# Interview with John Milnor

## Martin Raussen and Christian Skau

John W. Milnor is the recipient of the 2011 Abel Prize of the Norwegian Academy of Science and Letters. This interview, conducted by Martin Raussen and Christian Skau, took place in Oslo in conjunction with the Abel Prize celebration on May 25, 2011, and originally appeared in the September 2011 issue of the *Newsletter of the European Mathematical Society.*

**Raussen & Skau:** *Professor John Milnor, on behalf of the Norwegian and Danish Mathematical Societies, we would like to congratulate you for being selected as the Abel Prize Laureate in 2011.*

**Milnor:** Thank you very much.

### Student at Princeton University

*R & S: What kindled your interest in mathematics and when did you discover that you had an extraordinary aptitude for mathematics?*

**Milnor:** I can place that quite clearly. The first time that I developed a particular interest in mathematics was as a freshman at Princeton University. I had been rather socially maladjusted and did not have too many friends, but when I came to Princeton, I found myself very much at home in the atmosphere of the mathematics common room. People were chatting about mathematics, playing games, and one could come by at any time and just relax. I found the lectures very interesting. I felt more at home there than I ever had before and I have stayed with mathematics ever since.

*R & S: You were named a Putnam Fellow as one of the top scorers of the Putnam competition in mathematics in 1949 and 1950. Did you like solving mathematics problems and puzzles?*

**Milnor:** I think I always approached mathematics as interesting problems to be solved, so I certainly found that congenial.

*R & S: Your first important paper was accepted already in 1949 and published in 1950 in the prestigious journal* Annals of Mathematics. *You were only 18 years of age at the time and this is rather exceptional. The title of the paper was "On the Total Curvature of Knots". Could you tell us how you got the idea for that paper?*

**Milnor:** I was taking a course in differential geometry under Albert Tucker. We learned that Werner Fenchel, and later Karol Borsuk, had proved the following statement: the total curvature of a closed curve in space is always at least $2\pi$, with equality only if the curve bounds a convex subset of some plane. Borsuk, a famous Polish topologist, had asked what one could say about total curvature if the curve was knotted? I thought about this for a few days and came up with a proof that the total curvature is always greater than $4\pi$. (I think I did a poor job explaining the proof in the published paper, but one has to learn how to explain mathematics.) The Hungarian mathematician István Fáry had produced a similar proof at more or less the same time, but this was still a wonderful introduction to mathematics.

*R & S: That was quite an achievement! When you started your studies at Princeton in 1948, you met John Nash, three years your senior, who was a Ph.D. student. John Nash is well known through the book and movie* A Beautiful Mind. *Did you have any interaction with him? And how was it to be a Princeton student?*

**Milnor:** As I said, I spent a great deal of time in the common room, and so did Nash. He was a very interesting character and full of ideas. He also used to wander in the corridors whistling things like Bach, which I had never really heard before—a strange way to be introduced to classical music!

I saw quite a bit of him over those years and I also became interested in game theory, in which he was an important contributor. He was a very interesting person.

*R & S: At Princeton, you played Kriegspiel, Go, and a game called Nash?*

**Milnor:** That is true. Kriegspiel is a game of chess in which the two players are back-to-back and do not see each other's boards. There is a referee who tells whether the moves are legal or not. It is very easy for the referee to make a mistake, and it often happened that we could not finish because he got confused. In that case we said that the referee won the game! It was a marvelous game.

The game of Go was also very popular there. My first professor, Ralph Fox, was an expert in Go. So I learned something of it from him and also from

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many other people who played. The game that we called Nash had actually been developed earlier in Denmark by Piet Hein, but Nash invented it independently. This game, also called Hex, is based on topology. It is very interesting from a mathematical point of view. It is not hard to prove that the first player will always win if he plays correctly, but there is no constructive proof. In fact, when you play, it often happens that the first player does not win.

*R & S: You even published some papers on game theory with John Nash?*

**Milnor:** We often talked about game theory, but there was only one joint paper. Together with C. Kalish and E. D. Nering, we carried out an experiment with a group of people playing a many-person game. This experiment convinced me that many-person game theory is not just a subject of mathematics. It is also about social interactions and things far beyond mathematics, so I lost my enthusiasm for studying it mathematically.

One paper written on my own described a theoretical model for the game of Go. This was further developed by Olof Hanner and much later by Berlekamp and Wolfe. (John Conway's construction of "surreal numbers" is closely related.)

