# Claude LeBrun

Dr. Claude LeBrun received his PhD from the University of Oxford in 1980 under the supervision of Roger Penrose. In the same year, he became a faculty member at Stony Brook. Since then, he has also held positions at the Institut des Hautes Études Scientifiques, the Mathematical Sciences Research Institute, and the Institute for Advanced Study. LeBrun is working on Riemannian geometry especially on the 4-manifold, Einstein metrics, Yamabe invariants and related topics in complex and differential geometry.

LeBrun was an invited speaker at the 1994 International Congress of Mathematicians. In 2012, he became a Fellow of the American Mathematical Society. In 2018, he became a Simons Foundation Fellow in Mathematics.

## Encounters with Shing-Tung Yau

I belong to a generation of differential geometers whose worldview has been deeply influenced by the mathematical ideas of S.-T. Yau. This applies to all of my contemporaries, and not just to me. But in my case, there is an added personal dimension, because I have also had the good fortune to get to know Yau well enough to become his friend.

I first met Yau in the late 1970s, when I was a graduate student at Oxford. My supervisor, Roger Penrose, had invited him to speak about his work with Rick Schoen on the positive mass theorem, and this afforded me the opportunity to sit across the table from Yau at lunch. While he made a point of politely asking about my own work, I doubt I told him anything that day that made much of an impression. However, the flow of ideas in the opposite direction was substantial, and I learned things during Yau's visit that helped pave the way for my own work on the mass of ALE manifolds a few years later.

However, I was still completely unaware of Yau's solution of the Calabi conjecture, and I only really began to acquire a fragmentary understanding of his amazing work on Kähler-Einstein metrics a few years later, when I was a postdoc at the IHES. This new interest turned out to come with an added bonus. The IHES's small library prominently displayed every new book upon arrival, so Yau's *Seminar on Differential Geometry* immediately caught my eye, and I ended up returning to the library time and time again to peruse the volume. This book contained a wealth of important articles, and several of them, such as Calabi's *Extremal Kähler Metrics*, would eventually play an important role in my own work. But, like so many other ambitious young mathematicians, my attention was first drawn to the *Problem Section*, which listed a number of open questions that seemed incredibly fascinating, but impossibly difficult. It would frankly come as a real surprise when I was actually able to settle a couple of these questions a few years later!

I think the next time I actually got to talk to Yau was the next year, when I had moved on to a postdoc at MSRI. By then, he was a Fields medalist, and arguably the most famous differential geometer in the world. Yau was already starting to the think about Kähler-Einstein metrics in the positive-scalar-curvature case, and he was specifically interested in proving the existence of such metrics on del Pezzo surfaces. He expressed a certain amount of concern, though, regarding the blow-up of $\mathbb{CP}_2$ at one point, which couldn't admit such a metric by a theorem of Matsushima. I then pointed out that this space nonetheless does admit a non-Kähler Einstein metric, because an explicit one had been constructed a few years before by Page. Yau's reaction was that this was irrelevant, and that one probably couldn't learn anything from such a special example. What neither of us knew at the time, though, was that Derdziński had recently discovered that Page's metric is *conformally* Kähler; indeed, it is actually a conformal rescaling of one of Calabi's explicit extremal Kähler metrics! It would take me a quarter of a century to show, in collaboration with Chen and Weber, that this general-

ization of the Kähler-Einstein condition allows one to find Einstein metrics on every del Pezzo surface. Fortunately, differential geometry is such a vast subject that there is plenty of room in it for those of us who, unlike Yau, need to chew on ideas for decades before we make any substantial progress!

Not long after this, Yau left Stanford to move to San Diego, where a group of mathematical stars was being assembled that for a time converted UCSD into one of the world's foremost centers for geometry and topology. This was fortuitous for me, because my parents lived near San Diego at the time. Thus, even though my job was on the East Coast, where I had accepted a tenure-track position at Stony Brook, I had many opportunities to visit UCSD in the mid-1980s. I also had the good fortune to have been befriended by Rick Schoen during my time at MSRI, and Rick didn't seem to mind at all if I dropped by on short notice. When I did so, I would also sometimes get a chance to briefly chat with Yau, whose presence at UCSD was attracting a constant stream of interesting geometers. One such visitor was Gene Calabi, who suddenly walked into Rick's office during one of my visits and immediately launched into an extemporaneous lecture on extremal Kähler metrics. The net effect was that I was soon dipping back into Yau's seminar volume, trying to understand the gist of what Gene had said!

