he defined a purely arithmetic avatar of a point on an algebraic Riemann surface.

Both definitions are remarkably similar; both try to conceptualize the intuition of what a concrete point does for you. On the real line, fixing a point can tell you, can it not, where to cut the line in two, and in Dedekind's analysis [17], the idea of continuity is precisely that every cut in the line is also afforded by a point. So, if you ban intuition but still want to define a point, a real number, in the linearly ordered continuum, with only rational numbers at your disposal, you just define the point by the cut, i.e., as being a partition of the rationals in two subsets, one of which has all its elements smaller than all the elements of the other. Likewise, on a algebraic Riemann surface, a point will be something where you can evaluate (sometimes getting the value $\infty \ldots$)rational functions living on the Riemann surface; and you know or postulate that rational functions ought to be sufficiently plentiful to separate points. So, if you can't see the Riemann surface, but still have its field of rational functions, define a point arithmetically as an evaluation homomorphism (including possible values ∞) on rational functions which leaves constant functions invariant. That is what Dedekind and Weber did in the second part of [20], showing subsequently that such an evaluation mapping defines a prime ideal in the coordinate ring, hence a maximal ideal (we are in dimension one), etc. They carried the theory as far as the Riemann-Roch Theorem, formulated and proved in this purely arithmetical setting. (A point in the sense of Dedekind and Weber was the first special case of what was later called a 'place' of a field; see for instance [97], p. 3.)

Both of these conceptual creations of points by Dedekind were *arithmetizations*. This label 'arithmetization' has been used in many different ways at the end of the 19^{th} century; we have explored in [60] the panoply of arithmetizing treatments of the continuum and how they were received and rewritten by various mathematicians and philosophers at the time. Suffice it to emphasize here that Dedekind's arithmetization is not an axiomatization. Dedekind cuts were invented to *define* real numbers in terms of infinite sets of rational numbers—never mind the debates about impredicative definitions or set theoretical paradoxes this approach would encounter later on. Also the definition of a point on an algebraic Riemann surface given in [20] plainly relies on an *arithmetization* of the theory of algebraic Riemann surfaces because Dedekind and Weber prepare this definition by a good fifty pages "of a purely formal nature" ([20], §14) which carry over to rings of (entire) algebraic functions most of the apparatus that Dedekind had developed earlier for the theory of algebraic integers in number fields. It is this theory which then allowed them, among many other things, to quickly deduce that every point—in the sense of evaluation mapping—corresponds to a prime ideal of the coordinate ring.

The paper by Dedekind and Weber has attracted considerable attention, and indeed praise, in the historical and philosophical literature. The philosopher David Corfield, for example, insists that the paper of Dedekind and Weber constitutes "a watershed in the use of analogy" and quotes Jean Dieudonné to the effect that "this article by Dedekind and Weber drew attention for the first time to a striking relationship between two mathematical domains up until then considered very remote from each other . . . " ([15], p. 96) Corfield then further embeds his argument into a "historical claim" about a broad mathematical road towards a "structural outlook." ([15], p. 98) However, his whole discussion of [20] starts not in the 19$^{\text{th}}$ century but as a remembrance of things to come, with a discussion of two well-known pieces by André Weil: the letter to his sister Simone [86], 1940a, from the beginning of World War II, on the analogies that were guiding Weil when he tried to develop the theory of correspondences on curves over finite fields, and Weil's even later variant of it [87], 1960a. Furthermore, the discussion of the article by Dedekind and Weber is mixed with a brief look at Kurt Hensel's different approach to the arithmetic of algebraic function fields.

One may of course sympathize with Dieudonné's statement quoted by Corfield, about the "very remote" mathematical domains that Dedekind and Weber managed to bring together, if one remembers Riemann's geometrical and topological (*analysis situs*) approach to Riemann surfaces, and the unique feature of Riemann's theory of abelian functions which actually obtains the rational functions on the algebraic Riemann surface from a combination of *transcendental* functions with specified local behaviour. But there are also good reasons to rethink—and rewrite—Dieudonné's and Corfield's conclusion. In fact, the tectonics of sub-disciplines of mathematics was very much in flux during the 19$^{\text{th}}$ century, and statements about the relative distance of two domains of mathematics at a given point in time raise intricate questions.

To start with the classification adopted by the mathematicians at the time: the *Jahrbuch über die Fortschritte der Mathematik* (vol. 14 for the year 1882, published in 1885) reviewed the article by Dedekind and Weber neither under the heading *Algebra* (section II) nor under *Number Theory* (section III), nor under any of the chapters on algebraic curves, surfaces, etc. in section IX (*Analytic Geometry*), but under section VII, *Function Theory*, chap. I: *Generalities.* This is reasonable in that the paper explicitly gives a new general treatment of the algebraic functions on Riemann's surfaces, as the reviewer (Otto Toeplitz's father Emil, teacher at a Gymnasium in Breslau) also duly points out. By the way, the subsequent review in this volume of the *Jahrbuch*, in the same section and chapter, is of Felix Klein's very pedagogical exposition of Riemann's theory with appeal to physical intuition. On the other hand, Dedekind published another big paper the same year, on the discriminant of an algebraic number field; it was classified in the *Algebra* section II, chap. 2 on the *Theory of forms.* But the Italian translation of Dirichlet's Lectures on Number Theory edited by Dedekind which also appeared in 1882 did make it into the *Number Theory* section. Clearly the perception of the mathematical sub-disciplines at the time was not ours, looking back from today.

Trying to describe as best we can the 'domain of mathematics' at the time *into which* Dedekind's and Weber's paper [20] integrated part of Riemann's theory of algebraic functions, we have to say that it was the arithmetic (or number theory) in the wake of Kummer's "ideal numbers." This specialty, it turns out, was practised by less than a handful of mathematicians between, say, 1860 and 1880 (see [31], chap. I.2, §3). Among them two researchers clearly stand out: Leopold Kronecker and Richard Dedekind. As for Kronecker, he assures his readers ([50], p. 197) that as early as 1858 he had actually communicated to Riemann the main result of an algebro-arithmetic investigation of his on the discriminant of algebraic functions in one variable, because it provided a better justification than the one Riemann had given for a simplifying assumption concerning the ramification points, which Riemann used throughout his theory of abelian functions. Kronecker also tells us he discussed these ideas with Weierstrass who folded them into his Berlin lectures on abelian functions. At any rate, Kronecker did not publish a paper on this at the time, although he did include his ideas in several lecture courses he gave in Berlin, as I was able to confirm in handwritten notes from Kronecker's lectures which are kept at the Mathematics Library of IRMA, Strasbourg. In 1880 however, when Weber submitted the manscuript of [20] to *Crelle's Journal*, Kronecker apparently decided to only look at it after publishing in the upcoming volume 91 (1881) what he presented as his old write-up from 1862 ([50], pp. 193–236) together with a preface explaining its history. Dedekind's and Weber's paper was thus delayed until the subsequent, 1882 volume of *Crelle's Journal*, only to be again preceded there by yet another paper of Kronecker's closely related to the theory developed by Dedekind and Weber: a reprinting of his momentous 1881 *Grundzüge* [50], pp. 239–387. Now these *Grundzüge*—which the *Jahrbuch* classifies in the section on *Algebra*, chap. 1, *General theory of algebraic equations*—sketch a very complete, unified arithmetic theory of algebraic integers and algebraic functions (of arbitrary many variables). So Dedekind and Weber—who had come into closer contact when they were both collaborating to prepare Riemann's works and some of his unpublished papers for the edition of his Collected Papers—were not alone with their explicitly arithmetic approach to algebraic functions; in fact, all the usual suspects, so to say, were publishing along this line in the early 1880s, even if their papers ended up in different drawers of the classifiers.

