CHAPTER 11

A Historical Sketch of B. L. van der Waerden's Work in Algebraic Geometry: 1926–1946

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> I am simply not a Platonist. For me mathematics is not a contemplation of essences but intellectual construction. The *Tetragonizein te kai parateinein kai prostithenai* that Plato speaks of so contemptuously in *Republic 527A* is my element.[1]

Introduction

Algebraic geometry might be defined as the treatment of geometrical objects and problems by algebraic methods. According to this *ad hoc* definition,[2] what algebraic geometry is at a given point in history will naturally depend on the kind of geometrical objects and problems accepted at the time, and even more on the contemporary state of algebra. For instance, in Descartes's early seventeenth century, "algebraic geometry" (in the sense just defined) consisted primarily in applying the new algebra of the time to problems of geometrical constructions inherited mostly from antiquity. In other words, the "algebraic geometry" of early modern times was the so-called analytic art of Descartes, Viète, and others.[3]

The discipline which is called algebraic geometry today is much younger. It was first created by a process of gradual dissociation from analysis after the Riemannian revolution in geometry. Bernhard Riemann had opened the door to new objects that eventually gave rise to the various sorts of varieties—topological, differentiable, analytic, algebraic, etc.—which happily populate geometry today. After a

[1] "Ich bin halt doch kein Platoniker. Für mich ist Mathematik keine Betrachtung von Seiendem, sondern Konstruieren im Geiste. Das *Tetragonizein te kai parateinein kai prostithenai,* von dem Platon im Staat 527A so verächtlich redet, ist mein Element." Postscript of Bartel L. van der Waerden's letter to Hellmuth Kneser dated Zürich, 10 July, 1966, [NSUB, Cod. Ms. H. Kneser A 93, Blatt 19]. Van der Waerden begs to differ with the following passage of Plato's *Republic* (as it appears in Benjamin Jowett's translation): "Yet anybody who has the least acquaintance with geometry will not deny that such a conception of the science is in flat contradiction to the ordinary language of geometricians.—How so?—They have in view practice only, and are always speaking in a narrow and ridiculous manner, of *squaring and extending and applying* and the like—they confuse the necessities of geometry with those of daily life; whereas knowledge is the real object of the whole science." The italicized words are quoted in Greek by van der Waerden.

[2] This definition was suggested to me by Catherine Goldstein several years ago to fix ideas in the course of a discussion.

[3] Compare [Bos, 2001].

strong initial contribution by Alfred Clebsch and Max Noether as well as Alexander von Brill and Paul Gordan, the main development—important foreign influence notwithstanding, for instance by the Frenchman Émile Picard—came at the hands of Italian mathematicians such as the two leading figures in the classification of algebraic surfaces, Guido Castelnuovo and Federigo Enriques, as well as Eugenio Bertini, Pasquale del Pezzo, Corrado Segre, Beppo Levi, Ruggiero Torelli, and Carlo Rosati in his earlier works. This—I am tempted to say—golden period of Italian algebraic geometry may be argued to have more or less ended with World War I.[4] Yet, some of the authors, like Rosati, continued to be active and were joined by younger colleagues like Beniamino Segre. The strongest and most visible element of continuity of Italian algebraic geometry, after World War I and into the 1950s, however, was the towering figure of Francesco Severi, whose long and active life connects the golden first period with the following second period. At the end of this second period, Italian algebraic geometry essentially ceased to exist as a school identifiable by its method and production.

Meanwhile on an international scale, the discipline of algebraic geometry underwent a major methodological upheaval in the 1930s and 1940s, which today tends to be principally associated with the names of André Weil and Oscar Zariski. Subsequently, another rewriting occurred under Alexander Grothendieck's influence as of the early 1960s. Both of these twentieth-century upheavals redefined algebraic geometry, changing its methods and creating new types of mathematical practice. The second rewriting, at the hands of Grothendieck, also clearly changed the realm of objects; algebraic geometry became the theory of schemes in the 1960s. In contrast to this, the relevance of new objects for the rewriting of algebraic geometry in the 1930s and 1940s is less marked and depends in part on the authors and papers considered. At any rate, both rewritings appear to have preserved both the objects and the big problems studied in the previous incarnations of algebraic geometry. For example, the resolution of singularities for higher-dimensional algebraic varieties was prominent in Italian algebraic geometry, which claimed to have solved it up to dimension 2, and it continues to arouse interest even today. But new problems were added at the crossroads of history, either inherited from other traditions which had formerly not belonged to algebraic geometry—for instance, the analog of the Riemann Hypothesis for (function fields of) curves over finite fields—or created by the new methods—like Grothendieck's so-called "Standard Conjectures."

In this chapter, I discuss Bartel Leendert van der Waerden's contributions to algebraic geometry in the 1920s and 1930s (as well as a few later articles) with a view to an historical assessment of the process by which a new type of algebraic geometry was established during the 1930s and 1940s. The simultaneous decline of Italian algebraic geometry, its causes and the way it happened, is at best a side issue of the present chapter.[5] However, the relationship between new and old algebraic geometry in the 1930s and 1940s is at the heart of the discussion here, in part because of the interesting way in which van der Waerden's position with respect

[4]This point of view is also taken in [Brigaglia and Ciliberto, 1995].

[5]I plan to treat this in greater detail elsewhere. In fact, the present chapter on van der Waerden sketches only one slice of a larger project to study the history of algebraic and arithmetic geometry between 1919 (Noether's report on the arithmetic theory of algebraic functions in one variable) and 1954 (Weil's well-prepared coup against Severi at the International Congress of Mathematicians (ICM) in Amsterdam), that is, before the advent of cohomological methods in algebraic geometry.

to Italian algebraic geometry evolved in the 1930s (see the section on the years 1933 to 1939 below), but mostly because any historical account of the rewriting of algebraic geometry must answer the question of how the old and new practices related to each other.

A first explanation of this historical process could interpret the dramatic changes of the 1930s and 1940s as the natural consequence of the profound remodeling of algebra in the first third of the twentieth century; such an interpretation is perhaps suggested by the *ad hoc* definition in terms of objects, problems, and methods of algebraic geometry given above and by the fact that this rewriting essentially meant to preserve the objects and problems treated by the Italian authors. In this view, new, powerful algebra was being brought to bear on algebraic geometry, transforming this field so as to bring it closer to the algebraic taste of the day. The decline of Italian algebraic geometry around the same time might then simply express the failure on the part of the Italians to adopt that new way of doing algebra. Within this historical scheme, one would still wish to have a more specific explanation of why the Italian algebraic geometers failed to adapt to the new ways of algebra between the wars; for instance, some thought that algebraic geometry was a discipline separated from the rest of mathematics by a special sort of intuition needed to give evidence to its insights.[6] But even in the absence of this kind of a more detailed analysis, a plain historical mechanism—the adoption of a new algebraic methodology, the roots of which could be studied independently[7]—would be used to account for the rewriting of algebraic geometry in the 1930s and 1940s.

This first scheme of historical explanation would seem *a priori* to be particularly well adapted to an analysis of van der Waerden's contributions because the remodeling of algebra to which we have alluded was epitomized in his emblematic textbook *Moderne Algebra* [van der Waerden, 1930–1931]. Even though its author was but the skillful compiler and presenter of lectures by Emil Artin and Emmy Noether, he would obviously appear to have been particularly well placed to play an important role when it came to injecting modern algebra into algebraic geometry. As we will see in the next section, he appears to have set out to do precisely that. Moreover, main actors of the then modern and new development of algebra were aware of its potential usefulness for recasting algebraic geometry. This applies in the first place to Emmy Noether. As early as 1918, she had written a report for the *Deutsche Mathematiker-Vereinigung* (DMV) on the arithmetic theory of algebraic functions of one variable and its relation, especially, to the theory of algebraic number fields [Noether, 1919] and, in so doing, had complemented the earlier

[6]See, for example, [Weil, 1979, p. 555], where he states that "On the subject of algebraic geometry, some confusion still reigned. A growing number of mathematicians, and among them the adepts of Bourbaki, had convinced themselves of the necessity of founding all of mathematics on the theory of sets; others doubted that that would be possible. Exception was taken for probability, ..., differential geometry, algebraic geometry; it was held that they needed autonomous foundations, or even (confounding the needs of invention with those of logic) that the constant intervention of a mysterious intuition was required [Au sujet de la géométrie algébrique, il régnait encore quelque confusion dans les esprits. Un nombre croissant de mathématiciens, et parmi eux les adeptes de Bourbaki, s'étaient convaincus de la nécessité de fonder sur la théorie des ensembles toutes les mathématiques; d'autres doutaient que cela fût possible. On nous objectait le calcul des probabilités, ..., la géométrie différentielle, la géométrie algébrique; on soutenait qu'il leur fallait des fondations autonomes, ou même (confondant en cela les nécessités de l'invention avec celles de la logique) qu'il y fallait l'intervention constante d'une mystérieuse intuition]."

[7]These *have* been studied independently. See, for instance, [Corry, 1996/2003].

report of 1892–1893 by Alexander Brill and her father Max Noether [Brill and Noether, 1892–1893]. Noether had also actively helped introduce ideal-theoretic methods into algebraic geometry in the 1920s, in particular via her rewriting of Hentzelt's dissertation [Noether, 1923a] and her article on "Eliminationstheorie und allgemeine Idealtheorie [Elimination Theory and General Ideal Theory]" [Noether, 1923b], which inspired the young van der Waerden's first publication on algebraic geometry.

As we shall see below, however, this first scheme of explanation, according to which modern algebra is the principal motor of the process, does not suffice to account for van der Waerden's changing relationship with Italian algebraic geometry, let alone serve as an historical model for the whole rewriting of algebraic geometry in the 1930s and 1940s. Not only is the notion of applying modern algebra to algebraic geometry too vague as it stands, but following the first scheme carries the risk of missing the gossamer fabric of motivations, movements, and authors which renders the historiography of the first rewriting of algebraic geometry in the twentieth century so challenging and instructive.

Another explanation of this historical process, several variants of which are widespread among mathematicians, is implicit in the following quote by David Mumford from the preface to Carol Parikh's biography of Oscar Zariski:

> The Italian school of algebraic geometry was created in the late 19th century by a half dozen geniuses who were hugely gifted and who thought deeply and nearly always correctly about their field. ... But they found the geometric ideas much more seductive than the formal details of the proofs .... So, in the twenties and thirties, they began to go astray. It was Zariski and, at about the same time, Weil who set about to tame their intuition, to find the principles and techniques that could truly express the geometry while embodying the rigor without which mathematics eventually must degenerate to fantasy [Parikh 1991, pp. xxv–xxvi].

According to this view, the principal origin of the process lay in the lack of rigor on the part of the Italians; the injection of new algebraic techniques into algebraic geometry was simply necessary in order "truly" to bring out what the Italians had been trying to do with their inadequate methodology. Aside from the fact that no human mathematical formulation of a problem or phenomenon can ever reasonably be called the "true" one, Mumford's last sentence above is especially difficult to reconcile with the historical facts because of the considerable variety of ways to rewrite algebraic geometry which were under discussion in the 1930s and 1940s (compare the section on the years from 1933 to 1946 below).

The first part of Mumford's account, which isolates the Italians' lack of rigor as the principal motivation behind the development and interprets the rewriting of algebraic geometry as a reaction to it, has its origin in the experience of many mathematicians trying to work their way through the Italian literature on algebraic geometry. We shall see van der Waerden, too, was occasionally exasperated with the Italian sources. But there are two reasons why such an explanation of what happened in the 1930s and 1940s is insufficient. On the one hand, I will show on another occasion that these difficulties were not just due to a lack of rigor on the

Italian side, but can best be described as a clash of cultures of scientific publishing.[8] On the other hand, I shall sketch below—and this will show the need to correct both schemes of explanations discussed so far—how the rewriting of algebraic geometry was a much more complicated process in which several different mathematicians or mathematical schools, with different goals and methods, interacted, each in a different way, with Italian algebraic geometry. Political factors will be seen to play a non-negligible part in this dynamic. At the end of the day, Weil and Zariski indeed stand out as those who accomplished the decisive shift after which the practice of algebraic geometry could no longer resemble that of the Italian school.

Note, incidentally, that van der Waerden is not mentioned by Mumford as one of those who put algebraic geometry back on the right track. I am in no way pointing this out to suggest that Mumford did not want to give van der Waerden his due—in fact, he does mention him in a similar context in an article which is also reproduced in Parikh's biography of Zariski—but it seems to me that van der Waerden's sinuous path between algebra and geometry, which I will outline in this chapter, simply does not suggest Mumford's claim about "the principles and techniques that could truly express the geometry while embodying the rigor" [Parikh, 1991, p. 204]. Zariski's and Weil's (different!) algebraic reconstructions of algebraic geometry, on the other hand, may indeed convey the impression of justifying it because of the way in which these latter authors presented their findings. My main claim, then, which will be developed in this chapter at least as far as van der Waerden is concerned, is that the difference, especially between van der Waerden and Weil, is less a matter of mathematical substance than of style.

Indeed, compared to Weil's momentous treatise *Foundations of Algebraic Geometry* [Weil, 1946a], van der Waerden's articles on algebraic geometry may appear piecemeal, even though they do add up to an impressive body of theory,[9] most, if unfortunately not all,[10] of which has been assembled in [van der Waerden, 1983]. This piecemeal appearance may be related to van der Waerden's "non-platonic" way of doing mathematics as he described it to Hellmuth Kneser in the postscript chosen as the epigraph of this chapter. Van der Waerden was quite happy to develop bit by bit the minimum techniques needed to algebraize algebraic geometry, but he left the more essentialist discourse to others. Later in his life, he would feel that he was world-famous for the wrong reason—namely, for his book on algebra—whereas his more original contributions, especially those he had made to algebraic geometry, were largely forgotten.[11]

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[8] It therefore goes without saying that I do not go along with the caricature of Italian algebraic geometry presented in [de Boer, 1994].

