Irena Peeva *Editor*

Commutative Algebra

Expository Papers Dedicated to David Eisenbud on the Occasion of his 75th Birthday

Springer

Commutative Algebra

Irena Peeva
Editor

Commutative Algebra

Expository Papers Dedicated to David Eisenbud on the Occasion of his 75th Birthday

Springer

Editor
Irena Peeva
Department of Mathematics
Cornell University
Ithaca, NY, USA

ISBN 978-3-030-89693-5 ISBN 978-3-030-89694-2 (eBook)
https://doi.org/10.1007/978-3-030-89694-2

This Springer imprint is published by the registered company Springer Nature Switzerland AG
The registered company address is: Gewerbestrasse 11, 6330 Cham, Switzerland

Dedicated to David Eisenbud on the occasion of his 75th birthday.

Biosketch of David Eisenbud

David Eisenbud received his PhD in mathematics in 1970 from the University of Chicago under Professor Saunders MacLane and Professor Chris Robson. He was in the faculty at Brandeis University from 1970 until becoming Professor of Mathematics at UC Berkeley in 1997. Eisenbud has been a visiting professor at Harvard, and in Bonn and Paris.

His mathematical interests range widely over commutative and non-commutative algebra, algebraic geometry, topology, and computer methods.

Eisenbud served as the director of the Mathematical Sciences Research Institute from 1997 to 2007 and 2013 to 2022. He worked for the Simons Foundation between 2009 and 2011, creating the Foundation's grant program in mathematics and the physical sciences. He is currently on the board of directors of the Foundation, and is also a director of Math for America, a foundation devoted to improving mathematics teaching.

Eisenbud has been a member of the Board of Mathematical Sciences and their Applications of the National Research Council, and the U.S. National Committee of the International Mathematical Union.

He currently chairs the editorial board of the *Algebra and Number Theory* journal, which he helped found in 2006. He serves on the board of the *Journal of Software for Algebra and Geometry*, as well as Springer-Verlag's book series Algorithms and Computation in Mathematics and Graduate Texts in Mathematics.

In 2006, Eisenbud was elected a Fellow of the American Academy of Arts and Sciences. He won the 2010 Leroy P. Steele Prize for Mathematical Exposition for his book *Commutative Algebra, with a View toward Algebraic Geometry* and the 2020 Award for Distinguished Public Service, both from the American Mathematical Society.

Eisenbud's interests outside of mathematics include theater, music, and juggling. He loves photography and music, and sings Bach, Brahms, Schubert, and Schumann . . .

Mostly Mathematical Fragments of Autobiography

David Eisenbud

I was born on April 8, 1947, to Leonard Eisenbud, a mathematical physicist (and former student of Eugene Wigner), who was then working at the Oak Ridge National Laboratory, and Ruth-Jean Eisenbud, a psychologist-psychoanalyst (and former student of Robert White) with a large private practice.

The family soon moved to Long Island, where my father worked at Brookhaven National Laboratory, and I developed an early love for the water—I have a photo of my mother lying on the sand at the edge of the waves with me on her back, grinning.

When I was three, we moved to the Swarthmore area and stayed there eight years. My father worked for a research lab, and we lived initially on the edge of the property where the lab had a van de Graaf particle accelerator. I was captivated by the big machine, which my father patiently explained. I attended a public elementary school, and then the tiny progressive "School in Rose Valley." I apparently had such a poor sense of pitch in second grade that I was forbidden to sing with the rest of the class, but my music teacher in Rose Valley rescued me and taught me to hold a tune—a fateful development. Art was an unsolved problem for me too: terminally stuck on what painting to contribute to a frieze about world history, my teacher took pity and suggested I paint "The Dark Ages"—all black.

When I was seven, my parents took me to my first Shakespeare play—Macbeth—preceded by my first lobster dinner, at the original Bookbinders' Restaurant, a classic that is no more. My parents had prepared me for the play as best they could, but my mother told me later that she worried how I would take all the violence. She was relieved when I leaned over during the play and whispered "They forgot one of the murders!" Whether or not I was correct, the experience began a lifelong love of theatre (lobster, too).

We moved back to Long Island when I was 11. There, my father helped found the Stony Brook University physics department, where he worked until he retired, and my mother joined the faculty of the NYU postdoctoral program in psychoanalysis. I had an academically excellent but socially difficult middle school experience at the Friends Academy in Locust Valley, and then the opposite at the public Huntington High School.

When I was about 12, I began announcing, without much apparent cause, that I wanted to be a mathematician. I read Thomas' Calculus at 13, and then began to study mathematical topics proposed by one of my father's colleagues—Mendelson's book on topology, Wilder's book on the foundations of mathematics. Another colleague introduced me to the games of Go and Shogi. My father showed me how to use vectors and algebra to prove simple theorems in plane geometry, which I found very exciting. I entered some science/math fairs with this, with an analogue computation of logarithms, and with a calorimeter. I took folk guitar lessons, and my first serious girlfriend introduced me to the flute—I briefly considered trying to become a professional flutist (luckily, I stuck with math!).

Having exhausted the math and much else in high school I asked, late in my junior year, to leave for college, and was accepted at the University of Chicago, where I entered at 16 (not by any means exceptional there) and left with my PhD at 23. Despite the famed breadth of education at Chicago, I quickly focused on mathematics and music. Fortunately, the music came with some breadth, and I had wonderful mentoring from the well-known musicologist Howard Mayer Brown and from Brown's political scientist partner Roger Strauss, in whose home we practiced. I sang in Brown's small chorus, and played early instruments—recorders, krummhorns, Quantz flute, bass viola da gamba—in Brown's *Collegium Musicum* all seven years I was in Chicago. The group was the first to systematically record the pieces from the Historical Anthology of Music, a collection used by every musicology graduate student, and we gave a formal concert each quarter in the beautiful Bond Chapel. The most memorable concert for me was Schütz' Christmas Oratorio, in which I played the recorder. (I listen to a recording of this piece every December.) Music has remained a passion: after years of serious flute study, I started voice lessons in 1982, focusing on German Lieder, and I still spend many hours a week enjoying this art. The mathematician, pianist, and cellist Arthur Mattuck, my music partner for years, referring to the characteristic subjects of these songs, once wrote that I was "singing songs of puberty in a baritone Schuberty."

Among the math courses I took as an undergraduate, three stand out as exciting and inspiring. They could not have been less alike. The first was taught by Otto Kegel, a postdoc who was a student (and then *Assistent*, in the German sense) in the group of Reinhold Baer, in Frankfurt. Kegel taught a second semester linear algebra course using sesquilinear instead of merely bilinear forms, and with other (too) modern flourishes. In a subject where most objects are called V or W, Kegel was still struggling with the transition from German, where the word for W is pronounced "Vay." These things, combined with Kegel's almost illegible handwriting, made the course extremely hard to follow. Nevertheless, Kegel imbued it with such wonderful excitement that it was a peak experience.

The other two courses were excellent in a more standard way: in one, the famous analyst Antoni Zygmund told a highly polished and perfected version of the story of the Lebesgue integral, and in the other, Felix Browder laid down the basics of the theory of functions of one complex variable. To this day I am amazed by the consequences that flow from a simple hypothesis in that subject. With my inseparable undergraduate friend Joe Neisendorfer, I wrote notes for Browder's course (in pencil, and with plenty of erasures!).