Furthermore, the arithmetic rewriting of Riemann's analytic theory was not an unlikely idea at the time because competing digests of Riemann's theory were around which, even if they were not arithmetic, all started, unlike Riemann, from the explicitly given algebraic functions: see the overview in [11], p. 287. That the two arithmetic attempts were, in spite of the tensions between Dedekind and Kronecker, akin in many ways is also confirmed by Weber's textbook presentation of the whole paper [20] in [83], pp. 623–707, which follows the original rather faithfully, except that Weber replaced Dedekind's method of ideals by Kronecker's forms (just as Hilbert had done in his *Zahlbericht* [44], pp. 63–363, in the very proof of the uniqueness of factorization of ideals into prime ideals in the ring of integers of an algebraic number field, cf. [66]).

Finally, it should not be overlooked—because it emphasizes the algebro-arithmetic nature of the paper—that, in spite of the impressive theorems which they manage to prove so neatly by the arithmetic method, Dedekind's and Weber's paper does remain incomplete in that the naked abstract 'surface' of which they have defined the points is not endowed with any sort of topological or analytic structure. This is not an anachronistic comment of mine because the last sentences of the introduction show that the authors wanted to come back to this, and it is a pity that neither of them ever did.

For a better understanding of what is at stake here, let us look at a broader time scale. When Catherine Goldstein and I were preparing the first two chapters of [31], we sifted through 19$^{\text{th}}$ century papers directly or indirectly taking up Gauss's *Disquisitiones Arithmeticae*, and discovered for the approximate period 1825–1860 a domain of research connected with Gauss's work that "knit together reciprocity laws, infinite series with arithmetical interpretations, elliptic functions and algebraic equations." ([31], p. 52) We called this domain *Arithmetic Algebraic Analysis* and we argued ([31], pp. 24–38, 52–55) that it constituted a (research) *field*, in the sense of Bourdieu: "all the people who are engaged in [this] field have in common a certain number of fundamental interests, viz., in everything that is linked to the very existence of the field," and one can uncover "the presence in the work of traces of objective relations … to other works, past or present, [of the field]." ([7], p. 115) We had indeed found that the main actors of this domain were "linked by a dense communication network, both personal and mathematical. Their published papers would meet with prompt reactions. … An interesting characteristic feature was the production of new proofs of the central results." ([31], p. 52) And we explained ([31], p. 54) that the coming together of very different types of objects, methods and results does *not* suggest calling the field of Arithmetic Algebraic Analysis a mathematical *discipline*.

Kronecker had participated in that field; Richard Dedekind in his younger years had still seen it in action; and for such a versatile mathematician as Heinrich Weber—who moved from first papers in mathematical physics to being the author of the 3 volume *Lehrbuch der Algebra* where he would rewrite, among other things, all the main results obtained by Arithmetic Algebraic Analysis—the heritage of this field held no secrets. Insofar as Dedekind and Weber managed to provide an alternative proof of Riemann's theorems by means of an arithmetico-algebraic method, one might therefore be tempted to consider their paper [20] a late contribution to the practice of Arithmetic Algebraic Analysis. But this is not so. Times had changed. Not only had the field of Arithmetic Algebraic Analysis largely died out ([31], chap. I.2); but at least since the 1870s, arguing about the adequacy of the method employed had become an important issue for many mathematicians, esp. in Germany. Dedekind and Kronecker were no exception. Recall how Dedekind argued in [17] that he had uncovered with his cuts the true conceptual essence of the intuitive idea of continuity, or completeness, of the real line. On Kronecker's side, his bitter controversy of 1874 with Camille Jordan about the proper way to set up the theory of bilinear forms and their normal forms confirms this point perfectly—see Frédéric Brechenmacher's detailed analysis [10] of this controversy and of the different images of bilinear algebra which it brought to the fore.

In that controversy, Kronecker insisted very much on the *generality* of a theorem of Weierstrass's which did not have to exclude, or treat separately, the case of characteristic polynomials with multiple roots. He also claimed a superiority of the *arithmetic* point of view, which is more difficult to pinpoint precisely. The same values are also appealed to by Dedekind and Weber when they describe what their treatment avoids: "In previous investigations of the subject, certain restrictive assumptions have usually been imposed on the singularities of the functions studied, and these so-called exceptional cases are either obtained parenthetically as limit cases, or simply excluded. In the same way, certain principles about continuity and existence of [series] expansions are admitted whose evidence relies on various sorts of geometric intuition." Furthermore, there is the value of *simplicty* which, when it is coupled with the said generality or *completeness*, seems to have captured the highest standards of a scientific treatise (see [35] for a discussion of the scientific values at the time from the point of view of the humanities). Kronecker explicitly refers to the physicist Gustav Kirchhoff for these values ([50], pp. 353–354), and Dedekind & Weber announce their "simple, and at the same time rigorous and completely general point of view" right away in the first sentence of [20]. Finally, Dedekind struck a similar note when he graciously thanked his younger colleague after two years of work on the joint project, quoting from Pascal's letter to Fermat: "...I see that the truth is the same in Toulouse as in Paris", and then commenting on their collaboration "which after various oscillations increasingly took on the character of intrinsic necessity." ([19], p. 488) And sure enough, on the page just quoted from Kronecker, [50], p. 354, he also claims necessity for his method, i.e., the above-mentioned forms as alternative to Dedekind's ideals.

All this rhetoric leads us away from the inspirational play of fruitful analogies. It points towards establishing arithmetic as a model approach to the theory of algebraic functions. But this model stood not alone; other methods rivalled or complemented it. Apart from the analytic approach and the algebraic geometry of the time, there was in particular the theory of Hensel and Landsberg—their book [42] was dedicated to Dedekind on the occasion of the fiftieth anniversary of his doctorate—who used local series expansions as input for an otherwise purely arithmetic theory, but then went further than Dedekind and Weber, treating abelian integrals. Emmy Noether would call this the "accretion of Weierstrass and Dedekind-Weber" ([57], p. 273), but in view of Hensel's own mathematical history as a student of Kronecker's, this is not a final historical assessment.

Even though Emmy Noether would increasingly see herself in the line of thought of Dedekind during the 1920s, in 1919 she published the well-balanced report [57] which supplied the chapters deliberately left out of the report that Brill and her father had compiled a generation earlier [11]. The first part of Emmy Noether's report sets an impressive example of a dense, virtuoso and impartial comparison of the existing theories, concise and yet explicit down to the different arrangements of proof for corresponding theorems. In the final section 8, her report briefly explores analogies within the theory of algebraic functions, of transcendental problems in the theory of algebraic numbers. These analogies lay outside of the scope of Dedekind and Weber; they had been briefly hinted at in Hilbert's statement of his twelveth problem in 1900, [45], pp. 312–313. A budding new sub-discipline which started from this sort of analogy, but then converted to basic tenets of the Dedekind-Weber programme was the arithmetic theory of algebraic function fields of one variable over a finite field of constants. It first began after 1900, and then afresh after World War I, from an analogy for fields of algebraic functions of the analytic theory of algebraic number fields, in particular the analytic class number formula. Then, reversing prior practice in this new field, F.K. Schmidt decided in 1926 to work with *all* points afforded by the field, according to the first principles of Dedekind and Weber, and to change the definition of the zeta function of the field accordingly. (See [64], p. 571–572; cf. [31], pp. 174–178)

4. Holistic Points

We have seen that Kronecker and Dedekind attached great importance to certain methodological values. To be sure, all scientists have values they try to live up to in their work and, just like the values shared by Kronecker and Dedekind, these will usually not be limited to one branch of science and will typically be in tune with the ambient culture. In this section, I take one more look at Dedekind to draw attention to a basic concept of his mathematics which has a holistic ring to it. This will then be the occasion to look at holistic tendencies in mathematics in general, and in particular in the 20$^{\text{th}}$ century.