But my own work at the time was largely focused on twistor methods in differential geometry, and it took me quite a while to find the sweet spot for my methods. At the time, tenure at Stony Brook was hardly a foregone conclusion, and I grew more and more nervous as I witnessed fellow assistant professors being terminated. By the time I came up for tenure, I was therefore completely terrified. Even though I was finally beginning to make some breakthroughs in my work, it was unclear whether my senior colleagues would feel I was a safe bet and take a favorable view of my case. Fortunately, Yau seems to have written a persuasively positive letter on my behalf. (Of course, his name had been redacted from the transcribed version I was eventually allowed to read, but I have now read enough of Yau's recommendations to be able to easily identify his style.) I remain grateful for Yau's support at this critical juncture of my career, because I believe it played a key role in swaying the committee in my favor.

My good fortune then continued, because one of the most creative periods of my mathematical life began as soon as I was relieved from the stress of tenure-review. My work around this time focused on scalar-flat Kähler surfaces, which I had previously observed to be anti-self-dual 4-manifolds, and thus to have twistor spaces. Shortly before I came up for tenure, I had constructed ALE scalar-flat Kähler metrics on negative line bundles over $\mathbb{CP}_1$; and because most of these turned out to have negative mass, they incidentally provided counter-examples to a conjecture of Hawking and Pope. But I soon realized that the key to further progress might be to next thoroughly understand the explicit construction of Ricci-flat Kähler manifolds found by Gibbons and Hawking. Doing so led rather quickly to the writing of two papers (one joint with Anderson and Kronheimer) that gave negative answers to questions on Yau's problem list. Yau reacted to these developments with interest and magnanimous expressions of support. This was not only deeply appreciated, but also came as something of a relief, because, by contrast, Hawking had reacted to my earlier paper by first denying that he had ever made such a conjecture, and then, upon being presented with a highlighted copy of his own article, by trying to blame the whole thing on his co-author!

These events were followed by my wonderful first sabbatical, which was primarily spent at the IAS. The previous summer, I had carried out preliminary calculations regarding an explicit construction of scalar-flat Kähler metrics, while happily teaching an introductory graduate course in Perugia, Italy. Because my focus there had been on mastering Italian, with only a bit of time dedicated to doing creative mathematics during my afternoon siestas, I had left Italy pleased with my newly acquired command of the language, but with little expectation that my summer's calculations would ever amount to much of anything. This changed dramatically shortly after I arrived in Princeton and promptly came down with a high fever. In a fever dream, I wrestled with an angel who insisted that the essence of the entire construction hinged on harmonic functions in hyperbolic 3-space, while expressing utter contempt for my futile protestations to the contrary. Well, I have had plenty of dreams about mathematical ideas that made perfect sense to my sleeping mind, but then proved to be total nonsense once my critical faculties were reawakened. But this strange experience was completely different. To my amazement, the angel in my dream had in fact spoken the plain truth!

The upshot was that I spent my time at Princeton writing a series of papers constructing explicit anti-self-dual metrics on compact 4-manifolds. One of these articles constructed scalar-flat Kähler metrics on blow-ups of ruled surfaces. Here, one of Yau's early papers, written at Stony Brook and published in *Inventiones*, provided a vital piece of the groundwork. Yau had given an elegant proof that a compact complex surface with total scalar curvature zero would necessarily either be Calabi-Yau or else be obtained from a geometrically ruled surface by blowing up. What I managed to show, in this and in a later related

paper with Kim and Pontecorvo, was roughly that everything Yau had not excluded does in fact actually happen.

