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Interview Luc Illusie

Tales from the golden age of algebraic geometry

Last December the University of Utrecht organized a new edition of the Kan memorial lectures. Founded in 2015 by Ieke Moerdijk in honour of the famous Dutch topologist Daniel Kan, these lecture series are intended to highlight various aspects of algebraic topology. In each edition an acclaimed expert is invited to give one public lecture and two more specialized lectures about a topic of his choice. This year the French algebraic geometer Luc Illusie gave three lectures on the de Rham complex. Nieuw Archief seized the opportunity to talk with him about his life, mathematics and the golden age of algebraic geometry in the sixties.

From Nantes to Paris

Luc Illusie was born on 2 May 1940, just days before the start of World War II. By the middle of June his hometown of Savenay, a village in the West of France near the estuary of the Loire, had been conquered by the Germans.

"I was born in Nantes, but I spent my first five years in Savenay, a small village 30 kilometers north of Nantes. We were occupied by the Germans at the time, but by January 1945 some parts of France had been liberated. Together with my parents and my brother we escaped German controlled Savenay and we moved to Nantes. There I attended primary school and secondary school up to 1957.

Both my parents were teachers. My father taught history and literature, a combination that was still possible at the time. I owe him a lot because he knew so many things. He had an extraordinary memory. He could listen to a speech and then recite it afterwards. He knew so many pieces of the great poets.

My mother was teaching mathematics. She helped me discover the joys of algebra. When I was nine years old, she taught me how to put concrete problems into equations. She was my mathematics teacher in school when I was between 10 and 12 years old. She taught me geometry and I was fascinated by the beautiful properties of triangles. I think that today many boys and girls find mathematics a little boring in secondary school, not just because the foundations are messy. The point is that they are not presented with very beautiful results. They see Thales' and Pythagoras' theorems but these are not so exciting. However when you look at triangles, they have a lot more fascinating properties, such as the Euler line or the Euler circle or many other mysterious configurations. You start with something very irregular and you find a certain harmony hidden in plain sight."

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Luc Illusie

Although he was already fascinated by mathematics at an early age, Illusie had diverse interests and a career in mathematics was at that time not on his mind.

"I thought mathematics was fun, it was really a wonderful thing, it was challenging but it did not take me too much time. What took the most of my time was French, Latin and Greek, and the humanities. My teachers were fantastic and I was very involved in those subjects. I remember when I was 15, I could spend hours at night writing some French texts on literature, such as poems by Victor Hugo and other people. I enjoyed that very much, so in view of that I should have continued in literature or humanities.

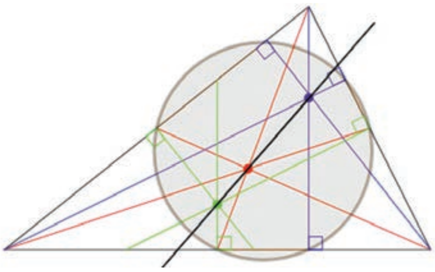


Illustration: eulercircle.com

The Euler line and Euler circle of a triangle

At that time, when you were 16 or 17 you had to pass the Baccalauréat. This was a serious examination in two parts spread over two years and in the second part you had to choose between literature or science. I chose science because I thought there were more opportunities for jobs in mathematics and physics. The idea was that I would compete for the École Normale or École Polytechnique afterwards. Therefore my parents decided it was better for me to go to a good school in Paris. So we all moved to Paris and I enrolled in the Lycée Louis-le-Grand. Getting into that lycée was not so easy, but I had excellent marks in Latin and Greek. Today I have forgotten all my Latin and all my Greek.

At the lycée I had an extraordinary teacher of mathematics in the second year. His name was André Magnier and in fact he knew Grothendieck very well. He was from Montpellier, where Grothendieck had studied, and he helped him come to Paris. He discovered that he got a special talent and then had him contact Cartan and the Bourbaki group. The rest is history… Magnier was a very good teacher and it is certainly thanks to him that I also got drawn into mathematics.”

Cartan and the caimans

Despite his teacher's connections, Illusie's personal contacts with Cartan, Grothendieck and the rest of the mathematics scene in Paris would only start later, during his student years at the École Normale.

“At the École Normale there were students called caimans (crocodiles) who would take care of the younger students. My first caiman was Adrien Douady. He was a wonderful character, he knew a lot of things, especially a lot of examples and counterexamples. He had a way of explaining very abstract things in a very concrete way. It was a marvelous person and I discovered afterwards that he was at Bourbaki. From time to time he would give me some secret papers of Bourbaki. The Boubaki group was not so secret, actually these secret papers were circulating quite a lot. Roger Godement, who was teaching me analysis at the time, was also giving me some Bourbaki papers about commutative algebra.