Dedekind's very basic and very successful concept which I am alluding to is *Körper*. Unfortunately, this is nowadays called a *field* in English, which may remind one, if not of Bourdieu, of agriculture or cricket. Earlier, feeble attempts to introduce the word *corpus*, and its plural *corpora*, into English mathematical terminology instead (see for instance [80], [41]) have never caught on. What is totally lost in the translation *field* is Dedekind's motivation for choosing the term *Körper*: "Similarly as in the sciences, in geometry and in the life of human society, this name is to denote also in the present context a system which exhibits a certain completeness, perfection, self-containedness through which it appears as an organic whole, a natural unity." ([19], p. 20)

Dedekind's *Körper* were not our fields, neither mathematically nor philosophically. Mathematically, his *Körper* consisted of complex numbers; and he called "finite field" (*endlicher Körper*) a finite extension of the rational numbers, i.e., an algebraic number field. Never did he take the step—although he knew of course Galois's *imaginaires de la théorie des nombres*—to extend what we view as the general field axioms to infinite and finite sets alike. (The first one who published such a parallel treatment of fields with infinitely many and finitely many elements would be Heinrich Weber [82]. The systematic exploration of the modern axiomatic notion of field is due to Steinitz [79] and was prompted by the advent of a new type of examples: Hensel's p-adic fields.) Various possible reasons for Dedekind's failure to really go structural here, are discussed in [38], pp. 108–109. The very intimate connection between his "finite fields" and the theory of algebraic numbers presumably had an important part in it. Dedekind's vision of his *Körper* as a basic object of arithmetic is also reflected in the fact that he described the inclusion of fields as a division. ([19], p. 409; [38], pp. 106–107; [16], Part I, chap. 2.) For Dedekind, the importance of fields was not that they represented a basic algebraic structure, whereas he did appreciate groups for this ([38], pp. 107–108). He treated his *Körper* as the active entities on which algebra and number theory rest. In a letter to Lipschitz from ~~197~~[illegible] alluded to the intrinsic possibility of defining inside an algebraic number field its ring of algebraic integers as the "number-theoretic capabilities of the field" (*zahlentheoretische Fähigkeiten des Körpers*), which are wasted once one fixes a primitive element to deal with a finite extension of the rationals.

For reasons that I find impossible to trace, but which might have to do with the holistic ring of the word, Dedekind's *Körper* had a direct impact on the immediately following generation, unlike Kronecker's terminology from his *Grundzüge*, and unlike other parts of Dedekind's theory. While Hurwitz, Weber and Hilbert would time and again substitute some of Dedekind's arguments in the theory of ideals by Kroneckerian arguments using the adjunction of indeterminates (a method that Dedekind abhorred as being not intrinsic), Dedekind's "finite fields" were turned into the pivotal notion of Hilbert's *Zahlbericht* ([44], pp. 63–363) whose full title reads "The theory of algebraic number fields." (Addressing their rings of integers directly, as a primary object of study, would become common only as a result of Emmy Noether's works from the 1920s on the axiomatics of commutative rings and their ideals.)

Sticking for simplicity with literature in German, the *Fortschritte* database lists 69 papers between the first one in 1882 (an article by Dedekind) and 1914 which have the word *Körper* (in the algebraic sense) or the word *Zahlkörper* already in the title. The little industry really took off around 1900. The arithmetic model also induced Felix Hausdorff to introduce analogous terminology into set theory "on the basis of a vague analogy from which one should not demand too much": a *Mengenkörper* is a set of sets which, with two member-sets, also contains their union and difference. ([39], p. 115) It was by this bias that the word *Körper* even made it into Kolmogorov's 1933 axiomatization of probability, see [49], p. 2.

And when Deuring, Hasse and his collaborators embarked as of 1936 on translating from the theory of Riemann surfaces into the theory of function fields the notion of a correspondence between two Riemann surfaces, or two algebraic curves, they studied the arithmetic of the field generated over the fixed field of constants K by (algebraically independent) generators of the two given function fields of one variable. They called this field of transcendence degree 2 over K the *Doppelkörper* attached to the situation. André Weil would not tire of deriding the clumsiness of this method—see [86], p. 253, from the text quoted by Corfield; [87], p. 14: "notably unsuccessful paper of Deuring"; [6], p. 104, note 18: "...orthodox successors of Dedekind"; cf. [68]. Weil embarked instead soon after 1940 on a fundamental algebraic rewriting of Algebraic Geometry to which we will return below. At least Deuring himself may not have been such an "orthodox successor of Dedekind" after all; he had apparently proved that the ring of correspondences of an algebraic curve over a finite field had characteristic zero by first lifting the curve to characteristic zero, doing an analytic argument on the associated Riemann surface, and then reducing back to the original finite characteristic. According to [37], p. 347, he deleted this argument—which may appeal to us today but which Hasse qualified as "unfair"—from the galley proofs of the second installment of his paper [21]. (Cf. [68])

Let us stop this field trip here. What would Dedekind say, if he knew that today his organic fields were, each single one of them, just a point, in Grothendieckian Algebraic Geometry, and not even a thick one?

Holism in the sciences has been studied especially in the history of the life sciences. A good example of such a study is Anne Harrington's *Reenchanted Science* [36]. Starting from Kant's Third Critique (*Kritik der Urteilskraft*) and Goethe's *Farbenlehre*, continued in the early 19$^{\text{th}}$ century by the idealist philosophers' quest for completed systems in the face of political fragmentation of Germany, holistic ideas within the sciences in Germany increasingly turned into a revolt against the machine image of life towards the end of the 19^{th} century. Nor was this only a German phenomenon: the French philosopher Henri Bergson clearly owed much of his popularity to the timeliness of his message, when he pointed for example to the incompatibility between our inner sense of duration (*durée*) and movement on the one hand, and our daily life surrounded by time pieces and nature as described in mechanical treatises on the other—see for instance his *Essai sur les données immédiates de la conscience* [4], pp. 58–80. But an important point of Harrington's analysis is that holistic currents in the sciences responded (by way of metaphors) to political agenda in Germany. Harrington's case studies of holistic thinkers—whose careers often evolved outside the scientific mainstream, but who nonethelss marked the history of their disciplines—include the biologists Jakob v. Uexküll with his key notion of *Umwelt* and Hans Driesch; the neurologist Constantin von Monakow; the writer on Wagner and the Aryan race Houston Stuart Chamberlain; a list of *Gestalt*-psychologists from Christian von Ehrenfels to Max Wertheimer, Wolfgang Köhler and others; and the expert of brain research and therapy Kurt Goldstein.

Holistic influences in mathematics have not received comparable attention. And it may not be obvious at first, what holism would mean in mathematics and what it could do for the history of mathematics. Gerolamo Cardano for instance was clearly a holistic scientist; metals were for him inhabited by the soul of the world. But the bearing of this on the history of his publication of Tartaglia's formulae for solving a cubic equation by radicals seems negligible. All through the nineteenth century, it seems very problematic to attribute holistic tendencies to mathematicians, or to try and use this category to better understand major debates. The holistic element we stressed in Dedekind's choice of the word *Körper* is undoubtedly there, as shown by his own comment when he introduces it; but which role this particular emphasis given to the term played for the practice of this notion I find impossible to trace, and so the observation does not seem to add anything exploitable to the analysis of Dedekind's guiding values, and of his conceptual approach which he traced to his teachers Dirichlet and Riemann.

However, I contend that the attribute 'holistic' is well-suited and useful to explain certain mathematicians' attitudes in the 20^{th} century. Since the phenomenon clearly touched different branches of science at that time, something may then be gained by looking across disciplinary boundaries, and the history of philosophy during the period ought to be simply integrated into the history of sciences for the purpose. Two examples of mathematicians immediately suggest themselves: Luitzen Egbertus Jan Brouwer and Hermann Weyl. One might think of adding other mathematicians, Andrei Kolmogorov for instance (in view of the constructivist side of his œuvre) or Erich Kähler (considering his *magnum opus* [48]). But at least for this talk, I will concentrate just on Hermann Weyl.