## Knot Theory

*R & S: You wrote your Ph.D. thesis under the supervision of Ralph Fox; the title of the thesis was "Isotopy of links". Did you get the idea to work on this topic yourself? And what was the impact of this work?*

**Milnor:** Fox was an expert in knot theory, so I learned a great deal about knots and links from him. There were many people in the department then that were active in this area, although there were also other people at the department that considered it a low-class subject and not very interesting. I think it's strange that, although it wasn't considered a very central subject then, today it's a subject which is very much alive and active.

As one example, I often saw a quiet Greek gentleman Christos Papakyriakopoulus around the common room, but I never got to know him very well. I had no idea he was doing important work, but Fox had managed to find money to support him for many years, while he did research more or less by himself. He finally succeeded in solving a very important problem in knot theory which, perhaps, was the beginning of a rebirth of the study of three-dimensional manifolds as a serious part of mathematics. A paper in 1910 by Max Dehn had claimed to prove a simple property about knots. Essentially it said that if the fundamental group of the complement of a knot is cyclic, then the knot can be unknotted. This proof by Max Dehn had been accepted for almost twenty years until Hellmuth Kneser in 1929 pointed out there was a big gap in the argument. This remained a

famous unsolved problem until 1957, when Papakyriakopoulus developed completely new methods and managed to give a proof of "Dehn's Lemma" and related theorems.

That was a big step in mathematics and an example of a case in which someone working in isolation made tremendous progress. There are relatively few examples of that. Andrew Wiles's proof of Fermat's last theorem is also an example of someone who had been working by himself and surprised everyone when he came up with the proof. Another example is Grigori Perelman in Russia who was working very much by himself and produced a proof of the Poincaré hypothesis. These are isolated examples. Usually mathematicians work in a much more social context, communicating ideas to each other. In fact, ideas often travel from country to country very rapidly. We are very fortunate that mathematics is usually totally divorced from political situations. Even at the height of the Cold War, we received information from the Soviet Union and people in the Soviet Union were eagerly reading papers from outside. Mathematics was much more open than most scientific subjects.

*R & S: As a footnote to what you said: Max Dehn was a student of David Hilbert and he solved Hilbert's third problem about three-dimensional polyhedra of equal volume, showing that you cannot always split them up into congruent polyhedra. No wonder people trusted his proof because of his name.*

**Milnor:** It's a cautionary tale because we tend to believe in mathematics that when something is proved, it stays proved. Cases like Dehn's Lemma, where a false proof was accepted for many years, are very rare.

## Manifolds

*R & S: For several years after your Ph.D. your research concentrated on the theory of manifolds. Could you explain what a manifold is and why manifolds are important?*

**Milnor:** In low dimensions manifolds are things that are easily visualized. A curve in space is an example of a one-dimensional manifold; the surfaces of a sphere and of a doughnut are examples of two-dimensional manifolds. But for mathematicians the dimensions one and two are just the beginning; things get more interesting in higher dimensions. Also, for physicists manifolds are very important, and it is essential for them to look at higher-dimensional examples.

For example, suppose you study the motion of an airplane. To describe just the position takes three coordinates, but then you want to describe what direction it is going in, the angle of its wings, and so on. It takes three coordinates to describe the point in space where the plane is centered
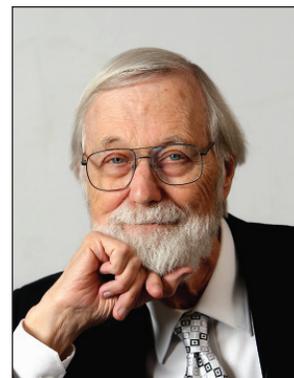


**John Milnor**

Photo by Knut Falch.

and three more coordinates to describe its orientation, so already you are in a six-dimensional space. As the plane is moving, you have a path in six-dimensional space, and this is only the beginning of the theory. If you study the motion of the particles in a gas, there are enormously many particles bouncing around, and each one has three coordinates describing its position and three coordinates describing its velocity, so a system of a thousand particles will have six thousand coordinates. Of course, much larger numbers occur, so mathematicians and physicists are used to working in large-dimensional spaces.

*R & S: The one result that made you immediately famous at age twenty-five was the discovery of different exotic structures on the seven-dimensional sphere. You exhibited smooth manifolds that are topologically equivalent to a seven-dimensional sphere but not smoothly equivalent, in a differentiable sense. Would you explain this result and also describe to us how you came up with the idea?*

**Milnor:** It was a complete accident, and certainly startled me. I had been working on a project of understanding different kinds of manifolds from a topological point of view. In particular, I was looking at some examples of seven-dimensional manifolds which were constructed by a simple and well-understood construction. They were explicit smooth objects which I would have thought were well understood, but looking at them from two different points of view, I seemed to find a complete contradiction. One argument showed that these manifolds were topological spheres and another very different argument showed that they couldn't be spheres.