I spent an exciting summer working at the University of Michigan as a counselor for a high-school math program and being tutored by family friend Paul Halmos, with problems from his manuscript *Hilbert Space Problem Book*. This gave me the idea that I wanted to study operator theory. By the end of my third year at Chicago I was taking only graduate math courses, and at the end of the year I officially became a graduate student.

The next summer, my parents treated me to a few months abroad. I chose to go and work with Otto Kegel, who was by this time back in Frankfurt. Kegel proposed a research problem about the order automorphisms of infinite ordered sets. I was possessed by the problem, and could talk of nothing else, no doubt tiresome to those around me! Though I had taken a German class in college (and gotten a very solid D), I unfortunately did not try to speak German, nor did I understand it—with one exception: Saunders MacLane, whom I knew from Chicago, came to visit. Though he spoke German easily, he had such thick American accent that he was easy for me to understand. I was relieved when I was given, as office-mate, a young English mathematician, but Bert Wehrfritz' cockney accent was almost as much of a problem for me as German. Peter Neumann was also a visitor to Frankfurt then, and when I went to England at the end of the summer, he kindly invited me to Oxford and took me to lunch at the High Table. In that hot weather I was living in a youth hostel, and I'll never forget that first time drinking cold hard cider from an ancient silver mug.

Back in Chicago, I was uncertain what direction to study—neither permutation groups nor operator theory were represented on the faculty. Advised by Neisendorfer to choose a thesis advisor first and subject second, I gravitated to Saunders MacLane and—thus—category theory. However, this was not to be my thesis: during MacLane's sabbatical I made friends with J. C. (Chris) Robson, former student of Alfred Goldie in Leeds, who was in Chicago as a postdoc with Israel Herstein. In an intense and exciting (for me) collaboration, Robson and I developed a noncommutative analogue of the theory of Dedekind domains.

At a memorable dinner that spring, the graduate student across the table from me said something implying that the work with Robson would be my thesis. I began to protest. . .when inspiration struck, and I realized how nice it would be to have a thesis done without a "thesis neurosis"! MacLane and Robson were generous, and I was done. Since this was already at the end of the spring term, it made sense to take an extra year, having only the (then) light responsibility of a graduate student and the freedom of a postdoc.

In the spring of 1968, MacLane took me along to a conference on category theory at the Batelle Institute in Seattle. David Buchsbaum, whose thesis had laid the foundation of Abelian categories, was to give a series of lectures on commutative algebra, and MacLane advised me to prepare for these lectures (at the time I knew no commutative algebra at all) and follow them closely. I was strongly drawn to

Buchsbaum for his great warmth and humanity and was also fascinated by his treatment of homological commutative algebra.

I volunteered to write the notes for the lectures and worked with Buchsbaum on them. Things were good in the first lectures—that treatment of the Koszul complex is preserved in my own book on commutative algebra. But in the last lectures, Buchsbaum turned to his thoughts on the resolution of lower-order minors of a generic matrix, then an open problem. I found the lectures muddy, impressionistic, and confusing, and suggested a reorganization. This did not go over well! Buchsbaum and I ultimately agreed to simply leave that material out.

The contact with Buchsbaum was decisive: I decided I would like to go to work near him, at Brandeis University, in Waltham near Boston. The academic job market in 1970, when I got my PhD, was quite different than it is today. This was just at the end of the period, sparked by the U.S. investment in research following Sputnik, when jobs in the sciences and mathematics were plentiful. There was no "Mathjobs," and people applied to few places. I initially applied only to Brandeis, but Nathan Jacobson, who knew of some of my work, wrote to Kaplansky to suggest that I apply to Yale too, and I followed Kap's advice. With offers from both places, I kept to my plan and accepted Brandeis.

I had met Monika Schwabe, a medical student, at the wedding of my cousin Bob to Monika's college friend Karen in the spring of 1966. I was 19 and living in Chicago. Monika was 23, living in NYC, and involved with others. I was interested, but the relationship did not develop. However, a few years later Monika thought that, after all, I had possibilities, and a courtship began. Ultimately Monika braved the disapproval of her medical school and her mother to take a year off to live with me in Chicago. At the end of the year, during a backpacking trip in the high Sierra, we decided to get married, and Monika returned to finish medical school in New York. In the Spring of 1970 I got my PhD, Monika got her MD, and we wed, in quick succession. We packed up and moved to Boston to take up my job at Brandeis, Monika's residency in child psychiatry at the Beth Israel Hospital, and a new life in an apartment in Central Square, Cambridge, a few blocks from the city hall where my parents had been married.

It was not only the job market that was different in 1970. Brandeis had been welcomed as the third member of the former Harvard-MIT colloquium, and the talks rotated, every third week in each place. At least as important for Monika and me as newcomers: there was a large and elaborate colloquium party, often with 30–50 people, nearly every week, at which we met "everyone" in the area. I was only later aware how much this institution, immensely valuable to me as a young mathematician, depended on the non-working spouses—wives, in every case—of senior members of the community. While the mathematician husbands listened to great (or not-so-great) talks, these women prepared and set out great quantities of food and drink, and smilingly welcomed the guests—who were their friends, too. The "job" of Faculty Wife is nearly gone, and largely unlamented, but in this regard, it served the mathematical world well, and certainly not only in Boston.

Mathematically speaking I was quite lonely during the first half-year in Boston. My thesis on noncommutative rings had led to a collaboration with Phillip Griffith (no s) about Artinian rings. Surprising as it now seems, at the time the only people in the Boston area interested in finite dimensional algebras, or indeed in any non-commutative algebra, were Bhama Srinivasan and the great but already elderly Richard Brauer—not a community for me. After a semester I figured out what to do: I went to Buchsbaum and brashly told him that I had a lot of energy that I would like to use to work on a problem with him! He accepted this proposition, and we began an intense collaboration of nearly 10 years, including some of my best work. In my second year at Brandeis Graham Evans arrived for a second postdoc at MIT. I knew Graham and his wife Kaye from graduate school—he graduated a year ahead of me, and we were good friends. Monika and I had admired their early married arrangements, unusual among the students then. During the summer, I joined Graham most days in his office at MIT. We ran a seminar together that included some odd characters. Once, one of the members came to the seminar with a bowl of water and a towel; as the seminar began (I was the speaker), he carefully washed and dried his face, folded his arms on his desk, put his head down, and went to sleep. Graham had a secretary/technical typist to himself that summer, and when we finally produced a manuscript (Basic Elements) she seemed glad to have something to do at last: she drew a cherub, celebrating with a trumpet on the cover page. More importantly, the next academic year we collaborated in solving a famous problem, proving that *Every Algebraic Set in n-space is the Intersection of n Hypersurfaces*. In the end, I think that this is what earned me tenure at Brandeis.

I had another stroke of good luck in my second year at Brandeis: an invitation to a workshop in Oberwolfach. At that time there were (informally!) two kinds of full professors in Germany: those with and those without an annual week reserved in Oberwolfach for them, their groups of students, and their invitees. Baer had such a week, and I had gone along the summer I visited Kegel. Now I was invited to the annual workshop run by Kasch, Rosenberg and Zelinsky—(I later learned that Kasch had noticed a paper I'd written as a graduate student giving a homological proof of a known theorem about when subrings of Noetherian rings are Noetherian.)