[9] Elements of this body continue to be used today in research to great advantage. For instance, Chow coordinates have had a kind of renaissance recently in Arakelov theory as seen, for example, in [Philippon, 1991–1995], and transcendence techniques have been improved using multi-homogeneous techniques first developed by van der Waerden. See, for instance, the reference to [van der Waerden, 1928c] in [Rémond, 2001, p. 57].

[10] Van der Waerden's papers sadly and surprisingly missing from the volume [van der Waerden, 1983] include: [van der Waerden, 1926b; 1928b; 1928c; 1941; 1946; 1947b; 1948; 1950a; 1950b; 1956a; 1956b; and 1958].

[11] Compare Hirzebruch's *Geleitwort* to the volume [van der Waerden, 1983, p. iii].

1925: Algebraizing Algebraic Geometry *à la* Emmy Noether

On 21 October, 1924, Luitzen Egbertus Jan Brouwer from Laren (Nord-Holland) wrote a letter to Hellmuth Kneser, then assistant to Richard Courant in Göttingen, announcing the arrival of Bartel Leendert van der Waerden:

> In a few days, a student of mine (or actually rather of Weitzenböck's) will come to Göttingen for the winter term. His name is van der Waerden, he is very bright and has already published things (especially about invariant theory). I do not know whether the formalities a foreigner has to go through in order to register at the University are difficult at the moment; at any rate, it would be very valuable for van der Waerden if he could find help and guidance. May he then contact you? Many thanks in advance for this.[12]

About ten months after his arrival in Göttingen, on 14 August, 1925, the twenty-two-year-old van der Waerden submitted his first paper on algebraic geometry to the *Mathematische Annalen* with the help of Emmy Noether: "Zur Nullstellentheorie der Polynomideale" [van der Waerden, 1926a]. Its immediate reference point was [Noether, 1923b], and its opening sentences sound like a vindication of the thesis indicated above that the development of algebraic geometry reflects the state of algebra at a given time. This interpretation was also endorsed by the author himself when he looked back on it forty-five years later: "Thus, armed with the powerful tools of Modern Algebra, I returned to my main problem: to give algebraic geometry a solid foundation."[13]

Van der Waerden opened his article in no uncertain terms:

> 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 lefthand sides of all equations that follow from the given ones form a polynomial ideal.
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> However, this foundation can be formulated more simply than it has been 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.[14]

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[12] "In einigen Tagen kommt ein Schüler von mir (oder eigentlich mehr von Weitzenböck) nach Göttingen zum Wintersemester. Er heisst van der Waerden, ist sehr gescheit und hat schon einiges publiziert (namentlich über Invariantentheorie). Ich weiss nicht, ob für einen Ausländer, der sich immatrikulieren will, die zu erfüllenden Formalitäten momentan schwierig sind; jedenfalls wäre es für van der Waerden von hohem Wert, wenn er dort etwas Hilfe und Führung fände. Darf er dann vielleicht einmal bei Ihnen vorsprechen? Vielen Dank im Voraus dafür" [NSUB, Cod. Ms. H. Kneser].

[13] See [van der Waerden, 1971, p. 172]. This passage goes on to recount the genesis and the main idea of [van der Waerden, 1926a].

[14] "Die exakte Begründung der Theorie der algebraischen Mannigfaltigkeiten in n-dimensionalen Räumen kann nur mit den Hilfsmitteln der Idealtheorie geschehen, weil schon die Definition einer algebraischen Mannigfaltigkeit unmittelbar auf Polynomideale führt. Eine Mannigfaltigkeit heißt ja algebraisch, wenn sie durch algebraische Gleichungen in den n Koordinaten bestimmt wird, und die linken Seiten aller Geichungen, die aus diesen Gleichungen folgen, bilden ein Polynomideal.

As we shall soon see, van der Waerden would change his discourse about the usefulness—let alone the necessity—of ideal theory for algebraic geometry quickly and radically. Looking back, he wrote on 13 January, 1955 in a letter to Wolfgang Gröbner (who, contrary to van der Waerden, adhered almost dogmatically to ideal theory as the royal road to algebraic geometry practically until his death): "Should one sacrifice this whole comprehensive theory only because one wants to stick to the ideal-theoretic definition of multiplicity? The common love of our youth, ideal theory, is fortunately not a living person, but a tool, which one drops as soon as one finds a better one."[15]

This statement belongs to a debate about the correct definition of intersection multiplicities (a first stage of which will be discussed in the next section). But one might actually wonder whether van der Waerden *ever* fully embraced the first sentence of his paper [van der Waerden, 1926a] about the necessity of ideal theory as the foundation of algebraic geometry. In all probability, in fact, the young author did not write the introduction. As van der Waerden states in his obituary for Emmy Noether, it was her habit with papers of her young students to write their introductions for them. In that way, she could highlight their main ideas, something they often could not do themselves [van der Waerden, 1935, p. 474]. Also, the fact that he felt or kept a certain distance from her can be gathered from remarks that van der Waerden made at different times. For instance, in a letter written on 26 April, 1926 to Hellmuth Kneser (then absent from Göttingen), van der Waerden wrote: "But you may be able to imagine that I value a conversation with you more highly than the one with Emmy Noether, which I am now facing every day (in complete recognition of Emmy's kindheartedness and mathematical capacities)."[16] And the obituary for his Jewish teacher—while in itself an act of courage in Nazi Germany, considering, in particular, the difficulties that local party officials at Leipzig created for van der Waerden then and afterwards[17]—insisted so strongly on how very special and different from ordinary mathematicians, and therefore also

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"Die Begründung kann nur einfacher gestaltet werden als es bisher geschehen ist, nämlich ohne Hilfe der Eliminationstheorie, ausschließlich auf dem Boden der Körpertheorie und der allgemeinen Idealtheorie in Ringbereichen" [van der Waerden, 1926a, p. 183].

[15] "Soll man nun diese ganze umfassende Theorie opfern, nur weil man an der idealtheoretischen Multiplizität festzuhalten wünscht? Unsere gemeinsame Jugendliebe, die Idealtheorie, ist zum Glück kein lebender Mensch, sondern ein Werkzeug, das man aus der Hand legt, sobald man ein besseres findet" [ETHZ, Nachlass van der Waerden, HS 652:3107]. I thank Silke Slembek, who first pointed out this correspondence to me.

[16] "Dennoch werden Sie sich vielleicht vorstellen können[,] daß ich Ihre Unterhaltung höher schätze als diejenige Emmy Noethers, die mir jetzt täglich wartet (mit vollständiger Anerkennung von Emmy's Herzensgüte und mathematische Kapazitäten)" [NSUB, Cod. Ms. H. Kneser A 93, Blatt 3].

[17] Van der Waerden's personal file in the University Archives at Leipzig [UAL, Film 513] records political difficulties he had especially with local Nazis. After initial problems with Nazi students in May of 1933 and after the refusal of the ministry in Dresden to let him accept an invitation to Princeton for the winter term of 1933–1934, an incident occurred in a faculty meeting on 8 May, 1935 (that is, less than a month after Emmy Noether's death and slightly more than a month before van der Waerden submitted his obituary to the *Mathematische Annalen*).

Van der Waerden and the physicists Heisenberg and Hund inquired critically about the government's decision to dismiss four "non-Aryan" colleagues in spite of the fact that they were covered by the exceptional clause for World War I Frontline Fighters of the law of 7 April, 1933, and van der Waerden went so far as to suggest that these dismissals amounted to a disregard of the law on the part of the government. Even though he took this back seconds afterwards when attacked by a colleague, an investigation into this affair ensued which produced evidence that from him, she had been that it makes her appear almost outlandish. Consider, for instance, the following passage (in which the gothic letters alluded to were at the time the usual symbols to denote ideals):

> It is true that her thinking differs in several respects from that of most other mathematicians. We all rely so happily on figures and formulæ. For her these utilities were worthless, even bothersome. She cared for concepts only, not for intuition or computation. The gothic letters which she hastily jotted on the blackboard or the paper in a characteristically simplified shape, represented concepts for her, not objects of a more or less mechanical computation.[18]

Regardless of van der Waerden's later opinions on the general relevance of ideal theory, in his first paper on algebraic geometry [van der Waerden, 1926a], he applied ideal theory to the very first steps of the theory of algebraic varieties. In so doing, he all but stripped it of elimination theory with which it was still intimately linked via Noether's immediately preceding works. More precisely, van der Waerden reduced to that of a mere tool the role of elimination theory in algebraic geometry, whereas ever since Kronecker, elimination theory had been an essential ingredient in the arithmetico-algebraic treatment of it. As van der Waerden put it: "Elimination theory in this setting is only left with the task to investigate how one can find in finitely many steps the variety of zeros of an ideal (once its basis is given) and the bases of its corresponding prime and primary ideals." He later repeated this move, as noted above, with respect to ideal theory.[19]

The key observation of the paper, which introduced one of the most fundamental notions into the new algebraic geometry, is today at the level of things taught in a standard algebra course. Paraphrasing §3 of [van der Waerden, 1926a], if $\Omega = \mathbf{P}(\xi_1, \ldots, \xi_n)$ is a finitely generated extension of fields, then all the polynomials f in $R = \mathbf{P}[x_1, \ldots, x_n]$, for which one has $f(\xi_1, \ldots, \xi_n) = 0$, form a prime ideal $\mathfrak{p}$ in R, and Ω is isomorphic to the field of quotients Π of the integral domain $R/\mathfrak{p}$, the isomorphism sending $\xi_1, \ldots, \xi_n$ to $x_1, \ldots, x_n$. Conversely, given a prime ideal $\mathfrak{p}$ in R (and distinct from R), there exists an extension field $\Omega = \mathbf{P}(\xi_1, \ldots, \xi_n)$ of finite type such that $\mathfrak{p}$ consists precisely of the polynomials f in $R = \mathbf{P}[x_1, \ldots, x_n]$

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local Nazis thought him politically dangerous, citing also his behavior at the Bad Pyrmont meeting of the DMV in the fall of 1934. Van der Waerden continued not to be authorized to attend scientific events abroad to which he was invited; he was allowed neither to attend the ICM in Oslo (1936) nor events in Italy (1939, 1942). The Nazi *Dozentenbund* in April of 1940 considered van der Waerden unacceptable as a representative of "German Science," and thought him to be "downright philosemitic." I sincerely thank Birgit Petri who took the trouble to consult this file in detail.

[18] "Ihr Denken weicht in der Tat in einigen Hinsichten von dem der meisten anderen Mathematiker ab. Wir stützen uns doch alle so gerne auf Figuren und Formeln. Für sie waren diese Hilfsmittel wertlos, eher störend. Es war ihr ausschließlich um Begriffe zu tun, nicht um Anschauung oder Rechnung. Die deutschen Buchstaben, die sie in typisch-vereinfachter Form hastig an die Tafel oder auf das Papier warf, waren für sie Repräsentanten von Begriffen, nicht Objekte einer mehr oder weniger mechanischen Rechnung" [van der Waerden, 1935, p. 474].

[19] "Die Eliminationstheorie hat in diesem Schema nur die Aufgabe, zu untersuchen, wie man (bei gegebener Idealbasis) in endlichvielen Schritten die Nullstellenmannigfaltigkeit eines Ideals und die Basis der zugehörigen Primideale und Primärideale finden kann" [van der Waerden 1926a, pp. 183-184]. We do not discuss here the gradual shift from elimination to ideals from Kronecker, via König, Macaulay, and others, to Emmy Noether and her Dedekindian background. This history will, however, be treated for our larger project.

for which one has $f(\xi_1, \ldots, \xi_n) = 0$; indeed, it suffices to take $\xi_i = x_i \pmod{\mathfrak{p}}$ in $R/\mathfrak{p}$.

These constructions suggest a crucial generalization of the notion of zero, and thereby of the notion of point of an algebraic variety. The field Ω associated with $\mathfrak{p}$, which is unique up to isomorphism, "is called the *field of zeros* of $\mathfrak{p}$. The system of elements $\{\xi_1, \ldots, \xi_n\}$ is called a *generic zero* of $\mathfrak{p}$.[20] A *zero* (without further qualification) of an ideal $\mathfrak{m}$ is by definition any system of elements $\{\eta_1, \ldots, \eta_n\}$ of an extension field of $\mathbf{P}$, such that $f(\eta_1, \ldots, \eta_n) = 0$ whenever $f \equiv 0$ ($\mathfrak{p}$). A zero which is not generic is called *special*."[21] In a footnote to this passage, van der Waerden noted the analogy with the terminology of generic points used by (algebraic) geometers. He further developed this point in geometric language in §4, with reference to an affine algebraic variety M in affine n-space $C_n(\mathbf{P})$ over an algebraically closed field $\mathbf{P}$, defined by the ideal $\mathfrak{m}$:

> If M is irreducible, so that $\mathfrak{m}$ is prime, then every generic zero of the ideal $\mathfrak{m}$ is called a *generic point of the variety M*. This terminology agrees with the meaning that the words generic and special have in geometry. Indeed, by generic 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, that is, points that lie in a space $C_n(\Omega)$, where Ω is a transcendental extension of $\mathbf{P}$. But requiring of a point of $C_n(\Omega)$ that it be a zero of all those and only those polynomials of $\mathbf{P}[x_1, \ldots, x_n]$ that vanish at all points of the variety M yields precisely our definition of a generic point of the variety M.[22]

[20]Literally, van der Waerden speaks of "allgemeine Nullstelle," that is, "general zero," and continues to use the adjective "general" throughout. Our translation takes its cue from the English terminology which was later firmly established, in particular by Weil, and which echoes the Italian "punto generico."