It is well-known that Weyl went through different periods in his thinking about the foundations of mathematics. (Cf. [69]) In spite of the fact that as early as 29 July 1910 he would write to Piet Mulder in Holland (I owe this quote to Dirk van Dalen): "I have recently thought about the foundations of set theory and were led to views which diverge rather strongly from Zermelo's, coming close in a certain sense to the point of view of Borel and Poincaré which is generously derided around here" (i.e., in Göttingen), his *Habilitation* lecture that same year, "On the defintion of the fundamental notions of mathematics", did not come to a very skeptical conclusion about the possibility of founding all of mathematics on set theory, but rather ended on a note which for me is typical of the first period of Weyl's thinking: "May we say—as is suggested by what we have developed—that mathematics is the science of ϵ [i.e., the element relation in set theory] and of those relations which can be defined from this notion via the principles discussed? Maybe such an explanation does actually determine mathematics correctly *as for its logical substance*. However, I see the proper value and the meaning proper of the system of notions of logicised mathematics thus constructed in that its notions may also be interpreted *intuitionwise* without affecting the truth of the statements about them. And I believe that the human spirit has no other way to ascend to mathematical notions but by digesting the given reality." ([91], p. 304) And Weyl would play on the same theme in the 1913 preface to his book *Die Idee der Riemannschen Fläche* [88]. In this book, Weyl famously rewrote Riemann's ideas on the basis of an abstract notion of two-dimensional manifold ([88], §4), and used very recent analytic results to secure the existence of functions via Dirichlet's principle. (A slightly different axiomatic description of a topological manifold, also in terms of neighbourhoods, was conceived independently at about the same time by Hausdorff—see the comments in [39], pp. 712–718.) One key message from Weyl's preface is that, for reasons of rigour, there is no alternative to building up very technical, abstract theories, even though one has to be aware of the fact that this necessity has "also brought about unhealthy phenomena. Part of the mathematical production has lost .... the connection with the living stream of science." Therefore, "... to grasp what accounts for the life, the true substance, the inner value of the theory: for this a book (and even a teacher) can only provide scanty indications; here everyone has to wrestle himself afresh to gain understanding."

In other words, Weyl suffered from the apparent incompatibility between the human, intuitive, ideal core of mathematics, and the artificial scaffolding we have to erect in order to obtain a sound scientific theory. But he is not prepared yet to move the holistic reaction against this difficult, potentially inhuman state of affairs into the formal mathematical work. The holistic conception remains an individual task to be mastered beyond and in spite of the modern, distorting presentation of the theory. The dilemma is relegated to prefaces or concluding exhortations.

World War I would change this, but at first in the direction of an even bigger divide between rigorous mathematics and the human mathematical activity. This is characteristic of the second phase in Weyl's thinking about the foundations. In 1913, Hermann Weyl married Helene Joseph in Göttingen and was appointed professor at ETH Zürich. However, in May 1915 he was drafted into German military service. It did not involve actual fighting though, just a stay at a garrison near Saarbrücken. An article on Riemann surfaces that would appear in 1916 ([91], pp. 600–613) was written there without access to mathematical literature. A year later, the Swiss authorities managed to obtain his release from his military duties, the Weyls returned to Zürich, but work did not go on where Weyl had left it. In 1916, Weyl started reading J.G. Fichte and Meister Eckart with a philosopher colleague in Zürich, Fritz Medicus. Meanwhile the courses that he taught indicate his new orientation: In the summer of 1917 he lectured on *Raum, Zeit, Materie*, and in the following winter on the *Logical foundations of mathematics*. ([27]) These courses gave each rise to a book published in 1918: [89] and [90]. And it is the latter which reflects the war experience within the foundations of mathematics.

Mathematically, the *Kontinuum* [90] constructs a viable but deliberately poor version of the continuum which systematically and carefully avoids all impredicative definitions, i.e., all quantification over sets of primitvely defined objects. (See Solomon Feferman's analysis in his article "Weyl vindicated" in [25], pp. 249–283, which elaborates on the surprising logical efficiency of Weyl's poor analysis.) Since the notion of upper bound of an infinite set of real numbers, analysed in terms of Dedekind cuts, involves such a higher order quantification, the existence of an upper bound cannot be proved for *every* bounded set in Weyl's poorer continuum, although it can be established for denumerable subsets of real numbers. Two lines of philosophical and rhetorical arguments stand out in Weyl's book: (i) the claim that the uncontrolled, impredicative usage of Dedekind cuts introduces a vicious circle into analysis, and that in order to prevent that theory from falling to pieces, one has no choice but to be content with the poorer continuum presented in this book; (ii) discussions of the problematic applications of the poor continuum to physics, i.e., to an arithmetized model of space-time.

Point (i) was criticized already at the time—see Weyl's reply to a letter from Otto Hölder [92], pp. 43–50—and until very recently; Paolo Mancosu, for instance, expressed the opinion that Weyl's contention "is a far cry from pointing out a vicious circle in the foundations." ([54], p. 75) His Zürich colleague Georg Pólya apparently did not believe in the vicious circle either, as is confirmed by a wager between Weyl and him, which other Zürich colleagues signed as witnesses, dated 9 February 1918 (see [65], p. 15, for the original text). As is to be expected from the book [90], the text of the wager confirms that Weyl at the beginning of 1918 saw no way of proving along traditional lines the existence of the precise upper bound of a bounded set of real numbers. But the wager went beyond the book in making predictions about the rewriting of the foundations of mathematics to be expected over 20 years, until 1937. Here Weyl seems prepared for coming research which could produce new, precise theories of the continuum where the existence theorem of the upper bound would not hold in general. If, however, that general existence theorem could actually be established by 1937 in a rigorous way—without any *circulus vitiosus*—then this would only be possible because of a truly original rewriting of the foundations in a way impossible to imagine now, i.e., in 1918.

An illuminating perspective on Weyl's ideas at the time can be gleaned from a postcard he wrote to Pólya on 29 December 1919. I found it quoted in Reinhard Siegmund-Schultze's thorough analysis of the correspondence between Pólya and Richard von Mises [75], p. 472. In my reading of this postcard, Weyl compares "two things": his own earlier debate with Pólya which led to the wager, and Pólya's ongoing debate with Richard von Mises about the latter's foundations of probability theory. Weyl writes that these two issues are more closely related than Pólya may have originally thought, but that in his ongoing debate with von Mises, Pólya finds himself on the side which corresponds to Weyl's part in their earlier debate about the foundations of analysis. Now Pólya's foremost criticism of von Mises's axiomatics concerned the irregularity axiom for collectives, [56], p. 57, *Forderung II*; see also [61], p. 184, and [40], pp. 825–833. I therefore think that Weyl is alluding to the analogy that, both in the definition of the upper bound of a set of real numbers (given as Dedekind cuts) and in von Mises's second axiom, a property has to be checked for an infinity of sequences or sets of objects satisfying certain requirements. If Pólya finds this "mathematically not viable" (*mathematisch untauglich*, as he writes to von Mises, [75], p. 501), then this strikes Weyl as very much analogous to his own criticism of a vicious circle on the ground that the notion "property of rational numbers" is not extensionally definite (*umfangsdefinit*, [92], p. 45). A much later rewriting, from the 1960s, of the theory of collectives at the hands of Kolmogorov and in terms of the algorithmic complexity of subsequences would resuscitate the theory of collectives in a new mathematical outfit. ([61], pp. 233–237) As for the rewritings of the foundations of the continuum predicted by Hermann Weyl, we shall encounter one anon.

We have seen that Weyl's claim of a vicious circle met with various ciriticisms. But I am reading his line of thought (i) in the book on the continuum as a reaction to the abysmal cultural experience of World War I, transposed into the problems about the foundation of mathematics. The way I see it, Weyl was closing the shutters because of the storm outside. He had been reading Fichte's relentless scrutiny of the act of judgment and the potential evidence provided by intellectual intuition which Fichte construed in analogy with proofs by geometric construction ([90], p. 2). Fichte was also the author of the *Reden an die deutsche Nation* which had helped to rally resistance against the French troops in 1807–1808. I conjecture that, in a similar vein, the return to the rock-bottom of absolute evidence in the face of potentially shaky foundations, and the restriction to tightly controlled methods of object construction was for Weyl a natural rejoinder to a war whose visible effects were increasingly hard to reconcile with the origins of the civilization that had unleashed it. This part of the book was not a move towards a more holistic, humane way of doing mathematics; it was a rescue operation, faced with a world which was threatening to go to pieces.

