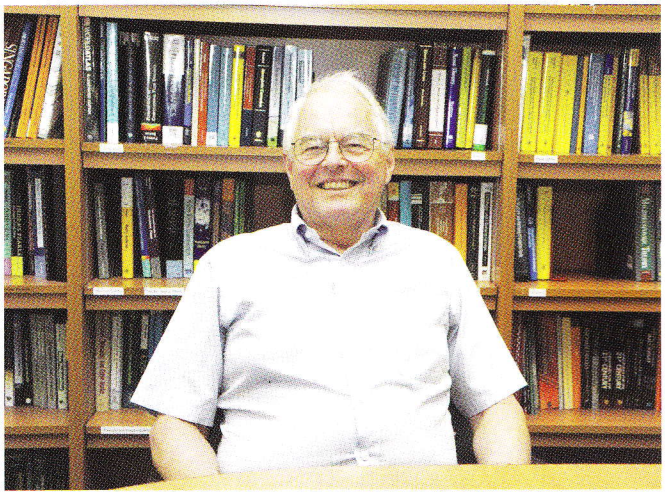
# Ralph T. Rockafellar: Convexity, Optimization, Risk >>>



Ralph T. Rockafellar

Interview of Ralph Tyrrell Rockafellar by Y.K. Leong

Ralph Tyrrell Rockafellar made pioneering and significant contributions to convex analysis, variational analysis, risk theory and optimization, both deterministic and stochastic.

Rockafellar had his undergraduate education and PhD from Harvard University with a one-year Fulbright scholarship break at University of Bonn. Except for an initial short stint at University of Texas at Austin, he has taught at the University of Washington at Seattle since 1966. He became professor emeritus there in 2003 and was concurrently appointed as an adjunct research professor at the University of Florida at Gainesville. During his distinguished career, he has been invited to numerous scientific meetings in various parts of Europe and has held positions as visiting professor and researcher in Denmark, France and Austria. During the last decade, however, he has spread his wings of mathematical research and scientific collaboration also to South America, especially Chile and Brazil, and to emerging centers of scientific activities in Japan, China and Taiwan. He is fluent in German and knowledgeable in French and Russian.

He has served on the editorial boards of numerous international journals on applied mathematics, optimization and mathematical finance and continues to do so for at least four major journals. He has contributed organizational services to the Mathematical Programming Society, the International Institute for Applied Systems Analysis, Institut des Sciences Mathématiques (Montreal) and the FONDAP Program in Applied Mathematics in Chile.

His total research output in the form of research papers, scholastic articles and books exceeds 230 in number; his collaborative research is prodigious. His most famous book *Convex Analysis*, published in 1970, is the first book to systematically develop that area in its own right and as a framework for formulating and solving optimization problems in economics and engineering. It is one of the most highly cited books in all of mathematics. (Werner Fenchel (1905-1988) was generously acknowledged as an "honorary co-author" for his pioneering influence on the subject.) In addition, Rockafellar has written five other books, two of them with research partners. His books have been influential in the development of variational analysis, optimal control, mathematical programming and stochastic optimization. In fact, he ranks highly in the Institute of Scientific Information (ISI) list of citation indices.

Rockafellar is the first recipient (together with Michael J.D. Powell) of the Dantzig Prize awarded by the Society for Industrial and Applied Mathematics (SIAM) and the Mathematical Programming Society (MPS) in 1982. He received the John von Neumann Citation from SIAM and MPS in 1992. His scientific contributions have been further recognized by honorary doctorates from universities in the Netherlands, France, Spain and Chile.

In 1965 Rockafellar began a long period of collaboration with Roger J-B Wets. That led eventually to the unified development of a new field which they termed "variational analysis". It extends the concepts and methodology of classical calculus and convex analysis to cover, among other things, broader problems of optimization that require set-valued convergence and generalized differentiation. The resulting monograph *Variational Analysis*, which earned the 1997 Frederick W. Lanchester Prize from the Institute for Operations Research and the Management Sciences (INFORMS), systematically laid out that subject. This was followed in 1999 by INFORMS's award of the John von Neumann Theory Prize to the joint authors.