I was even given the opportunity to speak, and I explained my newest paper with Buchsbaum, *What Makes a Complex Exact*. Maurice Auslander, my senior colleague at Brandeis, was in the audience, and seemed impressed as well. Ever since, Oberwolfach has seemed a magical place for me, and I have made a point of going back whenever I could—at least 30 times over the intervening 50 years. With perhaps the best mathematical library on the planet, and a perfect setting for walks and afternoon cake, it is a great place to work with others as well as to listen to talks.

After the workshop I was invited to go for a week to Regensburg to visit Juergen Herzog, in the group under Ernst Kunz. I stayed with Juergen, and we became good friends. It was in his household that I first had to try to speak German—a poor showing. I lectured at the University (in English!), and Kunz was extremely kind to me.

Monika and I spent the summer of 1972 traveling. In my mind from that summer are the pleasure of the St. Andrews Mathematical Colloquium in Scotland (Halmos was the principal speaker) and a lecture by Verdier on a very general form of the Riemann-Roch theorem, in Aarhus, Denmark. I knew next to nothing about algebraic geometry, but I dared to approach Verdier afterwards and asked him what the Riemann-Roch theorem was good for. I got no answer—the question left him speechless with disbelief! We then spent the fall in Leeds, England where I visited Chris Robson and Monika worked as a "Registrar" (= Resident) at High Royds psychiatric hospital; she reported that all activity stopped, daily, for afternoon tea, just as in the math department. The earliest notes for Commutative Algebra with a View toward Algebraic Geometry, finally published in 1995, came from lectures I gave there (on Noether normalization).

In our study of free resolutions Buchsbaum and I made many computations by hand, using a method he knew. We hired Ray Zibman, an undergraduate, to program it, and quickly learned that it was NOT an algorithm—without human curation it often looped. At the same time Graham Evans at Urbana hired Mike Stillman to program the computation of free resolutions of homogeneous ideals "up to a given degree" by ordinary linear algebra. Fast forward to 1983, when Mike came to graduate school at Harvard. There he met Dave Bayer and learned about Gröbner bases: soon the program Macaulay was born. Mike was later a postdoc with me, and I felt that I was for many years Macaulay's "uncle," collaborating often with Bayer and Stillman on computations (in recent years, collaborating with Mike and Dan Grayson, I became a member of the Macaulay2 team itself.) Macaulay, Macaulay2, and the computations they enabled have played a major role in my mathematical career. As I said at a Bayer-Stillman 60th Birthday party, Macaulay is the only video game to which I've ever been addicted!

Backing up to 1974, it was time for me to run the tenure gauntlet at Brandeis. Given that I had a powerful advocate in Buchsbaum, one might think that it would be an easy process, and perhaps compared to other tenure processes it was but. . .during it one senior colleague told me outright that he would not vote for me—because I might attract students away from him! Another threatened to vote against me because of an old disagreement with Buchsbaum. These threats could have been fatal, since at that time the Brandeis department operated on unanimity. After the first threat I made a trip to Montpellier, where Buchsbaum was on sabbatical, to tell him of the situation and seek his help—he calmed me down. In the end, neither of the threats was realized, and the vote of the department was positive.

The university still had to grant me tenure, and the Dean proposed to delay a year because of the number of cases pending. I was eager to put it all behind me, and in the end the Dean (whom I didn't yet know) backed down. Dining with Department Chair Jerry Levine a week later, Jerry pointed out the Dean across the room and asked whether I wanted to go and say hello, or perhaps say thank you, but the situation was still so charged for me that I proposed to go and punch him, instead! (I did not do it). These experiences left me highly sympathetic with the bright young researchers who are regularly tortured before promotion.

Tenure gained, Monika and I went for a year to Paris. We traveled on the Queen Mary, and I watched her with pleasure as she drowsed, pregnant with our first child, on the deck. I had a Sloan Fellowship and was a visitor at the IHES; Monika practiced her French as a visitor to the famous Salpêtrière hospital and studied for her psychiatric Board exams, scheduled the same day that Daniel was supposed to be born! Daniel was 3 days late, and Monika, though great with child, took and passed the exam on schedule.

During the first days in Paris, I ran into Harold (Hal) Levine, a colleague from Brandeis. Dining at an old-fashioned restaurant on the left bank of the Seine, he told me a mathematical problem: how could one compute the local degree of a finite map germ? After a few experiments, I had a glimmer of an idea, and over a sleepless night I became sure: the degree would appear as the signature of a natural quadratic form. Hal and I worked this out over the next days.

Arriving for the first time, in the late afternoon, at the Institut des Hautes Etudes Scientifiques, the first person I encountered was Pierre Deligne, only three years my senior but already famous for his proof of the Weil conjectures just a year before. A person of the utmost kindness, Deligne made me feel at home. Though I was in awe and addressed him with "vous," he explained to me that all the French mathematicians "se tutoyent"—that is, use the familiar form of "you"—to one another, because, in the (rather recent) "old days" all the research mathematicians in France had been graduates of one school, the Ecole Normale Superieur. The former students treated each other familiarly (and no doubt lent a hand to each other in careers—the "old boy" network realized on top of Napoleon's system of meritocracy.) Deligne also took me for a wild bicycle ride down paths in the forest nearby—the first time I had done such a thing. I felt that I could ask Deligne any mathematical question, and get an illuminating answer tuned to my state of ignorance.

Of course I told Deligne about the computation of the local degree, the paper with Hal. He immediately asked how we took care of a certain point... that I had not noticed! I stumbled for a while, and finally came up with a plausible fix. Deligne had far more technique than I, and he saw that it could be made rigorous—but I had some learning to do to write the final version of the paper.

I once went with Monika to attend a presentation at the Salpêtrière, and the event left an impression beyond that of any math lecture: one after another, the presenting pathologist would fish a tagged brain from a barrel, and begin slicing with a chef's knife until he came to the fatal lesion, meantime telling the patient's final story ("Entered hospital at 4pm complaining of terrible headache, dead at 6pm... Mais oui!—now you see the cause!")

Mathematically, I had hoped to work with Lucien Szpiro, the most active person in French commutative algebra, but Szpiro ran a seminar listed on the bulletin board as "by invitation only," and when I asked for an invitation...he said, "No!" This rebuff proved a blessing: I fell in with a group around Bernard Teissier, Norbert A'Campo and Monique Lejeune-Jalabert, and began to broaden my interests into singularity theory, initially from Milnor's wonderful book. These became great friends, from whom I found a warm welcome that offset Szpiro's coldness.

When I wasn't going to Teissier's seminar at Paris 7, I would walk in the morning across the Luxembourg gardens to the Metro and take the train to Bures-Sur-Yvette and the IHES. Two seminar experiences stand out from that time:

Renee Thom was still active in that period, and at the first lecture of the year in his seminar he was the speaker. He began by writing down a result on the blackboard and saying that the seminar that semester would be devoted to the consequences of that result, of which the proof had been the subject of the previous semester. Someone in the back of the room raised his hand and proposed a counterexample to the theorem. This was discussed for a few minutes, and the conclusion was: yes, it is a counterexample. Unfazed, Thom continued: "Now we will get to the applications...."