[21]"Der nach **3** für jedes von R verschiedene Primideal $\mathfrak{p}$ konstruierbare, nach **1** auch nur für Primideale existierende, nach **2** bis auf Isomorphie eindeutig bestimmte Körper $\Omega = \mathbf{P}(\xi_1, \ldots, \xi_n)$, dessen Erzeugende ξ_i die Eigenschaft haben, daß $f(\xi_1, \ldots, \xi_n) = 0$ dann und nur dann, wenn $f \equiv 0$ ($\mathfrak{p}$), heißt Nullstellenkörper von $\mathfrak{p}$; das Elementsystem $\{\xi_1, \ldots, \xi_n\}$ heißt *allgemeine Nullstelle* von $\mathfrak{p}$. Unter *Nullstelle* schlechthin eines Ideals $\mathfrak{m}$ verstehen wir jedes Elementsystem $\{\eta_1, \ldots, \eta_n\}$ eines Erweiterungskörpers von $\mathbf{P}$, so daß $f(\eta_1, \ldots, \eta_n) = 0$, wenn $f \equiv 0$ ($\mathfrak{p}$). Jede nicht allgemeine Nullstelle heißt *speziell*" [van der Waerden, 1926a, p. 192].

[22]"Ist M irreduzibel, also $\mathfrak{m}$ prim, so heißt jede allgemeine Nullstelle des Ideals $\mathfrak{m}$ *allgemeiner Punkt der Mannigfaltigkeit M*. Diese Bezeichnung ist in Übereinstimmung mit der in der Geometrie geläufigen Bedeutung der Wörter allgemein und speziell. Man versteht doch meistens, wenn es auch nicht immer deutlich gesagt wird, unter einem allgemeinen Punkt einer Mannigfaltigkeit einen solchen Punkt, der keiner einzigen speziellen Gleichung genügt, außer denjenigen Gleichungen, die in allen Punkten erfüllt sind. Diese Forderung kann natürlich ein bestimmter Punkt von M niemals erfüllen, und so ist man genötigt, Punkte zu betrachten, die von hinreichend vielen Parametern abhängen, d.h. in einem Raum $C_n(\Omega)$ liegen, wo Ω eine transzendente Erweiterung von $\mathbf{P}$ ist. Fordert man aber von einem Punkt von $C_n(\Omega)$, daß er Nullstelle ist für alle die und nur die Polynome von $\mathbf{P}[x_1, \ldots, x_n]$, die in allen Punkten der Mannigfaltigkeit M verschwinden, so kommt man gerade auf unsere Definition eines allgemeinen Punktes der Mannigfaltigkeit M" [van der Waerden, 1926a, p. 197].

This builds a very elegant bridge from the classical to the new usage of the word. The meaning of "generic," however, was not formally defined, as van der Waerden himself remarked, in terms of parameters, even though objects depending on parameters are fairly ubiquitous in the geometric literature.[23] The word appears to have been considered as already understood, and therefore in no need of definition. Still, it is to the more philosophically minded Federigo Enriques that we owe a textbook explanation of what a generic point is that does not agree with van der Waerden's interpretation:

> The notion of a *generic* "point" or "element" of a variety, that is, 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.[24]

Contrary to van der Waerden's notion of generic points, Enriques's "points" are always points with complex coordinates, and genericity has to do with negligible exceptional sets, not with introducing parameters. This provides a first measure for the *modification* of basic notions that the rewriting of algebraic geometry entailed; defining a generic point as van der Waerden did brought out the aspect that he explained so well, but is quite different from Enriques's narrower notion of point. At the same time, the new framework of ideal theory barred all notions of (classical, analytic) continuity as, for example, in the variation of parameters; it made sense over arbitrary abstract fields.

The modest *ersatz* for classical continuity offered by the Zariski topology[25] was partially introduced in [van der Waerden, 1926a, p. 25], where the author defined the "*algebraische Abschließung*"[26] of a finite set of points to be what we would call their Zariski closure. He appended an optimistic footnote, in which he said, in

[23]To cite an example at random from the Italian literature, Severi's *Trattato* [Severi, 1926], which appeared in the same year as van der Waerden's paper under discussion, opened with a chapter on linear systems of plane curves. In the chapter's second section, the discussion of algebraic conditions imposed on curves in a linear system quickly turned to the case [Severi, 1926, p. 23] where the conditions vary (continuously), giving rise to the distinction between particular and general positions of the condition. The context there, as well as in many other texts of the period, was the foundation of enumerative geometry, a problem in which van der Waerden was especially interested. Compare the section on the years 1933–1939 below.

[24]"La nozione di 'punto' o 'elemento' *generico* di una varietà, cioè la distinzione fra proprietà spettanti *in generale* ai punti d'una varietà e proprietà che spettano solo a punti eccezionali, acquista ora un significato preciso per tutte le varietà algebriche.

"Si dice che una proprietà spetta in generale ai punti d'una varietà V_n, ad n dimensioni, se i punti di V_n per cui essa non è soddisfatta formano—entro V_n—una varietà a meno di n dimensioni" [Enriques and Chisini, 1915, p. 139].

[25]This is, of course, our modern terminology, not van der Waerden's in 1926. As is well known, it was actually Zariski who formally introduced this topology on his "Riemann manifolds" of function fields (the points of which are general valuations of the field) in [Zariski, 1944].

[26]The only reasonable translation of this would be "algebraic closure." However, van der Waerden used a participle of the verb "to close" instead of the noun "closure," presumably in order to avoid confusion with the algebraic closure (*algebraischer Abschluß*) of a field.

particular, that "as far as algebra is concerned, the algebraic closure is a perfect substitute for the topological closure."[27]

Finally, the *dimension* of a prime ideal $\mathfrak{p}$ (notations as above) was defined by van der Waerden, in classical geometrical style, to be the transcendence degree of the corresponding function field Ω over $\mathbf{P}$. Emmy Noether had given her "arithmetical version of the notion of dimension" via the maximum length of chains of prime ideals in §4 of [Noether, 1923b] under slightly more restrictive hypotheses, and van der Waerden generalized her results to his setting in [van der Waerden, 1926a, pp. 193-195]. He added in proof a footnote which sounded a word of caution against using chains for the notion of dimension in arbitrary rings. As is well known, this step was taken by Wolfgang Krull more than ten years later in [Krull, 1937].

As the section title just quoted from [Noether, 1923b] shows, and as repeatedly used in [van der Waerden, 1926a], developments using ideal theory were called *arithmetic* by Emmy Noether and her circle.[28] In this sense, van der Waerden's first paper on algebraic geometry provides an *arithmetization* of some of its basic notions. This terminology was made more precise by Krull, who reserved it for methods having to do with the multiplicative decomposition of ideals or valuations,[29] and from there it was adopted by Zariski for his way of rewriting the foundations of algebraic geometry as of 1938. It sounds out of place today; we would rather speak of *algebraization*. But taking the old terminology seriously and using it to a certain extent actually helps the historic analysis.

More precisely, van der Waerden's first contribution to the rewriting of algebraic geometry announced a transition from the *arithmetization* to the *algebraization* of algebraic geometry. The methods he used were undoubtedly called arithmetical at the time and place where the paper was written. The basic new notions that he brought to algebraic geometry, above all the notion of generic point, however, did not appeal to the more properly arithmetic aspects of ideal theory (like prime or primary decomposition), that is, they did not appeal to those aspects which are nowadays treated under the heading of "commutative algebra." With the success of "modern algebra," the general theory of fields as it was first presented by Steinitz, which was still considered an arithmetic theory in the 1920s, would simply be incorporated into algebra, as most of it became preparatory material for the modern treatment of the resolution of algebraic equations. Since I will describe van der Waerden's later contributions to algebraic geometry as a specific form of *algebraization*, the article [van der Waerden, 1926a] can be considered with hindsight as a first step in the direction that he would take, increasingly freeing himself from a more specifically arithmetic heritage.

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[27] "Die algebraische Abschließung kann aber für die Algebra die Stelle der topologischen Abschließung vollständig vertreten" [van der Waerden, 1926a, pp. 197-198 (note 15)].

[28] It would be very interesting to study Emmy Noether's usage of the word "arithmetic" in detail. One might be able to argue that she tended to use the word as a synonym of "conceptual," taken in the sense that those coming after Emmy Noether have used to characterize her approach. A rather extreme example of such a characterization appeared in the passage from van der Waerden's obituary quoted above.

[29] See, in particular, [Krull, 1937, p. 745 (note 2)]: "Unter Sätzen von ausgesprochen 'arithmetischem' Charakter verstehe ich Sätze, die in den Gedankenkreis der 'multiplikativen', an Dedekind anknüpfenden Richtung der Idealtheorie und der Bewertungstheorie gehören ...."

1927–1932: Forays into Intersection Theory

It is probably not known what high or conflicting intentions the parents of H. C. H. Schubert had, in the proud town of Potsdam back in the turbulent year of 1848, when they christened their son Hermann Caesar Hannibal, but he who was thus named created a theory—the calculus of enumerative geometry—which, had it not been created, should have to be invented for the sake of historians of mathematics. For, like no other purely mathematical theory of the late nineteenth century, the so-called Schubert calculus can be regarded as an expression, in the realm of pure mathematics, of the mindset of contemporaneous industrialization. Consequently, later criticism of this theory—for what were viewed as its shaky foundations and/or for the occasional malfunctioning of its machinery at the hands of its practitioners—would eventually be cast in terms of metaphors of cultural critique.

Since the focus here, however, is on van der Waerden, I will not go into the history of the Schubert calculus. Suffice it to say that the precise goal of the theory was effectively to determine the number (not the nature!) of all the geometric objects satisfying a set of conditions, which, taken together, admit but finitely many solutions. Examples include: "(1) to find the number of circles tangent to 3 given circles, which Apollonius investigated about 200 B.C.; (2) to find the number of arbitrary conics—ellipses, parabolas and hyperbolas, as well as circles—tangent to 5 conics, which Steiner proposed in 1848 as a natural generalization of the problem of Apollonius; (3) to find the number of twisted cubics tangent to 12 quadratic surfaces, whose remarkable solution, published only in the book [Schubert, 1879] (culminating on p. 184), won Schubert the gold medal in 1875 from the Royal Danish Academy."[30] (Steiner thought the solution to (2) was $6^5 = 7776$, but was corrected by Chasles in 1864 who came up with the right answer of 3264. The prizeworthy number of solutions to (3) that Schubert found is 5,819,539,783,680.) Schubert constructed his theory as a special kind of propositional calculus—influenced by Ernst Schröder's logic, that is, by the continental counterpart of British developments in the algebra of logic—for geometric conditions. A key ingredient in building this effective calculus was Schubert's "principle of the conservation of number," which postulates the invariance—as long as the total number of solutions remains finite—of the number of solutions (always counted with multiplicities), when the constants in the equations of the geometric conditions vary.

The calculus works well and produces enormous numbers, digesting amazingly complicated situations. Its theoretical justification remained problematic, though, and in a very prominent way: David Hilbert's 15th problem in his famous 1900 ICM address called for the "rigorous foundation of Schubert's enumerative calculus," and, following artfully constructed counterexamples to Schubert's principle proposed as of 1903 by Gustav Kohn, Eduard Study, and Karl Rohn, even Francesco Severi admitted that the desire to secure the exact range of applicability of Schubert's principle was "something more than just a scruple about exaggerated rigor."[31] Severi, in the paper just quoted, reformulated the problem in terms of algebraic

[30] Quoted from Kleiman's concise introduction to the centennial reprint of [Schubert, 1979, p. 5]. This may also serve as a first orientation about the history of Schubert calculus.

[31] "Comunque, in questo caso si tratta di qualcosa più che un semplice scrupolo di eccessivo rigore; e la critica non è poi troppo esigente se richiede sia circoscritto con precisione il campo di validità del principio" [Severi, 1912, p. 313].

correspondences,[32] thereby providing one of the many reasons for the importance and increasing impetus of this subject in the algebraic geometry of the first half of the twentieth century. During World War I, Study's critique became more bitter, probably reflecting the fact that large numbers, without regard for the individuals in the masses that were counted, were acquiring a bad taste at that time.[33]

Van der Waerden first became acquainted with Schubert calculus, and indeed with algebraic geometry, in a course on enumerative geometry given by Hendrik de Vries at the University of Amsterdam, before he went to Göttingen.[34] He returned to this subject—apparently influenced by discussions with Emmy Noether[35]—in a paper that he submitted to the *Mathematische Annalen* just as [van der Waerden, 1926a] appeared. It is in this second paper on algebraic geometry, [van der Waerden, 1927], that one finds explicitly for the first time the other key ingredient, besides generic points, which characterized van der Waerden's rewriting of algebraic geometry, namely, what he called "*relationstreue Spezialisierung* [relation-preserving

[32]"I first observe, what is also implicit in Schubert's statement, that every variable condition [also of dimension less than k] imposed on the objects Γ of an algebraic variety V, ∞^k, translates into an algebraic correspondence between the elements Γ of V and the elements Γ' of another algebraic variety V' whose dimension k' has *as priori* nothing to do with k. Fixing one of the elements Γ', the elements Γ in correspondence with the given Γ' are those which satisfy a *specialization* of the variable condition.

"Thus, for example, the condition imposed on a line Γ in space to trisect an algebraic curve Γ' of given order n translates into an algebraic correspondence between the variety V_4 of all lines Γ and the algebraic variety V' (which in general is reducible and even consists of parts of different dimensions) of the curves Γ' of order n, by letting a line Γ and a curve Γ' be in correspondence if Γ trisects Γ' [Comincio dall'osservare che, come del resto è implicito nell'enunciato di Schubert, ogni condizione variabile [anche di dimensione inferiore a k] imposta agli enti Γ d'una varietà algebrica V, ∞^k, si traduce in una corrispondenza algebrica tra gli elementi Γ di V e gli elementi Γ' di un'altra varietà algebrica V', la cui dimensione k' non ha a priori alcuna relazione con k. Fissando uno degli elementi Γ', i Γ omologhi del dato Γ', son quelli che soddisfanno ad una *particolarizzazione* della condizione variabile.