While he was attending the Third Sino-Japan Optimization Meeting (31 October – 2 November 2005), which was organized by the National University of Singapore and the first such meeting to be held outside China and Japan, that meeting celebrated his 70th birthday. In January 2011, he was invited to NUS's Risk Management Institute, its Department of Decision Sciences and its Institute for



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Mathematical Sciences. He was also an invited speaker at the Institute's Workshop on the Probabilistic Impulse behind Modern Economic Theory, held from 11 to 18 January 2011. On behalf of *Imprints*, Y.K. Leong took the opportunity to interview him on 18 January 2011. The following is an edited and enhanced transcript of a lively interview in which he traces his early years in the United States and Europe and imparts the passion of a trail blazer of a path less trodden. One also sees a less well-known side of him – a spirit of physical adventure that is closely intertwined with the spirit of mathematical exploration.

***Imprints:*** You went to University of Bonn on a Fulbright Scholarship in 1957. What attracted you to Bonn then?

***Ralph Tyrrell Rockafellar:*** That goes back to the early days of my career when I did not know anything except that I was a good student and did well. I had come from a limited background. No member of my family had a college education. In my undergraduate years at Harvard, I already took two of the main sequences in graduate studies of mathematics, in real analysis and algebra. But I didn't know I wanted to be a mathematician. What I wanted was to have a year abroad while making up my mind. The easiest way to do this was to get a Fulbright Scholarship. I had a good chance and did get one, but then had to choose the country to go to. (The university would then be assigned automatically.) I chose Germany because I knew a lot of German, which I had learned by myself as a teenager. (My home city of Milwaukee, Wisconsin, has a strong German background.) I was very interested in languages, perhaps even more than in mathematics. Nevertheless I worked very hard on mathematics that Fulbright year at the University of Bonn, although I hardly went to classes at the university. In those days there were no exams at German universities, only lectures until a final graduation period, but the effort was exceedingly important to me. I learnt in fact that I really wanted to be a professional mathematician.

***I:*** You were in Bonn for a number of years, isn't it?

***R:*** I was in Bonn for just one year. It seemed that right afterwards I would have to go to the army, which I dreaded. It would be two years of service, but three years if you volunteered so as to maybe get into military intelligence and work with languages. I concluded my time in Germany, being reconciled to going to the army in some way or other, instead of proceeding with education. However, Sputnik then went up and everything changed. I found out that I could be deferred from the military once more. It was already too late to apply to Harvard for graduate studies, so I spent a year in my hometown, Milwaukee, teaching at Marquette University. There I learnt from a statistician friend, Joseph Talacko, about optimization, which was a new field, and that was extremely formative for me. For the following year opportunities came up to go either to Harvard or to Princeton. I decided on Harvard because I knew the place well, had friends there, and enjoyed the cultural life in Boston. (Princeton is in a small town more than an hour south of New York City.)

***I:*** You took your PhD at Harvard University. What was the topic of your PhD thesis and who was your thesis advisor?

***R:*** The topic was in optimization, which as a subject was only about 8 years old at that time and totally unrepresented at Harvard. Before writing a thesis I had to take the standard graduate courses along with various electives and had to pass the required comprehensive exams for a PhD. Then, since there was nobody in the mathematics department who had any idea about optimization, I basically had to do my research on my own. My designated advisor was Garrett Birkhoff [(1911-1996)], who was a specialist in lattice theory and differential equations but knew nothing about the topics I was exploring. I anyway completed a thesis in optimization and got it approved with the help of kind words from [Albert W.] Tucker [(1905-1995)] at Princeton, who had heard about my interests from Talacko at Marquette.