The École Normale offers a broad degree, which not only contains mathematics but also physics and chemistry. In the first year I was still hesitating between physics and mathematics. My physics teachers were quite interesting. One was Alfred Kastler, the Nobel Prize winner, and the other Yves Rocard, the father of the former prime minister Michel Rocard. Both were very brilliant, Kastler taught physics a little bit like mathematics. Rocard on the other hand was not so neat. I said to myself: ‘This is not the type of thing I want to do.’

Then I discovered the teachings of Cartan and this was a revelation for me. In high school you learn to do many calculations, but the theory and the conceptual aspect of mathematics is lacking. In the first course by Cartan, he introduced abstract concepts like the characteristic of a field. Cartan was marvelous. Sometimes he had not prepared so much and he would get stuck, but I really liked his approach to the matter.”

The fifties and sixties were the haydays of the French mathematical seminars. They were joint efforts of students and researchers who came together under the lead of one of the university professors to study a contemporary topic in mathematics. The most influential one was the seminar of Henri Cartan and it was there that Illusie made his first marks as a mathematical researcher.

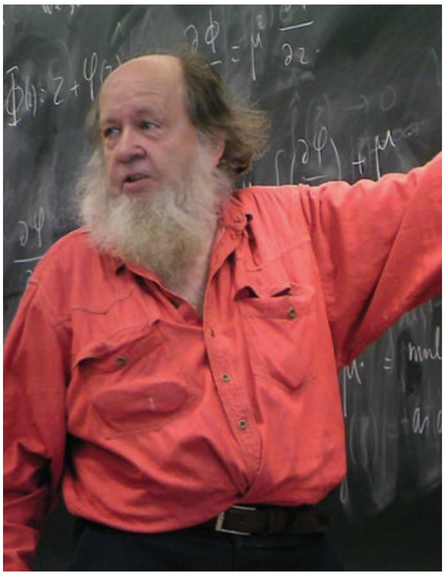


Photo: Wikipedia.org/François Tisseyre

Adrien Douady

“At the end of the fourth year, Cartan had arranged for me to have a temporary position at CNRS and he enrolled me in his seminar on the Atiyah–Singer index formula [1,9]. It was here that I first felt that maybe I could become a mathematician. Sometimes I made a new observation of which Atiyah, Singer and other very impressive people had not thought. Although it was maybe an epsilon, it gave me great confidence that I could continue.

The idea of the Cartan seminar was a continuation of Hadamard's seminar. We took on a big subject, a new theory, and we started all from the beginning. Black boxes were not allowed, everything had to be proven. For the Atiyah–Singer formula we had to discuss characteristic classes and how they are defined. I remember that the first talk Cartan asked me to give was on the Chern character and the Todd class. I told him: ‘Well Mr. Cartan, I do not know any of this. I cannot do it.’ He said: ‘It is not that difficult. Just come to my office and I will explain it to you.’ For one hour he explained to me classifying spaces, their cohomology and characteristic classes. It was so clear and simple.

Eventually I could give my talk, but then there was a constraint. You had to write it up in one month. My handwriting was not so good and Cartan said that I should get a typewriter and type it. I bought a German typewriter, an enormous machine, and I learned how to type with ten fingers. I typed my piece on the Chern character and then I handed it in. To my surprise, he said it was okay apart from a few technical remarks here and there.

At the seminar, there was one student who was very impressive. After my talk he came to me and we discussed passionately for maybe half an hour. At the end I asked ‘who is this young guy’ and it turned out to be Jean-Louis Verdier. There was also Demazure, who was of course two three years in advance and knew much more than I did. I talked with all these people and I learned how to do mathematics and how to write it down.”

Meeting the mentor

The seminar brought Illusie into contact with many of the great mathematicians of


Photo: mathshistory.st-andrews.ac.uk

Henri Cartan

his time, but the meeting that influenced his career the most was the one with Alexandre Grothendieck, the father of modern algebraic geometry.

"The big turning point came when I met Grothendieck. Under Cartan I had started working on a relative version of the Atiyah–Singer index formula. I had done some work on Hilbert bundles in the wake of what Atiyah had done. Unfortunately, I got stuck and Cartan said: 'Perhaps you should ask Grothendieck. Maybe he has some idea about that.' I contacted Grothendieck and he suggested that I use sheaves because they are very powerful and allow to treat singularities. This was somehow revolutionary: sheaves had been used for ten years in complex analytic geometry but not in the C-infinity context. Although his suggestion worked I did not continue with it because by then Atiyah and Singer had already solved the relative formula by other means.