Late in my year in Paris, Daniel Quillen proved that projective modules over polynomial rings are free, solving a famous problem that had been proposed by Jean-Pierre Serre. The proof was, in the end, surprisingly direct, and I was appointed to give an exposition in the main seminar — with Serre himself in the audience. That I was nervous is a gross understatement, and indeed there came a point in the proof when I clutched and couldn't see how to proceed. . .for just a moment. In the end, all was well.

Our son Daniel was born in June. Monika and I returned to Boston soon afterwards. We had bought a small house in a beautiful setting, next to the Charles River at its widest part in Newton, just opposite Brandeis. We could canoe through most of the year—indeed, I took to commuting to Brandeis by canoe—and skate on the ice the rest of the time. Since neither of us could bear the idea of moving out of that spot, we eventually enlarged the house, and our daughter Alina was born. Monika had by this time finished her training (in both Child and Adult psychiatry) and had an active practice in a private office nearby.

During that period I taught a course from Milnor's book on hypersurface singularities and discovered what are now well-known as the matrix factorizations associated with a hypersurface. (This suddenly became my most quoted paper in 2004, when some physicists discovered that matrix factorizations could be used in String Theory.) I also chanced to hear a lecture at MIT by a young postdoc, Joe Harris, which changed my direction again: Harris spoke about the equations of canonical curves (are the quadrics generated by those of rank at most 4? Yes, as Mark Green subsequently proved.) He explained that lots of rank 4 quadrics come from special varieties, called rational normal scrolls, that contain the curves. I recognized the equations of the scrolls as being determinantal, and since Buchsbaum and I had often discussed determinantal ideals, I felt I had something to contribute. We chatted briefly after the lecture. Not much came of the conversation until later, though I did write my first algebraic geometry paper, using scrolls to give the equations of hyperelliptic curves soon afterwards.

During those first 10 years at Brandeis, the work with David Buchsbaum was by no means our only contact. David was deeply committed to Brandeis and to the Brandeis math department, which he had helped to build, and we spoke a great deal about department and university politics. Though I would not have guessed it then, these lessons were the beginning of my interest in such topics, leading much later to my work at MSRI and presidency of the American Mathematical Society. David told me of past struggles on behalf of the department with deans and provosts; of meetings with the President of Brandeis; and of tensions and repercussions within the department itself.

I found all this quite interesting, as a game of chess is interesting. But the first time I was chair of the department, in 1981–1982, it felt very heavy when I had to act myself! Worst of all were the negotiations over salaries. Brandeis math salaries were very low (we thought) compared to what they should have been, and the department's egalitarian culture prevented much forward motion. It seemed strategic to propose a larger increase for a smaller group, hoping to equalize another group in the next round. In my naiveté I found it dismaying that no one was willing not to be in the first group. . .so the plan caused only bad feelings, and never got off the ground. That was the only time in my career when I regularly came home thinking "I need a drink!"

Curiously, that first experience inoculated me against the stress: when I was department chair again in the 90's (and much later director of MSRI) I could more easily act as if the issues were burningly important, and then turn away and be free of the care when I didn't need to be "on." This skill has gotten stronger and stronger, and served me well over the years—though there are still issues that can keep me awake at night.

My second sabbatical was at the Sonderforschungsbereich (forerunner of the Max Planck Institute) headed by Friedrich Hirzebruch at Bonn University. Monika, who was born in Germany, was eager to spend a year nearer her origins and some of her German family with our two children, then 1 and 3 years old, and this helped determine the place. Chance again did its work in my favor: Antonius van de Ven, a well-known Dutch algebraic geometer, was visiting for most of the period, and we fell into a very pleasant collaboration. We would meet in the late morning at the Institute and work together until hunger reached us around 3 or 4; then we would stroll into town for food, and best of all, coffee and cake at one of the many Konditoreien, on which van de Ven was expert.

Van de Ven taught me a great deal about algebraic geometry, as Buchsbaum had about commutative algebra, and changed my direction again. Later in the year, Walter Neumann also spent some time in Bonn, and we began a collaboration that led to a year-long visit by Walter to Brandeis, and our book on knot theory.

It's perhaps worth saying something about my earlier attempts to learn algebraic geometry, as well. When I was a student at Chicago there was no algebraic geometer on the faculty, but I listened to two one-quarter courses that were relevant. In one, Kaplansky lectured from Chevalley's book on algebraic curves. . .except that there were no curves, only fields and valuations. I learned very little. Then the book of Demazure and Gabriel appeared: schemes as functors. MacLane, who liked anything with functors, convinced Swan to give a course on this approach. The high point of the course, reached after a long slog, was to prove: The Grassmannian Exists! Again, I learned nothing that could be called geometry.

When I came to Brandeis I was determined to keep trying. I listened to Paul Monsky's algebraic geometry course first. It was from the Weil foundations, already a little old-fashioned. Big fields and small ones but. . .no geometry that I could discern. Things went better as I listened to Mumford's course from what was to be his book, "Complex Projective Varieties I"—finally, some geometry! But I found I still could not understand any of the frequent algebraic geometry seminars in the area, all cohomology and schemes. Then in 1977 Hartshorne's book appeared. Since my background in commutative algebra was by then strong, I found it relatively easy to read, and over a summer I studied it end-to-end, doing nearly all the problems. That fall, at last, the subject was open to me, though I still had not done anything in it myself. I think of this 10-year effort, and eventual apprenticeship with van de Ven, when people tell me of their troubles in learning this many-sided subject!

Returning from Bonn, I reconnected with Joe Harris, and enjoyed a long and very fruitful collaboration with him around the applications of limit linear series—in particular, the proof that the moduli space of curves of genus $g \geq 24$ is of general type! Joe and I occasionally played Go—he was a much stronger player—and I imagined us growing old together, playing Go in the sun in Harvard square. This was not to be.

In the light of what was to come it is worth mentioning my two longest stays in Berkeley before coming here in earnest in 1997. In 1986 we moved across the country from Boston for a year's sabbatical. We loved living in Berkeley, and it was a particularly productive time for me mathematically. I was a member of what is now called the "complementary program" at MSRI, though I was well-connected to some of the people in the algebra program. At the end of the year Monika and I wondered whether we should try to return—but there seemed no ready way. We happily went back to Boston, and I to Brandeis. Again in 1994 I was a visitor to MSRI, for 7 weeks during a program related to algebraic geometry. I felt at the time that the program badly lacked senior presence. For example, there were no organizers in residence for most of the time I was there, and I was asked, even as a short-term visitor, to run the main seminar. Nevertheless, Berkeley/MSRI was a very attractive place to be (it helped that I house-sat in a wonderful old Berkeley mansion).

Since my contact with MSRI was so slight, it seemed a great stretch when I applied for the position of Director, a dark-horse candidate, 5 years later.

Before getting to that, I want to fill in a few relevant events. The first has to do with my book, *Commutative Algebra with a View Toward Algebraic Geometry*, published in 1995, and now by far my most quoted work. Writing this occupied me off and on for over 20 years: the earliest written material (on Noether normalization) is from a course I gave during my 1972 sabbatical in Leeds, and the ideas in my exposition of the Koszul complex date from my still earlier writing of the notes for Buchsbaum's lectures in 1968. Some of the chapters carry distinctive memories. For example, I can still picture a certain cafe near the Lago Maggiore where I sat for many hours figuring out how to write about Gorenstein rings, after a memorable workshop at the Monte Verità conference center! Springer was happy to publish the book, but the proofreading was a nightmare: for a book with both "Algebra" and "Algebraic" in the title, some typesetter decided that only one was necessary, and changed all occurrences of "Algebraic" with the push of a button. Unfortunately, I wasn't experienced enough to simply say "No!—start again," and instead spent painful hours unsuccessfully trying to catch all the changes and change them back. (As many readers will know, alas, many other slips remained.) Of course there are things I would write differently if I were starting over, but I feel very good about the success the book has had. It won the AMS' Steele Prize for Exposition in 2010. I hope someday to write a short version.