Mathematicians get very unhappy when they have apparently good proofs of two contradictory statements. It's something that should never happen. The only way I could get out of this dilemma was by assuming there was an essential difference between the concept of a topological sphere (homeomorphic to the standard sphere) and the concept of a differentiable sphere (diffeomorphic to the standard sphere). This was something which hadn't been expected, and I am not aware that anybody had explicitly asked the question; we just assumed the answer was obvious. For some purposes one assumed only the topology and for other purposes one assumed the differentiable structure; but no one had really considered the possibility that there was a real difference. This result awakened a great deal of interest and a need for further research to understand exactly what was going on.

*R & S: You were certainly the driving force in this research area and you applied techniques both from differential geometry and topology, and also from algebraic topology, to shed new light on manifolds. It is probably fair to say that the work of European mathematicians, and especially French mathematicians like René Thom and Jean-Pierre Serre, who, by the way, received the first Abel Prize in 2003, made very fundamental contributions and made your ap-*proach possible. How did the collaboration over the Atlantic work at the time?*

**Milnor:** It was very easy to travel back and forth and I found French mathematicians very welcoming. I spent a great deal of time at the IHES [Institut des Hautes Études Scientifiques] near Paris. I hardly knew Serre (until much later), but I admired him tremendously, and still do. His work has had an enormous influence.

René Thom I got to know much better. He was really marvelous. He had an amazing ability to combine geometric arguments with hard algebraic topology to come up with very surprising conclusions. I was a great admirer of Thom and found he was also extremely friendly.

*R & S: Building on the work of, among others, Frank Adams from Britain and Stephen Smale from the United States, you, together with the French mathematician Michel Kervaire, were able to complete, to a certain extent, the classification of exotic structures on spheres. There are still some open questions concerning the stable homotopy of spheres, but at least up to those, we know what differentiable structures can be found on spheres.*

**Milnor:** That's true, except for very major difficulties in dimension four and a few problems in high dimensions (notably, the still unsolved "Kervaire Problem" in dimension 126). There are very classical arguments that work in dimensions one and two. Dimension three is already much more difficult, but the work of Bill Thurston and Grisha Perelman has more or less solved that problem. It was a tremendous surprise when we found, in the 1960s, that high dimensions were easier to work with than low dimensions. Once you get to a high enough dimension, you have enough room to move around so that arguments become much simpler. In many cases, one can make such arguments work even in dimension five, but dimension four is something else again and very difficult: neither high-dimensional methods nor low-dimensional methods work.

*R & S: One seems to need much harder pure analysis to work in dimensions three and four.*

**Milnor:** Well, yes and no. Michael Freedman first proved the topological Poincaré hypothesis in dimension four, and that was the very opposite of analysis. It was completely by methods of using very wild topological structures with no differentiability. But the real breakthrough in understanding differential 4-manifolds was completely based on methods from mathematical physics: methods of gauge theory and later Seiberg-Witten theory. Although motivated by mathematical physics, these tools turned out to be enormously useful in pure mathematics.

*R & S: Terminology in manifold theory is graphic and down to earth. Some techniques are known as "plumbing". Also "surgery" has become a real industry in mathematics, and you have written a*

*paper on "killing", but of course just homotopy groups. May we ask to what extent you are responsible for this terminology?*

**Milnor:** To tell the truth, I'm not sure. I probably introduced the term "surgery", meaning cutting up manifolds and gluing them together in a different way (the term "spherical modification" is sometimes used for the same thing). Much later, the idea of quasiconformal surgery played an important role in holomorphic dynamics.

Simple graphic terminology can be very useful, but there are some words that get used so much that one loses track of what they mean (and they may also change their meaning over the years). Words like "regular" or "smooth" are very dangerous. There are very many important concepts in mathematics, and it is important to have a terminology which makes it clear exactly what you are talking about. The use of proper names can be very useful because there are so many possible proper names. An appropriate proper name attached to a concept often pins it down more clearly than the use of everyday words. Terminology is very important; it can have a very good influence if it's successfully used and can be very confusing if badly used.

*R & S: Another surprising result from your hand was a counterexample to the so-called Hauptvermutung, the "main conjecture" in combinatorial topology, dating back to Steinitz and Tietze in 1908. It is concerned with triangulated manifolds or, more generally, triangulated spaces. Could you explain what you proved at the time?*

**Milnor:** One of the important developments in topology in the early part of the twentieth century was the concept of homology, and later cohomology. In some form, they were already introduced in the nineteenth century, but there was a real problem making precise definitions. To make sense of them, people started by cutting a topological space up into linear pieces called simplexes. It was relatively easy to prove that homology was well defined on that level, and well behaved if you cut the simplexes into smaller ones, so the natural conjecture was that you really were doing topology when you defined things this way. If two simplicial complexes are homeomorphic to each other, then you should be able to cut them up into pieces that corresponded to each other. This was the first attempt to prove that homology was topologically invariant, but nobody could quite make it work. Soon they developed better methods and got around the problem. But the old problem of the Hauptvermutung, showing that you could always find isomorphic subdivisions, remained open.