Instead I kept working with Grothendieck on other projects. Cartan was really generous because he did not object to my going to Grothendieck and working with him. He could have said: 'I am losing a student and this is not good', but he was not like that at all."

Parallel to Cartan, Grothendieck ran his own seminar: the Séminaire de Géométrie Algébrique du Bois Marie (SGA). The seminar took place at the Institut des Hautes Études Scientifiques (IHÉS), a newly founded mathematical research institute south of Paris in Bures sur Yvette. The institute was situated in a forested domain, hence the name of the seminar. For ten years Grothendieck and his collaborators rewrote the foundations of algebraic geometry and incorporated many of the recent developments in the fast evolving subject.

In the fall of 1964 Grothendieck started a seminar on ℓ-adic cohomology and asked me to write down notes for some exposés [8]. I objected 'This is impossible! I know almost nothing in algebraic geometry, even the concept of an affine scheme is not so clear to me', but he took my 'no' for an agreement and replied: 'You will learn quickly!'

In the seminar he took the pain of writing all the definitions and explaining how they worked. It was crystal clear and gradually I learned the necessary basic material. For example, at the time I did not know anything about Zariski's main theorem. At the same time I was reading the preprints that came from for SGA4 [11], the basic theorems in étale cohomology and the general formalism of sites and topoi, which was abstract and quite hard. But Grothendieck had a way of making that somehow easy.

Grothendieck was a new spirit, a new flame, because of the functorial language. This functorial language is a whole new world. Grothendieck topologies are amazing, you see, so I was fascinated by that. The other aspect was derived categories. I had studied Cartan and Eilenberg [4] at the École Normale, but I found this new approach so wonderfully simple. For example the spectral sequence of the composition of two functors is complicated but in derived categories it just becomes $R(G \circ F) = RG \circ RF$.

Writing down the notes turned out to be not difficult at all, because his lectures were so clear. I would write down a first draft and present it to him. Then he would invite me to his place and we would go line by line through it. He had many comments on details or spelling and general comments on presentations and proofs. It usually took the whole afternoon and evening.

Grothendieck would not accept a pack of notes of less than 15 pages. Once one of my former caimans at the École Normale handed him maybe 10 or 15 pages and he said: 'When you have elaborated on it, come back again.' It had to be at least 40 or 50 pages.

That was how I learned how to work and how to write. Even though Cartan and Douady had already been very strict and taught me how to write in the Bourbaki style, Grothendieck was at another level. I think I can write in French reasonably well, but he was also making comments on that and although French was not his mother tongue he was always right."

While the first interactions between Illusie and Grothendieck concentrated on the redactions of the exposés of SGA5 and SGA6, Illusie also had to find a good topic for a PhD thesis. This turned out to be more tricky than expected.

"You could say that Grothendieck was quite unsuccessful with me. I was a bad student. He asked me several problems

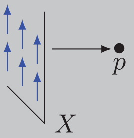

Photo: www.thetimes.co.uk

Grothendieck lecturing at IHES

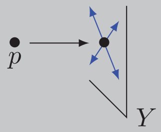
What is the cotangent complex?

In his thesis, which was later published in *Springer Lecture Notes in Mathematics* [7], Illusie constructed a broad generalization of the cotangent bundle. For each morphism $X \to Y$ of ringed topoi he defined a complex $L_{X/Y}$ that captures the deformation theory of that morphism. More precisely $L_{X/Y}$ is a complex of sheaves over X such that $\mathrm{Hom}(L_{X/Y}, O_X)$ describes the infintesimal automorphisms of $X \to Y$, $\mathrm{Ext}^1(L_{X/Y}, O_X)$ the first order deformations, and the higher exts the obstructions.

Let us look at some examples to get a feeling of what is going on. There are two extreme cases: the map of a smooth variety to a point, $X \to p$, and the embedding of a point in a smooth variety, $p \to Y$.



$$L_{X/p} = \Omega^1_X$$



$$L_{p/Y} = T^*_p Y[1]$$

In the former case, the infinitesimal automorphisms are given by vector fields on X and there are no deformations. This shows that (as an object in the derived category of X) $L_{X/p}$ is isomorphic to the cotangent bundle of X concentrated in degree 0. In the latter case, the morphism $p \to Y$ has no infinitesimal automorphisms but its infinitesimal deformations are unobstructed and given by tangent vectors at p, so $L_{p/Y}$ is the cotangent space to Y at p concentrated in degree -1.

