

CLASSICS



Shreeram Abhyankar has made significant contributions to many areas of mathematics, especially, to algebraic geometry, commutative algebra, theory of functions of several complex variables, invariant theory, and combinatorics. His work on the Resolution of Singularities began with his path-breaking PhD thesis work in 1953–54 and continued during 1954–1969. This topic always remained close to his heart and he returned to it time and again. He authored a riveting account of these, intermingled with personalised history. We reproduce excerpts from the first seven sections of this article; (for the references quoted, please refer to the original article). We hope this will whet the appetite of the readers, who can then go on to read the full article. The interested reader may benefit by reading another nice expository article by Abhyankar, Historical ramblings in algebraic geometry and related algebra, *Amer. Math. Monthly*, Vol.83, pp.409–448, 1976.

C S Yogananda

RESOLUTION OF SINGULARITIES AND MODULAR GALOIS THEORY

By SHREERAM S. ABHYANKAR

Section 0: Preamble

I want to tell you the story of the problem of resolution of singularities in algebraic geometry and its intimate relationship with Galois theory and group theory. With a view towards making the subject more approachable to prospective students, my story will be intermingled with personalized history.

I shall start off in Section 1 by giving examples of singularities of curves and surfaces. Then in Section 2 we shall see what it means to resolve these singularities. A bit of history will trace my mathematical lineage and its links to the resolution problem in particular and algebraic geometry in general. In Section 3 we shall make the passage from characteristic zero to

Excerpts from S.S. Abhyankar, 'Resolution of singularities and modular Galois theory.' *Bull. Amer. Math. Soc.* 38 (2001): 131–169. (C) 2000 American Mathematical Society. Courtesy: AMS.



CLASSICS

positive characteristic. In Section 4 I shall illustrate the traditional Indian method of learning by recounting my experiences with my father. Then I shall show how my journey from India to the United States for doing graduate work paralleled the march of algebraic discoveries from India through Arabia to Europe in ancient times. This resulted in my meeting my Guru Zariski at Harvard. Section 4 is concluded by describing how I found my Ph.D. problem in Zariski's address at the International Congress of 1950.

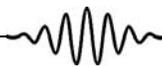
Section 5 gives more details of my early training under Zariski. This was marked by the reading of the fundamental work of Krull on generalized valuations and their use by Zariski to explain birational transformations. Then in Section 6 we come to the high regard held by Zariski for the works of Jung and Chevalley. Jung's work on complex surface singularities dates back to 1908. Chevalley's fundamental paper on local rings appeared in 1943. Zariski asked me to put these two together. In this an important role is played by Zariski's theory of normalization which, following Dedekind, gets rid of singularities in codimension 1. Also topological ideas come to the fore. They tell us the Galois structure of singularities. This amalgamates the ideas of Galois and Riemann.

In Section 7, we come to Zariski's returning from an Italian trip, and my making counter-examples. These counter-examples show a fundamental difference between the Galois theories of singularities in characteristic zero and prime characteristic p . As a result, for resolving singularities, it becomes necessary to augment the abstract college algebra arsenal of local rings and valuations by the explicit manipulations of high-school algebra. It took another ten years to inject more and more high-school algebra for converting the characteristic p surface desingularization to arithmetic surfaces and characteristic p solids. ... Some progress in these was done by some of my former students. Indeed, as I shall relate throughout the paper, in addition to having an illustrious line of teachers and teacher's teachers, I have been blessed with numerous brilliant students who continue pushing the frontiers of the subject. It is a fulfilling pleasure to be sandwiched in between.

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Section 1: What are singularities?

At a singularity a curve crosses itself or has a special beak-like shape or



CLASSICS

both. For instance the alpha curve $y^2 - x^2 - x^3 = 0$ crosses itself at the origin, where it has the two tangent lines $y = \pm x$. Such a singular point is called a node. The cuspidal cubic $y^2 = x^3$ has a beak-like shape at the origin. Such a singularity is called a cusp. Most points of a curve are simple points. Singularities are those points where the curve has some special features. The vertex of the quadratic cone $z^2 = x^2 + y^2$ is an example of a surface singularity. Thus the origin $(0, 0, 0)$ is a double point of the cone, and all other points of the cone are simple points.