I ran into an example where you could prove that it could not work. This was a rather pathological example, not about manifolds; but about ten years later, counterexamples were found even for nicely triangulated manifolds. A number of people worked on this, but the ones who finally built a really satisfactory theory were Rob Kirby and my student Larry Siebenmann.

*R & S: Over a long period of years after your thesis work, you published a paper almost every year, sometimes even several papers, that are known as landmark papers. They determined the direction of topology for many years ahead. This includes, apart from the themes we have already talked about, topics in knot theory, three-dimensional manifolds, singularities of complex hypersurfaces, Milnor fibrations, Milnor numbers, complex cobordism, and so on. There are also papers of a more algebraic flavor. Are there any particular papers or particular results you are most fond or proud of?*

**Milnor:** It's very hard for me to answer; I tend to concentrate on one subject at a time so that it takes some effort to remember precisely what I have done earlier.

## Geometry, Topology, and Algebra

*R & S: Mathematics is traditionally divided into algebra, analysis, and geometry/topology. It is probably fair to say that your most spectacular results belong to geometry and topology. Can you tell us about your working style and your intuition? Do you think geometrically, so to speak? Is visualization important for you?*

**Milnor:** Very important! I definitely have a visual mind, so it's very hard for me to carry on a mathematical conversation without seeing anything written down.

*R & S: On the other hand, it seems to be a general feature, at least when you move into higher-dimensional topology, that real understanding arises when you find a suitable algebraic framework which allows you to formulate what you are thinking about.*

**Milnor:** We often think by analogies. We have pictures in small dimensions and must try to decide how much of the picture remains accurate in higher dimensions and how much has to change. This visualization is very different from just manipulating a string of symbols.

*R & S: Certainly, you have worked very hard on algebraic aspects of topology and also algebraic questions on their own. While you developed manifold theory, you wrote, at the same time, papers on Steenrod algebras, Hopf algebras, and so on. It seems to us that you have an algebraic mind as well?*

**Milnor:** One thing leads to another. If the answer to a purely topological problem clearly requires algebra, then you are forced to learn some algebra. An example: in the study of manifolds one of the essential invariants—perhaps first studied by Henry Whitehead—was the quadratic form of a four-dimensional manifold, or more generally a $4k$-dimensional manifold. Trying to understand this, I had to look up the research on quadratic forms. I found this very difficult until I found a beautiful exposition by Jean-Pierre Serre which provided exactly

what was needed. I then discovered that the theory of quadratic forms is an exciting field on its own. So just by following my nose, doing what came next, I started studying properties of quadratic forms. In these years, topological $K$-theory was also developed, for example by Michael Atiyah, and was very exciting. There were beginnings of algebraic analogs. Grothen-dieck was one of the first. Hyman Bass developed a theory of algebraic $K$-theory, and I pursued that a bit further and discovered that there were relations between the theory of quadratic forms and algebraic $K$-theory. John Tate was very useful at that point, helping me work out how these things corresponded.

*R & S: John Tate was last year's Abel Prize winner, by the way.*

**Milnor:** I made a very lucky guess at that point, conjecturing a general relationship between algebraic $K$-theory, quadratic forms, and Galois cohomology. I had very limited evidence for this, but it turned out to be true and much later was proved by Vladimir Voevodsky. It's very easy to make guesses, but it feels very good when they turn out to be correct.

*R & S: That's only one of the quite famous Milnor conjectures.*

**Milnor:** Well, I also had conjectures that turned out to be false.

*R & S: Algebraic K-theory is a topic you already mentioned, and we guess your interest in that came through Whitehead groups and Whitehead torsion related to $K_1$.*

**Milnor:** That is certainly true.

*R & S: It is quite obvious that this is instrumental in the theory of nonsimply connected manifolds through the s-cobordism theorem. That must have aroused your interest in general algebraic K-theory where you invented what is today called Milnor K-theory. Dan Quillen then came up with a competing or different version with a topological underpinning….*

**Milnor:** Topological $K$-theory worked in all dimensions, using Bott periodicity properties, so it seemed there should be a corresponding algebraic theory. Hyman Bass had worked out a complete theory for $K_0$ and $K_1$, and I found an algebraic version of $K_2$. Quillen, who died recently after a long illness, provided a satisfactory theory of $K_n$ for all values of $n$. Quillen's $K_2$ was naturally isomorphic to my $K_2$, although our motivations and expositions were different. I did construct a rather ad hoc definition for the higher $K_n$. This was in no sense a substitute for the Quillen $K$-theory. However, it did turn out to be very useful for certain problems, so it has kept a separate identity.