I made a couple of mathematical visits to IMPA, in Rio, and something happened during one of these visits that strongly influenced my future. I was a speaker at a national meeting where Vladimir Arnol'd (Dima to his friends) was giving a series of lectures; I listened with delight. I was lucky enough to stay in the same hotel, and one day at breakfast he mentioned a conjecture that he had made, having to do with the rigidity of algebras filtered by a sequence of ideals with 1-dimensional quotients. I thought the conjecture should be false and produced a counterexample a few days later. Arnol'd was very aware of the stylistic differences between the mathematics in different countries, and I think he was surprised, not so much that there was a counterexample, but that an American should have gotten his hands dirty enough to find it!

Dima and I became good friends, and had some adventures together: for example, during the conference there was a storm, despite which we went swimming together in the sea near the hotel. The waves were big, and the water was very rough. We were separated by a big wave, and when I dragged myself out onto the beach, I looked around. . .and didn't see Dima! I thought "Oh, no! has he drowned?" but he appeared, intact, a few moments later. We didn't go back in. . .. Later I visited Dima and his wife Ella in their flat in Paris. Rather than going 'round the corner to buy the wonderful cheese or croissants, Dima took me on a bike trip to collect berries and wild vegetables on the outskirts of Paris. Visiting Paris, a little later, I was a faithful member of his seminar. Though he sometimes didn't let the lecturer finish a sentence, his explanations were so good—and generally so much more intuitive than the lecturer's—that it was easy for me to forgive him. Another time Dima and Ella visited Monika and me at our vacation cottage in New Hampshire. We all liked to collect mushrooms—but Dima and Ella were far more efficient and far less fussy; they came home with much bigger bags, worms and all.

I'm convinced that Arnol'd's warm letter of recommendation—because I was the American who dared to challenge his conjecture, but also because of the work I had done with Harold Levine on topological degree—was one of the main reasons I was eventually hired at Berkeley and MSRI.

In 1996 I got a letter that changed my life, with the subject line "Retire in Berkeley?" Here's the background: on a visit to Berkeley a year or two before I spent a very pleasant evening over dinner with Bernd Sturmfels and his wife, Hyungsook Kim. I mentioned that Berkeley would be a great place to retire someday (an idea that Marie France Vigneras had once put forward to me). Now Bernd was suggesting that I apply for the job of MSRI Director! Brandeis had been in hard times financially for years, and there had been serious cuts in the mathematics department; I was thoroughly sick of fighting a losing battle to keep the department strong, and the idea of moving to Berkeley was extremely attractive.

However, I wasn't as sure that the administrative job of Director was a good fit for me. I had made only two visits, both times as a peripheral member of programs. And the administrative jobs I'd had—as department chair, as organizer of scientific meetings—were far smaller and simpler than the MSRI Directorship. It seems that the search committee agreed: I was not the first choice for the position, but when the first candidate withdrew, I was apparently the best candidate in a weak field. . .and got the job! Next, I needed a Deputy Director. Hugo Rossi had been Chair at Brandeis when I was first hired, and was now at the University of Utah. A wonderful inspiration led me to phone and offer him the position, and he accepted 24 hours later. Hugo was much a much more experienced administrator than I, and it proved a successful partnership.

There were bad feelings at the time between the Board of Trustees, headed by Elwyn Berlekamp, and the directorate, led by Bill Thurston. A charismatic and immensely brilliant mathematician, Thurston had succeeded in broadening the focus of MSRI in a very positive way, but the tension with the trustees was proving destructive. Fortunately the differences were not very deep, and the rifts were soon mended. Perhaps because of the contrast with Thurston, I got more credit than I deserved.

An immediate problem I faced was a new policy at the NSF: after 15 years of regular renewals, the NSF had decided that in 2000 there should be a "recompetition"—everyone in the world could apply to take the place of MSRI. The NSF was quite aware of the difficulties that MSRI had had under Thurston, and I felt that I might become "the Director who lost MSRI"! Our strongest competitor seemed to be the American Institute of Mathematics (AIM): John Fry, a wealthy businessman, had promised to put his money behind AIM, which was negotiating a partnership with Stanford University—a formidable coupling. Fortunately for MSRI, the AIM/Stanford partnership fell through. Moreover, Berlekamp and others on the Board contributed money to show that MSRI could also get non-government funding. To my great relief, MSRI won the recompetition.

My first two five-year terms at MSRI were intense and full of incident, which will have to be reported elsewhere. Joe Buhler, Michael Singer, Robert Megginson and Julius Zelmanowitz succeeded Hugo as Deputy Directors and I greatly enjoyed working with them. A first serious fundraising project, carried out with Development Director Jim Sotiros, gathered $12 million for a building expansion and renovation that included the grand Simons Auditorium and many other features. Ten years later architect William Glass and celebrated the achievement with a large-format book describing that process and some of the ideas that went into the design. When I retired from the Directorship in 2007, Jim Simons, whom I had recruited to the Board, gave MSRI its first major endowment gift: $5 million outright plus $5 million to match.

In 2007 Robert Bryant became Director of MSRI, and I happily began life as a regular Berkeley professor, but this did not last very long. Shortly before I retired as Director, Jim Simons had inquired about my plans, and soon asked me whether I would come to New York as Director of the Simons Foundation! At the time the Foundation was a very much smaller and less active organization than today: there was a group funding research on Autism, and a group running Math for America—technically a separate foundation. After looking at the situation, I said no.

A couple of years later Jim asked me to come and found a new Division of Mathematics and Physical Sciences (MPS) within the foundation, focusing on fundamental math, physics and computer science. By this time the Foundation had developed further and was about to move into much bigger quarters. My task would be to create a program that could grow over a few years to spend $40 million annually. This was too exciting to pass up! Starting in 2010, I began to spend about half time in NYC, and eventually worked on Foundation business full time. Collaborating with Jim on plans for the MPS program while enjoying the wonderful atmosphere created by Marilyn Simons' management of the Foundation proved a capstone experience.

In 2012–2013 MSRI hosted a year-long program on Commutative Algebra, and already when I joined the Simons Foundation I had decided to return to Berkeley for that program. Jim and Marilyn asked me to stay at the Foundation in New York. Monika and I weighed the possibilities, but we ultimately decided that it would be too disruptive for our family, and I declined. When I stepped down as Director of MPS, Jim and Marilyn asked me to join the Board of the Foundation, and I was delighted to continue in that role.

Robert Bryant's term came to an end in the summer of 2013, and I put myself forward as a candidate to succeed him. I have served as Director for two more terms, but will retire from that job in August of 2022, 25 years after coming to MSRI.

Among the changes at MSRI that I've overseen in these years, several stand out. First, some measurable increases: the number of Academic Sponsor departments has gone from 28 to 110; the annual budget has gone from about $3 million in 1997–1998 to about $12 million in 2019–2020; and the building expansion roughly doubled the floorspace of MSRI, now renamed Chern Hall in honor of the founding director.

