# A Century of Legends: Remembering Calabi

Shing-Tung Yau

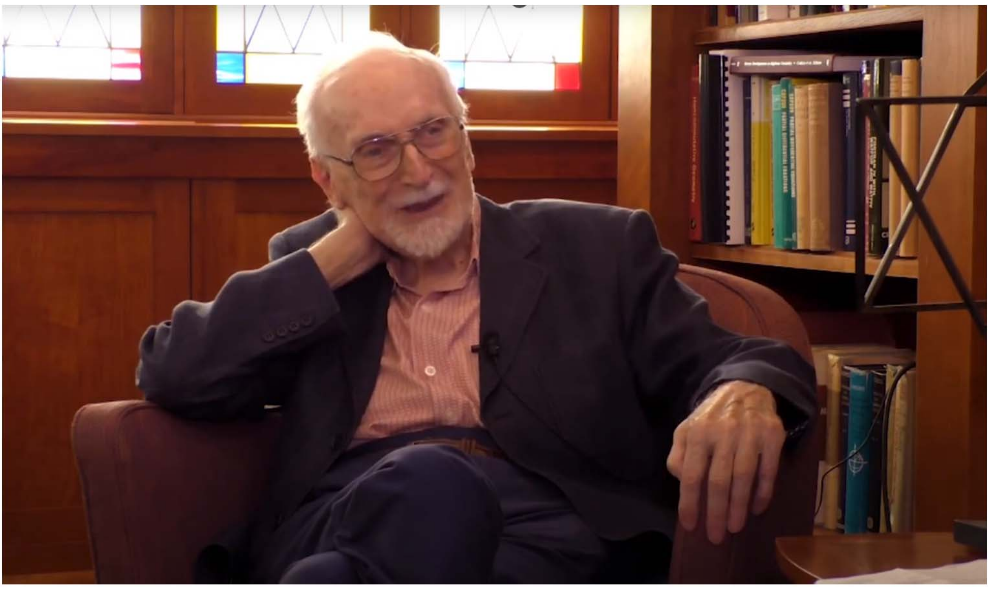

*Figure 1. Eugenio Calabi (May 11, 1923 – September 25, 2023). Credit: Simons Center for Geometry and Physics.*

Biographical Sketch. Eugenio Calabi passed away September 25, 2023. He was a member of the American National Academy of Sciences and an emeritus professor at the University of Pennsylvania. He obtained worldwide recognition for his revolutionary work in differential geometry, geometric flows, string theory, and other advanced areas of mathematics. In 1991, he was awarded the Steele Prize of the American Mathematical Society for his foundational work in global differential geometry. Calabi's name is associated with dozens of theorems, conjectures, properties, and principles, the most famous example being the Calabi-Yau manifolds. He collaborated with the best scientists and mathematicians in the world, and taught many remarkable scholars.

This article is an adapted translation of an article written this past summer in honor of Calabi's hundredth birthday.
*Translated and adapted by Nathan Thomas Carruth.*

May 11 of this year was Professor Eugenio Calabi's hundredth birthday. My old friend Jean-Pierre Bourguignon specially edited a collection in his honor, which contains many articles from friends and students.[1] Unfortunately, over the first part of this year I was busy with preparations for the International Congress of Basic Science to be held in Beijing, and did not have time to contribute anything. Bourguignon's collection has already been completed; but as I now have a bit of time, I would still like to write some of my thoughts, and express some of my feelings.

The first time I saw Calabi was in the spring of 1970. At that time Blaine Lawson had just graduated from Stanford and come to Berkeley as a lecturer. I took his course on elementary differential geometry, started working with him, and wrote an article. He told me that Calabi had given him some very helpful direction on his doctoral thesis, that Calabi was a highly original geometer, and that I ought to take advantage of the opportunity to talk with Calabi while he was visiting Berkeley.

It took some effort, but finally I found Calabi in a small lounge at the Berkeley Mathematics Department in Campbell Hall. He was wearing a pair of thick glasses (he was very nearsighted) and relaxedly talking with some young students while smoking. He had written a pile of equations on a napkin, and I had to go over and look closely to see that they were the equations describing minimal submanifolds. Not until I had had more contact with Calabi did I come to know that this was one of his specialties. He was often able to find analytical answers from complicated geometric phenomena. His doctoral thesis had been in the study of isometric embeddings of metrics on Riemannian surfaces into complex Euclidean spaces.