*R & S: Giving rise to motivic cohomology, right?*

**Milnor:** Yes, but only in the sense that Voevodsky developed motivic cohomology in the process of proving conjectures which I had posed.

*R & S: You introduced the concept of the growth function for a finitely presented group in a paper from 1968. Then you proved that the fundamental group of a negatively curved Riemannian manifold has exponential growth. This paved the way for a spectacular*

development in modern geometric group theory and eventually led to Gromov's hyperbolic group theory. Gromov, by the way, received the Abel Prize two years ago. Could you tell us why you found this concept so important?

**Milnor:** I have been very much interested in the relation between the topology and the geometry of a manifold. Some classical theorems were well known. For example, Preismann had proved that if the curvature of a complete manifold is strictly negative, then any Abelian subgroup of the fundamental group must be cyclic. The growth function seemed to be a simple property of groups which would reflect the geometry in the fundamental group. I wasn't the first to notice this. Albert Schwarz in Russia had done some similar work before me, but I was perhaps better known and got much more publicity for the concept.

I can bring in another former Abel Prize winner Jacques Tits, who proved what is now called the "Tits alternative" for finitely generated subgroups of algebraic groups. He proved that either there was a free subgroup or the group was virtually solvable. All the finitely generated groups I was able to construct had this property: either they contained a noncyclic free subgroup or else they contained a solvable subgroup of finite index. Such groups always have either polynomial growth or exponential growth. The problem of groups of intermediate growth remained unsolved for many years until Grigorchuk in Russia found examples of groups that had less than exponential growth but more than polynomial growth. It is always nice to ask interesting questions and find that people have interesting answers.

## Dynamics

*R & S: We jump in time to the last thirty years in which you have worked extensively on real and complex dynamics. Roughly speaking, this is the study of iterates of a continuous or holomorphic function and the associated orbits and stability behavior. We are very curious to hear why you got interested in this area of mathematics.*

**Milnor:** I first got interested under the influence of Bill Thurston, who himself got interested from the work of Robert May in mathematical ecology. Consider an isolated population of insects where the numbers may vary from year to year. If there get to be too many of these insects, then they use up their resources and start to die off, but if there are very few, they will grow exponentially. So the curve which describes next year's population as a function of this year's will have positive slope if the population is small and negative slope if the population gets too big. This led to the study of dynamical properties of such "unimodal" functions. When you look at one year after another, you get a very chaotic looking set of population data. Bill Thurston had gotten very interested in this

problem and explained some of his ideas to me. As frequently happened in my interactions with Bill, I first was very dubious and found it difficult to believe what he was telling me. He had a hard time convincing me, but finally we wrote a paper together explaining it.

*R & S: This was a seminal paper. The first version of this paper dates from around 1977. The manuscript circulated for many years before it was published in the* Springer Lecture Notes *in 1988. You introduced a new basic invariant that you called the "kneading matrix" and the associated "kneading determinant". You proved a marvelous theorem connecting the kneading determinant with the zeta function associated to the map, which counts the periodic orbits. Browsing through the paper, it seems to us that it must have been a delight to write it. Your enthusiasm shines through!*

**Milnor:** You said that the zeta function describes periodic orbits, which is true, but it omits a great deal of history. Zeta functions were first made famous by Riemann's zeta function (actually first studied by Euler). Zeta functions are important in number theory, but then people studying dynamics found that the same mathematical formalism was very useful for counting periodic orbits. The catalyst was André Weil, who studied an analog of the Riemann zeta function for curves over a finite field, constructed by counting periodic orbits of the Frobenius involution.

So there is a continuous history here from pure number theory, starting with Euler and Riemann and then André Weil, to problems in dynamics in which one studies iterated mappings and counts how many periodic orbits there are. This is typical of something that makes mathematicians very happy: techniques that are invented in one subject turn out to be useful in a completely different subject.

*R & S: You must have been surprised that the study of a continuous map from an interval into itself would lead to such deep results?*

**Milnor:** Well, it was certainly a very enjoyable subject.

*R & S: Your work with Bill Thurston has been compared to Poincaré's work on circle diffeomorphisms 100 years earlier which led to the qualitative theory of dynamical systems and had a tremendous impact on the subject.*

## Use of Computers in Mathematics

*R & S: This leads to another question. There is a journal called* Experimental Mathematics. *The first volume appeared in 1992 and the first article was written by you. It dealt with iterates of a cubic polynomial. The article included quite a lot of computer graphics. You later published several papers in this journal. What is your view on computers in mathematics?*



Abel interview, from left to right: Martin Raussen, Christian Skau, and John Milnor.