In a similar way $L_{X/Y}$ will be the conormal sheaf concentrated in degree -1 for an embedding of smooth varieties, and the sheaf of relative differentials concentrated in degree 0 for a smooth morphism (e.g a flat family of smooth varieties).

Things become trickier if X, Y are singular schemes or even more complicated objects such as algebraic sets, stacks or ringed topoi.

which were all interesting, but I could not get anywhere. For example, he knew that I loved derived categories so he asked me to develop a formalism that could do away with the restrictions on the degrees. Think of the choice between D^-, D^+ or D^b. At the time pro- and ind-objects were very popular, so I tried something along these lines but it did not work. In fact we had to wait for Spaltenstein in the 1980s to get a real new approach.

Another very important problem concerned Kunneth's formula. I loved this formula. In its treatment in EGA3 there are maybe a dozen spectral sequences. This is ridiculous, so he asked me to clean that and write it down neatly in the form of derived categories. But again I was stuck. In Kunneth's formula the ring structure is important, but there is no good ring structure in the derived setting. A few years later it was solved by Quillen with homotopical algebras.

A third problem concerning the notion of properness in algebraic geometry also led to nowhere and only very late in 1968 he proposed to me the questions on deformation theory that would lead to my work on the cotangent complex. But then in a few months I had essentially all the basics theorems of the first part of my thesis."

Carpets

The circle around Grothendieck included mathematicians from all over the world and Illusie has fond memories of interacting with many of them. Two of them stand out: Pierre Deligne, who was his mathematical brother and Daniel Quillen, whose work formed the basis for his PhD thesis and who invited Illusie to visit MIT in 1970.

"Starting in 1965, there was a new timid looking young man attending the seminar: Pierre Deligne. From then onwards, anytime I was stuck in some place I did not understand I asked him to explain it to me. He knew everything already and it was just marvelous. He would come to my place, we would discuss mathematics the whole afternoon and then we would move to Parc Montsouris for a walk and further discussions. I had a carpet in my place in Paris and he liked to lie down on the carpet and do the pear tree. You put your head on the carpet and your legs in the air to make the blood run to your brain.

Quillen was also a wonderful person. In 1968 when I discovered a very simple way to extend his construction to topoi, I wrote him a letter. He replied immediately and invited me to visit MIT, but before I went there I already had the opportunity to meet him in person at IHES. Grothendieck was very impressed actually by Quillen. He wrote some notes entitled 'Tapis de Quillen' (Quillen's carpet). Marchand de Tapis was the Bourbaki term for someone who had a new theory and who wanted to sell it to everybody.

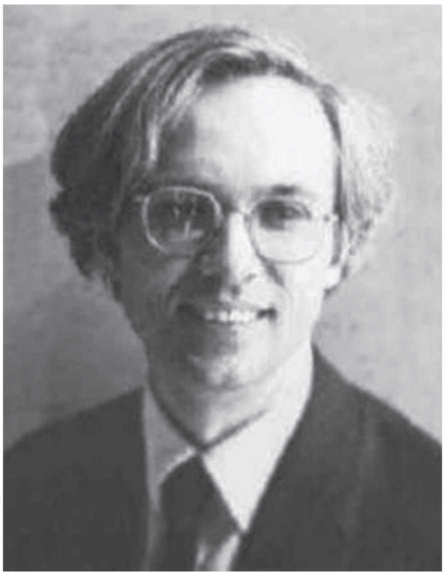
Anyway, I went to MIT and Quillen invited me to give a course on my thesis and we discussed it a lot. It was fabulous, he was extremely clear and precise. He knew a lot of algebraic topology, a lot of number theory, finite groups and algebraic geometry. All that was coming together in his mind wonderfully. I arrived in September 1970 but I could not stay as long as I wanted because my mother had had a stroke. I had to come back in the spring of 1971 and then I defended my thesis at that time. My visit to MIT was really quite a personal discovery."

Writing and redacting

In 1976 Illusie became a professor at the Université Paris-Sud in Orsay and remained there until his retirement in 2005.