But the second line of thought (ii) mentioned above went beyond the immediate purpose of saving a minimal secure form of analysis. Here Weyl took stock of just how far mathematics had gone astray as a consequence of its modern development: "If we make precise the notion of set in the way here proposed then the claim that to every point on the line . . . correspond a real number, and vice versa, acquires a profound content. It establishes a peculiar link between what is given in our intuition of space and what is construed in a logical-conceptual manner. But this claim obviously leaves entirely the scope of what intuition teaches us or may teach us about the continuum; it is no longer a morphological description of what intuition offers us . . . ." ([90], p. 37) And on p. 68: "It is the great merit of Bergson's philosophy to have emphasized this profound alienation of the world of mathematical concepts from the immediately experienced continuity of the phenomenon of time." The security of sound foundations is thus obtained in *Das Kontinuum* at the high price of violating even more our intuition of space and time. The continuum is all but holistically satisfying for the mathematician-physicist Hermann Weyl. Seeing no way out of this dilemma during the war, he resigned himself to sketching the principles of physical applications of the poor continuum. To even start doing this, to determine a point, one has to refer to a coordinate system: "The coordinate system is the inevitable residue of the annihilation of the ego in that geometrical-physical world which reason carves out of what is given under the norm of 'objectivity'; the last meagre symbol even in this objective sphere for the fact that existence is only given and can only be given as intentional content of the conscious experience of a pure, sense-creating ego." ([90], p. 72) Never did the reality of space seem further from our mathematical models of it to Hermann Weyl than at the end of World War I. The choosing of a point, i.e., the very beginning of the soulless arithmetization of what once was a lived intuition, is the only act remaining to remind the mathematician-physicist of his creative self.

Meeting Brouwer in the Engadin in the Summer of 1919 liberated Weyl from this dehumanized, atomistic mathematical universe and started the short period, his third, during which he believed in holistic analysis. The articles he wrote to propagate this view are full of political metaphors reflecting the collapse of the German empire as well as the ensuing revolution and inflation; in this way Weyl's holistic turn is made to reflect, to match the historical moment. These passages are well-known or easy to find. Let me rather quote from the holistic rhetoric here: "Mathematics is, as Brouwer occasionally puts it, more of an activity than a doctrine. ... Brouwer's view ties together the highest intuitive clarity with freedom. It must have the effect of a deliverance from a nightmare for whoever has maintained any sense for intuitively given facts in the abstract formalism of mathematics." ([92], p. 157/179) "The ice cover has burst into floes, and now the element of flux was soon altogether master over the solid. L.E.J. Brouwer sketches a rigorous mathematical theory in which ... the continuum is not conceived as a rigid being but as medium of free becoming. With this we also regain our freedom as concerns number sequences and sets of numbers. We no longer try to gain a yes or no answer ... by stretching the sequences on the Procrustean bed of construction principles. With Brouwer, mathematics gains the highest intuitive clarity ... " ([92], p. 528/530)

The technical gadget which Weyl received from Brouwer were the choice sequences, *Wahlfolgen*, which in general are eternally in the making; only finite beginnings of them can be considered given. A point is defined by a choice sequence of natural numbers that encode nested intervals: "The whole admits parts" replaces the principle that a set has elements. ([92], p. 177) "The continuum appears as something which is infinitely in the making inside." ([92], p. 172) A precise point, for instance $x = 0$, does not cut the continuum in two, because whether an arbitary other point is or is not equal to x may be undecided. The new continuum is uncuttable. When we want to study a function on it, we have to "hover over" the new continuum, because we cannot "sit down" on a arbitrary point of if. ([92], p. 179) If ever there was 'reenchanted mathematics', an enchanted continuum—in analogy with the title of [36]—this is one. Considering how much more readily a biologist or a neurologist can deliver scientific verdict on organic connections or expressions of life, Weyl's "medium of free becoming", as he calls the intuitionist continuum, strikes me as a remarkably coequal holistic notion for a mathematician. (Cf. [36], pp. xxviii–xix)

Weyl's allusions to the postwar situation place his holistic articles in a period of time which was favourable for holistic writers, at least in Germany. The following is an extract from a petition of his students in Zürich dated 6 May 1920 which was written in order to prevent Weyl's leaving Zürich for Göttingen or Berlin: "Our conviction that Herr Prof. Weyl is irreplaceable has its source in the following reasons: We admire in him the ingenious creator of new cultural values which consist in that the exact sciences come into fruitful interaction with life itself. It is this exceedingly fortunate fusion of the man and the scholar in Herr Prof. Weyl which inspires in each one of us a sense of liberation ... and seems to us to guarantee most surely that whole men will emerge from the eighth section", i.e., the Mathematics Institute. ([27], pp. 43–44)

Possibly the most far-reaching consequence that Hermann Weyl was seriously considering in pursuing his holistic mathematics was the *inherently probabilistic universe*. In a way, Weyl carried the comparison he had made in his postcard to Pólya over to his continuum based on choice sequences, which begin to look like random variables: "the quantitative data in a piece of the (space-time) world S are known only approximately, with a certain margin, not only because my sensory organs have limited precision, but *they are affected in themselves by such a vagueness.* ... the future will continue working on the present; the past is not terminated. This lifts the rigid pressure of natural causality and opens up—irrespective of the validity of the laws of nature—a *space for autonomous decisions which are causally totally independent from each other* and which according to me take place in the elementary quanta of matter." ([92], p. 121–122; cf. p. 173; cf. [61], p. 68–70)

This may remind one of Paul Forman's old and oft debated thesis [26] to the effect that German physicists let go of traditional deterministic principles after World War I in order to accommodate to the Weimar Republic whose cultural climate was hostile to traditional scientific ideas such as determinism and whose societal reorganisation threatened the academic elite. I have not worked on such a grand scale. I have been following Hermann Weyl's individual path and found the more literary passages in his works explicit enough to link them to the war, resp. to the postwar period. His being in tune with the historical events surely helps to explain the students' petition. On the other hand, Weyl seems himself only half convinced that probabilism is really a corollary of his "medium of free becoming", i.e., of the holistic continuum based on choice sequences. ([92], p. 122, footnote) And he does not seem to take up this hypothesis again in later articles. So Weyl may well be an individual case matching Forman's old thesis. But I find it more remarkable that holism and probabilism are tentatively linked by Weyl via the notion of choice sequence.

At any rate, Weyl's holistic continuum was a fairly ephemeral phenomenon. In fact, Weyl's partisanship for the intuitionism lasted less than 10 years, and even during this time, his mathematical research outside of the foundational articles shows hardly any sign of intuitionist practice. Also, in the second half of the 1920s he tried to steer a mediating course between Brouwer and Hilbert. The reasons for Weyl's final abandonment of Brouwer's cause are not clear and deserve further historical investigation. Mancosu [54], pp. 80–81, discusses this completely from the point of view of the relationship with Hilbert. Epple [24] has suggested that intuitionism itself simply did not manage to live up to the high standards of proof that it called for. Remembering the frequent interaction of holistic ideas with the ambient cultural and political climate, and the fact that for Hermann Weyl, unlike other holistic scientists, taking the Nazi turn in the 1930s was never an option, for both political and personal reasons, the course of general history may also have contributed to the brief duration of Weyl's holistic mathematics.