Quite generally most points of a hypersurface in n -space given by a polynomial equation $f(x_1, \dots, x_n) = 0$ are simple points. At a simple point P at least one of the partial derivatives of f is not zero. Say, the partial of f with respect to x_1 is not zero at P . Then by the implicit function theorem we can solve $f = 0$ near P by expressing x_1 as a function of x_2, \dots, x_n . When we cannot do this, we have a singular point. Thus the singular points of the hypersurface are where f as well as all its partials are zero. Here by partials we mean the first partials.

If all the first partials of f are zero at a point P , but some second partial is not, then P is called a double point. Similarly for triple or 3-fold, 4-fold, \dots , e -fold points. Thus simple means 1-fold. If the locus in n -space we are studying is given by several equations $f_1 = 0, \dots, f_m = 0$ in x_1, \dots, x_n , then instead of taking the n first partials of the single polynomial f , we take the $m \times n$ jacobian matrix of the first partials of the polynomials f_1, \dots, f_m with respect to the variables x_1, \dots, x_n . Now the singular points are defined to be those where the rank of this matrix is not maximal. Alternatively, simple points are those where its rank is maximal, and then by the multivariate implicit function theorem, near a simple point, all of the variables can be expressed as functions of some of them, say d of them. The given locus consisting of the common solutions of the simultaneous equations $f_1 = 0, \dots, f_m = 0$ is called an algebraic variety of dimension d .

Section 2: What does it mean to resolve them?

Let us make the substitution $x = x'$ and $y = x'y'$ in the equation of the nodal cubic, i.e., the alpha curve given by $y^2 - x^2 - x^3 = 0$. This gives us $y^2 - x^2 - x^3 = x'^2 y'^2 - x'^2 - x'^3 = x'^2 (y'^2 - 1 - x')$. Discarding the extraneous factor x'^2 we get the proper transform $y'^2 - 1 - x' = 0$, which being a parabola



CLASSICS

has no singularities. Thus the singularity of the nodal cubic is resolved by one quadratic transformation. The inverse of the quadratic transformation is given by $x' = x$ and $y' = y/x$. The indeterminate form y/x indicates that, as x and y both approach zero, y' takes all possible values. In other words, the origin $(0, 0)$ of the (x, y) -plane blows-up into the line $x' = 0$ of the (x', y') -plane which we call the exceptional line. It is this explosion that unravels the singularity. By putting $x' = 0$ in the original equations $x = x'$ and $y = x'y'$ we directly see that the exceptional line shrinks to the origin $(0, 0)$ of the (x, y) -plane. The total transform of the nodal cubic consists of the exceptional line together with the proper transform which is a parabola. At any rate a quadratic transformation and its inverse both involve only rational expressions, and so a quadratic transformation is birational. A birational transformation of a plane is called a Cremona transformation in honor of the originator of such transformations. As a piece of history, Cremona was my triple-parama-guru. Veronese was a pupil of Cremona. Castelnuovo was a pupil of Veronese. My own guru Zariski was a pupil of Castelnuovo. This makes Castelnuovo my parama-guru and Veronese my parama-parama-guru.

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Section 3: Resolution from Riemann through Zariski to Hironaka

Thus Riemann's topological and function-theoretic construction of the Riemann surface of $f(x; y) = 0$ was algebracized (or geometrized) by Noether in resolving the singularities of the curve $C : f(x, y) = 0$ by a succession of Cremona quadratic transformations. Another way of algebracizing the Riemann construction was put forward by Dedekind in the Dedekind-Weber paper [DWe] of 1882. To wit, Dedekind took the integral closure of the affine coordinate ring of C in its quotient field and, lo and behold, the singularities of C are resolved in one fell swoop. Emmy Noether [NoE], the daughter of Max, followed Dedekind in developing the college algebra of groups-rings-fields by adding to it a rich chapter of ideal theory. On the other hand, Max Noether's algebraization belonged to the high-school algebra of polynomial and power series manipulation. Thus Max Noether, in addition to being a possible father of algebraic geometry, could also be called a grandfather of modern algebra.