**Milnor:** I was fascinated by computers from the very beginning. At first one had to work with horrible punch cards. It was a great pain, but it has gotten easier and easier. Actually, the biggest impact of computers in mathematics has been just to make it easier to prepare manuscripts. I always have had a habit of rewriting over and over, so in the early days I drove the poor secretaries crazy. I would hand in messy longhand manuscripts. They would present a beautiful typescript. I would cross out this, change that, and so on. It was very hard on them. It has been so much easier since one can edit manuscripts on the computer.

Of course, computers also make it much easier to carry out numerical experiments. Such experiments are nothing new. Gauss carried out many numerical experiments, but it was very difficult at his time. Now it's so much easier. In particular, in studying a difficult dynamical system it can be very helpful to run the system (or perhaps a simplified model of it) on a computer. Hopefully this will yield an accurate result. But it is dangerous. It is very hard to be sure that round-off errors by the computer, or other computing errors, haven't produced a result which is not at all accurate. It becomes a kind of art to understand what the computer can do and what the limitations are, but it is enormously helpful. You can get an idea quickly of what you can expect from a dynamical system and then try to prove something about it using the computer result as an indication of what to expect. At least, that's in the best case. There's also the other case where all you can do is to obtain the computer results and hope that they are accurate.

*R & S: In a sense, this mathematical discipline resembles what the physicists do when they plan their experiments and when they draw conclusions from the results of their experiments….*

**Milnor:** There is also the intermediate stage of a computer-assisted proof where (at least if you believe there are no mistakes in the computer program or no faults in the hardware) you have a complete proof.

But the assumption that there are no mistakes is a very important one. Enrico Bombieri had an experience with this. He was using a fancy new high-speed computer to make experiments in number theory. He found that in some cases the result just seemed wrong. He traced it back and traced it back and finally found that there was a wiring mistake in the hardware!

*R & S: Do you have examples from your own experience where all experiments you have performed indicate that a certain conjecture must be true but you don't have a way to prove it in the end?*

**Milnor:** In my experience, computer experiments seldom indicate that something is definitely true. They often show only that any possible exception is very hard to find. If you verify a number-theoretical property for numbers less than $10^{10}$, who knows what would happen for $10^{11}$. In dynamics, there may be examples where the behavior changes very much as we go to higher dimensions. There is a fundamental dogma in dynamics, saying that we are not interested in events which happen with probability zero. But perhaps something happens with probability $10^{-10}$. In that case, you will never see it on a computer.

## Textbooks and Expository Articles

*R & S: You have written several textbooks which are legendary in the sense that they are lucid and lead the reader quickly to the point, seemingly in the shortest possible way. The topics of your books deal with differential topology, algebraic K-theory, characteristic classes, quadratic forms, and holomorphic dynamics. Your books are certainly enjoyable reading. Do you have a particular philosophy when you write mathematical textbooks?*

**Milnor:** I think most textbooks I have written have arisen because I have tried to understand a subject. I mentioned before that I have a very visual memory and the only way I can be convinced that I understand something is to write it down clearly enough so that I can really understand it. I think the clarity of writing, to the extent it exists, is because I am a slow learner and have to write down many details to be sure that I'm right and then keep revising until the argument is clear.

*R & S: Apart from your textbooks and your research contributions, you have written many superb expository and survey articles which are a delight to read for every mathematician, expert or nonexpert.*

*Two questions come to mind. Do you enjoy writing articles of a historical survey type? You certainly have a knack for it. Do you think it is important that articles and books on mathematics of a popular and general nature are written by prominent mathematicians like yourself?*

**Milnor:** The answer to your first question is certainly yes. Mathematics has a rich and interesting history. The answer to the second question is surely no. I don't care who writes an article or a book. The issue is: is it clearly written, correct, and useful.

*R & S: Are you interested in the history of mathematics also—following how ideas develop?*

**Milnor:** I certainly enjoy trying to track down just when and how the ideas that I work with originated. This is, of course, a very special kind of history, which may concentrate on obscure ideas which turned out to be important, while ignoring ideas which seemed much more important at the time. History to most scientists is the history of the ideas that worked. One tends to be rather bored by ideas that didn't work. A more complete history would describe how ideas develop and would be interested in the false leads also. In this sense, the history I would write is very biased, trying to find out where the important ideas we have today came from—who first discovered them. I find *that* an interesting subject. It can be very difficult to understand old papers because terminology changes. For example, if an article written 100 years ago describes a function as being "regular", it is hard to find out precisely what this means. It is always important to have definitions which are clearly written down so that, even if the terminology does change, people can still understand what you were saying.