With these new resources, the major scientific programs (typically two in each semester) have been significantly enriched. These already had an excellent reputation but, as mentioned above, they didn't always have enough senior participation, and this aspect has improved as we have moved resources from the "Complementary Program" into the main programs and raised endowment and other funds to improve the support of the members. We have emphasized long stays since these are the most productive. We have greatly increased the number of graduate summer schools we offer, now held all over the world, and started new programs emphasizing wide participation from currently under-represented groups, so that (in non-pandemic times) our building is full throughout the year. We have also added programs to serve mathematics in other ways, such as the support of Numberphile, the Mathical Book Prize, the National Math Festival, the prize for mathematical economics given jointly with the Chicago Mercantile Exchange, and the twice-yearly Congressional Briefings in Washington, DC. In these ways MSRI has strengthened both its core missions and its impact on the wider community.

Many people share the credit for these achievements: MSRI has a strong and well-functioning staff, and successive Deputy Directors, and especially Hélène Barcelo, Deputy Director for the last 10 years, have contributed immensely.

Until about 2000, the National Science Foundation was essentially the only financial supporter of MSRI, and it continues to be the most important source. Soon after I came to MSRI, the NSA began to contribute significantly, and continues to do so. Now many private individuals and foundations add their support, amounting to roughly half our budget, a healthy diversification.

I believe that a substantial endowment will be necessary to ensure MSRI's continued ability to serve the community no matter what shifts in federal funding there may be over time. In 2007 we had virtually no endowment. In 2021 the endowment (counting pledges) has reached about $75 million, and our current goal is $100 million.

One sign of the current health of MSRI is the great strength of the field of applicants to succeed me as Director in August 2022. I'm delighted that Tatiana Toro, the Craig McKibben & Sarah Merner Professor of Mathematics at the University of Washington was chosen, and that she has agreed to become MSRI's next Director! The Institute will be in good hands.

As for my own future, I'm looking forward to going back to the life of an ordinary professor at Berkeley, and to being back in the classroom. Despite the scarcity of time to concentrate on mathematics, I've managed to keep up research over these 25 years. From my work in this period, I'm particularly proud of the proof of the Boij-Soederberg conjecture and the analysis of Chow forms (both with Frank Schreyer, continued in work with Daniel Erman); of matrix factorizations for Cohen-Macaulay modules over complete intersections (with Irena Peeva); of the work on residual intersections (with Marc Chardin and Bernd Ulrich); and of the book on intersection theory, "3264 And All That" (with Joe Harris). I've had great pleasure throughout my career in collaborations (you can find the full list of my collaborators in my MathSci record), and I take a special pleasure in those with former students, with many of whom I've kept a close relationship. I look forward particularly to continuing collaborations and to advising of PhD students in the next years.

Berkeley, CA, USA David Eisenbud
August 2021

Contents

Bernstein-Sato Polynomials in Commutative Algebra

Josep Àlvarez Montaner, Jack Jeffries, and Luis Núñez-Betancourt

Dedicated to Professor David Eisenbud on the occasion of his seventy-fifth birthday.

2010 Mathematics Subject Classification Primary: 14F10, 13N10, 13A35, 16S32; Secondary: 13D45, 14B05, 14M25, 13A50

1 Introduction

The origin of the theory of D-modules can be found in the works of Kashiwara [70] and Bernstein [9, 10]. The motivation behind Bernstein's approach was to give a solution to a question posed by I. M. Gel'fand [55] at the 1954 edition of the International Congress of Mathematicians regarding the analytic continuation of the complex zeta function. The solution is based on the existence of a polynomial in a

Josep Àlvarez Montaner: Partially supported by grants 2017SGR-932 (AGAUR) and PID2019-103849GB-I00 (AEI/10.13039/501100011033).

Jack Jeffries: Partially supported by National Science Foundation grant DMS-1606353 and CAREER Award DMS-2044833.

Luis Núñez-Betancourt: Partially supported by the CONACYT Grant 284598 and Cátedras Marcos Moshinsky.

J. Àlvarez Montaner
Departament de Matemàtiques and Institut de Matemàtiques de la UPC-BarcelonaTech (IMTech), Universitat Politècnica de Catalunya, Barcelona, Spain
e-mail: josep.alvarez@upc.edu

J. Jeffries
University of Nebraska-Lincoln, Lincoln, NE, USA
e-mail: jack.jeffries@unl.edu

L. Núñez-Betancourt (✉)
Centro de Investigación en Matemáticas, Guanajuato, Gto., Mexico
e-mail: luisnub@cimat.mx

I. Peeva (ed.), *Commutative Algebra*, https://doi.org/10.1007/978-3-030-89694-2_1

single variable satisfying a certain functional equation. This polynomial coincides with the b-function developed by Sato in the context of prehomogeneous vector spaces and it is known as the *Bernstein-Sato polynomial*.

The theory of D-modules grew up immensely in the 1970s and 1980s and fundamental results regarding Bernstein-Sato polynomials were obtained by Malgrange [91–93] and Kashiwara [71, 72]. For instance, they proved the rationality of the roots of the Bernstein-Sato polynomial and related the roots to the eigenvalues of the monodromy of the Milnor fiber associated to the singularity. Indeed this link is made through the concept of V-filtrations and the Hilbert-Riemann correspondence.

The theory of D-modules burst into commutative algebra through the seminal work of Lyubeznik [85] where he proved some finiteness properties of local cohomology modules. Nowadays, the theory of D-modules is an essential tool used in the area and has a prominent role. For example, the smallest integer root of the Bernstein-Sato polynomial determines the structure of the localization [143], and thus, using the Čech complex, it is a key ingredient in the computation of local cohomology modules [107–109, 111]. In addition, several results regarding finiteness aspects of local cohomology were obtained via the existence of the Bernstein-Sato polynomial and related techniques [1, 106]. Finally, there are several invariants that measure singularity that are related to the Bernstein-Sato polynomial [36, 38, 51, 102].

In this expository paper we survey several features of the theory of Bernstein-Sato polynomials relating to commutative algebra that have been developed over the last fifteen years or so. For instance, we discuss a version of Bernstein-Sato polynomial associated to ideals was introduced by Budur, Mustaţă, and Saito [36]. We also present a version of the theory for rings of positive characteristic developed by Mustaţă [100] and furthered by Bitoun [14] and Quinlan-Gallego [114]. Finally, we treat a recent extension to certain singular rings [1, 2, 63]. In addition, we discuss relations between the roots of the Bernstein-Sato polynomial and the poles of the complex zeta function [9, 10] and also the relation with multiplier ideals and jumping numbers [36, 38, 51].

In this survey we have extended a few results to greater generality than previously in the literature. For instance, we prove the existence of Bernstein-Sato polynomials of nonprincipal ideals for differentiably admissible algebras in Theorem 5.6. In Proposition 8.2, we show that Walther's proof [143] about generation of the localization as a D-module also holds for nonregular rings. In Theorem 8.6 we observe conditions sufficient for the finiteness of the associated primes of local cohomology in terms of the existence of the Bernstein-Sato polynomial; this covers several cases where this finiteness result is known. We point out that these results are likely expected by the experts and the proofs are along the lines of previous results. They are in this survey to expand the literature on this subject.