"Così per esempio la condizione imposta ad una retta Γ dello spazio di trisecare una curva algebrica Γ' di dato ordine n, si traduce in una corrispondenza algebrica tra la varietà V_4 delle rette Γ e la varietà algebrica V' (generalmente riducibile e costituita anche da parti di diverse dimensioni) delle curve Γ' di ordine n, assumendosi omologhe una retta Γ ed una curva Γ', quando Γ triseca Γ']" [Severi, 1912, p. 314f].

[33]"In the case at hand, what is at issue is not only the massive figures produced by some representatives of the enumerative geometry, which one may or may not find interesting, but the methodology of algebraic geometry itself. ... The said 'principle' has also been applied in places where the usual means of algebra, applied in a thorough effort, would not only have been sufficient, but would have yielded *much more*. When one is interested in such and such 'results,' any method is welcome which appears to produce them as quickly and abundantly as possible [Im vorliegenden Fall handelt es sich nicht nur um die von einzelnen Vertretern der abzählenden Geometrie produzierten gewaltigen Zahlen, für die man sich interessieren mag oder nicht, sondern um die Methodik der algebraischen Geometrie überhaupt. ... Man hat das in Rede stehende 'Prinzip' auch da angewendet, wo, bei eingehenderer Bemühung, die gewöhnlichen Mittel der Algebra nicht nur ausgereicht, sondern auch sehr viel mehr geleistet haben würden. Man interessiert sich für diese oder jene 'Resultate', jede Methode ist willkommen, die sie möglichst geschwind und reichlich zu liefern scheint]" [Study, 1916, p. 65-66].

[34]In 1936, de Vries published a textbook in Dutch, *Introduction to Enumerative Geometry*, which van der Waerden reviewed very briefly for *Zentralblatt* (15, p. 368-369), writing in particular that, according to his own experience, there was no better way to learn geometry than to study Schubert's *Kalkül der abzählenden Geometrie*.

[35]Compare [van der Waerden, 1927 (note 5)].

specialization]." André Weil would later, in his *Foundations of Algebraic Geometry*, simply write "specialization."[36]

There is, however, a slight technical difference between the basic notion of specialization *à la* Weil—replacing one affine point ξ with coordinates in some extension field of the fixed ground field, which we call $\mathbf{P}$ as before, by another one η in such a way that every polynomial relation with coefficients in $\mathbf{P}$ involving the coordinates of ξ also holds for the coordinates of η—and the concept that van der Waerden introduced in his 1927 paper. Van der Waerden worked with multi-homogeneous coordinates in order to control the simultaneous specialization of a finite number of projective points (which will be taken to be all the generic solutions of an enumerative problem). More precisely,[37] starting from the ground field $\mathbf{P}$ and adjoining h unknowns (parameters) $\lambda_1, \ldots, \lambda_h$, he worked in some fixed algebraically closed extension field Ω of $\mathbf{P}(\lambda_1, \ldots, \lambda_h)$. Given q points

$$X^{(1)} = (\xi_0^{(1)} : \cdots : \xi_n^{(1)}), \ \ldots \ , \ X^{(q)} = (\xi_0^{(q)} : \cdots : \xi_n^{(q)})$$

in projective n-space over the algebraic closure $\overline{\mathbf{P}(\lambda_1, \ldots, \lambda_h)}$ inside Ω, a "*relationstreue Spezialisierung*" of $X^{(1)}, \ldots, X^{(q)}$ for the parameter values $\mu_1, \ldots, \mu_h \in \Omega$ is a set of q points

$$Y^{(1)} = (\eta_0^{(1)} : \cdots : \eta_n^{(1)}), \ \ldots \ , \ Y^{(q)} = (\eta_0^{(q)} : \cdots : \eta_n^{(q)})$$

in projective n-space over Ω such that, for any polynomial g in the variables $x_0^{(1)}, \ldots, x_n^{(1)}; x_0^{(2)}, \ldots, x_n^{(2)}; \ldots; x_0^{(q)}, \ldots, x_n^{(q)}; \lambda_1; \ldots; \lambda_h$ with coefficients in $\mathbf{P}$ which is homogeneous in each of the packets of variables separated by semicolons, and such that when

$$g(\xi_0^{(1)}, \ldots, \xi_n^{(1)}; \ldots; \xi_0^{(q)}, \ldots, \xi_n^{(q)}; \lambda_1; \ldots; \lambda_h) = 0,$$

one also has

$$g(\eta_0^{(1)}, \ldots, \eta_n^{(1)}; \ldots; \eta_0^{(q)}, \ldots, \eta_n^{(q)}; \mu_1; \ldots; \mu_h) = 0.$$

Van der Waerden uses this notion to analyze problems with Schubert's principle of the conservation of number in a way vaguely reminiscent of the avoidance of Russell's paradox by a theory of types; in order to make sense of the number of solutions which will be conserved, one has to specify the generic problem from which the given problem is considered to have been derived via specialization of parameters. Just as in the case of the theory of types, the prescribed diet makes it a little hard to survive. Thus, van der Waerden mentioned the example of the multiplicity of an intersection point of an r-dimensional with an $(n-r)$-dimensional subvariety in projective n-space, which, according to his analysis, is not well-defined (if none of the subvarieties is linear) as long as one has not specified the more general

[36]See [Weil, 1946a, Chap. II, §1]. In the introduction to this book, Weil acknowledged that "[t]he notion of specialization, the properties of which are the main subject of Chap. II, and (in a form adapted to our language and purposes) the theorem on the extension of a specialization ... will of course be recognized as coming from van der Waerden" [Weil, 1946a, p. x].

[37]Here, I am paraphrasing the beginning of §3 in [van der Waerden, 1927].

algebraic sets of which the given subvarieties are considered to be specializations.[38] We will soon encounter this example again.

On the positive side, given the reference to a generic problem, van der Waerden could simply define the multiplicity of a specialized solution to be the number of times it occurs among the specializations of all generic solutions. (This multiplicity can be zero, for generic solutions that do not specialize; see [van der Waerden, 1927, p. 765].) In this way, the "conservation of number" was verified by construction, and van der Waerden managed to solve a certain number of problems from enumerative geometry by interpreting them as specializations of generic problems which are completely under control. For instance, in the final §8, he demonstrated his method for lines on a (possibly singular) cubic surface over a base field of arbitrary characteristic.[39]

The technical heart of [van der Waerden, 1927] is the proof of the possibility and unicity (under suitable conditions) of extending ("*ergänzen*") a specialization from a smaller to a larger finite set of points. It is for this that van der Waerden resorted to elimination theory (systems of resultants). The necessary results had been established in [van der Waerden, 1926b] which, as noted above, is strangely missing from [van der Waerden, 1983]. It is part of well-known folklore in algebraic geometry that André Weil in his *Foundations* would "finally eliminate ... the last traces of elimination theory" [Weil, 1946a, p. 31 (note)], at least from this part of the theory, using a trick of Chevalley's. As of the fourth edition of 1959, van der Waerden also dropped the chapter on elimination theory from the second volume of his algebra book. In the papers by van der Waerden to which I now turn, however, algebraic techniques become even more diverse, but this will be short-lived, for he ultimately settled on his own sort of minimal algebraization of algebraic geometry (see the next section).

Having seen how van der Waerden reduced the problem of Schubert's principle to that of a good definition of intersection multiplicity, it is not surprising to find him working on Bezout's Theorem in two papers the next year: the long article [van der Waerden, 1928a] as well as the note [van der Waerden, 1928c]. (This note is also not contained in [van der Waerden, 1983].) In the simplest case, Bezout's Theorem says that two plane projective curves of degree n, respectively m, intersect in precisely $m \cdot n$ points of the complex projective plane, provided one counts these points with the right multiplicities. In the introduction to [van der Waerden, 1928a], van der Waerden first recalled a "Theorem of Bézout in modern garb" following Macaulay, to the effect that the sum of multiplicities of the points of intersection of n algebraic hypersurfaces $f_i = 0$ in projective n-space equals the product of the degrees $\deg f_i$, provided the number of points of intersection remains finite. Here, the multiplicities are defined in terms of the decomposition into linear

38 "The principle of specifying the generic problem has often been violated. For instance, one talks without definition of the multiplicity of the point of intersection of two varieties, of dimensions r and $n-r$ in the projective space P_n. But the generic sets of which M_r and M_{n-r} are considered to be specializations are not given [Gegen diesen Grundsatz ist oft verstoßen worden. Man redet z.B. ohne Definition von der Multiplizität eines Schnittpunktes zweier Mannigfaltigkeiten der Dimension r und $n-r$ im projektiven Raum P_n. Es wird dabei nicht angegeben, aus welchen allgemeineren Gebilden man die M_r und die M_{n-r} durch Spezialisierung entstanden denkt]" [van der Waerden, 1927, p. 766].

39 In this paper, van der Waerden called hypersurfaces "principal varieties" because their corresponding ideals are principal. In a funny footnote [van der Waerden, 1927, p. 768], he even proposed to call them simply "*Häupter*," that is, "heads."

forms of the so-called u-resultant of the system of hypersurfaces, that is, of the resultant of $(f_1, \ldots, f_n, \sum u_k x_k)$, where the u_k are unknowns and $x_0, \ldots, x_n$ are the projective coordinates. This entailed the "conservation of number" in the sense of the article discussed above, namely, the sum of multiplicities in each special case equals the number of solutions in the generic case (when the coefficients of the f_i are unknowns). Van der Waerden preserved this property as a guiding principle for generalizing Bezout's Theorem. As a consequence, for every application of the theorem, he had to define the "generic case" that is to be taken as reference.

Van der Waerden mentioned the general problem already encountered in [van der Waerden, 1927]: to define the multiplicity of the intersection of an r-dimensional subvariety and an $(n-r)$-dimensional subvariety in projective n-space. Again, he criticized earlier attempts to generalize Bezout's Theorem to this situation for their failure to make the notion of multiplicity precise. He solved the problem using a method which went back to Kronecker, and which used the wealth of automorphisms of projective space: transform the two subvarieties which we want to intersect via a sufficiently general matrix U of rank $n-r+1$, so that they are in general position to each other. Re-specializing U to the identity matrix will then realize the original problem as a special case of the generic one. Bezout's Theorem then states that the number of generic intersection points is just the product of the degrees of the two subvarieties (the degree of a k-dimensional subvariety being defined as the number of intersection points with a generic $(n-k)$-dimensional linear subspace).

The technical panoply employed in [van der Waerden, 1928a] was rich and varied: more Noetherian (and Noether-Hentzeltian) ideal theory than in the parsimonious [van der Waerden, 1926a], Macaulay's homogeneous ideals, David Hilbert's and Emmanuel Lasker's results about dimension theory with "Hilbert's Function,"[40] and linear transformations. Incidentally, van der Waerden performed all the constructions of §6 of the paper in what Weil later called a *universal domain* Ω, that is, an algebraically closed field of infinite transcendence degree over the base field:

> Ω then has the property that every time when, in the course of the investigation, finitely many quantities have been used, there will still be arbitrarily many unknowns left which are independent of those quantities. Fixing this field Ω once and for all saves us adjoining new unknowns time and again, and all constructions of algebraic extensions. If in the sequel at any point "unknowns from Ω" are introduced, it will be understood that they are unknowns which are algebraically independent of all quantities used up to that point.[41]

In spite of the considerable algebraic apparatus that van der Waerden brought to bear on the problems of intersection theory, his results remained unsatisfactory:

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[40]Compare also the slightly later [van der Waerden, 1928c] in which another case of Bezout's Theorem was established, concerning the intersection of a subvariety with a hypersurface in projective space.

[41]"Ω hat dann die Eigenschaft, daß es immer, wenn im Laufe der Untersuchung endlichviele Größen aus Ω verwendet worden sind, noch beliebig viele neue, von diesen Größen unabhängige Unbestimmte in Ω gibt. Die Zugrundelegung des ein für allemal konstruierten Körpers Ω erspart uns also die immer erneute Adjunktion von Unbestimmten und alle Konstruktionen von algebraischen Erweiterungskörpern. Wenn im Folgenden an irgendeiner Stelle 'Unbestimmte aus Ω' eingeführt werden, so sind damit immer gemeint solche Unbestimmte von Ω, die von allen bis dahin verwendeten Größen aus Ω algebraisch-unabhängig sind" [van der Waerden, 1928a, p. 518].

> As far as it went, the algebraic method had a greater generality than any analytic one, since it was applicable to arbitrary abstract geometries (belonging to abstract fields). But in transferring the methods to varieties of lines and the like, the proofs encountered ever mounting difficulties, and for ambient varieties which do not admit a transitive group of transformations like projective space, the transfer of the above notion of multiplicity is altogether excluded.[42]

Thus, van der Waerden changed horses:

> But topology has a notion of multiplicity: the notion of index of a point of intersection of two complexes, which has already been applied with success by Lefschetz [1924] to the theory of algebraic surfaces as well as to correspondences on algebraic curves.
>
> ...
>
> But topology achieves even more than making a useful definition of multiplicity possible. At the same time it provides plenty of means to determine in a simple manner the sum of indices of all the intersection points, or the "intersection number," the determination of which is the goal of all enumerative methods. For it shows that this sum of indices depends only on the homology classes of the varieties that are being intersected, and for the determination of the homology classes, it puts at our disposal the whole apparatus of "combinatorial topology."[43]

Van der Waerden was not the only mathematician involved in algebraic geometry to be tempted by Solomon Lefschetz's topology. Oscar Zariski's topological period around this same time, for instance, was brought about by immediate contact with Lefschetz and lasted roughly from 1928 until 1935. Interestingly, Lefschetz was skeptical of algebraic geometry, but did not so much bemoan its lack of rigor as deplore the amount of special training needed to practice this discipline in the traditional way. His idea was to incorporate algebraic geometry into more accessible mainstream mathematics, that is, into analysis in a broad sense. As he wrote to Hermann Weyl:

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[42] "Soweit sie reichte, hatte die algebraische Methode eine größere Allgemeinheit als jede analytische, da sie auf beliebige abstrakte Geometrien (die zu abstrakten Körpern gehören) anwendbar war. Aber bei der Übertragung der Methoden auf Varietäten von Geraden u.dgl. stieß die Durchführung der Beweise auf immer wachsende Schwierigkeiten, und für solche Gebilde, die nicht wie der Projektive Raum eine transitive Gruppe von Transformationen in sich gestatten, ist die Übertragung der obigen Multiplizitätsdefinition ganz ausgeschlossen" [van der Waerden, 1929, p. 338].