*R & S: Is it also important to communicate that to a wider mathematics audience?*

**Milnor:** It is important to communicate to a wide audience what mathematics is and does. However, my own expositions have always been directed to readers who already have a strong interest in mathematics. In practice, I tend to write about what interests me, in the hope that others will also be interested.

## Academic Work Places

*R & S: You started your career at Princeton University and you were on the staff for many years. After some intermediate stages in Los Angeles and at MIT, you went back to Princeton but now to the Institute for Advanced Study. Can you compare the Institute and the university and the connections between them?*

**Milnor:** They are alike in some ways. They have close connections; people go back and forth all the time. The big difference is that at the university you have continual contact with students, both in teaching and with the graduate students, and there is a fair amount of continuity since the students stay around, at least for a few years. The Institute is much more peaceful, with more opportunity for work and more idyllic circumstances, but there is a continually rotating population, so almost before you get to know people, the year is over and they move on. So it's unsatisfactory in that way. But they are both wonderful institutions and I was very happy at both.

*R & S: In the late 1980s you left for Stony Brook, to the State University of New York, where you got*

*in contact with students again, as an academic teacher.*

**Milnor:** Yes, that was certainly one strong motivation. I felt that the Institute was a wonderful place to spend some years, but for me it was, perhaps, not a good place to spend my life. I was too isolated, in a way. I think the contact with young people and students and having more continuity was important to me, so I was happy to find a good position in Stony Brook.

There were also domestic reasons: my wife was at Stony Brook and commuting back and forth, which worked very well until our son got old enough to talk. Then he started complaining loudly about it.

*R & S: A colleague of mine and I had an interview with Atle Selberg in Princeton in 2005. He told us, incidentally, that he thought Milnor would never move from the Institute because his office was so messy that just to clean it up would take a tremendous effort. But you moved in the end….*

**Milnor:** I don't know if the office ever got cleaned up. I think it was moved into boxes and stored in our garage.

## Development of Mathematics

*R & S: Are there any mathematicians that you have met personally during your lifetime who have made a special, deep impression on you?*

**Milnor:** There are many, of course. There were certainly the professors at Princeton. Ralph Fox, Norman Steenrod, and Emil Artin all made a strong impression on me. Henry Whitehead, I remember, invited a group of very young topologists to Oxford. This was a wonderful experience for me when I was young. I mentioned René Thom. More recently Adrien Douady was a very important influence. He was an amazing person, always full of life and willing to talk about any mathematical subject. If you had a question and emailed him, you would always get an answer back within a day or so. These are the names that occur most prominently to me.

*R & S: When we observe mathematics as a whole, it has changed during your lifetime. Mathematics has periods in which internal development is predominant and other periods where a lot of momentum comes more from other disciplines, like physics. What period are we in currently? What influences from the outside are important now and how would you judge future developments?*

**Milnor:** I think the big mystery is how the relation between mathematics and biology will develop.

*R & S: You mentioned ecology as an example.*

**Milnor:** Yes, but that was a discussion of a very simplified mathematical model. It's clear that most biological problems are so complex that you can never make a total mathematical model. This is part of the general problem in applied mathematics; most things that occur in the real world are very complicated. The art is to realize what the essential variables are, in order to construct a simplified model that can still say something about the actual more complex situation. There has recently been tremendous success in the understanding of large data sets (also in statistical analysis). This is not a kind of mathematics I have ever done but, nevertheless, it's very important. The question of what kind of mathematics will be useful in biology is still up in the air, I think.

## Work Style

*R & S: You have proved many results that are described as breakthroughs by mathematicians all around. May we ask you to recall some of the instances when an idea struck you that all of a sudden solved a problem you had been working on? Did that rather occur when you had been working on it very intensely or did it often happen in a relaxed atmosphere?*

**Milnor:** Here is one scenario. After a lot of studying and worrying about a question, one night you go to sleep wondering what the answer is. When you wake up in the morning, you know the answer. That really can happen. The other more common possibility is that you sit at the desk working and finally something works out. Mathematical conversations are definitely very important. Talking to people, reading other people's work, and getting suggestions are usually very essential.

*R & S: Talking, very often, makes ideas more clear.*

**Milnor:** Yes, in both directions. If you are explaining something to someone else, it helps you understand it better. And certainly, if someone is explaining something to you, it can be very important.

*R & S: Is the way you do mathematics today any different from how you did mathematics when you were thirty or forty?*

**Milnor:** Probably, yes.

*R & S: How many hours per day do you work on mathematics?*

**Milnor:** I don't know. I work a few hours in the morning, take a nap, and then work a few hours in the afternoon. But it varies. When I was younger I probably worked longer hours.