We have attempted to collect as many examples as possible. In particular, Sect. 4 is devoted to discuss several examples for classical Bernstein-Sato polynomials. In Sect. 5, we also provide several examples for nonprincipal ideals. In addition, we tried to collect many examples in other sections. We also attempted to present this material in an accessible way for people with no previous experience in the subject.

The theory surrounding the Bernstein-Sato polynomial is vast, and only a portion of it is discussed here. Our most blatant omission is the relation of the roots of Bernstein-Sato polynomials with the eigenvalues of the monodromy of the Milnor fiber [90]. Another crucial aspect of the theory that is not touched upon here is mixed Hodge modules [119]. We also do not discuss the different variants of the Strong Monodromy conjecture which relate the poles of the p-adic Igusa zeta function or the topological zeta function with the roots of the Bernstein-Sato polynomial [48, 68, 105]. We also omitted computational aspects of this subject [13, 107]. We do not discuss in depth several recent results obtained via representation theory [83, 84]. We hope the reader of this survey is inspired to learn more and we enthusiastically recommend the surveys of Budur [31, 33], Granger [57], Saito [122], and Walther [52, 144] for further insight.

2 Preliminaries

2.1 Differential Operators

Definition 2.1 Let $\mathbb{K}$ be a field of characteristic zero, and let A be either

- $A = \mathbb{K}[x_1, \dots, x_d]$, a polynomial ring over $\mathbb{K}$,
- $A = \mathbb{K}[\![x_1, \dots, x_d]\!]$, a power series ring over $\mathbb{K}$, or
- $A = \mathbb{C}\{x_1, \dots, x_d\}$, the ring of convergent power series in a neighborhood of the origin over $\mathbb{C}$.

The ring of differential operators $D_{A|\mathbb{K}}$ is the $\mathbb{K}$-subalgebra of $\operatorname{End}_{\mathbb{K}}(A)$ generated by A and $\partial_1, \dots, \partial_d$, where ∂_i is the derivation $\frac{\partial}{\partial x_i}$.

In the polynomial ring case, $D_{A|\mathbb{K}}$ is the Weyl algebra. We refer the reader to books on this subject [46], [96, Chapter 15] for a basic introduction to this ring and its modules. The Weyl algebra can be described in terms of generators and relations as

$$D_{A|\mathbb{K}} = \frac{\mathbb{K}\langle x_1, \dots, x_d, \partial_1, \dots, \partial_d\rangle}{(\partial_i x_j - x_j \partial_i - \delta_{ij} \mid i, j = 1, \dots, d)},$$

where δ_{ij} is the Kronecker delta. As $D_{A|\mathbb{K}}$ is a subalgebra of $\operatorname{End}_{\mathbb{K}}(A)$, $x_i \in D_{A|\mathbb{K}}$ is the operator of multiplication by x_i. The ring $D_{A|\mathbb{K}}$ has an order filtration

$$D^i_{A|\mathbb{K}} = \bigoplus_{\substack{a_1,\dots,a_d \in \mathbb{N} \\ b_1+\cdots+b_d \le i}} \mathbb{K} \cdot x_1^{a_1} \cdots x_d^{a_d} \partial_1^{b_1} \cdots \partial_d^{b_d}.$$

The associated graded ring of $D_{A|\mathbb{K}}$ with respect to the order filtration is a polynomial ring in $2d$ variables. Many good properties follow from this, for instance, the Weyl algebra is left-Noetherian, is right-Noetherian, and has finite global dimension.

In the generality of Definition 2.1, the associated graded ring of $D_{A|\mathbb{K}}$ with respect to the order filtration is a polynomial ring over A.

Rings of differential operators are defined much more generally as follows.

Definition 2.2 Let $\mathbb{K}$ be a field, and R be a $\mathbb{K}$-algebra.

- $D^0_{R|\mathbb{K}} = \mathrm{Hom}_R(R, R) \subseteq \mathrm{End}_{\mathbb{K}}(R)$.
- Inductively, we define $D^i_{R|\mathbb{K}}$ as

$$\{\delta \in \mathrm{End}_{\mathbb{K}}(R) \mid \delta \circ \mu - \mu \circ \delta \in D^{i-1}_{R|\mathbb{K}} \text{ for all } \mu \in D^0_{R|\mathbb{K}}\}.$$

- $D_{R|\mathbb{K}} = \bigcup_{i \in \mathbb{N}} D^i_{R|\mathbb{K}}$.

We call $D_{R|\mathbb{K}}$ the ring of ($\mathbb{K}$-linear) differential operators on R, and

$$D^0_{R|\mathbb{K}} \subseteq D^1_{R|\mathbb{K}} \subseteq D^2_{R|\mathbb{K}} \subseteq \cdots$$

the order filtration on $D_{R|\mathbb{K}}$.

We refer the interested reader to classic literature on this subject, e.g., [58, §16.8], [16], [104], and [96, Chapter 15]. We now present a few examples of rings of differential operators.

(i) If A is a polynomial ring over a field $\mathbb{K}$, then

$$D^i_{A|\mathbb{K}} = \bigoplus_{a_1+\cdots+a_d \leq i} A \cdot \frac{\partial_1^{a_1}}{a_1!} \cdots \frac{\partial_d^{a_d}}{a_d!},$$

where $\frac{\partial_i^{a_i}}{a_i!}$ is the $\mathbb{K}$-linear operator given by

$$\frac{\partial_i^{a_i}}{a_i!}(x_1^{b_1} \cdots x_d^{b_d}) = \binom{b_i}{a_i} x_1^{b_1} \cdots x_i^{b_i - a_i} \cdots x_d^{b_d}.$$

Here, we identify an element $a \in A$ with the operator of multiplication by a. In particular, when $\mathbb{K}$ has characteristic zero, this definition agrees with Definition 2.1.

(ii) If R is essentially of finite type over $\mathbb{K}$, and $W \subseteq R$ is multiplicatively closed, then $D^i_{W^{-1}R|\mathbb{K}} = W^{-1}D^i_{R|\mathbb{K}}$. In particular, for $R = \mathbb{K}[x_1, \dots, x_d]_f$,

$$D^i_{R|\mathbb{K}} = \bigoplus_{a_1+\cdots+a_d \le i} K[x_1, \dots, x_d]_f \cdot \frac{\partial_1^{a_1}}{a_1!} \cdots \frac{\partial_d^{a_d}}{a_d!}.$$

(iii) If A is a polynomial ring over $\mathbb{K}$, and $R = A/\mathfrak{a}$ for some ideal $\mathfrak{a}$, then

$$D^i_{R|\mathbb{K}} = \frac{\{\delta \in D^i_{A|\mathbb{K}} \mid \delta(\mathfrak{a}) \subseteq \mathfrak{a}\}}{\mathfrak{a}D^i_{A|\mathbb{K}}}.$$

In general, rings of differential operators need not be left-Noetherian or right-Noetherian, nor have finite global dimension [12].

We note that if R is an $\mathbb{N}$-graded $\mathbb{K}$-algebra, then $D_{R|\mathbb{K}}$ admits a compatible $\mathbb{Z}$-grading via $\deg(\delta) = \deg(\delta(f)) - \deg(f)$ for all homogeneous $f \in R$.