[43] "Aber die Topologie besitzt einen Multiplizitätsbegriff: den Begriff des Schnittpunktes von zwei Komplexen, der schon von Lefschetz [1924] mit Erfolg auf die Theorie der algebraischen Flächen sowie auf Korrespondenzen auf algebraischen Kurven angewandt wurde. ... Die Topologie leistet aber noch mehr als die Ermöglichung einer brauchbaren Multiplizitätsdefinition. Sie verschafft zugleich eine Fülle von Mitteln, die Indexsumme aller Schnittpunkte oder 'Schnittpunktzahl', deren Bestimmung das Ziel aller abzählenden Methoden ist, in einfacher Weise zu bestimmen, indem sie zeigt, daß diese Indexsumme nur von den Homologieklassen der zum Schnitt gebrachten Varietäten abhängt, und indem sie für die Bestimmung der Homologieklassen den ganzen Apparat der 'kombinatorischen Topologie' zur Verfügung stellt" [van der Waerden, 1929, pp. 339-340].

> I was greatly interested in your "Randbemerkungen zu Hauptproblemen ..." and especially in its opening sentence.[44] 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. 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.[45]

Van der Waerden was apparently the first to realize Schubert's formal identities in the homology ring of the ambient variety:

> In general, each homology relation between algebraic varieties gives a symbolic equation in Schubert's sense, and these equations may be added and multiplied *ad libitum*, just as in Schubert's calculus. And the existence of a finite basis for the homologies in every closed manifold implies furthermore the solvability of Schubert's 'characteristics problems' in general.
>
> ...
>
> I hope to give on a later occasion applications to concrete enumerative problems of the methods which are about to be developed here.[46]

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[44]This refers to [Weyl, 1924, p. 131]: "Next to such works, which—exploding in all directions and therefore followed with a lively interest by only a few—explore new scientific territory, reflections like those presented here—which care less for augmenting than for clearing up and reformulating in a way as simple and adequate as possible results already obtained earlier—also have their right, if they focus on main problems that are of interest to all mathematicians who deserve to be called by this name [Neben solchen Arbeiten, die—in alle Richtungen sich zersplitternd und darum jeweils auch nur von wenigen mit lebhafterem Interesse verfolgt—in wissenschaftliches Neuland vorstoßen, haben wohl auch Betrachtungen wie die hier vorgelegten, in denen es sich weniger um Mehrung als um Klärung, um möglichst einfache und sachgemäße Fassung des schon Gewonnenen handelt, ihre Berechtigung, wenn sie sich auf Hauptprobleme richten, an denen alle Mathematiker, die überhaupt diesen Namen verdienen, ungefähr in gleicher Weise interessiert sind]."

[45]From page 4 of a long letter by Solomon Lefschetz to Hermann Weyl, dated 30 November, 1926 [ETHZ, HS 91:659]. Hearty thanks to David Rowe for pointing out this magnificent quote to me.

[46]"Allgemein ergibt jede Homologierelation zwischen algebraischen Varietäten eine symbolische Gleichung im Schubertschen Sinn, und man darf diese Gleichungeen unbeschränkt addieren und multiplizieren, wie es im Schubertschen Kalkül geschieht. Aus der Existentz einer endlichen Basis für die Homologien in jeder geschlossenen Mannigfaltigkeit ergbt sich weiter allgemein die Lösbarkeit der Schubertsche 'Charakteristikenprobleme.' ... Anwendungen der hier zu entwickelnden Methoden auf konkrete abzählende Probleme hoffe ich später zu geben" [van der Waerden, 1929, p. 340]. An example of such a concrete application is contained in the paper "Zur algebraischen Geometrie IV": [van der Waerden, 1983, pp. 156-161].

The article was written in the midst of the active development of topology. For example, in a note added in proof, van der Waerden put to immediate use van Kampen's thesis, which had just been completed.[47]

The whole topological approach, of course, only works over the complex (or real) numbers; it does not work in what was called at the time "abstract" algebraic geometry, over an arbitrary (algebraically closed) field, let alone over one of characteristic $p \neq 0$. There is, however, no reason to discard this work from the history of algebraic geometry simply because it seems to lead us away from a purely algebraic or arithmetic rewriting of it. Both Zariski and van der Waerden took the topological road for a while; and Italian algebraic geometry had never done without analytical or continuity arguments when needed. In fact (as a smiling Richard Pink once pointed out to me), algebraic topology meets the *ad hoc* definition of algebraic geometry with which this chapter opened: the treatment of geometrical objects and problems by algebraic methods.

Clearly, van der Waerden held no dogmatic views about arithmetic or algebraic approaches. He had tried the algebraic muscle on the problem of defining intersection multiplicities as generally as possible, and the result had not been conclusive. The fact that I have anticipated here and there how André Weil picked up van der Waerden's most basic ideas in his *Foundations of Algebraic Geometry* (1946) must, of course, not create the impression of an internal sense of direction for the history of algebraic geometry. At the end of the 1920s, that history remained wide open, full of different options, and—to anticipate once more—in the 1950s, topological (Hirzebruch) and analytical (Kodaira and Spencer) methods would make their strong reappearance in a discipline which had just been thoroughly algebraized.

History must also have seemed particularly open from the personal point of view of the young, brilliant van der Waerden, who, newly married, had started his first professorship in 1928 at Groningen, and had become Otto Hölder's successor in Leipzig in May of 1931. He had plenty of different interests. He was most attracted to Leipzig because of the prospect of contact with the physicists Heisenberg and Hund. While his *Moderne Algebra* appeared in 1930 (vol. I) and 1931 (vol. II), the following year of 1932 saw the publication of his book on group-theoretic methods in quantum mechanics. Within another five years, he had added statistics to his active research interests, and had even started to publish on the ancient history of mathematics.

Nevertheless, algebraic geometry, including topological methods when necessary, remained one of his chief research interests. Thus, following a tiny, four-page paper emending an oversight of Brill and Noether[48] and obviously confident that he had already explored and secured the methodological foundations for broad research in the field, van der Waerden launched in 1933 (paper submitted on 12 July, 1932) his series "Zur algebraischen Geometrie," or ZAG for short, coming back in the first installment to the problem of defining multiplicities, with a relatively light use of algebra, this time in the special case where one of the intersected varieties is a hypersurface.[49] This ZAG series, which appeared in the *Mathematische Annalen*

[47]See [van der Waerden, 1929, p. 118 (note 20)]. I will not go into the technical details of van der Waerden's topological work here.

[48][van der Waerden 1931] was submitted on 19 November, 1930. Severi later scolded van der Waerden for criticizing his elders. See the final footnote in [Severi, 1933, p. 364 (note 31)], respectively, [Severi, 1980, p. 129].

[49]See [van der Waerden, 1933].

and which was incorporated in the volume [van der Waerden, 1983], ran from the article ZAG I (1933) just mentioned, all the way to ZAG 20 which appeared in 1971. (Although it is only fair to say that the penultimate paper of the series, ZAG 19, had appeared in 1958.) Van der Waerden opened the series this way: "In three preceding articles in the *Annalen*, I have developed several algebraic and topological notions and methods upon which higher dimensional algebraic geometry may be based. The purpose of the present series of papers 'On algebraic geometry' is to demonstrate the applicability of these methods to various problems from algebraic geometry."[50]

We shall skip over the details of this paper as well as over the quick succession of ZAG II (submitted 27 July, 1932/appeared 1933), ZAG III (27 October, 1932/1933), ZAG IV (27 October 1932/1933), and ZAG V (8 October, 1933/1934), in order to get to the historically more significant encounter of van der Waerden with the Italian school of algebraic geometry, and the corresponding ripples in the mathematical literature.

1933–1939: When in Rome ... ?

The following remarkably dry account, taken from [van der Waerden, 1971, p. 176], is surely an understatement of what actually happened during and after that meeting between the twenty-nine-year-old Bartel L. van der Waerden and the impressive and impulsive fifty-three-year-old Francesco Severi:

> At the Zürich International Congress in 1932 I met Severi, and I asked him whether he could give me a good algebraic definition of the multiplicity of a point of intersection of two varieties A and B, of dimensions d and $n-d$, on a variety U of dimension n, on which the point in question is simple. The next day he gave me the answer, and he published it in the *Hamburger Abhandlungen* in 1933. He gave several equivalent definitions ....

In the absence of any first-hand documentary evidence about their relationship in the thirties,[51] one can only say that Severi's presence effectively confronted van der Waerden with the reality of Italian algebraic geometry for the first time in his life. This confrontation had an attractive and a repellent aspect. The attraction is clearly reflected in van der Waerden's desire to spend some time in Rome. In fact, just about a month before he had to abandon his function as director of the Göttingen Mathematics Institute, Richard Courant wrote a letter to Wilbur E. Tisdale at the Rockefeller Foundation in Paris in which he explained that

[50] "In drei früheren Annalenarbeiten habe ich einige algebraische und topologische Begriffe und Methoden entwickelt, die der mehrdimensionalen algebraischen Geometrie zugrunde gelegt werden können. Der Zweck der jetzigen Serie von Abhandlungen 'Zur Algebraischen Geometrie' ist, die Anwendbarleit dieser Methoden auf verschiedene algebraisch-geometrische Probleme darzutun" [van der Waerden 1933].

[51] All of van der Waerden's correspondence before December 1943 seems to have burned with his Leipzig home in an air raid. On the other hand, Italian historian colleagues have assured me that, in spite of years of searching, they have never found any non-political correspondence of Severi's—except for those letters that were kept by the correspondents. A fair amount of later correspondence between Severi and van der Waerden, in particular in the long, emotional aftermath of the events at the 1954 ICM in Amsterdam, is conserved at ETHZ.

> Prof. Dr. B. L. van der Waerden, at present full professor at the University of Leipzig, about 30 or 31 years old, former Rockefeller fellow, has asked me to sound out whether the Rockefeller Foundation could arrange a prolonged sojourn in Italy for him.
>
> In spite of his great youth, van der Waerden is today one of the outstanding mathematicians in Europe. He was one of the three candidates of the Faculty for Hilbert's successor. For a few years now, van der Waerden has started to study the problems of algebraic geometry, and he seriously intends to promote the cultivation of this domain in Germany. As a matter of fact, the geometric-algebraic tradition is all but dead in Germany whereas it has come to full blossom in Italy over the past few decades. Several young mathematicians, for instance Dr. Fenchel and Dr. Kähler have spent time in Italy on a Rockefeller grant and have successfully studied algebraic geometry there. But for the advancement of science, it would be effective on quite a different scale, if such an outstanding man as van der Waerden could establish the necessary link on a broad basis.
>
> It is for these scientific reasons that van der Waerden has developed the wish to work for some time especially with Prof. Severi in Rome, and to then transplant the results back to Germany.[52]

In fact, van der Waerden did not get the Rockefeller grant, and he traveled neither to Italy nor to the United States in the 1930s, at least in part because of the travel restrictions that the Nazi Regime imposed on him.[53]

As to the repellent side of the encounter with Severi, Leonard Roth (who had spent the 1930–1931 academic year in Rome) left this analysis in his obituary of Severi. He explained that "[p]ersonal relationships with Severi, however complicated in appearance, were always reducible to two basically simple situations: either he had just taken offence or else he was in the process of giving it—and quite often genuinely unaware that he was doing so. Paradoxically, endowed as he was with even more wit than most of his fellow Tuscans, he showed a childlike incapacity

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[52]"Prof. Dr. B. L. van der Waerden, gegenwärtig Ordinarius an der Universität Leipzig, etwa 30 oder 31 Jahre alt, früherer Rockefeller fellow, hat mich darum gebeten, die Möglichkeit zu sondieren, ob ihm von der Rockefeller Foundation ein längerer Aufenthalt in Italien ermöglicht werden kann.

"van der Waerden ist trotz seiner grossen Jugend einer der hervorragenden Mathematiker, die es augenblicklich in Europa gibt. Er war bei der Neubesetzung des Hilbertschen Lehrstuhls einer der drei Kandidaten der Fakultät. Nun hat van der Waerden seit einigen Jahren erfolgreich begonnen, sich mit den Problemen der algebraischen Geometrie zu beschäftigen, und es ist sein sehr ernstes Bestreben, die Pflege dieses Gebietes in Deutschland wirklich zu betreiben. Tatsächlich ist die geometrisch-algebraische Tradition in Deutschland fast ausgestorben, während sie in Italien im Laufe der letzten Jahrzehnte zu hoher Blüte gelangt ist. Schon mehrere junge Mathematiker, z.B. Dr. Fenchel und Dr. Kähler sind mit einem Rockefellerstipendium in Italien gewesen und haben dort erfolgreich algebraische Geometrie studiert. Aber es würde für die wissenschaftliche Entwicklung von ganz anderer Wirksamkeit sein, wenn ein so hervorragender Mann wie van der Waerden die notwendige Verbindung auf einer breiteren Front herstellen könnte.