Amongst the three approaches to curve resolution, namely that of Riemann,



CLASSICS

Noether, and Dedekind, it was primarily Noether's method which found its flowering in resolution of singularities of surfaces and solids (= 3-dimensional objects) by Zariski [Za2], [Za4] in 1939-1944, and then in 1964 by Hironaka [Hir] for all dimensions. However, the Zariski-Hironaka work was restricted to characteristic zero, i.e., when the coefficients of the polynomials defining the variety are in a field of characteristic zero such as the field of real or complex numbers. A field is a set of elements with the operations of addition and multiplication defined on them. It is of characteristic zero means that in it $1 + 1 + \dots + 1$ is never zero. It is of prime characteristic p if in it $1 + 1 + \dots + 1$, taken p times, equals zero. For example we could add and multiply any two of the integers $0, 1, \dots, 6$ and then replace the result by the remainder obtained after dividing it by 7; this gives us a field of characteristic 7; in it, for instance, we have $4 + 4 = 1$ and $4 \times 4 = 2$. So what about resolving singularities of surfaces, solids, and higher dimensional varieties, over a field of characteristic p ? This is where I entered the picture.

Section 4: How I got interested in doing resolution

To start at the beginning, my father taught me mathematics by the ancient Indian method. Thus he would recite to me a few lines from the geometry and algebra books written by Bhaskaracharya around 1100 A.D. These books, called *Leelavati* and *Beejganit*, being the first two parts of his five-part treatise on astronomy, are in Sanskrit verse. I would then commit those lines to memory by repeating them several times. My father would follow it up by reciting a few more lines. With the implicit faith that their sound would eventually reveal their meaning, I would repeat the new lines several times, and so on. The solution of a quadratic equation by completing the square which I had memorized in this manner is reproduced in my 1982 Springer Lecture Notes on Canonical Desingularization [A12]. There I go on to say that the entire essence of Hironaka's proof lies in generalizing this completing of the square method.

This is an example of the dictum that history should be interesting and inspiring, though not necessarily completely true. What I am referring to is the fact that although I did learn from my father, when I was about ten, Bhaskaracharya's book on geometry called *Leelavati*, it was much later when I was about forty-five that at my request my father made a Marathi translation of Bhaskaracharya's book on algebra called *Beejganit*, and it was



CLASSICS

only then that I memorized the verse giving the completion of the square method of solving quadratic equations.

At any rate, armed with the high-school algebra (i.e., manipulative algebra) training from my father, in July 1951, when I was twenty, I set out for America by boat.

Embarking from the Bombay pier my boat took the same path by which algebra had traveled from India to Europe via Arabia, where it acquired its current Arabic-derived name as opposed to its original Sanskrit name Beejganit. Refueling at the Arabian Sea port of Aden, the boat proceeded to touch land next in Italy, where, in the sixteenth century, cubic and quartic equation were solved by Cardano and Ferrari. Next stop was Marseille, in France. There I said, “Aha, here is where the youthful Galois, in 1830, proved the impossibility of solving quintic equations by introducing the Galois group.” Once again this is an example of making history colorful, because actually by the time the boat touched port in Marseille I had become unconscious because of typhoid fever, which I caught on the boat. Also the boat may have landed in Italy only in my imagination. From there we went on to England, where I was stranded for two months in the Seaman’s Hospital. In my hospital bed I may have been (?) absorbing the sympathetic waves radiated by Caley-Sylvester-Salmon as they went about creating algebraic geometry from 1840 to 1880. Was I also imbibing the spirit of Newton, which is perennially alive in his binomial theorem with exponents integral or fractional and in his fractional power series expansions, which were rediscovered one hundred fifty years after him by Puiseux?