I began to get to know Yau much better personally several years later, during our extended stays at the newly-opened Isaac Newton Institute in Cambridge, England. I quickly discovered that Yau's broad knowledge and profound curiosity were not limited to the mathematical realm, but extended to a broad range topics in the sciences and humanities, so that our discussions concerned not only gauge theory, extremal Kähler metrics, and special holonomy, but also string theory, quantum gravity, world history, world religions, and so forth. I fondly remember Yau treating me, Nick Shepherd-Barron, and a few other mathematicians to a fine Chinese meal at a restaurant in Cambridge where he apparently knew the owner. Meanwhile, the program at the Institute featured parallel lecture series by Hawking and Penrose that eventually formed the basis of a semi-popular book. As for me, I spent far too much of my time in Cambridge carefully preparing my slides for my upcoming talk at the 1994 ICM in Zurich—an effort that turned out to be largely wasted, because Nicos Kapouleas, mistaking my briefcase for his own, would walk off with my slides just before the beginning of my talk!

Later that year, the Seiberg-Witten equations suddenly appeared on the scene, and it immediately became clear that the relationship between scalar curvature and Kodaira dimension that Yau had pointed out in the Kähler context actually reflected a more general interaction between the Riemannian geometry and differential topology of 4-manifolds. My very first paper in the area generalized the Miyaoka-Yau inequality for complex surfaces of general type to the setting of 4-dimensional Einstein manifolds with a non-trivial Seiberg-Witten invariant. This in particular implied a uniqueness theorem for Einstein metrics on ball quotients of complex dimension 2.

Perhaps because of this, Yau invited me to give a series of talks at a conference in Hong Kong the very next summer. This was a fantastic experience for me, both mathematically and culturally. I was particularly interested in Yau's work on string theory and enumerative geometry on Calabi-Yau 3-folds, but there were also many other excellent Kähler geometers at the conference. I particularly enjoyed the gracious hospitality extended to me by S.-Y. Cheng, who showed me around both Hong Kong and Macau. I also had interesting conversations with Mok and Leung, among many others. Hong Kong was still a British colony at the time, so many of the participants from the Chinese mainland seemed to consider the place to be just as fascinatingly exotic as I did, albeit for opposite reasons. I still vividly recall how thrilled one of the mainland participants was to be allowed to choose a several-hundred-pound Vietnamese lake fish for consumption at the banquet, and how surprised I was to then witness the struggling leviathan's public decapitation by meat-cleaver.

By the next fall, my work in Seiberg-Witten theory had produced new obstructions to the existence of Einstein metrics on suitable 4-manifolds, in a way that depended on diffeotype rather than homeotype. (Over the next few years, I was able to significantly improve these results by showing that the Seiberg-Witten equations imply estimates for quantities involving the Weyl curvature as well as the scalar curvature.) Soon after, I also showed, in joint work with Fabrizio Catanese, that there are high-dimensional smooth compact manifolds that admit pairs of Einstein metrics with opposite signs of the Einstein constant. The proof was highly dependent on Yau's results, because the relevant Einstein metrics were actually Kähler-Einstein, albeit with respect to wildly different complex structures.

I was also pushing my work in Seiberg-Witten theory in other directions, such as calculating the Yamabe invariant (or sigma constant) for complex surfaces of Kähler type. The latter was particularly tricky in the positive case, but fortunately I was in contact with Cliff Taubes, who provided some key guidance regarding an important technical subtlety. I therefore felt very lucky when Cliff invited me, first to visit Harvard to give a talk, and then to spend my next sabbatical there.

Fortunately, Yau and other members of the Harvard Mathematics Department also supported the idea. The logistics remained non-trivial, though, because my wife, Dolores Augustine, is also an academic, and we already had a young son by that time. However, my wife and I had managed to sync our sabbaticals, the Harvard History department agreed to host my wife, the Harvard Law School day-care center offered a place for my son, and the math department offered to house us in an apartment that was usually reserved for Jean-Pierre Serre. With the stars aligned in this way, we were therefore able to spend a happy and productive semester in Cambridge, Massachusetts, in the Fall of 1998. I was especially pleased to give a talk in Yau's seminar, and the semester gave me many opportunities to discuss mathematics with Yau. Of course, I was also excited to discuss mathematics with a number of his amazing colleagues. In fact, the main problem was that there was so much going on at Harvard and MIT that I found it difficult to just sit and write my own papers. However, I absorbed a number of exciting ideas that semester that played important roles in articles that I eventually wrote after returning home to New York.