"Aus solchen sachlichen Erwägungen ist van der Waerdens Wunsch entstanden, insbesondere in Kontakt mit Prof. Severi in Rom eine gewisse Zeit zu arbeiten und dann das Gewonnene hier nach Deutschland zu verpflanzen" (my translation). The letter is dated 2 March, 1933. Compare [Siegmund-Schultze, 2001, pp. 112-113]. I thank Reinhard Siegmund-Schultze for providing me with the original German text of the letter

[53]Recall the discussion of this point in note 17 above.

either for self-criticism or for cool judgement" [Roth, 1963, p. 307]. At the same time, such psychological observations must not obscure the fact that Severi wielded real academic power in the fascist Italy of the thirties, after having turned his back on his former socialist convictions and anti-fascist declarations when the possibility arose to take Enriques's seat at the Academy in Rome. For example, beginning in 1929 and in concert with the regime's philosopher Giovanni Gentile, Severi was actively preparing the transformation (which became effective in August of 1931) of the traditional professors' oath of allegiance into an oath to the fascist regime.[54]

The papers of van der Waerden that appeared before 1934 contain only very occasional references to Italian literature, and only one to Severi [van der Waerden, 1931, p. 475 (note 6)]. Severi's irritated reaction to this—and more generally to the content of van der Waerden's series of papers on algebraic geometry—shows clearly through the sometimes barely polite formulations in his German paper [Severi, 1933]. As Hellmuth Kneser nicely put it in his *Jahrbuch* review of this article, "[g]eneral and personal remarks scattered throughout the article impart even to the non-initiated reader a lively impression of the peculiarity and the achievements of the author and the Italian school."[55] Severi's overall vision of algebraic geometry and its relationship to neighboring disciplines is made clear straight away in the introductory remarks:

> I claimed that all the elements required to define the notion of "intersection multiplicity" completely rigorously and in the most general cases have been around, more or less well developed, for a long time in algebraic geometry, and that the proof of the principle of the conservation of number that I gave in 1912 is perfectly general. In order to lay the foundation for those concepts in a way covered against all criticism, it is therefore not necessary, as Mr. van der Waerden and Mr. Lefschetz think, to resort to topology as a means that would be particularly adapted to the question. Lefschetz's theorems ... and van der Waerden's applications thereof ... are undoubtedly of great interest already in that they demonstrate conclusively that fundamental algebraic facts have their deep and almost exclusive foundation in pure and simple continuity. ... As I already said in my ICM talk, it is rather topology that has learned from algebra and algebraic geometry than the other way around, because these two disciplines have served topology as examples and inspiration.[56]

[54]See [Guerraggio and Nastasi, 1993, pp. 76-83 and 211-213].

[55]"Allgemeine und persönliche Bemerkungen, die durch die Abhandlung verstreut sind, vermitteln auch dem Fernerstehenden einen lebhaften Eindruck von der Eigenart und den Leistungen des Verf. und der italienischen Schule."

[56]"... behauptete ich, daß sich in der algebraischen Geometrie schon seit längerer Zeit in mehr oder weniger entwickelter Form alle Elemente vorfinden, die den Begriff 'Schnittmultiplizität' mit aller Strenge und in den allgemeinsten Fällen zu definieren erlauben; und dass ferner der von mir 1912 gegebene Beweis für das Prinzip der Erhaltung der Anzahlen vollkommen allgemein ist. Es ist demnach nicht nötig, wie die Herren van der Waerden und Lefschetz meinen, zur Topologie als dem der Frage vor allem angemessenen Hilfsmittel zu greifen, um eine gegen alle Einwände gedeckte Begründung jener Begriffe zu geben. Die Sätze von Lefschetz ... und die Anwendungen, die Herr van der Waerden davon ... gemacht hat, bieten unzweifelhaft grosses Interesse, schon weil sie in erschöpfender Weise zeigen, daß fundamentale algebraische Tatsachen ihren tiefen und fast ausschließlichen Grund in der reinen und einfachen Kontinuität finden. ... Wie ich bereits

Mathematically, Severi's construction for the intersection multiplicity amounts to the following.[57] He wanted to define the intersection multiplicity of the two irreducible (for simplicity) subvarieties V_k (indices indicate dimensions) and W_{r-k} of a variety M_r, which, in turn, is embedded in projective d-space S_d at a point P of their intersection which is simple on M. Then Severi chose a generic linear projective subspace S_{d-r-1} in S_d, and took the corresponding cone N_{d-r+k} over V_k projected from S_{d-r-1}. Writing the intersection cycle $N \cap M = V + V'$ and observing that V' does not pass through P, he then defined the intersection multiplicity of V, W at P to be the intersection multiplicity of N, W at P. This thus reduced the problem to the intersection of subvarieties of complementary dimensions in projective d-space, where he argued with generic members of a family containing N, or alternatively, of a family on M containing $V + V'$. The definition was then supplemented by showing its independence of choices, within suitable equivalence classes.[58]

We have used here, for the convenience of the modern reader, the word "cycle" (instead of "variety") to denote a linear combination of irreducible varieties. Such a distinction was absent from the terminology of the thirties, and was only introduced in Weil's *Foundations*. Still, even if the word is anachronistic relative to the early thirties, the concept is not. Severi had just opened up a whole "new field of research" in 1932, which today would be described as the theory of rational equivalence of 0-cycles.[59] It is important to underscore Severi's amazing mathematical productivity during those years, and even later, lest one get a wrong picture about what it meant to *re*write algebraic geometry at the time.

Van der Waerden's reaction to Severi's explanations and critique was twofold: he was annoyed, but he heeded the advice. Both reactions are evident in his paper ZAG VI, that is, [van der Waerden, 1934]. Mathematically, van der Waerden reconstructed here a good deal of Severi's theory of correspondences and of the

in meinem [ICM-] Vortrag sagte, hat eher die Topologie von der Algebra und der algebraischen Geometrie gelernt als umgekehrt" [Severi, 1933, p. 335].

It is instructive to compare this passage to Dieudonné's account of the history of intersection theory. See [Dieudonné, 1974, pp. 132-133], where he says that "[t]he works of Severi and Lefschetz bring to light the essentially topological nature of the foundations of classical algebraic geometry; in order to be able to develop in the same manner algebraic geometry over any field whatsoever, it will be necessary to create purely algebraic tools which will be able to substitute for the topological notions .... It is to van der Waerden that the credit goes for having, beginning in 1926, placed the essential markers for this path [Les travaux de Severi et de Lefschetz mettaient donc en évidence la nature essentiellement topologique des fondements de la Géométrie algébrique classique; pour pouvoir développer de la même manière la Géométrie algébrique sur un corps quelconque, il fallait créer des outils purement algébriques qui puissent se substituer aux notions topologiques .... C'est à van der Waerden que revient le mérite d'avoir, à partir de 1926, posé les jalons essentiels dans cette voie]." Although globally correct, this analysis leaves Severi back in 1912 and glosses over van der Waerden's multifarious methods.

[57] We paraphrase [Severi, 1933, no. 8].

[58] In the endnote Severi added to his 1933 article in 1950 obviously under the influence of Weil's *Foundations* (see [Severi, 1980, pp. 129-131]), Severi observed (which he had not done explicitly in 1933) that the intersection multiplicity he defined was symmetric in the intersecting subvarieties. He went on to comment on Weil's definition of intersection multiplicity, in the same way as in many other papers of his from the 1950s, calling it "static" rather than dynamic.

[59] Since Severi is not the main focus of this article, I shall not go into this here. I refer the reader instead to the best available study of this aspect of Severi's work: [Brigaglia, Ciliberto, and Pedrini, 2004, pp. 325-333]. Compare also van der Waerden's account in [van der Waerden, 1970].

"principle of conservation of number" with his own, *mild algebraic* methods (that is, without elimination or other fancy ideal theory, but also without topology). The paper digests substantial mathematical input coming more or less directly from Severi (not only from Severi's article just discussed) and sticks again to exclusively algebraic techniques.

As for the annoyance, the first paragraph of the introduction announced a surprising change of orientation with political overtones which could not have been suspected after all his previous papers on algebraic geometry:

> The goal of the series of my articles "On Algebraic Geometry" (ZAG) is not only to establish new theorems but also to make the far-reaching methods and conceptions of the Italian geometric school accessible with a rigorous algebraic foundation to the circle of readers of the Math. Annalen. If I then perhaps prove again something here which has already been proved more or less properly elsewhere, this has two reasons. Firstly, the Italian geometers presuppose in their proofs a whole universe of ideas and a way of geometric reasoning with which, for instance, the German man of today is not immediately familiar. But secondly, it is impossible for me to search, for each theorem, through all the proofs in the literature in order to check whether there is one among them which is flawless. I rather formulate and prove the theorems my own way. Thus, if I occasionally indicate deficiencies in the most widely circulated literature, I do not claim in any way that I am the first who now presents things really rigorously.[60]

The fairly aggressive wording in this passage may not quite show in the English translation, but the other element of linguistic taint of the time, namely, the fact that the readers of the *Mathematische Annalen* are represented by "*der Deutsche von heute*," gives a distinctly national vocation to the international journal and is obvious enough. In order to understand this peculiar twist of van der Waerden's anger, one may recall that in October of 1933, when the paper was submitted, the Berlin–Rome axis was still a long way in the future, and Italy's foreign politics looked potentially threatening to German interests, not only in Austria. Thus, van der Waerden, momentarily forgetting that he was himself a foreigner in Germany, having been criticized by a famous Italian colleague, comfortably used for his own sake the favorite discourse of the day: that Germany had to concentrate on herself to be fortified against attacks from abroad.

[60]"Das Ziel der Serie meiner Abhandlungen 'Zur Algebraischen Geometrie' (ZAG) ist nicht nur, neue Sätze aufzustellen, sondern auch, die weitreichenden Methoden und Begriffsbildungen der italienischen geometrischen Schule in exakter algebraischer Begründung dem Leserkreis der Math. Annalen näherzubringen. Wenn ich dabei vielleicht einiges, was schon mehr oder weniger einwandfrei bewiesen vorliegt, hier wieder beweise, so hat das einen doppelten Grund. Erstens setzen die italienischen Geometer in ihren Beweisen meistens eine ganze Begriffswelt, eine Art geometrischen Denkens, voraus, mit der z.B. der Deutsche von heute nicht von vornherein vertraut ist. Zweitens aber ist es mir unmöglich, bei jedem Satz alle in der Literatur vorhandenen Beweise dahin nachzuprüfen, ob sich ein völlig einwandfreier darunter befindet, sondern ich ziehe es vor, die Sätze in meiner eigenen Art zu formulieren und zu beweisen. Wenn ich also hin und wieder eimal auf Unzulänglichkeiten in den verbreitetsten Darstellungen hinweisen werde, so erhebe ich damit keineswegs den Anspruch, der erste zu sein, der die Sachen nun wirklich exakt darstellt" [van der Waerden, 1934, p. 168].

I emphasize here that van der Waerden somewhat surprisingly does *not* insist in the introduction to [van der Waerden, 1934] on the extra generality achieved by his methods. After all, Italian geometers had never proved (nor wanted to prove) a single theorem valid over a field of characteristic p. The whole presentation of this article—in which van der Waerden begins to develop his treatment of some of the most central notions of Italian geometry, like correspondences and linear systems—seems remarkably close in style to the Italian literature, much more so than the previous articles we have discussed. For instance, the field over which constructions are performed is hardly ever made explicit.

At the end of the introduction to this article, van der Waerden stated that "[t]he methods of proof of the present study consist firstly in an application of *relationstreue Spezialisierung* over and over again, and secondly in supplementing arbitrary subvarieties of an ambient variety $\mathfrak{M}$ to complete intersections of $\mathfrak{M}$ by adding residual intersections which do not contain a given point.[61] This second method I got from Severi [1933]."[62] The first and the last sentences of this introduction, taken together, can well serve as a motto for almost all of van der Waerden's ZAG articles in the 1930s, more precisely, for ZAG VI–ZAG XV with the exception of ZAG IX. The author enriched his own motivations and resources by Italian problems and ideas, and he wrote up his proofs with the mildest possible use of modern algebra, essentially only using generic points and specializations to translate classical constructions. A particularly striking illustration of this is ZAG XIV of 1938 [van der Waerden, 1983, pp. 273-296]. There, van der Waerden returned to intersection theory and managed to translate not only Severi's construction of 1933 but also a good deal of the latter's theory of equivalence families into his purely algebraic setting, while, at the same time, excising all of the fancier ideal theory of his earlier papers [van der Waerden, 1927] and [van der Waerden, 1928a].

There is, however, one fundamentally new ingredient, which I have not yet mentioned, that enters in the mathematical technology of ZAG XIV. It is due to the one article excluded above, namely, the brilliantly original and important ZAG IX written jointly with Wei-Liang Chow [Chow and van der Waerden, 1937]. As Serge Lang concisely described this work:

> To each projective variety, Chow saw how to associate a homogeneous polynomial in such a way that the association extends to a homomorphism from the additive monoid of effective cycles in projective space to the multiplicative monoid of homogeneous polynomials, and ..., if one cycle is a specialization of another, then the associated Chow form is also a specialization. Thus varieties of given degree in a given projective space decompose into a finite number of algebraic families, called Chow families. The coefficients of the Chow form are called the Chow coordinates of the cycle or of the variety. ... He was to use them all his life in various contexts dealing with algebraic families.

[61] These "residual subvarieties" are like the cycle V' in our sketch of Severi's argument above. Adding them is all that is meant here by obtaining a "complete intersection."

[62] Die Beweismethoden der vorliegenden Untersuchung bestehen erstens in einer immer wiederholten Anwendung der 'relationstreuen Spezialisierung' und zweitens der Ergänzung beliebiger Teilmannigfaltigkeiten einer Mannigfaltigkeit $\mathfrak{M}$ zu vollständigen Schnitten von $\mathfrak{M}$ durch Hinzunahme von Restschnitten, welche einen vorgegebenen Punkt nicht enthalten. Die zweite Methode habe ich von Severi [1933] übernommen" [van der Waerden, 1934, p. 137].