5. Generic Points

Still in the holistic vein and trying to address as large a part of the mathematical community as possible, still treating foundational issues at the end and suggesting a mediation between Brouwer's and Hilbert's programmes, Hermann Weyl published in 1924 an article "Marginal notes on main problems of mathematics" ([92], pp. 433–452) which revisited, in a new and transparent presentation, a few problems that, according to Weyl, interest "all those who deserve to be called mathematicians, in essentially the same way." Solomon Lefschetz from Princeton reacted to this project in a letter of 30 November 1926 (HS 91:659 in the Archives of ETH Zürich): "...For any sincere mathematical or scientific worker it is a very difficult and heartsearching question. What about the young who are coming up? There is a great need to unify mathematics and cast off to the wind all unnecessary parts leaving only a skeleton that an average mathematician may more or less absorb. Methods that are extremely special should be avoided. Thus if I live long enough I shall endeavor to bring the theory of Algebraic Surfaces under the fold of Analysis and An.[alysis] Situs as indicated in Ch. 4 of my Monograph [52]. The structure built by Castelnuovo, Enriques, Severi is no doubt magnificent but tremendously special and requires a terrible 'entraînement'. It is significant that since 1909 little has been done in that direction even in Italy. I think a parallel edifice can be built up within the grasp of an average analyst."

So Lefschetz was ready to rewrite Algebraic Geometry, or more precisely the major Italian work in Algebraic Geometry, i.e., above all the classification of algebraic surfaces, in his topologico-analytical approach. Lefschetz felt that in this way Algebraic Geometry could be reconnected to the hard core of mathematics. Note that such a rewriting would not amount to an algebraization of the Italian body of knowledge. About ten years later, when the founding fathers of Bourbaki started working towards their encyclopedic project, a few established sub-disciplines of mathematics, specifically probability theory and algebraic geometry, were still often thought (if not by the Bourbakists themselves) to be not amenable to insertion into a project like the *Eléments*, built on axiomatics starting with (logic and) set theory. The "terrible *entraînement*" needed to penetrate work of the Italian school, as Lefschetz had felt, was thought to be due to some specific intuition employed in this discipline, which would make it not reducible to logic and set theory. ([86], p. 555) As is well-known, the sub-discipline of Algebraic Geometry was in fact completely rewritten and remodelled, essentially between 1925 and 1950, by various mathematicians, and not within the Bourbaki project although Bourbaki members did play an important role, notably Weil [85]. To conclude my talk I would like to briefly discuss ways to describe this major rewriting historically. Before doing so, however, let us make sure that what we are talking about really makes sense.

First of all, that a mathematical sub-discipline of Algebraic Geometry with its own history did indeed exist, say around 1930, is documented in particular

- by a string of reports which took stock of the domain: Brill & Noether (1892–93) [11], Castelnuovo & Enriques (1914) [12], Emmy Noether (1919) [57], Snyder *et.al.* (1928/34) [78], Berzolari (1933) [9], Commessati (1932) [14], Geppert (1931) [28];

- by a string of monographs which highlight both the field and a wide range of interactions between different approaches; examples include: Schubert (1879) [70], Picard & Simart (1897–1906) [62], Bertini (1907) [8], Hensel & Landsberg (1902) [42], Severi (1908/1921) [71], Zeuthen (1914) [98], Enriques & Chisini (1915–1924) [22], Lefschetz (1924) [52], Jung (1925) [47], Severi (1926) [73], Coolidge (1931) [13];

- by ongoing production as evidenced for instance in the first volumes of *Zentralblatt* (founded in 1931). Various subsections have to be surveyed here in order to gather all the aspects of the domain we would like to trace, also in anticipation of the later rewriting: in the first place those for algebraic geometry, algebraic surfaces, algebraic curves, birational transformations; and then increasingly also sections on the theory of fields and rings. The rewriting that has taken place since can also be judged from the fact that certain authors stood out in the early thirties as particularly prolific in the bibliographical record whom the memory of the community has not conserved according to the number of their publications, Lucien Godeaux for example;

- by Hilbert's fifteenth problem: rigorous foundation of Schubert's calculus of enumerative geometry. Not only was this a problem in the domain of Algebraic Geometry, but is was a foundational problem, of which Severi for instance had admitted in 1912 that it was "something more than just a scruple about exaggerated rigour." [72] In that same paper, Severi reformulated the problem in terms of algebraic correspondences which considerably enhanced its link with ongoing work in the field.

Furthermore, speaking of the *Italian school of algebraic geometry* also makes good historical sense because, after a strong initial contribution by Alfred Clebsch, Max Noether, as well as Alexander v. Brill and Paul Gordan, the main development—important foreign influence notwithstanding, for instance by Emile Picard—did lie in the hands of Italian mathematicians such as—apart from the three names mentioned by Lefschetz—Eugenio Bertini, Pasquale del Pezzo, Corrado Segre, Beppo Levi, Ruggiero Torelli, Carlo Rosati. These Italian mathematicians formed a social web and often published in not very international Italian journals. ([76], pp. 100–104) At least until the early 1930s, Italy was the place for many to go and learn Algebraic Geometry. Finally, by the 1930s, there was one uncontested leader governing the school: Francesco Severi after his fascist turn, and finally director of the newly founded *Istituto Nazionale di Alta Matematica* inaugurated on 15 April 1940. ([34], *passim* and in particular p. 272)

So who was attacking, or approaching, from where and how this international sub-discipline, and in particular its Italian branch, with a view to rewriting it? Lefschetz's monograph [52] already contained such a partial topological rewriting, concerning algebraic surfaces and correspondences on curves. This lead was followed by Oscar Zariski's papers on the fundamental group mostly from the 1920s, and by Bartel L. van der Waerden's topological solution of Hilbert's 15$^{\text{th}}$ problem from 1929, which used intersections in the homology ring of the ambient variety. ([67], pp. 260–264)

But arguably the first attempt at an explicit *refoundation* of Algebraic Geometry grew out of Emmy Noether's work on the ideal theory of rings, and was published by van der Waerden in 1926 where we read the lines that were presumably written by Emmy Noether herself: "The rigorous foundation of the theory of algebraic varieties in n-dimensional spaces can only be given in terms of ideal theory, because the definition of an algebraic variety itself leads immediately to polynomial ideals. Indeed, a variety is called algebraic, if it is given by algebraic equations in the n coordinates, and the left hand sides of all equations that follow from the given ones form a polynomial ideal. However, this foundation can be formulated more simply than it has been done so far, without the help of elimination theory, on the sole basis of field theory and of the general theory of ideals in ring domains." ([67], p. 251) From this resulted a new notion of point on an (affine, say) algebraic variety which van der Waerden called *allgemeine Nullstelle*, i.e., a general zero (of a set of algebraic equations).

Here is in essence van der Waerden's simple observation (for a more complete analysis of this paper, see [67]): If K is a field and $\Omega = K(\xi_1, \dots, \xi_n)$ a finitely generated extension of it, then all polynomials f in $R = K[x_1, \dots, x_n]$ such that $f(\xi_1, \dots, \xi_n) = 0$ form a prime ideal $\wp$ in R, and Ω is isomorphic via $x_i \mapsto \xi_i$ to the integral domain $R/\wp$. Conversely, given a prime ideal $\wp$ in R, then there exists an extension field $\Omega = K(\xi_1, \dots, \xi_n)$ of finite type such that $\wp$ consists precisely of the polynomials f in R such that $f(\xi_1, \dots, \xi_n) = 0$; indeed, it suffices to take $\xi_i = x_i \pmod{\wp}$ in $R/\wp$. Such a system $(\xi_1, \dots, \xi_n)$ in an extension field of finite type of K is called a general zero of the ideal $\wp$, or a *general point* of the variety in affine n-space over K defined by the prime ideal $\wp$. Even though all this looks extremely elementary today, the definition, together with the notion of specialization, i.e., van der Waerden's *relationstreue Spezialisierung*, is one of the central notions of the algebraic rewriting of algebraic geometry in the 1930s and 1940s. Proofs of theorems in the rewritten algebraic geometry typically involve choosing general points of all varieties with which one has to work.

Significantly, van der Waerden when defining these general points also offered a bridge linking them to the traditional terminology of algebraic geometers saying that the general point just defined "…agrees with the meaning that the words general and special have in geometry. Indeed, by a general point of a variety, one usually means, even if this is not always clearly explained, a point which satisfies no special equation, except those equations which are met at every point. For a specific point of M, this is of course impossible to fulfil, and so one has to consider points that depend on sufficiently many parameters, i.e., points that lie in a space $C_n(\Omega)$ [affine n-space], where Ω is a transcendental extension of K. But requiring of a point of $C_n(\Omega)$ that it be a zero of all those and only those polynomials of $K[x_1, \dots, x_n]$ that vanish at all points of the variety M yields precisely our definition of a general point of the variety M." In other words, van der Waerden claimed that he was really only rewriting in modern algebraic language what Italian geometers for instance had meant. He also said that the traditional literature was not particularly clear on this.