***I:*** Did you pick up the research problem yourself?

***R:*** Exactly, exactly. In the year before I had gone on to graduate studies, but had heard about optimization at Marquette, I learned that it had a phenomenon called duality. Mathematicians are generally familiar with some aspects of this, such as dual vector spaces, but this was a new and intriguingly different kind of duality. I was told it was well understood for "linear" problems of optimization but nobody knew how to extend it to "nonlinear" problems of optimization. That got me fired up and put me on my own track. I worked on it by myself even during the first two years back at Harvard, devoted to courses and exams. But later I got some help from the outside, not in research but in support from Tucker at Princeton, who was one of the

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founders of optimization. It was he who had arranged that I could have pursued graduate studies at Princeton instead. He was able to tell my advisor sometimes: "He's okay. What this young man is doing is good. Don't worry." My advisor himself did not know how to deal with me except always to say "Work harder, work longer." That's how I was able to finish. And a couple of years after I got out, Tucker invited me to be a visiting professor at Princeton, which was very important to my career.

***I:*** How did you know him [Tucker]?

***R:*** The statistician I knew back in my hometown, Talacko, had contacted him at an early meeting in optimization in the year before I returned to Harvard [1959]. He told him about me and that got the relationship started. Tucker was shown some of the duality work I was doing, and he was impressed. It's very good, by the way, to see professors who encourage young people, especially young people who are trying to cross an academic ocean all by themselves. In those days I didn't even know I wanted to be a professor. To me a professor seemed just like one step above a high school teacher, maybe with more prestige. I knew very little about the life of a professor or the academic world.

***I:*** If I understand it correctly, convex optimization or programming generalizes many different kinds of programming methods. Would that mean that in some sense convex optimization unifies a number of areas and would that also not mean that the most important or central problems in optimization are those in convex optimization?

***R:*** This is a good question because it helps me explain a lot of conceptions that people may have about optimization and programming. The original idea of "programming" was synonymous with optimization. The only way "programming" turned out to be computer programming as we know it now is that "programming" was connected with running or managing a government program on a computer. Early examples, like food distribution programs, very much involved optimization in the sense of finding the best ways to do a job, and computers were essential for that. It was then called computer programming. The word "programming" as a synonym for optimization is going out of fashion, though. In fact, there is an organization called the Mathematical Programming Society which this year changed its name to Mathematical Optimization Society because they found itself more and more uncomfortable with some misunderstanding created increasingly by its original name. "Programming" as optimization referred to a new kind of mathematics which demanded fresh ways of looking at things. I got fascinated by the theory behind it – how to create the needed mathematics. I've always been more of a theory builder. I like the idea that mathematics can organize ideas across disciplines. People in some particular application area may have a very narrow view of what they are doing, but a mathematician can see similarities and analogies and can put together structures that will work for many different practical purposes and at the same time generate deeply interesting mathematical concepts and results.

***I:*** Just to pursue it a bit further, does convex optimization sort of unify different kinds of things?

***R:*** I would like to put it differently. I have to explain what optimization is about. First let me say that in mathematics most people make a distinction between linear and nonlinear things, but in optimization it's between convex and non-convex things. This means that the core entities for which the theory is nicest and computation is the easiest in optimization are those that have properties of convexity, just as in engineering and physics it's the linear things that mostly serve for approximations and computations. But this had to be discovered. Optimization was not a known subject in those days, and its essentials had to be found out. In some way the impetus for that started with computers. Anytime there is a decision and choices have to be made, you want to make a better decision, or in other words, optimize. Once computers came in, people were able to look at problems on an entirely different scale of magnitude. In huge problems of optimization, inequalities are very important. You are not modelling with equations. You have certain ranges in which you can do things – not too much, not too little. You have a large number of these one-sided constraints, but you don't know which ones will ultimately be active. Maybe some of them are superfluous in determining the solution, but there is no way to know that in advance of laborious computations. Optimization draws on many different things, including a new kind of geometry, but relatively little of classical mathematics. The new geometry centers on convexity but carries over then to the treatment of functions as well.

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**I:** You wrote a book on convexity. Was that the first book on convexity?