> In Grothendieck's development of algebraic geometry, Chow coordinates were bypassed by Grothendieck's construction of Hilbert schemes whereby two schemes are in the same family whenever they have the same Hilbert polynomial. The Hilbert schemes can be used more advantageously than the Chow families in some cases. However, as frequently happens in mathematics, neither is a substitute for the other in all cases [Lang, 1996, pp. 1120-1121].

Wei-Liang Chow, born in Shanghai, was van der Waerden's doctoral student in Leipzig (although he was actually more often to be found in Hamburg). He submitted his dissertation [Chow, 1937] in May of 1936. In it, he gave a highly original—in some ways amazing—example of rewriting algebraic geometry in van der Waerden's way (including the so-called "Chow forms" and a subtle sharpening of Bertini's Theorem). The thesis reproved the whole theory of algebraic functions of one variable—the theory of algebraic curves—over a perfect ground field of arbitrary characteristic, and it did so all the way to the Riemann-Roch Theorem, following for much of the way Severi's so-called "*metodo rapido*."[63] This may seem like a modest goal to achieve. However, Chow got there without ever using differential forms. As van der Waerden wrote in the evaluation of this work, contrasting its algebraic-geometric approach with the approach via function field arithmetic by Friedrich Karl Schmidt, "[a]ltogether, this has established a very beautiful, self-contained and methodologically pure construction of the theory."[64]

These examples should suffice to convey the general picture of van der Waerden's algebraization of algebraic geometry in his Leipzig years. It produced often brilliantly original, and always viable and verifiable, theorems about exciting questions in algebraic geometry with a modicum of algebra. And even the algebra that was used no longer looked particularly modern at the time: just polynomials, fields, generic points, and specializations.

This *modest algebraization* of algebraic geometry, as it may be styled, did a lot to restore harmony with the Italian school. In 1939, van der Waerden published his textbook *Einführung in die algebraische Geometrie*, which digested a great deal of classical material from old algebraic geometry, but also included the results of a number of his articles of the thirties. The style is particularly pedagogical, going from linear subspaces of projective space to quadrics, etc., from curves to higher dimensional varieties, from the complex numbers to more general ground fields. In his preface, van der Waerden stated that "[i]n choosing the material, what mattered were not aesthetic considerations, but only the distinction: necessary–dispensable. Everything that absolutely has to be counted among the 'elements,' I hope to have taken in. Ideal theory, which guided me in my earlier investigations, has proved dispensable for the foundations; its place has been taken by the methods of the Italian school which go further."[65] The echo from Rome was very encouraging:

[63]This presentation of the theory of algebraic curves goes back to [Severi, 1920], and Severi himself returned to it several times. See, in particular, [Severi, 1926, pp. 145-169] and [Severi, 1952]. On a later occasion, I hope to publish a detailed comparison of Severi's method with other treatments from the 1930s, in particular André Weil's. See [Weil, 1938b], and compare [van der Waerden, 1959, chapter 19].

[64]"Insgesamt ist so ein sehr schöner, in sich geschlossener und methodisch reiner Aufbau der Theorie entstanden" [UAL, Phil. Fak. Prom. 1272, Blatt 2].

[65]"Bei der Auswahl des Stoffes waren nicht ästhetische Gesichtspunkte, sondern ausschliesslich die Unterscheidung: notwendig–entbehrlich maßgebend. Alles das, was unbedingt zu

> This volume, devoted to an introduction to algebraic geometry, shows some of the well-known characteristics of the works of its author, namely, the clarity of exposition, the conciseness of the treatment, kept within the limits of a severe economy, and the constant aspiration for rigor and transparency in the foundations. However, one does not find that dense game of abstract concepts which is so typical of the "Modern Algebra," and renders the latter so hard to read without extensive preliminary preparation. ... This remarkable book of van der Waerden will undoubtedly facilitate learning the methods of the Italian school, and contribute to a mutual understanding between the Italian geometers and the German algebraists, thus fulfilling a task of great importance.[66]

A letter from 1950 of van der Waerden to Severi (the latter had invited van der Waerden to come to Rome for a conference and to give a talk on abstract algebra) rings like an echo both of Conforto's words about van der Waerden's algebraic geometry and of Weil's recollection (recall the introductory section):

> I do not think I can give a really interesting talk on abstract algebra. The enthusiasm would be lacking. One knows me as an algebraist, but I much prefer geometry.
>
> In algebra, not much is marvelous. One reasons with signs that one has created oneself, one deduces consequences from arbitrary axioms: there is nothing to wonder about.
>
> But how marvelous geometry is! There is a preestablished harmony between algebra and geometry, between intuition and reason, between nature and man! What is a point? Can one see it? No. Can one define it? No. Can one dissolve it into arbitrary conventions, like the axioms of a ring? No, No, No! There is always a mysterious and divine remainder which escapes both reason and the senses. It is from this divine harmony that a talk on geometry derives its inspiration.
>
> This is why I ask you to let me talk on:
>
> 1) The principle of the conservation of number (historic overview)
> or else
> 2) The theory of birational invariants, based on invariant notions.

den 'Elementen' gerechnet werden muß, hoffe ich, aufgenommen zu haben. Die Idealtheorie, die mich bei meinen früheren Untersuchungen leitete, hat sich für die Grundlegung als entbehrlich herausgestellt; an ihre Stelle sind die weitertragenden Methoden der italienischen Schule getreten" [van der Waerden, 1939, p. v].

66 "Questo volume, dedicato ad un'introduzione alla geometria algebrica, presenta alcune delle ben note caratteristiche delle opere del suo Autore, e precisamente la nitidezza dell'esposizione, la rapidità e compattezza della trattazione, tenuta nei limiti di una severa economia, e la costante aspirazione al rigore ed alla chiarezza nei fondamenti. Non si trova invece quel serrato giuoco di concetto astratti, così caratteristico della 'Moderne Algebra,' che rende quest'ultima di difficile lettura per chi non abbia un'ampia preparazione preliminare. ... il notevole libro di van der Waerden agevolerà senza dubbio la conoscenza dei metodi della scuola italiana e coopererà ad una reciproca comprensione tra i geometri italiani e gli algebristi tedeschi, assolvendo così un compito di grande importanza." This passage is taken from the review of the book by Fabio Conforto (Rome) in *Zentralblatt* 21, 250.

I found this very recently, stimulated by a discussion with you at Liège.[67]

1933–1946: The Construction Site of Algebraic Geometry

Having traced the development of van der Waerden's research in algebraic geometry, the issue now becomes to attempt to situate his contributions with respect to other contemporaneous agendas in the area. This more global picture must of necessity remain sketchy here and will highlight only a few of the other relevant actors.[68] Among them, however, as we saw in the previous section, the Italians figure prominently; Fabio Conforto underscored this relationship in his review of van der Waerden's 1939 textbook on algebraic geometry, by referring to it as a contribution "to a mutual understanding between the Italian geometers and the German algebraists, thus fulfilling a task of great importance." Moreover, once the "Axis Berlin–Rome," as Mussolini termed it, was in place—that is, after the summer of 1936—it could also provide at least a metaphorical background for and justification of official invitations attempting to promote scientific exchange between Germany and Italy. The related activities on the German side actually constitute an interesting prelude to the war attempts to set up a European scientific policy under German domination.[69]

Van der Waerden's position in this miniature replica of a great political game was certainly handicapped by the hurdles that local Nazi officials created for him in Leipzig. Even if this had not been the case, however, that is, even if he could have engaged in direct contact at will, the strategy he followed after 1933 with respect to Italian algebraic geometry might have done him a disservice. As intellectually flexible as he was, he managed to present his rewritten algebraic geometry in a way that outwardly conformed, to a large extent, to the Italian model. It may have been his personal mathematical temperament, as reflected in the epigram with which this chapter opened, that made him place more emphasis on the rich geometric ideas and techniques than on the radically new kind of theory in which he was executing his constructions. He made it very easy for the Italians to consider him almost as a disciple, and as the later letters between him and Severi show, he never betrayed his loyalty to the Italian master. For instance, at one of the crisis points in their correspondence (after the 1954 ICM), Severi accused van der

[67]"Mon trés cher collègue. Je ne crois pas que je puisse présenter une conférence vraiment intéressante sur l'Algèbre abstraite. Il y manquera l'enthousiasme. On me connaît comme algébriste, mais j'aime la géométrie beaucoup plus. — Dans l'algèbre, il n'y a que peu de merveilleux. On raisonne sur des signes qu'on a créé[s] soi-même, on déduit des conséquences d'axiomes arbitraires: il n'y a pas de quoi s'étonner. — Mais la géométrie, quel[le] merveille! Il y a une harmonie préétabli[e] entre l'algèbre et la géométrie, entre l'intuition et la raison, entre la nature et l'homme! Qu'est-ce que c'est un point? Peut-on le voir? Non. Peut-on le définir? Non. Peut-on le résoudre en des conventions arbitraires, comme les axiomes d'un anneau? Non, non, non! Il y a toujours un reste mystérieux et divin, qui échappe à la raison comme aux sens. C'est de cette harmonie divine que s'inspire une conférence géométrique. — C'est pourquoi je vous propose de me laisser parler sur: 1) Le principe de la conservation du nombre (aperçu historique), ou bien: 2) La théorie des invariants biration[n]els basée sur des notions invariant[e?]s. J'ai trouvé cela tout récemment, stimulé par une discussion avec vous à Liège" [ETHZ, Nachlass van der Waerden, HS 652:11960]. This is a draft of a letter from van der Waerden to Severi, dated 15 February, 1950.

[68]I plan to return to this matter in the context of my larger research project.

[69]Compare [Siegmund-Schultze, 1986] and [Remmert, 2004].

Waerden of not sufficiently acknowledging the priority and accuracy of his ideas. For his part, however, van der Waerden was ready to plead with Severi, by pointing out that he had documented his complete confidence in Severi's approach as early as 1937.[70] Being the younger of the two, van der Waerden could appear as a junior partner, rewriting algebraic geometry; thus, at the beginning of his long review of van der Waerden's *Introduction to Algebraic Geometry* in volume sixty-five of the *Jahrbuch über die Fortschritte der Mathematik* for 1939, Harald Geppert attributed the fact that the foundations of algebraic geometry had now finally attained the necessary degree of rigor, mainly "to the works of Severi and of" van der Waerden.[71] Bearing this in mind, let us now consider some of the other mathematicians busy at the construction site.

Helmut Hasse and his school of function field arithmetic developed an increasing demand for ideas from algebraic geometry after Max Deuring had the idea, in the spring of 1936, to use the theory of correspondences in order to generalize Hasse's proof of the analog of the Riemann hypothesis for (function fields of) curves over finite fields from genus one to higher genera. Hasse organized a little conference on algebraic geometry in Göttingen on 6-8 January, 1937, with expository talks by Jung, van der Waerden, Geppert, and Deuring. The politically prestigious bicentennial celebration of Göttingen University in June of 1937 next provided the opportunity for Hasse and Severi to meet, and the mathematical and personal contact between them grew more intense from then on.

A few days after the Munich summit on the Bohemian crisis—the summit where Mussolini had used his unexpected role as a mediator to favor Hitler—Hasse wrote an amazing letter to Severi in which a political part, thanking "your incomparable Duce" for what he has done for the Germans, is followed by a plea for a corresponding mathematical axis. In particular, he mentioned a plan to start a German-Italian series of monographs in algebra and geometry with the goal of synchronizing the two schools.[72] Hasse and his school had a much more definite methodological paradigm than van der Waerden, however; they foresaw an arithmetic theory of function fields in the tradition of Dedekind and Weber, Hensel and Landsberg, etc. Translating ideas from classical algebraic geometry into this framework could not be presented as a relatively smooth transition as in van der Waerden's case. The "axis" between the schools of Hasse and Severi therefore took the form of expository work on function field arithmetic sent to or delivered in Italy, and published in Italian, as well as lists of bibliographical references about the classical theory of correspondences going the other way.

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[70]As van der Waerden put it: "As far as I am concerned, I already wrote (ZAG XIV, Math. Annalen **115**, p. 642) with complete confidence in 1937: 'The calculus of intersection multiplicities can be used for the foundation of Severi's theory of equivalence families on algebraic varieties.' This means that I stressed the importance of your fundamental ideas and developed at the same time an algebraic apparatus to make them precise in an irrefutable manner [Quant à moi j'ai écrit déjà en 1937 (ZAG XIV, Math. Annalen **115**, p. 642) avec confiance complet [sic]: 'Der Kalkül der Schnittmannigfaltigkeiten kann zur Begündung der Severischen Theorie der Äquivalenzscharen auf algebraischen Mannigfaltigkeiten verwendet werden.' Cela veut dire que j'ai souligné l'importance de vos idées fondamentales et en même temps développé un apparat algébrique pour les préciser d'une manière irréfutable]" [ETHZ, Nachlass van der Waerden, HS 652:8394, page 3]. This is a draft of a letter from van der Waerden to Severi, dated "'Mars 1955."

[71]Es ist hauptsächlich den Arbeiten Severis und des Verf. zu danken, dass heute in den Grundlagen die erforderliche Exaktheit erreicht ist.

[72]See the appendix for the text (and a translation) of this remarkable archive.

In spite of the small Göttingen meeting mentioned above, collaboration inside Germany between van der Waerden and the Hasse group remained scant. A revealing exception to this occurred in the last few days of 1941, when van der Waerden sat down and worked out, in his way of doing algebraic geometry, the proofs of three theorems in [Hasse, 1942] that Hasse had been unable to prove in his set-up. Hasse was overjoyed[73] and asked van der Waerden to publish his proofs alongside his article. Van der Waerden only published them in 1947, however.[74] This was in another mathematical world, one in which Hasse, ever since his dismissal from Göttingen by the British military authorities in 1945, no longer had much institutional power. Van der Waerden was thus free[75] to criticize what he considered Hasse's inadequate approach. His criticism not only showed the distance between van der Waerden and Hasse when it came to algebraic geometry, but confirmed once more van der Waerden's dogmatically conservative attitude with respect to fundamental notions of algebraic geometry.[76] The episode suggests that the war and political or personal factors—that made effective collaboration between the two German groups difficult—mixed with differences of mathematical appreciation in an intricate web of relations which is not always easy to untwine.