Thus I was a month and a half late arriving at Harvard. It happened to be a Saturday, when normally professors do not come to the department. Luckily for me on that Saturday the departmental secretary was there and pointed out to me, “Mr. Zariski is here, so you may go and talk to him.” I proceeded to have a long two-hour conversation with Zariski. He asked me many questions and I reciprocated, not yet being exposed to the western etiquette of avoiding personal questions. So he found out that my father was a math professor, and I found out that he was brought up by his mother in a town where the borders changed between Russia and Poland, but he regarded himself as a Russian. He told me that his mother had a cloth shop. My teachers in Bombay had suggested I take three elementary courses



CLASSICS

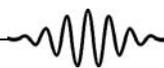
and one advanced course, as was allowed to entering graduate students at Harvard, but after talking to me Zariski completely changed my plan, and so I ended up with three advanced courses and only one elementary course and that too only because Zariski was teaching it. Thus, I never took a basic course on linear algebra. I at once proceeded to take Zariski's advanced course based on notes of his forthcoming book, which eight years later metamorphed into his two-volume treatise [ZSa] with Samuel. The elementary course which I took with Zariski was Math 103 on projective geometry.

On that same eventful Saturday, at Zariski's suggestion, I walked to his house to borrow his copy of Veblen and Young's book [VYo] on projective geometry, which was being used in the course. This was the first of many trips to his house, later accompanied by meals there due to the kindness of Mrs. Yole Zariski. Especially I remember the tasty salads made with romaine lettuce.

So my relationship with Zariski started the very first day I arrived in America.

At the end of the first semester, seeing how excited I had become about the projective geometry course, Zariski said to me, "Projective geometry is a beautiful dead subject. Do not try to do research in it." Nevertheless, in my second semester, being scared out of Whitney's topology course, I took yet another semester of higher dimensional projective geometry with Zariski, in which he explained Grassman coordinates.

During the summer between my first and second years in graduate school I spent most of my time, almost twelve hours a day, in the library looking through many of Zariski's papers. I came upon Zariski's hour lecture at the International Congress of Mathematicians held two years before in Cambridge. It was the first such meeting after the sixteen-year interruption caused by the war and its aftermath. Zariski's paper [Za5] was on ideas of abstract algebraic geometry. In it he described how he resolved the singularities of surfaces and solids in characteristic zero and declared the problem in characteristic p to be intractable even for the innocent-looking surface $z^p = f(x, y)$. He continued by saying, "It is not a problem for the geometer, but it is a problem for the algebraist with a feeling for all the



CLASSICS

unpleasant things which can happen in characteristic p ." On reading this, I immediately said to myself, "This is what I am going to do for my thesis."

Section 5: Zariski gives me a reading course

Towards the end of my first summer at Harvard, i.e., late August or early September of 1952, one day I met Zariski riding home on his bicycle. Seeing me he got down, and we chatted a bit. Then I asked him if I could have a reading course with him that fall. Whereupon he asked me if I was going to register for his course on algebraic curves. When I said I did not know, he responded that then he would not give me a reading course. To that I shrugged my shoulders, and he started riding away. After going a bit he returned. Getting down from his bicycle, he asked me whether I was going to work with him. Again, when I said I did not know, he repeated that he would not give me a reading course and started riding away. After riding off a short distance he again returned. Again getting down from his bicycle, he said, "All right, you may have the reading course," and he rode away. So in the end I did register for his course on algebraic curves and for the reading course. I found his very first lecture on algebraic curves, full of places and valuations, so exciting that at the end of it I went up to him and told him that yes, I did want to work with him and have chosen as my thesis topic the problem of characteristic p resolution mentioned in his International Congress Address of 1950. To that he smiled and let it pass. During the next several months he tried to indicate that this was not a thesis problem and I should be working on something easier. Then in the spring he went off to Italy for a semester.