Traditional Algebraic Geometry had been particularly rich in all sorts of points: apart from just plain points, there were infinitely near points of various orders, intersection points of varying order, virtual double points, etc., and there were what the Italians called *punti generici*, a word that A. Weil in [85] imported into English as "generic point", but with the precise mathematical meaning of van der Waerden's general zero. The question that arises from our last quote from van der Waerden is how well the algebraic rewriting captures what is being rewritten. Let us look at a correspondence between the two actors who would finally impose the new Algebraic Geometry by the end of the 1940s, Oscar Zariski and André Weil (from the Zariski papers in the Harvard Archives). Both of them were using van der Waerden's general points, but Zariski called them 'general', Weil 'generic'.

On 25 March 1952, Weil writes to Zariski:

> ... I wonder whether it is not too late to persuade you to reconsider the use of the words 'general' and 'generic.' Any unnecessary discrepancy between our terminologies is bound to accentuate the all too prevalent impression that there is a sharp cleavage between your work and mine, which is simply not true. When I selected 'generic', I certainly was not unaware of the fact that 'generale' is quite as good Italian as 'generico'. But I don't think that the Italians ever gave a sharp definition for either word; they just used them loosely. I adopted 'generic' because it is a less common word than 'general', both in French and in English, and therefore seems to lend itself better to a strictly technical meaning. One does not need two words, I contend: some points are (in my sense) 'generic', relatively to a given field, i.e., 'general' in your sense; but no point is generic in your sense. If I understand you right (from your remarks in your Congress lecture), what you mean when you say that a property P holds 'at a generic point' seems to me to be much better expressed by saying that P holds on an open set (in your topology), or (as Seidenberg does) by saying that P holds almost everywhere. I doubt very much whether the Italians ever differentiated sharply between the two concepts. As you have seen them at much closer quarters than I ever did, I am willing to take your word as to what they thought about this or that; but this is psychology, not mathematics; and I do not think that it need bother us. What is far more important is not to create unnecessary difficulties to young people who are now trying to learn algebraic geometry from your work and from mine. ... Maybe you will ask why I don't adopt the sim-

> ple remedy of changing over to your terminology. Now: a) if I had found 'general' in common use in a well-defined technical sense, I should certainly not have tried to change it; this not being the case, I decided upon 'generic' for the reason indicated above, which is not a very strong one, I admit; b) . . . c) having, for the punishment of my sins, written and published a book, I am far more committed to my terminology than you who are yet to publish yours and therefore still enjoy far greater freedom in such matters.

Zariski's answer to Weil is dated 29 March 1952:

> I hope that you will not hold it against me if I say that you have not convinced me on the evidence in re general versus generic. I claim that from the work of the Italians it appears quite clearly (and objectively, not just as a matter of psychological interpretation) what they meant by the term 'generic.' Next I claim that, without reading a single line of the Italian papers but just using the fact that in the Italian school the ground field and the coordinate field were identical, namely the field of complex numbers, one must conclude with the corollary that their generic point could not possibly be the same thing as the 'allgemeiner Punkt' of van der Waerden. Finally, it is not quite true that no point is generic in my sense. I agree that no point is generic (in my sense) in itself, just as no point is generic (in your sense) in itself. Incidentally, I notice that also outside of algebraic geometry (for instance in function theory) mathematicians begin to use the term generic, and obviously not in your sense. . . .

Here Zariski could have gone to his bookshelf and quoted from [22], p. 139: "The notion of a *generic* 'point' or 'element' of a variety, i.e., the distinction between properties that pertain *in general* to the points of a variety and properties that only pertain to *exceptional* points, now takes on a precise meaning for all algebraic varieties. A property is said to pertain in general to the points of a variety V_n, of dimension n, if the points of V_n not satisfying it form—inside V_n—a variety of less than n dimensions." We see that the authors of this quote tacitly assume that the exceptional points form a subvariety; this point will be raised incidentally in Weil's answer of 15 April 1952:

> Dear Zariski, I have no remarks of mathematical interest to make at the moment, but I want to express my renewed doubts about what the Italians are supposed to have meant by 'generic'. Your arguments, purporting to show that they meant it in your sense, would indeed be decisive if they had been logical thinkers in such matters; but in that case they would have defined the word, and there would be no controversy. It is a plain fact (as again emphasized, and quite rightly, by Chevalley in a review of some article by Severi [74] in

> Math. Rev. last year, I think) that the Italians were of the opinion that every proper subset of a variety which is defined by algebraic geometric means is a union of subvarieties; this belief alone accounts for their obstinate contention that they knew all about the Chow coordinates, when in fact the main theorem to be proved there (viz., that there is an algebraic set, every point of which is the Chow point of some cycle of the given dimension) is entirely missing from their work. This clearly means that they were *essentially unable* to distinguish between your sense of the word 'generic' and mine. What they would do, of course, is to *prove* that a generic point in my sense has a certain property, and to *conclude* that a generic point in your sense has that property. Presumably I have been paying more attention to their proofs, and you to their statements, so that we may well both be right. Also, the argument based on the fact that their ground field and coordinate field were identical (viz., the complex numbers) would be valid only if they had thought clearly on these subjects. Not only with them, but in the greater part of classical mathematics, a 'variable' is essentially a transcendental element over the field of complex numbers, even though it is never defined that way but usually as 'an arbitrary complex number'; it follows that classical mathematics, including of course Picard and the Italians, is full of contradictions which cannot be disentangled unless one reinterprets the word 'variable' as I have just said. As those people were no fools, one must conclude that they had some obscure notion of "a transcendental element over complex numbers" but lacked the algebraic language to express it. . . .

Do you want me to tell you who is right? That's easy: both are right (says the historian). The substantial difference between them is their relationship to Italian Algebraic Geometry.

Oscar Zariski—born in 1899 as Ascher Zaritski in the small town of Kobrin, then Russia, as of 1921 Poland, today Belarus—managed to go to Italy to study in 1921 and was trained in Rome, then the world center for Algebraic Geometry. Lefschetz had been visiting there before Zariski's arrival; Severi transferred to Rome in 1922. Zariski got his doctorate with Castelnuovo in 1924 and worked also for the philosophically enclined Enriques, preparing for instance an Italian translation of Dedekind's foundational writings, in particular of [17], with extensive commentary. Since he was not a naturalized Italian, university positions were closed to him. After two postdoc years in Rome on stipends of the Rockefeller Foundation, Castelnuovo obtained through Lefschetz for Zariski to go to the US, at first to Baltimore. Not surprisingly in view of Lefschetz's letter quoted above, Zariski published in 1928 a paper "On a theorem of Severi" [93] where he criticized a proof that Severi had given in 1913, and proposed a topological approach instead. He took the measure of his former masters on a much bigger scale in his 1935 *Ergebnisse* volume on Algebraic Surfaces [94] where the typical comments one finds are of the following sort. p. 18: "It is important, however, to bear in mind that in the theory of singularities the details of the proofs acquire a special importance and make all the difference between theorems which are rigorously proved and those which are only rendered highly plausible." p. 19: "In regard to the "accidental" singularities introduced by the quadratic and the monoidal transformations and in regard to the manner in which they should be eliminated by birational transformations, Levi's proof is not sufficiently explicit." p. 20/21: "... What matters, however, and is essential for the application which Severi makes of this lemma is that ... Hence the above formula is not correct. Since the composition indices are not diminished by projection, we can only write ... ", and so on.