"Around 1976 people at Orsay invited me to their university to become a professor there. At first I was a bit reluctant because at CNRS I had quite a lot of freedom to do research but eventually Raynaud and other colleagues convinced me to accept the position that they had for me. I had to do quite some teaching, sometimes to groups of 200 students. These were the beginning students who had not yet decided between math, physics or engineering. Some were very motivated and some were less motivated. I typed notes for them on a weekly basis and then at one point, in 1981 or 1982, I had a small group of very interested students and I said next time you will write the notes and I will teach you how to write properly in Bourbaki style. We would meet at the end of the day and go through the annotations I made, just like Grothendieck did when I was a student. After the corrections their notes were dis-

Pierre Deligne



Daniel Quillen

tributed to the students with their names on and they were very proud. That was a very nice experience.”

The Bourbaki style of writing mathematics has a mixed reputation. Some praise it for its precise statements and clear expositions, while others complain about the lack of motivation and examples. Illusie acknowledges this but adds that many good texts combine elements of Bourbaki with a more concrete approach.

“Well, let me give an example. Mumford and Atiyah’s way of writing is based on Bourbaki but also very much illuminated with examples and concrete applications. So it looks concrete though at the bottom it is still Bourbaki. So it is a question of style, where you put the emphasis. If you look at the text by Giraud for example, *Méthode de la Descente*, it is very abstract and very terse. That is not so nice, on the substance it is certainly a great text, but it lacks that balance. Even Grothendieck was able to see the difference if you take something very abstract like a topos. I remember that Verdier had written Exposé IV in SGA4 on topoi and there were categories and functors everywhere. Grothendieck said: ‘I cannot make sense of all that, you need to write in a more geometric language.’ Starting from Verdier’s initial text, Grothendieck wrote a completely new version with many examples. From this very abstract substance he made something which was still Bourbaki style but also motivated and concrete. I use the term motivated because I think this is what is often lacking in Bourbaki. Never complain, never explain; the reader will understand by himself why we are doing this. But Grothendieck was very careful to explain why he was doing things.

Today people like Bhargav Bhatt and Peter Scholze write in some kind of Bourbaki style illuminated with examples. Their theories are very abstract but very concrete at the same time with key examples and striking observations.”

The role of examples in theory building

Illusie also thinks that Grothendieck’s reputation of eschewing examples is misplaced.

“It is not correct to say that Grothendieck did not know any examples. He knew simple examples, maybe he did not know much about $E8$ or other special things, but he knew basic examples. For example he had a lot of knowledge about abelian varieties and algebraic groups and he was an expert in functional analysis and topology. This helped him a lot and he had a broad view of topology and differential geometry.

He started with simple examples. I think already with curves, abelian varieties and hypersurfaces you have enough. When you want to develop a big theory, the idea is to look for extreme examples where some abstract principle still works and you want to join those extremes together. Often it then turns out that the existing theory is not stable and you need to modify it. You can try to find a new definition which is much more amenable.

For example, in 1966 Grothendieck dreamed of a theory of the determinant of a perfect complex. He proposed this problem to Daniel Ferrand. When you have a short exact sequence, the determinant in the middle should be the product determinant of the two ends, so when you have a triangle then the determinant should be the product in a suitable functorial sense. Ferrand tried to work this out, but this turned out to be problematic. As you know the derivative of the determinant is the trace, so the trace in the middle should be the sum of the traces of the two sides. Alas, triangles in the derived category are not functorial. Nowadays, you pray that infinity categories will do the job, but at the time it was not the case. In fact Ferrand found that in general the trace in the middle is not always the sum of the traces. Usually when there was this sort of problem, you asked Deligne how to repair that. He came up with some sort of refined triangles, which he called true triangles, and then somehow the formula became correct.

I was looking at that and I thought, well, you cannot take tensor products of true triangles. If you have an exact sequence the left term is a subobject of the middle term. This is a small filtration, so we should take filtrations in many steps. This naturally led me to filtered derived categories, which was of course very successful afterwards and Deligne used it very much in Hodge theory. This is just an example of how you see something and want to make it more amenable to generalization and then you are sort of forced to do these things.”

Present and future

In the last decade there has been a renewed interest in Illusie’s work on the cotangent complex and the de Rham complex. This originated in the work of Alexander Beilinson who in 2011 gave a totally new proof of the p-adic comparison theorem [2].

“I am quite excited with the new developments, but I feel I am running after some people who are much faster than I. I was excited when in 2012 I received a preprint from Beilinson about his comparison theorem, which used to derive de Rham complex in a very striking way. That was fantastic but at the same time there was something I was very shocked about. In the paper he looked at some kind of a presheaf of categories and then he took the associated sheaf. To me this did not make sense because we all know that objects in derived categories do not glue. We have known that for sixty years.