Having taken three courses with Zariski and having gotten so interested in his work, why was I being so evasive about working with him? The problem was that Pesi Masani, my teacher in Bombay, was a student of Garrett Birkhoff. Masani had communicated with Birkhoff regarding my admission to Harvard. On the boat from Bombay I had solved one and a half unsolved problems listed in Birkhoff's book [Bir] on lattice theory. Birkhoff assumed that this would form the main part of my thesis. In later years I had many pleasant contacts with Birkhoff, and in fact my "Historical ramblings" article [A11] was written largely due to his constant encouragement. Moreover, my early training in lattice theory had gotten me interested in ordered structures. That is what immediately attracted



CLASSICS

me to the problem of resolution of singularities with its close connection to valuation theory which involves ordered abelian groups. The original construction of real valuations by Ostrowski [Ost] in 1918 was generalized by Krull in his 1932 paper [Kr1] on “Allgemeine Bewertungstheorie”, where he employed values in any ordered abelian group. These general valuations of Krull form the basis of Zariski’s paper [Za3] on birational correspondences, where he uses them to algebraicize the idea of limits.

Section 6: Zariski asks me to read Jung and Chevalley

In the spring of 1953, while Zariski was in Italy, we corresponded several times. Seeing that I was not giving up on characteristic p resolution, he wrote me a four-page letter suggesting possible approaches. Specially he suggested that I should read the 1908 paper [Jun] of Jung, where he proved a local version of resolution of singularities of complex surfaces. Zariski went on to say that I should also study the 1943 local rings paper [Che] of Chevalley and use it to algebraicize Jung’s proof with a view of adapting it to characteristic p Eventually I ended up by showing that Jung’s method cannot be adapted to characteristic p because, even at a simple point of the branch locus, the local fundamental group need not be abelian and actually may even be unsolvable, and points of the normal surface above it may be singular. . . .

Towards the end of his stay in Italy, Zariski sent me a postcard suggesting that I go over to his house in Cambridge to meet his son, Raphael, who would let me take whatever of Zariski’s reprints I could find, including the only remaining copy of his 3-dimensional resolution paper [Za4], and to get Raphael’s help for getting a summer job in the Harvard University Press, where he was working. In the same postcard Zariski informed me, referring to his recent ulcer operation, that a 3-dimensional singularity was removed from his stomach. This was a picture postcard of Michaelangelo’s statue of Moses. It is still in my possession. Ever since, in my mind, I have always identified Zariski with the wise Moses.

Section 7: I make counter-examples

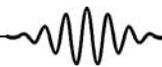
In the fall of 1953, when Zariski returned from Italy, I presented him with my 50-page essay on the algebraization of local fundamental groups as a family of Galois groups, which is how I paraphrased Jung’s ideas by using



CLASSICS

Chevalley's local rings paper supplemented by the equally fundamental 1938 local rings paper [Kr2] of Krull and the 1946 local rings paper [Coh] of Cohen. This essay was subtitled "My attempt at understanding your four-page letter" together with the sentence "Please tell me if I am studying in the right direction." I followed this up by biweekly ten-page bulletins entitled "On uniformization i " with $i = 1, 2, \dots, 9$. In Bulletin 1, I gave a counter-example to the local fundamental group at a normal crossing being abelian even in characteristic p . Then Zariski said that at least at a simple point of the branch locus the local fundamental group should be cyclic. In Bulletin 2, I showed that, in characteristic p , this need not even be solvable. Then Zariski suggested that at least points of the surface above a simple point of the branch locus should be simple. In Bulletin 3, I gave a counter-example to this also. After two or three further such negative bulletins, Zariski said that his ideas were exhausted. After a couple of more weeks of hard work I dialed TR(owbridge) 6-7938, which was Zariski's home phone number, still etched in my memory, and told him that I cannot do characteristic p resolution. Zariski responded that all right, after some time we could discuss a suitable thesis problem for me.