Professor Calabi's advisor was Salomon Bochner from Princeton University. Professor Bochner's work in differential geometry had a great impact on me. After he retired, he moved to Rice University in Houston, Texas. In 1978, Bochner really wanted to hire me, so he invited me to come to Houston to give a named lecture series. He and I got along well. He told me that Calabi had been his doctoral student, and because he was so outstanding, his colleague at the time, Don Spencer, had tried to convince Calabi to change to study under Spencer! After Bochner learned about it, he made sure that Calabi graduated quickly. Calabi introduced many important concepts in his doctoral thesis.

In May 1970, I began to be interested in a certain problem arising from general relativity: how to construct complete, nonflat metrics with zero Ricci curvature. This was a hard problem. While I was looking through some books in the library, I happened to come across an article Calabi had written in 1954, which introduced me to a spectacular conjecture he had proposed at the International Congress of

[1] *Translator's note:* See [1].

Mathematicians. One consequence of this conjecture was a solution to my original problem: any compact Kähler space with vanishing first Chern class should possess a complete, nonflat, Ricci-flat metric. Calabi's conjecture could be phrased in terms of the existence problem for a certain, very difficult, nonlinear partial differential equation. It was a complex Monge-Ampère equation, and at the time there were essentially no publications about it. (On the other hand, the study of Monge-Ampère equations in two real variables had a long history. The classical Minkowski and Weyl problems can both be described by different forms of that equation. In two variables, it has a close relation to complex analysis; and in general, it has deep connections to the theory of the affine sphere in affine geometry.) In the summer of 1973, I saw Calabi at a differential geometry conference, and told him that I was thinking about his conjecture. He told me that when the great mathematician André Weil had seen the associated equation, he had felt that solving it was far beyond the reach of PDE theory as it then stood.

As it happened, in the spring of 1970, I had taken a class on partial differential equations from Charles Morrey, an expert in the subject. At that time, the U.S. was supporting [South] Vietnam's invasion of Cambodia, and many students and faculty at Berkeley had walked out in protest. By the last class, I was the only student present; but Professor Morrey kept on teaching, though we had moved to his office. Because of this, I had learned a great deal about partial differential equations. Even so, when faced with this equation, I felt totally helpless!

From 1970 I began to be most interested in the Calabi conjecture. I felt that it would illuminate the deepest aspects of Kähler manifolds, and was also a central problem in the study of curvature. To make an analogy: if a great river is obstructed by mountains or boulders, they must be removed so that the water can flow freely, otherwise it cannot flow very far. At that point in the development of differential geometry, the study of curvature was mostly restricted to sectional curvature and comparison theorems, and nothing was known about how to deal with that most important curvature, Ricci curvature. Ricci curvature is the main bridge between differential geometry and physics, but there were no meaningful specific examples. I felt that differential geometry needed to flow together with theoretical physics, and the lack of good examples had become the main obstruction to the confluence of these two great sciences. It had to be removed. Professor Chern wanted me to work on the Riemann hypothesis, but I couldn't get up any excitement for it; on the other hand, for this geometric conjecture, I had a feeling like that in a song by Liu Bannong:[2] "teach me how to not think about her"!

I had the passion, but I didn't know how to start. Because I had no idea where to start, I decided to try for a quick resolution. Following the thinking of the time, I rolled up my sleeves and looked for a counterexample. At that point, everyone thought this conjecture was too elegant to hold. People had spent decades of effort and still had not found a single example of this kind of space, and yet this conjecture would give a multitude of them as a trivial consequence. Looking back,

[2] *Translator's note:* An early-20th century Chinese poet.

this kind of thinking was really just an attempt to avoid difficulties: nobody dared face the obstacles that would be encountered trying to find estimates for that complex Monge-Ampère equation. Be that as it may, I put all I had into finding a counterexample, and at first I thought I had been successful. In the summer of 1973 there was a meeting at Stanford, and I told Calabi that I had found a counterexample. Smoking, he – together with other researchers – listened carefully to my explanation, nodding the whole time. I was extremely excited, and thought it was correct.