**R:** It was, at least in combining convex sets with convex functions and in that way with analysis. The title of this book was suggested by Tucker in the year when he invited me to Princeton. I had already done a lot of work on convex sets, convex functions and applications to optimization. He suggested that I give a course on this, and when that was nearly over, with a set of lecture notes, he said, "Write it up as a book." The title "Convex Analysis" was prescient because this marks a transition from geometry to analysis in which profound changes take place. The way we look at functions ordinarily in calculus is through their graphs. Differentiation corresponds to approximations that linearize these graphs. But in much of optimization you shouldn't look at a function this way; you should focus instead on the epigraph – the set of points on and above the graph. Convex functions are characterized by the fact that the epigraph is a convex set. The epigraph may not have a smooth boundary suitable for linearization, but nonetheless there can be convex tangential approximations leading to new forms of differentiation, and so forth. This creates a wholly different outlook on analysis. The convex analysis book is the one people best know me for. I wrote it at the beginning of my thirties, very early in my career, and it put the subject on the map.



**I:** What about a later edition? Did you revise it?

**R:** Never had a chance to really revise it. Sorry about that, but there also didn't seem to be much need. I have a friend at *Mathematical Reviews*. He had access to certain databases that they keep, for example a list of the 100 most cited books, not just the most recent, but of all time. Out of the first 100, he told me – that was a few years ago – it was number 6. In 1997 that book also came out in paperback, still going strong some 27 years after its first printing, and it is probably one of the most enduring books published by Princeton University Press. Why? Because, besides being a unique contribution, it came at the beginning of a subject which was growing enormously with many applications, so it became a key reference for everybody. Later on, my idea was to unite convex analysis with classical analysis in some bigger and grander scheme. I was able finally to do that, along with the help of others, and the larger subject is called "variational analysis".

**I:** It seems that this book is more of a theory book than a methods book.

**R:** That's right. Optimization has an unusual status in mathematics. It really has to stand on three legs. One leg is some kind of basic theory like convex analysis. Another leg is the understanding of the various ways of formulating problems, what are the important things, not important things – in other words, artful mathematical modelling. What are the tools for that? In a new subject, you are obliged to develop new tools. The third leg on which optimization stands is computation. All three interact deeply. Computation is often based on optimality conditions, which come from analysis and especially duality. On the other hand, the models you set up should be ones suitable for finding solutions effectively. The challenges of modeling and computation inspire advances in theory. The trouble with this three-legged field of optimization, however, is that it doesn't fit into a single branch of mathematics, pure, applied or numerical. So it doesn't really have a home *[Laughs]*.

**I:** Or it belongs everywhere …

**R:** Belongs everywhere. If you look at the academic world there are traditions. Chemistry belongs to the school of science, but where is optimization? You find it in engineering schools, mathematics departments, business schools. You find it in all sorts of different locations. It has undergone a sort of random social development in different universities and countries.

**I:** That's very interesting. This book came out before personal computers came in, didn't it?

**R:** You want to go back earlier. Computers came out in the forties with those enormous things like ENIAC (Electronic Numerical Integrator and Computer). People like [John] von Neumann [(1903-1957)] were very much involved in computers. He was a mathematical genius looking at many things – quantum mechanics, mathematical economics, computers and early aspects of optimization. He was at Princeton and basically that's how optimization got going there – his connections with Tucker. Some foundational work in optimization was promoted at Princeton. The thing about computers is that they got involved early on with algorithms for certain classes of optimization problems –

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linear programming – there is something called the "simplex method" invented by George Dantzig [(1914-2005)], which turns out to be extremely efficient for solving many practical problems. These are problems we could never solve by hand. That is basically the spark that made everything grow, in 1949, eight years before I started to be involved.

***I:*** You have been Visiting Professor in quite a number of universities in Europe between 1964 and 1997. Does Europe hold a certain kind of attraction for you?