Remark 2.3 The ring R is tautologically a left $D_{R|\mathbb{K}}$-module. Every localization of R is a $D_{R|\mathbb{K}}$-module as well. For $\delta \in D_{R|\mathbb{K}}$, and $f \in R$, we define $\delta^{(j),f}$ inductively as $\delta^{(0),f} = \delta$, and $\delta^{(j),f} = \delta^{(j-1),f} \circ f - f \circ \delta^{(j-1),f}$. The action of $D_{R|\mathbb{K}}$ on $W^{-1}R$ is then given by

$$\delta \cdot \frac{r}{f} = \sum_{j=0}^{t} \frac{\delta^{(j),f}(r)}{f^{j+1}}$$

for $\delta \in D^t_{R|\mathbb{K}}$, $r \in R$, $f \in W$.

Definition 2.4 Let $\mathfrak{a} \subseteq R$ be an ideal and $F = f_1, \dots, f_\ell \in R$ be a set of generators for $\mathfrak{a}$. Let M be any R-module. The Čech complex of M with respect to F is defined by

$$\check{C}^\bullet(F; M): \quad 0 \to M \to \bigoplus_i M_{f_i} \to \bigoplus_{i,j} M_{f_i f_j} \to \cdots \to M_{f_1 \cdots f_\ell} \to 0,$$

where the maps on every summand are localization maps up to a sign. The local cohomology of M with support on $\mathfrak{a}$ is defined by

$$H^i_{\mathfrak{a}}(M) = H^i(\check{C}^\bullet(F; M)).$$

This module is independent of the set of generators of $\mathfrak{a}$.

As a special case, $H^1_{(f)}(R) = \dfrac{R_f}{R}$.

The Čech complex of any left $D_{R|\mathbb{K}}$-module with respect to any sequence of elements is a complex of $D_{R|\mathbb{K}}$-modules, and hence the local cohomology of any $D_{R|\mathbb{K}}$-module with respect to any ideal is a left $D_{R|\mathbb{K}}$-module.

2.2 Differentiably Admissible $\mathbb{K}$*-Algebras*

In this subsection we introduce what is called now differentiably admissible algebras. To the best of our knowledge, this is the more general class of ring where the existence of the Bernstein-Sato polynomial is known. We follow the extension done for Tate and Dwork-Monsky-Washnitzer $\mathbb{K}$-algebras by Mebkhout and Narváez-Macarro [98], which was extended by the third-named author to differentiably admissible algebras [106]. We assume that $\mathbb{K}$ is a field of characteristic zero.

Definition 2.5 Let A be a Noetherian regular $\mathbb{K}$-algebra of dimension d. We say that A is differentiably admissible if

(i) $\dim(A_{\mathfrak{m}}) = d$ for every maximal ideal $\mathfrak{m} \subseteq A$,
(ii) $A/\mathfrak{m}$ is an algebraic extension of $\mathbb{K}$ for every maximal ideal $\mathfrak{m} \subseteq A$, and
(iii) $\mathrm{Der}_{A|\mathbb{K}}$ is a projective A-module of rank d such that the natural map

$$A_{\mathfrak{m}} \otimes_A \mathrm{Der}_{A|\mathbb{K}} \to \mathrm{Der}_{A_{\mathfrak{m}}|\mathbb{K}}$$

is an isomorphism.

Example 2.6 The following are examples of differentiably admissible algebras:

(i) Polynomial rings over $\mathbb{K}$.
(ii) Power series rings over $\mathbb{K}$.
(iii) The ring of convergent power series in a neighborhood of the origin over $\mathbb{C}$.
(iv) Tate and Dwork-Monsky-Washnitzer $\mathbb{K}$-algebras [98].
(v) The localization of a complete regular rings of mixed characteristic at the uniformizer [86, 106].
(vi) Localization of complete local domains of equal-characteristic zero at certain elements [112].

We note that in the Examples 2.6(i)–(iv), we have that $\mathrm{Der}_{A|\mathbb{K}}$ is free, because there exists $x_1, \dots, x_d \in R$ and $\partial_1, \dots, \partial_d \in \mathrm{Der}_{A|\mathbb{K}}$ such that $\partial_i(x_j) = \delta_{i,j}$ [94, Theorem 99].

Theorem 2.7 ([106, Theorem 2.7]) *Let A be a differentiably admissible $\mathbb{K}$-algebra. If there is an element $f \in A$ such that $R = A/fA$ is a regular ring, then R is a differentiably admissible $\mathbb{K}$-algebra.*

Remark 2.8 ([106, Proposition 2.10]) Let A be a differentiably admissible $\mathbb{K}$-algebra. Then,

(i) $D^n_{A|\mathbb{K}} = (\mathrm{Der}_{A|\mathbb{K}} + A)^n$, and
(ii) $D_{A|\mathbb{K}} \cong A\langle \mathrm{Der}_{A|\mathbb{K}} \rangle$.

Theorem 2.9 ([106, Section 2]) *Let A be a differentiably admissible $\mathbb{K}$-algebra. Then,*

(i) *$D_{A|\mathbb{K}}$ is left and right Noetherian;*
(ii) *$\mathrm{gr}_{D^{\bullet}_{A|\mathbb{K}}}(D_{A|\mathbb{K}})$ is a regular ring of pure graded dimension $2d$;*
(iii) *$\mathrm{gl.\,dim}(D_{A|\mathbb{K}}) = d$.*

We recall that for Noetherian rings the left and right global dimension are equal. In fact, this number is also equal to the weak global dimension [116, Theorem 8.27].

Definition 2.10 ([98]) We say that $D_{A|\mathbb{K}}$ is a *ring of differentiable type* if

(i) $D_{A|\mathbb{K}}$ is left and right Noetherian,
(ii) $\mathrm{gr}_{D^{\bullet}_{A|\mathbb{K}}}(D_{A|\mathbb{K}})$ is a regular ring of pure graded dimension $2d$, and
(iii) $\mathrm{gl.\,dim}(D_{A|\mathbb{K}}) = d$.

By Theorem 2.9, the ring of differential operators of any differentiably admissible algebra is a ring of differentiable type.

2.3 Log-Resolutions

Let $A = \mathbb{C}[x_1, \ldots, x_d]$ be the polynomial ring over the complex numbers and set $X = \mathbb{C}^d$. A *log-resolution* of an ideal $\mathfrak{a} \subseteq A$ is a proper birational morphism $\pi : X' \to X$ such that X' is smooth, $\mathfrak{a} \cdot \mathcal{O}_{X'} = \mathcal{O}_{X'}(-F_\pi)$ for some effective Cartier divisor F_π and $F_\pi + E$ is a simple normal crossing divisor where $E = \mathrm{Exc}(\pi) = \sum_{i=1}^r E_i$ denotes the exceptional divisor. We have a decomposition $F_\pi = F_{exc} + F_{aff}$ into its *exceptional* and *affine* parts which we denote

$$F_\pi := \sum_{i=1}^{r} N_i E_i + \sum_{j=1}^{s} N'_j S_j$$

with N_i, N'_j being nonnegative integers. For a principal ideal $\mathfrak{a} = (f)$ we have that $F_\pi = \pi^* f$ is the total transform divisor and S_j are the irreducible components of the *strict transform* of f. In particular $N'_j = 1$ for all j when f is reduced.

The *relative canonical divisor*

$$K_\pi := \sum_{i=1}^{r} k_i E_i$$