I asked him and he told me that there is a general formalism by Lurie that enables you to do that. So then I wrote to Jacob Lurie and asked: 'Please help me. Teach me how this works.' I discovered you could in fact take the associated sheaf using infinity categories. The whole thing was completely rigorous. I also discovered that Bhargav Bhatt had some other approach, so I also exchanged a lot with him. Those guys are young, they work so fast and they do so much.

In the sixties things were also evolving very fast. Think of the revolution of étale cohomology, toposes, crystalline cohomology, semi-stable reduction, stacks and so on. At that time I thought it was all quite natural and it was only in retrospect that I realized I was so privileged to have been part of this exciting adventure. But it seems that the period we are now living through with all these developments in derived algebraic geometry and all the new concepts around Scholze and many others is also a very exciting period. I think it is quite similar in spirit to the so-called golden age of the sixties."

These new exciting developments were also the topic of the Kan lectures. Illusie gave two talks on de Rham complexes in mixed characteristic, in which he talked about new work due to Bhatt-Lurie [9], Drinfeld [6], and Petrov that builds further on his own work with Deligne about this topic [5]. These talks were accompanied by a lecture about the history of the de Rham complex.

From Hodge to de Rham in characteristic p

The de Rham complex $\Omega^{\bullet}_X$ originates in differential geometry, where it is used to measure the difference between closed ($d\omega = 0$) and exact ($\omega = df$) forms. In algebraic geometry one can define a purely algebraic version of the de Rham complex. Because it is a complex of sheaves, you have to compute the hypercohomology instead of the ordinary cohomology to combine the contributions of the differential d and the cohomology of the sheaves.

Let us illustrate this for the Riemann sphere $\mathbb{P}^1$. There are only two terms in the de Rham complex: the sheaf of functions and the sheaf of one forms. To calculate their sheaf cohomology we use the cover $\mathbb{P}^1 = \operatorname{Spec}\mathbb{C}[z] \cup \operatorname{Spec}\mathbb{C}\left[\frac{1}{z}\right]$. This results in the following hypercomplex and hypercohomology:

$$\begin{tikzcd}
\Omega^0_{\mathbb{P}^1} : \arrow[d, "d"] & \mathbb{C}[z] \oplus \mathbb{C}[\frac{1}{z}] \arrow[r] \arrow[dr, "d"] & \mathbb{C}[z, \frac{1}{z}] \arrow[dr, "d"] & \\
\Omega^1_{\mathbb{P}^1} : & & \mathbb{C}[z]dz \oplus \mathbb{C}[\frac{1}{z}]\frac{dz}{z^2} \arrow[r] & \mathbb{C}[z, \frac{1}{z}]dz \\
\mathbb{H}(\Omega^{\bullet}_{\mathbb{P}^1}, d) : & \mathbb{C}(1,1) & 0 & \mathbb{C}\frac{dz}{z}.
\end{tikzcd}$$

As expected for a sphere, the cohomology is 1-dimensional in degree 0 and 2 and 0 in degree 1.

Surprisingly, if you omit the d's and only calculate the sheaf cohomology, you will get the same result. This holds in general if you work over $\mathbb{C}$ and your scheme is smooth and complete. This is called the Hodge to de Rham degeneration.

$$H^{\bullet}_{dR}(X) = \mathbb{H}(\Omega^{\bullet}_X, d) \cong \mathbb{H}(\Omega^{\bullet}_X, 0) = H^{\bullet}_{Hodge}(X).$$

In characteristic p the situation is more complicated but in their seminal work of 1987 [5] Deligne and Illusie found conditions for which the Hodge to de Rham degeneration holds in characteristic p. They showed that for $p > \dim X$, it is sufficient that the scheme X lifts to a scheme $\tilde{X}$ which is defined modulo p^2 over the Witt ring.

"For the Kan Lecture I had prepared some slides but I did not use them. We put them on the web site but I consider in fact making a text from these slides. The problem is that in the coming months I am really too busy. I do not see the possibility of remodeling all that right now, but I hope I have a little more time next fall when my travel schedule is a bit more quiet.

I enjoy traveling. This is a privilege of mathematicians, that's why it's the greatest job of all. We can travel a lot and meet wonderful people. I was quite frustrated during the covid period not to be able to have contact in person. But now that we are back to normal, I would like to travel more again, as long as my health enables me."

With that, we wish Luc Illusie 'Bons voyages' and hope he will find some time to write his essay on the history of the de Rham complex.

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