***R:*** It does from a number of angles. One is that Europe is easy to understand beyond mathematics, even with my limited background growing up. I love to travel. I love languages, and as a graduate student in those days you needed to study European languages. I was fluent in German and I knew enough French to be a visiting professor in France and give lectures in French. I also learned a lot of Russian. I like that side of the world and jumped at the opportunity to spend time there. Another more important aspect of my connection with Europe is that in the United States there was hardly any activity [in convexity] except possibly at Princeton. It was different in Europe. In France the field of convex analysis quickly became very popular. In Russia, too, there was intense interest. Of course, because I was a founder of the subject, I got many invitations related to it to participate in meetings for which financing was available. You could say that this was in contrast to Asia at the time. There were not many things happening in mathematics in Asia in the `60s, certainly not the kind of things I've been connected with. Now I have many chances to go to both Asia and South America. I have connections with University of Chile in Santiago; I go there several times a year. I went to a conference in Brazil last month, and that wasn't the first time. I have strong research connections in that southerly direction and more recently also in the direction of Asia – China, Japan and Taiwan. Part of what we see in Asia is a wave of professors who studied in the United States, Canada and Europe and then came back to their home countries and made mathematics grow. If I may put a footnote somewhere, in the business school here, which I am visiting for two weeks, I have a former student [Jie Sun]. He was originally from mainland China, but had his PhD with me, worked in the United States for some time, and came to Singapore in 1992. He was then the only person here in optimization, but now there are probably 15 to 20 people, just in the business school at the National University of Singapore, who are connected in one way or another to optimization.

***I:*** Are you also interested in economics?

***R:*** From the very beginning I was interested in economics. One of the reasons is that a lot of mathematical economics involves convex sets and convex functions. Another fundamental reason is that a lot of economic ideas involve optimization, for example in maximizing utility or minimizing cost. That connection has continued to grow, and now I'm all the more engaged with economics. As you perhaps know, I was involved with an economics conference here just now. With me at the moment at NUS is a close collaborator [Alejandro Jofré] from Chile.

***I:*** Until your retirement, you were at the University of Washington (Seattle) for 40 years. Is there any particular reason for this attachment?

***R:*** We can start with why I went there in the first place. There were people on the faculty who had worked on convexity of a more geometric kind in a setting of functional analysis, for instance the study of the unit balls associated with norms. That enabled me to be invited there in the first place. Then I found a great working environment – not too many rules, the personal freedom to focus on research I liked, and the possibility of teaching courses on topics under development. Later I was able to set up a broad program of optimization-related courses in a department that supported me well, a department of mathematics where theory was welcome to thrive. I didn't want a job in a business school or an engineering school. I really felt myself to be a mathematician, so this was a wonderful thing. However, an underlying answer to your question about the 40 years is that I love the natural setting of Seattle. It has made me very much of an outdoor adventurer. I have spent many, many years climbing around in the wilderness, hiking in the mountains, fishing in high lakes with my family and friends, kayaking from one island to another, camping, catching crabs. This became an important part of me and the way I related to my family and friends, even students. After I became so tightly bound up with it, there was no substitute ever to be found in another location.



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*I:* Do you still do mountain climbing?

*R:* I still do mountain climbing in the form of off-trail hiking and exploring. I also do kayaking, but there are two kinds of that, like the two kinds of skiing – downhill skiing and cross-country skiing. With kayaking, one kind is white-water kayaking in fast-flowing rivers. That I don't do. What I do is the kind where you paddle in the sea from island to island or down a nice quiet river or around a lake, more or less to explore the shore. You can sometimes stop and camp. On a major island near Seattle, reachable by ferry, I have a second house which is right on a tidal beach, and my kayaks are there, ready to be put in the water whenever adventure calls.

*I:* No wonder you never left Seattle.

*R:* That is the real answer to your question. Fill your life with a lot of enthusiasms, and it will help you to be creative and keep a balance. I am often asked by people about hobbies. I always have my outdoor interests for that and have never pushed them aside. I have always kept them going, and they have helped my productivity despite the time they take. In fact, I have always done things I enjoy and think are important. I suppose I'm as competitive as anybody else and would like imagine being out in front in at least one form of recreation, but I don't even like to play a game of chess. I don't like one-on-one competition of that kind. I was never much into sports as a child, yet I have some physical endurance. I do well in the mountains and I feel I can excel in that scene. Another advantage offered by mountains is that you can escape from too much bother, too much noise, by going out into quiet, inspiring nature. You then get new ideas. In the past, before we depended on computers for writing, I could sit high on a peak for an entire day with a pile of papers, putting together an article.