But a few months later, Calabi wrote me a letter, pointing out some problems in my counterexample. As soon as I read the letter, I knew that I was in serious trouble. I spent two weeks trying every trick I could think of, but nothing could fix the issue with the counterexample; at the same time, I could not find any other counterexamples. After a lot of thought, I began to change my mind and feel that the conjecture should hold after all! Nevertheless, the three years' effort spent on looking for a counterexample was not entirely wasted: the ideas that I used in that search subsequently led to an important theorem in algebraic geometry. The deep connections of the Calabi conjecture with algebraic geometry also came from these same ideas.

This meant that I needed to study the theory of Monge-Ampère equations. Since convex functions are easier to deal with, my plan was to first understand real Monge-Ampère equations. On the other hand, I was facing a space of arbitrary dimension, so the techniques of complex function theory were not available. One known method for solving partial differential equations was through so-called "a priori estimates", obtaining higher estimates from lower ones in a step-by-step process. Most students of differential equations do not do analysis on manifolds, but I had started to study problems of analysis on manifolds in the spring of 1970. At that time I was studying harmonic functions and harmonic maps. I had obtained a gradient estimate on harmonic functions; that method influenced my later development in geometric analysis, but on the surface it had no close connection to Monge-Ampère equations.

At that time I was at Stanford, Shiu-Yuen Cheng was still at Berkeley (he hadn't graduated yet), and he often came to look for me. He was in the middle of taking Professor Chern's class on affine geometry, the high point of which was some theorems of Calabi's. Cheng and I studied Calabi's great work on affine geometry together. One important problem in affine geometry is the classification of affine spheres. One class of affine spheres is a generalization of parabolic surfaces, and the general conjecture is: all improper affine spheres are paraboloids. This question can be seen as the central question in the study of real Monge-Ampère equations. Around 1950, the German mathematician Konrad Jörgens proved this conjecture in two dimensions using the theory of complex functions. At the start of 1960, Calabi proved that the conjecture held in dimensions no greater than 5. Cheng and I discovered that my gradient estimate could be applied to completely resolve the conjecture. Just as we were reveling in the excitement of this breakthrough, we were startled to realize that the Soviet mathematician Aleksei Pogorelov was one step ahead of us and had already resolved the exact same conjecture. His proof used completely different methods, which were afterwards called Pogorelov estimates. My and Cheng's methods were described in geometrical language, and were not easy to understand, but were actually more general. For example, they could be used to solve Calabi's conjecture about hyperbolic affine spheres, which was an important piece of work, and could also be seen as giving estimates for Monge-Ampère equations. (We didn't imagine that later there would be a certain Chinese scholar who would find a small error in our article and try to claim credit for the whole work.)

We subsequently realized that Calabi had written an article in which he proved that in dimensions less than 5, any maximal spacelike hypersurface in Minkowski space must be a plane. Cheng and I extended his result to any dimension. Afterwards, my student Robert Bartnik further extended this to more general spacetimes, making important contributions to general relativity. Reading these articles fifty years later, I feel that they still have historical value. All of this was a gift we obtained from studying Calabi's works.

At the start of 1974, Professor S. S. Chern told me that Louis Nirenberg had planned to announce results he and Calabi had obtained about the boundary-value problem for the Monge-Ampère equation at the International Congress of Mathematicians in Vancouver that summer, but they had discovered that the proof had an error, and Chern suggested that we look over it. Cheng and I thought that our method could resolve the issue. Initially we ran into the same problem they had, but fortunately after an adjustment we obtained the correct answer, except that the regularity requirements for the boundary were imperfect. Because this was not that related to geometry, I didn't care that much, and only a very few people knew about this work. Ten years later, Luis Caffarelli, Louis Nirenberg, and Joel Spruck worked out the regularity of the boundary.

To address the Calabi conjecture, in addition to partial differential equation theory, one needed a sufficient knowledge of Riemannian geometry, the most important part of which for the Calabi conjecture was the nature of Ricci curvature on manifolds. Generally speaking, one considers the situation where the Ricci curvature is bounded below. In 1974 I used methods of geometric analysis to prove that when the Ricci curvature is positive, the volume of a complete, noncompact manifold is infinite and linearly growing.