After that phone call, continuously for three days and three nights, that is seventy-two hours at a stretch, I kept working on characteristic p surface resolution and got the first positive result by uniformizing valuations whose value group consists of rational numbers with unbounded powers of p in their denominators, since, in his 1950 International Congress Address, Zariski had declared these to be the most intractable. As a beginning I dealt with the case of $p = 2$. Excitedly, late at night I telephoned Zariski with the good news. Early next morning he came to the department, and I started explaining the matter to him on the blackboard. When I fumbled, Zariski said, "What is the matter with you, Abhyankar? In spite of having a fever I have come to listen to you." Fortunately my fumble was only temporary. Then in the next three or four biweekly bulletins I finished resolving singularities of characteristic p surfaces. This positive result appeared in my 1956 paper [A02]. The various counter-examples appeared in my 1955 paper [A01]. Two years later, in my 1957 paper [A05], by taking plane sections of the surfaces involved in these counter-examples, I was led to a conjecture about fundamental groups of affine curves in characteristic p . Then, in my 1959-60 papers [A06], I used the local fundamental group results of

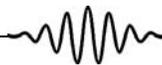


CLASSICS

[A01] to obtain some results on global fundamental groups of surfaces in characteristic p .

In April or May of 1954 Zariski said that I should submit my Ph.D. thesis, but I held off, saying that I wanted to do it for any dimension. By August, Zariski insisted I finish. In fact he had already applied to the newly formed National Science Foundation to grant me a post-doctoral fellowship for the academic year 1954-55, saying in the application that during that year I was well qualified to do the higher dimensional problem. When in September 1954 Zariski gave a dinner party to celebrate my thesis, he told me that he too was unhappy that I was about to give up the problem, because, although it was his duty to say that it was not a thesis problem, he had very much hoped I would be able to solve it. Another pleasant memory I have about my thesis is that John Tate joined the Harvard math department the day I defended my thesis, and I was able to answer all the questions he asked me in my oral exam. Yet another amusing memory is that when originally I had written only a two-page introduction to my thesis, Zariski wrote from his Nantucket vacation cottage that I must do a better job in my introduction, because that was the only part which Richard Brauer, who was the second reader, was expected to read. When I ended up writing a twelve-page introduction, Zariski was very happy. In spring 1953, when Zariski was in Italy, I had taken a course on noncommutative rings with Brauer, and when I proposed Cohen's paper [Coh] as a topic for the term paper for that course, Brauer said, "No, that is too much Zariski-type," and so I ended up writing about Hochschild cohomology. It took me another thirty-five years to realize what great group theory pioneering work Brauer had done. Also I have pleasant memories how, during my post-doctoral year 1954-55, Tate used to come over to my apartment to give me private lessons in algebraic number theory. During that year I also heard some inspiring MIT lectures of Iwasawa on infinite Galois theory of algebraic number fields. The influence of Brauer, Iwasawa, and Tate is evident in my work on algebraic fundamental groups.

In spite of Zariski saying to NSF that I was well equipped to do higher dimensional resolution during the academic year 1954-55, actually it took me ten more years to do characteristic p resolution for dimension 3, as reported in my 1966 Academic Press book [A08], and for dimensions higher than that the problem still remains open. In the meantime in my 1965 Purdue Conference paper [A07], I had proved resolution of singularities for arithmetical



CLASSICS

surfaces, i.e., surfaces defined by polynomials with integer coefficients, which in my 1969 Tata Institute Conference paper [A10] I extended to the still more general situation of 2-dimensional excellent schemes. Again the resolution problem remains open for three and higher dimensional arithmetical varieties or excellent schemes. Seeing that not much progress in the resolution problem was made after my 1966 book, Springer-Verlag put out a new edition [A31] of it in 1998. In this new edition, I have added an appendix giving a short proof of analytic resolution for all dimensions in characteristic zero. This proof is a consequence of a new avatar of an algorithmic trick which I had used in the original edition of the book. The same algorithmic trick was also used in my 1966 paper [A09], dedicated to the centenary of my parama-guru (= guru's guru) Castelnuovo, to prove some lemmas leading to local uniformization of valuations of maximal rational rank for all dimensions in characteristic p . This analytic proof should provide a good introduction to the great papers of Zariski and Hironaka. Hopefully it should also provide an impetus to young algebraic geometers to complete the resolution problem.