The next few years were particularly productive and satisfying ones for me. I proved increasingly stronger results on Einstein 4-manifolds using

Seiberg-Witten theory, and I still consider some of these papers to be among my very best. But I also wrote an unrelated series of papers with Lionel Mason in which we developed a new approach to Zoll manifolds and related phenomena by developing a variant of twistor theory based on moduli spaces of holomorphic disks rather than of rational curves. While Victor Guillemin had previously been able to prove an important result about the space of Zoll metrics on $S^2$ that were small perturbations of the standard metric, our techniques yielded results that applied to metrics that were far away from the standard example. This was in part made possible by Yau's beautiful theorem that there is only one complex structure on $\mathbb{CP}_2$. This link with Yau's mathematics may partly explain why the first of these papers eventually appeared in the Journal of Differential Geometry.

But, while all these other things were going on, I'd never forgotten about a small paper I had written back in 1995 for a volume commemorating a conference in Aarhus, Denmark (where I'd incidentally also had some brief but interesting discussions with Yau). This little paper had shown that if an Einstein metric on a compact complex surface was Hermitian, it would have to be conformal to an extremal Kähler metric, and that it would actually have to be Kähler-Einstein unless the complex surface were toric. The fact that this exception was essential was illustrated by the example of the Page metric on $\mathbb{CP}_2\#\overline{\mathbb{CP}}_2$. The paper then concluded with the speculation that there might be a companion metric on $\mathbb{CP}_2\#2\overline{\mathbb{CP}}_2$, and noted that showing this would hinge on constructing an extremal Kähler metric of positive scalar curvature in a specific irrational Kähler class identified in the paper. Fortunately, I was able to show this in 2007 in joint work with Xiuxiong Chen and his student Brian Weber, who had recently proved a weak compactness result for extremal Kähler metrics. When Yau invited me to speak at the Seventh Conference on Geometry and Topology at Harvard, I was therefore thrilled to be able to present this result and some of its consequences.

Over the next few years, I proved a number of uniqueness results concerning conformally Kähler, Einstein metrics, either from the point of view of complex geometry or in purely Riemannian terms. I also discovered a simpler, more conceptual existence proof that clarifies my earlier result with Chen and Weber. But existence results in this subject often crucially depend on a thorough understanding of ALE scalar-flat Kähler manifolds, which arise as bubbling modes for the problem, and so represent a potential obstruction that must be excluded. Conversations with Hajo Hein and his wife Bianca Santoro thus led me back to the problem of systematically understanding the mass of ALE Kähler manifolds, a problem that Hajo and I were able to solve after overcoming many subtle technical difficulties. I was therefore very excited when Yau invited me to Harvard to speak about our work. I do not know, but it is just possible that my talk there may have helped rekindle Yau's interest in in the high-dimensional positive mass theorem, since in the Kähler case Hajo and I showed that it followed from our mass formula. In any case, Yau and Rick Schoen finally proved the positive mass theorem in full generality a couple of years later. It was a real pleasure to then see them both at the Simons Center, where they were the stars of the show at a workshop on mass in general relativity and Riemannian geometry.

I have deeply appreciated the support and friendship that Yau has offered me throughout my career. My gratitude to him has gone hand-in-hand with the sense of wonder I still get from his many fundamental contributions to mathematics, as I have never ceased to be awed and amazed by the beauty of his best results. I can only hope that the anecdotes I have recounted here have made it clear how overwhelming the influence of Yau's ideas has been on geometers of my generation. But, above all, I hope that these small expressions of my esteem have meaningfully contributed to our communal project of wishing Shing-Tung Yau a very happy seventieth birthday!