*I:* Other than algorithmic or computational approaches, have new ideas in algebra, analysis and geometry contributed to fundamental advances in optimization?

*R:* I like this question because I can turn it completely around. I think it suggests that optimization is an area which takes existing mathematics and applies it. Actually it's quite the opposite. Optimization grew out of new demands in mathematical thinking. After all, a lot of mathematics was inspired by the challenges of astronomy, building pyramids, commerce, and the like. In physics everything is modeled by equations, but now in economics and systems management, for example, there are different needs. What I believe is that optimization theory has contributed to a new kind of mathematics, a new kind of analysis. I only wish that people in the pure mathematics departments had more access to knowing about this. A lot of mathematics tends to go on in a closed little world and in-group. You know, all areas of science and mathematics have a social component – revolving around who knows who. People just aren't aware of optimization-inspired developments. A good example of such developments is in my most recent book *Implicit Functions and Solution Mappings*, which came out in 2009, written with a colleague [Asen L. Dontchev] who is a Bulgarian-American, now in Michigan. In the mathematics of the past, the main model was solving a system of equations. Then if you wanted to know how the solution depends on parameters, you were led to the implicit function theorem. But now there are different models, such as problems of optimization or game structures in which several agents compete in optimizing from their own perspectives. How does the solution to the model depend on the model's parameters in such cases? You can't use the classical implicit function theorem. You need a new version for broader kinds of "solution mappings," which assign to a parameter vector the corresponding solution or set of solutions. What can be said about the derivatives of such a mapping? They have to be one-sided generalized derivatives. What does that mean? The book presents the kind of analysis that can serve in such a situation.

*I:* So it seems that optimization actually created new mathematics.

*R:* That is exactly what I believe very strongly. Nowadays there's convexity in statistics and set-based probability theory, along with many other areas. More recently I got into the theory of risk. I started out with a colleague [Stanislav Uryasev] a dozen years ago. He's 20 years younger than I am and he's at the University of Florida.

*I:* Your work is mainly in the theoretical aspects of optimization. Has the computer come up with results or scenarios that are counter-intuitive and not mathematically proved or understood?

*R:* That's not the way computers influence optimization, because we aren't working with classical conjectures

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or anything that resembles that. However, there's an important aspect which, as they say, boggles the mind. It is that optimization deals with incredibly large problems in which you can have millions of variables and millions of inequality constraints. How are you going to solve them? You can only hope to do it with careful attention to structure. That may involve discretization in space or time, or stochastic representation, or a more novel kind of approximation, and we get into territory beyond what people can usually imagine. With advances in computers, what is very important is the feedback from computational methods – what can be done and what can't be done. After all, optimization was originally inspired by broader capabilities in computing. As more computational ability comes up, it's not just that you compute bigger problems. It's that you have new ways of thinking about them, requiring extensive mathematical development.

***I:*** What do you think about quantum computers?

***R:*** We have problems in optimization which are still far beyond our ability to solve. They are problems with enormous numbers of variables and constraints, which arise for example through, discretization as already mentioned, or in stochastic representation. In Monte Carlo methods, say, you can generate approximations through statistical sampling, and an explosion in the dimensionality can occur. But if you have a system evolving in a lot of time periods and each period has such an explosion of stochastic branches through sampling, it's easy to see how fantastically huge a problem of optimizing or managing that system can become. How can you best model it? How can you effectively work with the huge model? So, the level of computing will have a big influence. Quantum computers could really help.

***I:*** What about parallel processing?

***R:*** That helps too. It's also important, but with parallel processing it's not just the ability of the computers to perform many actions simultaneously. How do they communicate with each other? Results have to be combined, and how do influences go back and forth? It's not just a matter of machine technology. It's a matter of understanding the design of the communications that take place. Parallel processing doesn't end the story by itself.

***I:*** I believe that you work on several projects with different people at the same time. What are the areas of application that you are working on now?