Only after submitting that manuscript did I see in the newest issue of the *Notices of the AMS* that Calabi had already published similar results. When I presented my results at Berkeley, M. Gromov didn't believe my analytical proof, and I spent a day using more geometrical methods to explain it to him. Several years later, in joint work with Jeff Cheeger, he used the methods I had described to him that day to generalize my and Calabi's results.

After this preparatory work, starting in 1975, I obtained a succession of estimates which were sufficient to prove the existence of negatively-curved Kähler-Einstein metrics. By September of 1976, when I was visiting UCLA on a Sloan Fellowship, I completed a full proof of the Calabi conjecture.

I used many different methods to try to verify the correctness of the proof, and because of this came up with important results in algebraic geometry. But I still wasn't entirely comfortable, and decided that in November I would go to Philadelphia to discuss my proof with Calabi. After a few days' discussion, everybody felt there were no mistakes. I accepted I. M. Singer's invitation to stay a month at MIT. At that point Singer's mind was elsewhere and, after having one dinner together, during which we discussed his article with M. Atiyah and N. Hitchin, he wasn't able to have any more lengthy discussions. Nevertheless, that discussion had a major impact on my later work with Karen Uhlenbeck on conformal fields.

I lived in a dormitory next to MIT, and started to write out a full proof of the conjecture. For several days the snow blew around outside my window, and the crows and sparrows were all silent, but in my room my thoughts ran quickly, and I wrote furiously. When I had time I went to Harvard to look for people to talk to. I told David Mumford that with the resolution of the Calabi conjecture, by applying Mostow's rigidity theorem, one could prove that quotients of spheres were rigid. He liked this result very much. I talked at Harvard; the room was full. They were extremely excited that I had applied nonlinear partial differential equations to algebraic geometry.

(Previously, in 1975, I had talked at a conference on several complex variables at Williamstown, and pointed out that one could use the curvature of complex manifolds to study their uniformization. Yum-Tong Siu was very interested, and suggested we collaborate, so on the way down from Boston, when I passed through New Haven, I spent a few days working with him. I pointed out that I had used the Calabi conjecture to prove the rigidity of quotients of spheres, and the same method should be applicable to general curved algebraic manifolds, as long as the dimension of the space was greater than 1. I suggested that we use methods from harmonic maps to complete this. By this point I had already, with Richard Schoen, used harmonic maps to prove many geometric theorems. But Siu was always opposed to this idea; he thought that this should be a result in the duality of coherent sheaves. Two years later, I persuaded Stanford to hire Siu, and discussed this problem again. It happened that Eric Bedford had provided a clue: in appropriate circumstances, and in the context of algebraic manifolds of strongly negative curvature and maps between them, harmonic maps could be proven to be either holomorphic or antiholomorphic, which resolved a part of my conjecture. The proof actually was fairly easy; it could be seen as an extension of standard methods in differential equations. But Siu, without asking my permission, decided to publish this by himself, and wrote a long article about it. At that point I was busy with extensions of the Calabi conjecture, and didn't come back to this other area until joint work with Jürgen Jost in the 80s.)

While in Cambridge, I walked alone along the Charles River, looked at the water flowing under the bridge, watched the birds flying, and saw the fine rain like silk, and the rich grass; it was a very pleasant environment. Later on, when the media interviewed me, they asked me how I felt when I proved the Calabi conjecture, and I used two sentences of Song prose to answer: *luo hua ren du li, wei yu yan shuang fei* (the flowers fall, I am alone; in the fine rain, the swallows fly in pairs).[3]

On my way back to UCLA, I decided to go to New York to look for Nirenberg, who was an authority on nonlinear partial differential equations. Calabi also came, and we arranged to meet at New York University.

On Christmas morning, 1976, I went to Nirenberg's office. (Being Jewish, they did not object to meeting on Christmas day.) I spent four hours explaining the proof of the conjecture in detail to him and Calabi. Nirenberg finally agreed that the proof had no mistakes, and expressed his admiration. (Afterwards, at the International Congress of Mathematicians, he strongly recommended me for the Fields Medal.)

When I left his office, I realized that the entire building was empty; the road was empty too, and all the restaurants were closed. As a result I decided to walk to Chinatown to eat at a Chinese restaurant Nirenberg frequented.