During his stay at the Institute for Advanced Study, Princeton, in 1935–1936 he came into contact with modern algebra and in particular read Wolfgang Krull's works. Building on this, he managed between 1937 and 1947 what he called himself an *arithmetization* of Algebraic Geometry. One of the basic ideas was to define points on what he called—alluding to Dedekind and Weber from a vastly more general situation—the arithmetic "Riemann surface" attached to a polynomial ring (or a ring of formal power series) by looking at valuations with general value groups (generalizing in this way the discrete rank one valuations associated to any of Dedekind's and Weber's points). In this way he managed in particular to build "an arithmetic theory parallel to the geometric theory of infinitely near points" on a smooth algebraic surface. ([95], p. 14) Other big achievements of this period include: the definition of the *normalization* of a projective variety, the resolution of singularities of surfaces (two different proofs) and threefolds, and the Zariski topology. ([95], [96], [59], [77], [76])

Zariski brought with him from Rome the training and the central problems in Algebraic Geometry. When the experience of his *Ergebnisse* volume suggested the necessity to rewrite a good deal of this corpus of knowledge, he was open and creative enough to use completely different methods—those which Krull had defined as properly "arithmetic" in his personal terminology ([51], p. 746, footnote 2)—but he would never betray the language in which he had first discovered the world of Algebraic Geometry. If a generic point was a complex point in general position back in Rome, it would still be so in the US. And when he introduced the concept of a normal projective variety—which would subsequently give him the process of normalization and thereby a completely new desingularization procedure, practically impossible to reconstruct in the language of Italian Algebraic Geometry—Zariski chose this very word 'normal' in analogy to a traditional terminology, as if this could smoothen the transition. ([95], p. 112; cf. [77]) This shows that the casting of a rewriting is influenced by *allegiances* (in the broadest sense). Zariski's allegiance was with his Rome education. When he was helping his Dutch colleague Kloosterman to organize the symposium on Algebraic Geometry at the Amsterdam ICM in 1954, he commented on a preliminary list of invited speakers: "There are several names I would add to your list. I am particularly worried by the omission of the name of

Severi. I think that Severi deserves a place of honor in any gathering of algebraic geometers as long as he is able and willing to attend such a gathering. We must try to avoid hurting the feelings of a man who has done so much for algebraic geometry. He is still mentally alert, despite his age, and his participation can only have a stimulating effect. I think he should be invited to participate." (Zariski to Kloosterman, 15 January 1954) But to be sure, as we have seen, the allegiance to his Rome education had long ceased to constrict Zariski's methods; it concerned a tradition, not a working environment. In Italy during the 1930s it was impossible to openly criticize Severi; ever since he had arrived in the US, Zariski enjoyed and used the freedom from this sort of allegiance and constraint.

André Weil's allegiances are less easy to detect and to describe. He had also been to Rome on a Rockefeller stipend, but only briefly. For the Amsterdam ICM, André Weil would negotiate wih Kloosterman a special session, within the Algebraic Geometry Symposium, on equivalence relations for algebraic cycles. This was first prompted by the announcement of Segre's ICM lecture (Weil to Zariski, 19 January 1954), but would finally result in a direct showdown between Severi and Weil on the subject. The ensuing voluminous correspondence between Severi, van der Waerden and Pierre Samuel (preserved in the van der Waerden papers at ETH Zürich) was finally condensed in an article printed in the ICM Proceedings, but Severi would carry his grief about Weil's attack in the discussion after his talk for years; see [5].

Weil was probably the most widely read of the Bourbaki members at the time; Bourbaki's historical endnotes [6] were his idea and many of them supplied by him. The argument developed in his second letter quoted above is perfectly compatible with the philosophy of these notes; the rewritten, the modern mathematical notion looks for subsumable elements in older texts. And the older literature does indeed speak routinely about moving points on a variety which are mapped somewhere etc. An element which is transcendental over the ground field can model this. It is not that Zariski is the more careful historian of the two; he just refuses to let new terminology interfere with ways of formulating to which he is attached. Weil had no such specific allegiance with the Italian school. For him this was one of several corpusses of texts from the recent history of mathematics with which he had gained a certain familiarity. He undoubtedly had an allegiance to the group of collective individualists ([32]) Bourbaki of which he had been a cofounder. This can be seen inside the history of the rewriting of Algebraic Geometry by following Weil's and Claude Chevalley's respective contributions to it; Chevalley is also mentioned in the above letter. On a less personal note, Weil's allegiance to Bourbaki is reflected in the format of his book [85]. It was one of the first unmistakably bourbakist books that appeared, even though it was not part of the *Eléments de mathématique.*

The word allegiance is unsuitable to describe the relationship between Weil and Zariski. But the evolution of this relationship and the way in which their two individual projects grew—without ever merging completely—into something that was finally perceived as one rewriting of Algebraic Geometry can be followed thanks to their correspondence.

There are different ways to tell the story of a rewriting. Part of the work is of course to follow the mathematical details of published papers and available correspondence carefully. I have been doing this for several years now concerning the rewriting of Algebraic Geometry in the 1930s and 1940s. One could think that this would do the job, all the more so as the number of rewriters in that period is not big, less than ten, and their works—even when crossed with quoted literature and with the considerable resilience of the Italian school, esp. through Severi's amazing production in the 1930s—are in principle surveyable, and since the rewriting took place under the motto of new rigour, the new methods, notions and objects brought into play are relatively easy to recognize and to describe mathematically. But working on this, one notices that the history of the phenomenon in the large cannot be captured in this fashion. A better historical account on the scale of this whole rewriting of Algebraic Geometry emerges by describing allegiances. This notion allows to treat factors like methodological preferences due to established values, personal respect or the relationship between teacher and student, academic power, political agenda, and others all at once. The picture obtained in this way is something like a graph, with a small number of actors with surprisingly few coalitions among them, but definite power flows along the various edges.

In [67] for instance, I have followed van der Waerden's seemingly erratic course in his long and rich series of articles on Algebraic Geometry. It falls into place in terms of his allegiances: He first became interested in Algebraic Geometry through a lecture by Hendrik de Vries at the University of Amsterdam on Schubert calculus. In Göttingen, he was part of the group around Emmy Noether (and Emil Artin in Hamburg); his first paper, where his general points are defined, was written in that situation, in particular it was written from outside of the Algebraic Geometry community. After meeting Severi in 1932, he drastically reduced the level of algebraic abstraction in his papers and used geometric intersection constructions which were due to Severi. But he could never take advantage of the friendly course he was steering with respect to his influential Italian colleague because his enemies in the Nazi administration made it impossible for him to travel abroad. For the same reason, he had to be careful when dealing with Hasse because of the latter's political influence in Germany until 1945; I have documented [67], p. 274, a case where, against Hasse's wish, van der Waerden did not publish a 1941 proof he had done in response to a query from Hasse; but after the war he used this proof to criticize Hasse's notion of point in the very article which he had helped Hasse to complete.

Helmut Hasse's more active interest in Algebraic Geometry goes back to Deuring's 1936 programme for proving the analogue of the Riemann Hypothesis for all algebraic curves over finite fields. His contact with Severi was more political than mathematical and started late, in the Spring of 1937. The projected axis of collaboration between the German algebraists and the Italian geometers, which they wanted to be parallel to the political axis between the two fascist states, hardly got off the ground. ([68])

The triangle van der Waerden — Severi — Hasse thus appears to have functioned in a way which effectively hampered the constitution of a new joint European research practice in Algebraic Geometry, in spite of the substantial string of papers *Zur Algebraischen Geometrie* and the excellent textbook [84] which van der Waerden published from his splendid isolation in Leipzig. The allegiances in the triangle and the political agenda which enforced them already before the war emerge even more clearly if one compares them to the absence of similar constraints which the ex-Europeans in the US—Zariski, Weil, Chevalley—were enjoying during the war. And during the last years of the war, there were hardly any actively competing rewriters in Europe. In this precise sense, the new Algebraic Geometry which would set the standard of the subdiscipline in the 1950s and until Grothendieck's second rewriting, was a product of the second World War, or more exactly of the World Wars, considering 1914–1945 as a single period of world history, marked by global warfare.

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