*R & S: Do you subscribe to Hardy when he said that mathematics is a young man's game? You seem to be a counterexample!*

**Milnor:** What can you say? Whatever age, do the best you can!

*R & S: In an article around fifteen years ago, you described several areas in mathematics that you first had judged as of minor interest but which later on turned out to be fundamental in solving problems that you had been working on yourself. I think Michael Freedman's work was one of the examples you mentioned. Do you have more examples and is there a general moral?*

**Milnor:** I think that one of the joys about mathematics is that it doesn't take an enormous grant and an enormous machine to carry it out. One person working alone can still make a big contribution. There are many

possible approaches to most questions, so I think it's a big mistake to have everything concentrated in a few areas. The idea of having many people working independently is actually very useful because it may be that the good idea comes from a totally unexpected direction. This has happened often. I am very much of the opinion that mathematics should not be directed from above. People must be able to follow their own ideas.

*R & S: This leads to a natural question: what is mathematics to you? What is the best part of being a mathematician?*

**Milnor:** It is trying to understand things, trying to explain them to yourself and to others, to interchange ideas and watch how other people develop new ideas. There is so much going on that no one person can understand all of it, but you can admire other people's work even if you don't follow it in detail. I find it an exciting world to be in.

*R & S: What's the worst part of being a mathematician, if there is any? Is competition part of it?*

**Milnor:** Competition can be very unpleasant if there are several people fighting for the same goal, especially if they don't like each other. If the pressure is too great and if the reward for being the successful one is too large, it distorts the situation. I think, in general, most mathematicians have a fair attitude. If two different groups produce more or less the same results at more or less the same time, one gives credit to everyone. I think it's unfortunate to put too much emphasis on priority. On the other hand, if one person gets an idea and other people claim credit for it, that becomes very unpleasant. I think the situation in mathematics is much milder than in other fields, like biology, where competition seems to be much more ferocious.

*R & S: Do you have the same interest in mathematics now as you had when you were young?*

**Milnor:** I think so, yes.

## Prizes

*R & S: You received the Fields Medal back in 1962, particularly for your work on manifolds. This happened in Stockholm at the International Congress and you were only thirty-one years old. The Fields Medal is the most important prize given to mathematicians, at least to those under the age of forty. The Abel Prize is relatively new and allows us to honor mathematicians regardless of age. Receiving the Fields Medal almost fifty years ago, do you remember what you felt at the time? How did receiving the Fields Medal influence your academic career?*

**Milnor:** Well, as you say, it was very important. It was a recognition and I was certainly honored by it. It was a marvelous experience going to Stockholm and receiving it. The primary motive is to understand mathematics and to work out ideas. It's gratifying to receive such honors, but I am not sure it had a direct effect.

*R & S: Did you feel any extra pressure when you wrote papers after you received the Fields Medal?*

**Milnor:** No, I think I continued more or less as before.

*R & S: You have won a lot of prizes throughout your career: the Fields Medal, the Wolf Prize, and the three Steele Prizes given by the American Mathematical Society. And now you will receive the Abel Prize. What do you feel about getting this prize on top of all the other distinctions you have gotten already?*

**Milnor:** It is surely the most important one. It is always nice to be recognized for what you have done, but this is an especially gratifying occasion.

*R & S: What do you generally feel about prizes to scientists as a means of raising public awareness?*

**Milnor:** It is certainly very successful at that. I'm not sure I like getting so much attention, but it doesn't do me much harm. If this is a way of bringing attention to mathematics, I'm all in favor. The danger of large prizes is that they will lead to the situations I described in biology. The competition can become so intense that it becomes poisonous, but I hope that will never happen in mathematics.

## Personal Interests

*R & S: Having talked about mathematics all the time, may we finish this interview by asking about other things you are interested in: your hobbies, etc?*

**Milnor:** I suppose I like to relax by reading science fiction or other silly novels. I certainly used to love mountain climbing, although I was never an expert. I have also enjoyed skiing. Again I was not an expert, but it was something I enjoyed doing…. I didn't manage it this winter but I hope I will be able to take up skiing again.

*R & S: What about literature or music?*

**Milnor:** I enjoy music but I don't have a refined musical ear or a talent for it. I certainly enjoy reading, although, as I said, I tend to read nonserious things for relaxation more than trying to read serious things. I find that working on mathematics is hard enough without trying to be an expert in everything else.

*R & S: We would like to thank you very much for this most interesting interview. This is, of course, on the behalf of the two of us but also on behalf of the Danish, Norwegian, and European Mathematical Societies. Thank you very much!*