I gained a lot from this trip, but I had been gone from my new wife for two months; my homing instinct awakened, and it was time to go home.

After I returned to Los Angeles, I realized that I had become famous overnight. But I kept quiet and stayed focused on research. I was surprised when Singer drove from Berkeley to Los Angeles to visit me and talk about research. He was a great mathematician, a good friend of Chern, and visiting Berkeley at the time. He asked whether I would consider staying at Berkeley. This left me both excited and unsettled.

Following him Calabi also came; I was extremely excited to have him visit. I looked up to all of them – Chern, Morrey, Nirenberg, Singer, and Calabi – as great mathematicians, but my thinking was closest to Calabi's, and my interactions with him were the most unrestrained. So every time I saw him, I would talk about absolutely everything. (He liked to fill napkins with equations at mealtimes, and I kept all of those napkins in a drawer. Then, about ten years ago, when I was department chair at Harvard, my office was piled high with books and papers, and my secretaries all protested, feeling that I didn't need to keep all these things which could be found online. With my reluctant agreement, they cleaned it all out. My friends were all excited when they saw my office all cleared out; they probably thought that they had finally "waited until the clouds parted and the moon shone". But every time I remember that these meaningful items are all gone, I feel a sense of sadness.)

This time, when Calabi came, he brought his brother, and my wife and I were happy to host them. Because the proof of the Calabi conjecture meant that there were a large number of Kähler manifolds with zero Ricci curvature, many people gained the confidence to try to write down such metrics explicitly (actually they used numerical computations; my original proof was already enough, but one had

[3] *Translator's note:* This is from Lin Jiang Xian, by the southern Song poet Yan Jidao (1038 – 1110).

to overcome computational problems in high-dimensional spaces). The physicists Stephen Hawking and Gary Gibbons obtained so-called gravitational instanton solutions via a Wick rotation from classical solutions in relativity, while Calabi used fiber bundle methods to construct many such metrics. Nevertheless, none of these concrete solutions were on compact spaces.

When I was writing up my proof of the Calabi conjecture, I had already considered the case of degeneracies and singularities in Kähler-Einstein metrics, because when one talks about certain spaces one must allow for such metrics. In actuality, the second half of my article discussed and completed the existence theorem for metrics with singular points. At first everyone focused on the case of metrics without singularities; in the past twenty years the study of metrics with singularities has become an important direction in research, but everyone has forgotten the early work I did in this area. In 1978, at a talk at the International Congress of Mathematicians in Helsinki, I mentioned the construction of complete but noncompact spaces; these results generalized the above-mentioned constructions of Calabi. Ten years later, I had my student Gang Tian write out the details for part of these results, which we later published together.

In the winter of 1977, the American Mathematical Society held a geometry meeting in Utah, where I met Calabi again. At the meeting I described my work with Richard Schoen on using harmonic maps to construct minimal submanifolds. I pointed out that the energy of these maps could be viewed as a Morse function on Teichmüller space, from which I proved that the space was contractible. An analogous way of thinking could also be applied to moduli of higher-dimensional spaces and other geometric structures.

That day it snowed a lot, and many people went skiing. Calabi and several other of us young workers in geometry talked around the stove. I told him that harmonic maps were very similar to harmonic 1-forms, which made vanishing theorems very useful. I had always been very interested in Yozo Matsushima's vanishing theorem on locally symmetric spaces, but this theory was restricted to symmetric spaces. Calabi was very excited and said that using pure differential geometric methods to derive the Matsushima theorem would be very nice. It wasn't until ten years later that Jost and I found how to apply Calabi's formula to harmonic maps, and use it to give a new proof of Grigory Margulis's superrigidity theorem.

The next time we met after this conversation was during the 1979-1980 Special Year in Geometry at the Institute for Advanced Study, which Armand Borel and myself organized. At that time geometric analysis was extremely hot, many articles were coming out, and many important problems were being solved; this area was moving the entire mathematical world. The IAS decided to hold a Special Year in Geometry and invite leading researchers from around the world with the goal of understanding the forefront of geometric analysis and finding directions for future developments. Essentially every scholar in geometric analysis was invited, and those who accepted the invitation came and lived in Institute housing. It could be called a gathering of excellencies old and young, for mutual refinement and inspiration. It was an unprecedented opportunity.