***R:*** My current work is in three separate areas basically. One is on the pure mathematical side in the sense of theoretical development such as in the book I told you about *[Implicit Functions and Solution Mappings]*, which is sort of outgrowth of convex analysis and variational analysis. By the way, the book *Variational Analysis* by Roger Wets [and me], written about 12 years ago, was already number 50 in that list of top 100 cited. This was only several years after it came out. So that's one side. Another side of my current work is economic modelling, specifically economic equilibrium in markets. That's mostly what I do in my collaboration in Chile and also now with Wets. The third side is the theory of risk which is focused at the University of Florida, and that is what is propelling a lot of the speaking invitations I get. I got interested in risk because I like to work on topics which have practical applications and at the same time exhibit the beauty of thought that I demand as a mathematician. You don't want to waste time on something that is ad hoc and temporary. You want to think you have found the real essence of a topic so you can put it together usefully for the next generation. The theory of risk is moreover deeply involved with convex analysis. That started in finance. I have long been working in optimization problems where there is uncertainty – you have make decisions in advance of fully knowing the future circumstances they will have to confront - but I discovered that people in finance had some fresh ideas. It became clear to me that these ideas could also be good in engineering, for example in reliability of design. Engineering design has many constraints – like requiring the probability that the bridge may collapse should be less than 1 percent, to give you some kind of indication. It turns out that such probabilistic conditions are very ill behaved mathematically. There are some better ways to look at the issue, and one of my passions at the moment is to try to convey this new way of looking at risk to the engineering community. I was involved with a conference in October in Minneapolis, Minnesota, where all kinds of engineers were talking about risk and uncertainty. This interest has also led to another collaboration, with a civil engineer and optimization specialist [Johannes Royset] who is at the Naval Postgraduate School in Monterey, California.



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***I:*** You are also involved in stochastic analysis, isn't it?

***R:*** That's right. This is another underpinning of work in risk, because optimization with uncertainty gets very much involved with stochastic analysis in the sense that, in making decisions that have consequences in the future, you have to use the available data to construct a good representation of what might happen in the future. Also, on a different plane, there are many, many situations in engineering where you have a statistical database from experimental tests, say, of the properties and behavior of some material according to the mix of ingredients you put into it, but no underlying theory. There is no law, even in physics, that explains the test results fully and can be extrapolated beyond the particular instances tested. What you get into is a lot of interplay between statistical estimation and optimization, and I was talking about that here at the National University [of Singapore]. It leads to new developments in statistics. For example, there is a classical form of regression, least-squares regression. But we found that if we are going to work with some kinds of risk we should do regression with a different measure of error other than a sum of squares. That is what I'm very much involved with. I also gave lectures on it to the Department of Statistics at Heidelberg University in July. I know it's at a late stage in my career, but the pleasure of being in that stage is that you have a broader view. If you are still very interested in a field, you may be able to bring ideas together and see them in a way that other people have not yet noticed. You can try to bring this to people's attention, and it can be a lot of fun when your later career gives you platforms for that.

***I:*** Have you done any consultation work with industry?

***R:*** There were various opportunities, but somehow they didn't really come to fruition until recently. I'm now doing some consulting with Codelco which is a gigantic copper company in Chile, and that's why I'm going back there in March. This has to do with risk, and reliable engineering is behind it as well. To see how all these things fit together, I'll describe briefly. Codelco has copper mines which can involve making a cavity in the mountain as large as 200 meters high from floor to ceiling – imagine, such an enormous cavity. They do this by blasting over many years. Once they have exhausted that cavity they start again at a lower level of the mountain. As they do this, there are micro-earthquakes caused by the blasting and there are cracks, shears, slips and all that. My colleagues in Chile are modelling the geophysics of it, but then risk comes in. First of all, where should they place sensors to monitor all the potentially dangerous activity? What would be the best locations? That is an optimization problem. Another is how to protect the tunnels that have the workers in them at the various levels of the mine. Nothing is perfect; you can never build a tunnel so heavily reinforced that nothing bad would ever happen. Or if you could, maybe you wouldn't have a budget big enough. What is the trade-off? That's where the theory of risk comes in, and that's what my consultation is about. ■