We have seen that van der Waerden had been on very good terms with Hellmuth Kneser. In the short note [Kneser, 1935], the latter *very* barely sketched a proof of the Local Uniformization Theorem for algebraic varieties of arbitrary dimension, in the complex analytic setting. Van der Waerden reacted immediately in a letter, inviting Kneser to publish a full account of the argument in the *Mathematische Annalen* and pointing out its importance by comparing it with Walker's analytic

[73]"Your letter was a great joy for me. You will not believe how happy I am that the statements I came up with are not only meaningful and correct, but that you taught me a method to attack these and similar questions. I am convinced that I will make substantial progress with this method, provided I one day have the time to take up my mathematical research work again with full sails [Mit Ihrem Brief haben Sie mir eine grosse Freude gemacht. Sie glauben gar nicht wie glücklich ich bin, nicht nur dass die von mir ausgesprochenen Behauptungen überhaupt sinnvoll und richtig sind, sondern dass ich durch Sie eine Methode gelernt habe, wie man diese und dann auch ähnliche Fragen angreifen kann. Ich bin überzeugt, dass ich mit dieser Methode in meinem Programm erheblich weiterkommen werde, wenn ich einmal die Zeit habe, die mathematische Forschungsarbeit wieder mit vollen Segeln aufzunehmen" [UAG Cod. Ms. H. Hasse 1:1794, van der Waerden, Bartel Leendert; Hasse to van der Waerden, 9 January, 1942].

[74]See [van der Waerden, 1947a].

[75]A letter to H. Braun dated Leipzig, 3 May, 1944 [ETHZ, HS 652 : 10 552] shows that van der Waerden, conscious of his political difficulties at Leipzig, tried—apparently in vain—during World War II to get help from Hasse as well as Wilhelm Süss.

[76]In evidence of this, consider, for example, the following critique of Hasse's notion of a point: "Calling these homomorphisms 'points' fits badly with the terminology of algebraic geometry. A point in algebraic geometry is not a homomorphism but a sequence of homogeneous coordinates or something which is uniquely determined by such a sequence, and so many other notions and notations hinge on this concept of 'point,' that it is impossible to use the same word in another meaning. What Hasse calls 'point' is, in our terminology, a *relationstreue Spezialisierung* $\zeta \to z$, i.e., the transition from a generic to a special point of an algebraic variety [Zu der Terminologie der algebraischen Geometrie paßt die Bezeichnung dieser Homomorphismen als 'Punkte' nicht. Ein Punkt ist in der algebraischen Geometrie kein Homomorphismus, sondern eine Reihe von homogenen Koordinaten oder etwas, was durch eine solche Reihe eindeutig bestimmt ist, und an diesem Begriff 'Punkt' hängen soviele andere Begriffe und Bezeichnungen, daß man dasselbe Wort unmöglich in einer anderen Bedeutung verwenden kann. Was bei Hasse 'Punkt' heißt, ist in unserer Bezeichnungsweise eine *relationstreue Spezialisierung* $\zeta \to z$, der Übergang von einem allgemeinen zu einem speziellen Punkt einer algebraischen Mannigfaltigkeit" [van der Waerden, 1947a, p. 346].

proof [Walker, 1935] of the resolution of singularities of algebraic surfaces.[77] Kneser did not comply. As a result, when van der Waerden reported on 23 October, 1941 at the meeting in Jena of the DMV about "recent American investigations," that is, about Oscar Zariski's arithmetization of local uniformization and resolution of singularities of algebraic surfaces [van der Waerden, 1942], and when he mentioned Kneser's work as a balm for his German audience, he was promptly criticized in a review by Claude Chevalley because that proof had never been published in detail.[78]

Zariski's stupendous accomplishments in the rewriting of algebraic geometry—which between 1939 and 1944 included not only the basic "arithmetic" theory of algebraic varieties but also a good deal of the theory of normal varieties (a terminology introduced by Zariski) as well as the resolution of singularities for two- and three-dimensional varieties—were based on Wolfgang Krull's general theory of valuations much more than on van der Waerden's approach. This heavier algebro-arithmetic packaging visibly separated Zariski's approach from the Italian style in which he had been brought up. The independence of the mature Zariski from his mathematical origins gave him a distinct confidence in dealing with Severi after World War II. For example, it was Zariski who suggested inviting Severi to the algebraic geometry symposium held at the Amsterdam ICM and organized by Kloosterman and van der Waerden.[79]

As noted above, van der Waerden's basic ideas for an algebraic reformulation of algebraic geometry—his generic points and specializations—account for a good deal of the technical backbone of André Weil's *Foundations of Algebraic Geometry*. Moreover, van der Waerden's success in rewriting much of algebraic geometry with these modest methods had, of course, informed Weil's undertaking. In trying to pin down the most important differences between the contributions of van der Waerden and Weil to the rewriting of algebraic geometry, then, the mathematical chronicler must first isolate innovations that Weil brought to the subject and that

[77]See van der Waerden to Kneser, 23 March, 1936 [NSUB, Cod. Ms. H. Kneser A 93, Blatt 10].

[78]See *Mathematical Reviews* 5 (1944), 11. "A previous solution of the problem [of local uniformization] is credited to Kneser [Jber. Deutsch. Math. Verein. 45, 76 (1935)]. This attribution of priority seems unfair. Kneser published only a short note in which he outlined the idea of a proof of the local uniformization theorem. Considering the great importance of the result the fact that Kneser never came back to the question makes it seem probable that he ran into serious difficulties in trying to write down the missing details of his proof."

Totally outside of the context of the resolution of singularities, but as another interesting illustration of the variety of approaches to algebraic geometry that were in the air in the 1930s and 1940s, we mention in passing Teichmüller's sketch [Tecihmüller, 1942] of how to derive the theory of complex algebraic functions of one variable from the uniformization theory of Riemann surfaces. This paper is probably both an attempt to promote his research program towards what is today called Teichmüller Theory, and an expression of Teichmüller's ideas about adequate methods in complex geometry. For the latter aspect, compare the somewhat ideological discussion of relative merits of various methods of proof, and in particular the preference for "geometric" reasonings, in [Teichmüller, 1944, §6].

[79]See the correspondence between Zariski and Severi in [HUA, HUG 69.10, Box 2, 'Serre - Szegö']. In a letter to Kloosterman dated 15 January, 1954 [HUA, HUG 69.10, Box 2, 'Zariski (pers.)'] Zariski wrote: "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."

went beyond what he found in his predecessors, namely, the local definition of intersection multiplicities, the proof of the Riemann Hypothesis, the formulation of the general Weil Conjectures, the use of abstract varieties, etc. But as in Zariski's case, where the valuation-theoretic language immediately created a sense of independence from predecessors or competitors (an independence, however, which would probably be considered pointless if it were not accompanied by mathematical success), Weil produced the same effect via the *style* of his *Foundations.* What struck many contemporaries (who had no notion yet of Bourbaki's texts) as a book full of mannerisms, effectively imposed a practice of doing algebraic geometry *à la* Weil.

Keeping both of these aspects in mind—the novelty of mathematical notions and the new style—is essential for a reasonable discussion of Weil's role in re-shaping algebraic geometry. For instance, pointing to the fact that Weil's *Foundations* get most of their mileage out of van der Waerden's basic notions, as does Serge Lang, does *not* suffice to invalidate Michel Raynaud's claim, quoted by Lang, that Weil's *Foundations* mark "a break (*rupture*) with respect to the works of his predecessors—B. L. van der Waerden and the German school" [Lang, 2002, p. 52]. In other words, Weil's book is a startling example showing how a history of mathematics that only looks at "mathematical content" easily misses an essential part of the story.

To fix ideas, consider the year 1947. A spectrum of five disciplinary practices of algebraic geometry exist:

(1) the classical Italian way,
(2) van der Waerden's way,
(3) the method of Weil's *Foundations*,
(4) Zariski's valuation-based arithmetization, and
(5) (only for the case of curves) the practice of function field arithmetic.

Given the force of the discourse about the lack of rigor in (1) compared to existing algebraic or arithmetic alternatives, and given the dimension-restriction of (5), the real competition took place between (2), (3), and (4). Then, the superficial resemblance between (2) and (1), on the one hand, and the fact, on the other hand, that the basic mathematical concepts of (2) are absorbed in (3), clearly left the finish between (3) and (4). This was precisely the constellation that Pierre Samuel described in the lovely beginning of the introduction to his thesis [Samuel, 1951, pp. 1-2] and with respect to which he opted for the more varied method of (4). A more precise analysis of the mathematical practice of each of the alternatives will yield interesting insights into one of the most spectacular developments in the history of pure mathematics in the twentieth century, but this chapter, it is to be hoped, represents at least a start down this historical path.

Appendix: Extract from a Letter from Hasse to Severi

Ew. Exzellenz und Hochverehrter Herr Kollege,

Es ist mir ein tiefes Bedürfnis, Ihnen heute endlich einen Brief zu schreiben, den ich eigentlich gleich im Anschluss an die Tagung in Baden-Baden schreiben wollte. Die grossen Ereignisse, die inzwischen eingetreten sind, rechtfertigen es wohl, wenn ich zunächst ein paar Worte an Sie als hervorragenden Vertreter Ihres Landes richte, ehe ich zu Ihnen als Mathematiker und Kollegen spreche. Uns Deutsche bewegt in diesen Tagen ein Gefühl tiefster Dankbarkeit für die Treue und Entschlossenheit, mit der Ihr unvergleichlicher Duce zu unserem Führer gestanden hat, und ebenso für die Einmütigkeit und Verbundenheit, mit der sich das ganze italienische Volk zu der Sache unseres Volkes bekannt hat. Es ist wohl auch dem letzten von uns in diesen Tagen klar geworden, dass wir das gesteckte Ziel, die Befreiung der Sudetendeutschen, niemals erreicht hätten, wenn nicht der unbeugsame Wille unseres Führers und unseres Volkes diese kräftige und entschlossene Stütze durch den anderen Pol unserer Axe gehabt hätte. Sie haben ja aus dem Munde unseres Führers gehört, wie er dies anerkennt und wie er bereit ist, auch seinerseits zu seinem Freunde, dem Duce zu stehen, sollte es einmal nötig sein. Sie dürfen überzeugt sein, dass auch hinter diesem Wort das ganze deutsche Volk aus innerster Überzeugung steht.

Dazu, dass auch in unserem Bezirk, der Mathematik, der herzliche Wunsch und das eifrige Bestreben besteht, das Fundament der politischen Axe auf kulturellem Boden zu unterbauen und zu festigen, hätte es wohl des kräftigen Anstosses der letzten Wochen schon gar nicht mehr bedurft. Ich hoffe, dass Sie in Baden-Baden gefühlt haben, wie wir deutschen Mathematiker in dieser Richtung denken und zu arbeiten gewillt sind. Ganz besonders habe ich mich gefreut, dort von dem Plan zu hören, durch eine Reihe von Monographien das gegenseitige Verstehen und die Gleichrichtung der beiderseitigen Schulen in der Algebra und Geometrie zu fördern. ...[80]

Translation

Your excellency, venerated colleague:

It is my deep-felt need at last to write you a letter today, which I had originally wanted to write just after the conference in Baden-Baden. The big events that have occurred in the meantime surely justify my addressing you first as an eminent representative of your country, before talking to you as a mathematician and colleague. All Germans are moved these days by the resolute faithfulness with which your incomparable *Duce* has stood beside our *Führer*, and by the united solidarity which the Italian people have acknowledged in the interest of our people. Down to the last one among us we have realized these days that the intended goal: the liberalization of the *Sudeten*-Germans, would never have been attained, if the unfaltering will of our *Führer* and our people had not enjoyed this strong and resolute support by the other pole of our axis. You have heard it from the mouth of our *Führer*, how he acknowledges this and how he is prepared also to stand by the side of his friend, the *Duce*, if ever this should prove necessary. You may be assured that the German people also stand behind this word with innermost conviction.

In order that also in our domain, mathematics, the heartfelt desire and arduous quest exist to underpin and stabilize the foundation of the political axis in the cultural terrain, the forceful impetus of the past weeks would not even have been necessary. I hope that you will have felt in Baden-Baden [at a meeting of the DMV where Severi had given an invited talk] how we, the German mathematicians, think and are willing to work. I was particularly glad to hear of the plan to enhance the mutual understanding and the synchronization of the schools on both sides in algebra and geometry. ...

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[80]See [UAG Cod. Ms. H. Hasse 1:1585, Severi, Francesco; Hasse to Severi, 3 October, 1938].

Nothing more about this planned series of monographs is known, yet Severi's answer to the spirit of Hasse's letter may be found in the conclusions of his Baden-Baden lecture. There, Severi expressed the "hope that the important progress that Germany has realized in modern algebra will enable your magnificent mathematicians to penetrate ever more profoundly into algebraic geometry, which has been cultivated in Italy over the last 40 years, and that the ties between German and Italian science which have already been so close in this area at the times of our masters will grow every day more intimate, as they are today in the political and general cultural domain" [Severi, 1939, p. 389].[81]

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[81] "Spero che i progressi tanto importanti che la Germania ha conseguiti nell'algebra moderna, consentiranno ai suoi magnifici matematici di penetrare sempre più a fondo nella geometria algebrica, quale è stata coltivata in Italia negli ultimi 40 anni; e che i legami fra la scienza tedesca e la scienza italiana, che furono già tanto stretti in questo dominio ai tempi dei nostri Maestri, divengano ogni giorno più intimi, come lo sono oggi sul terreno politico e culturale generale."

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