As far as I can recall, the attendees included Calabi and his wife, Richard Schoen and his wife, Leon Simon and his family, Peter Li and his wife, Karen Uhlenbeck and her boyfriend Robert Williams, Shiu-Yuen Cheng (then at Princeton University) and his wife, while the younger scholars included Robert Bryant, Michael Anderson, a group of theoretical physicists including Malcolm Perry and Alan Lapedes, and others who came for a time, including Nirenberg, Arthur Jaffe and Clifford Taubes, and Enrico Giusti and Roger Penrose.

Because my wife was working in San Diego, I made space in my three bedroom, one living room apartment for a graduate student I brought with me from Stanford. This gentleman loved parties; in Christmas of 1979, while I was in Hong Kong taking care of my older brother, he took advantage of my absence to hold a Christmas party in the apartment. After I came back, he gleefully told me that the party had been very successful, Calabi and his wife had danced, and Uhlenbeck had gotten drunk. It was so lively that the Institute complained to me that my apartment was too noisy.

Calabi and his wife invited me to dinner twice, once at the IAS and once at his home in Philadelphia. At the IAS, I organized several discussion groups, he spoke multiple times, and we also had many fruitful private interactions. We also discussed extremal metrics, which were very interesting. But my focus was always on the general nature and application of Einstein metrics, and I didn't put too much effort into extremal metrics.

As I look back on it today, the Special Year in Geometry at the IAS was extremely successful. Most of the participants are still alive, and they all have treasured memories of that year. Borel passed away a while ago; we were fortunate that Calabi stayed with us so long.[4]

That year I was hired by the IAS and settled down in Princeton. In 1981, my first son was born; my wife and parents-in-law took care of him in San Diego. In the fall of 1982, my wife took a leave of absence and brought our son to the east coast to spend six months with me; we decided to stay in Philadelphia. The house we stayed in was not far from Calabi's. During the move, we had our hands full taking care of our one-year-old. Calabi was much more handy than me: he brought a small bed they didn't need and set it up himself. At that time he was already in his 60s but still very strong and energetic. I was very touched. During this time, he and his wife Giuliana often came over to visit; I treasured their friendship.

In 1979 I came back to China for a visit, and in Guilin I met an artist named Huang. His daughter was very talented in drawing and wanted to go to the U.S. to study. Borel's wife introduced her to an art school in Philadelphia, but she needed someone to take care of her, so I introduced her to Calabi and his wife. Calabi handed the responsibility to his student Dajiang Yang, and, as the saying goes, the unobstructed tower first gets the moonlight: they very soon got married!

After 1982, we didn't see each other as much. When I went to Harvard, his daughter was living nearby, and sometimes I would run into him and his wife. In

[4] *Translator's note:* Calabi and Borel were both born in 1923.

the 1990s he had a bypass surgery, and I flew to Philadelphia to see him. The surgery was very successful and he had no subsequent problems.

After 1984, string theory as developed by mathematical physicists was based on the spaces which were found in the resolution of the Calabi conjecture, and they became known as Calabi-Yau spaces. When Calabi turned 80, I gave him a birthday toast at the Harvard Faculty Club. I made a joke: everyone said Calabi-Yau so much that I thought Calabi was my given name!

In the last years before his passing, we didn't see each other much, but I will always remember my friend and teacher, Professor Calabi. He spent his life in research; he didn't care about fame or personal benefit, and he was always happy to teach others. He was respected by his colleagues and admired by younger generations. His academic accomplishments were outstanding, and are engraved in the annals of the history of science. I would like to mark his hundredth birthday with these lines of Chu prose:[5]

> He is as immortal as heaven and earth,
> Shining as bright as the sun and the moon!

## References

[1] J.-P. Bourguignon and Balázs Szendrői, *Gene Calabi at 100 – Memorable encounters with Eugenio Calabi*, Eur. Math. Soc. Mag. **128** (2023), 30–35. MR4614846

Shing-Tung Yau
*styau@tsinghua.edu.cn*
YMSC, Tsinghua University, Beijing (China)

[5] *Translator's note:* These are by the 4th century (B.C.) writer Qu Yuan. The translation here is mine.