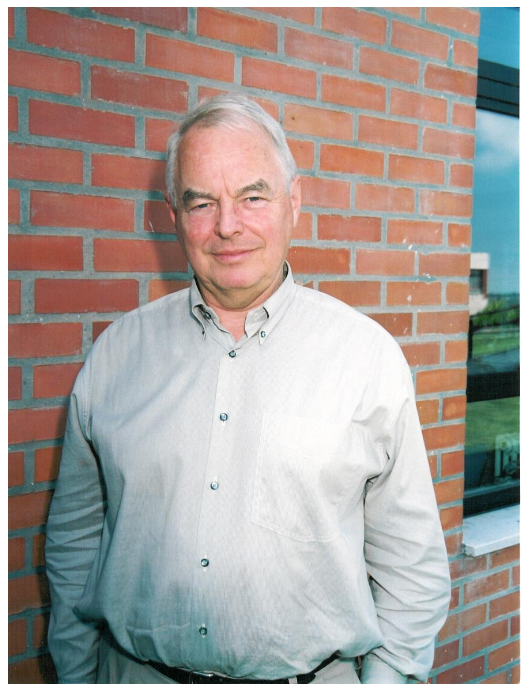
An Interview with with R. Tyrrell Rockafellar

There are obvious reasons for concern about the current excessive scientific specialization and about the uncontrolled breadth of research publication. Do you see a need for increasing coordination of events and publications in the mathematical community (in particular in the optimization community) as a way to improve quality?

There are too many meetings nowadays, even too many in some specialized areas of optimization. This is regrettable, but perhaps self-limiting because of constraints on the time and budgets of participants. In many ways, the huge increase in the number of meetings is a direct consequence of globalization—with more possibilities for travel and communication (e.g. e-mail) than before, and this is somehow good. The real problem, I think, is how to preserve quality under these circumstances. Meetings shouldn't just be touristic opportunities, and generally they aren't, but in some cases this has indeed become the case. I see no hope, however, for a coordinating body to control the situation.

An aspect of meetings that I believe can definitely have a bad effect on the quality of publications is the proliferation of "conference volumes" of collected papers. This isn't a new thing, but has gotten worse. In principle such volumes could be good, but we all know that it's not a good idea to submit a "real" paper to such a volume. In fact I often did that in the past, but it's clear now that such papers are essentially lost to the literature after a few years and unavailable. Of course, the organizers of a conference often feel obliged to produce such a book in order to justify getting the money to support the conference. But for the authors, the need to produce papers for that purpose is definitely a big distraction from their more serious work. Therefore it can have a bad effect on activities that are mathematically more important.

There are also too many journals. This is a difficult matter, but it may also be self-limiting. Many libraries now aren't subscribing to all the available journals. At my own university, for example, we have decided to omit many mathematical journals that we regard as costing much more than they are worth, and this even includes some older journals that are quite well known (I won't name names). And hardly a month goes by without the introduction of yet another journal. Besides the problem of paying for all the journals (isn't this often really a kind of business trick of publishers in which ambitious professors cooperate?), there is the quality problem that there aren't enough researchers to referee the papers that get submitted. Furthermore, one sees that certain fields of research that are perhaps questionable in value and content, start separate journals of their own and thereby escape their critics on the outside. The governments paying for all of it may some day become disillusioned, and that would hurt us all.



R. Tyrrell Rockafellar

Before I ask you questions about yourself and your work, let me pose you another question about research policy. How do you see the importance and impact of research in the professor's teaching activity? Do you consider research as a necessary condition for better university teaching?

Personally, I believe that an active acquaintance with research is important to teaching mathematics on many levels. The nature of the subject being taught, and the kind of research being done, can make a big difference in this, however. Ideally, mathematics should be seen as a thought process, rather than just as a mass of facts to be learned and remembered, which is so often the common view. The thought process uses logic but also abstraction and needs to operate with a clear appreciation of goals, whether coming directly out of applications or for the sake of more complete insights into a central issue.

Even with standard subjects such as calculus, I think it's valuable to communicate the excitement of the ideas and their history, how hard they were to develop and understand properly—which so often reflects difficulties that students have themselves. I don't see how a teacher can do that well without some direct experience in how mathematics continues to grow and affect the world.

On the higher levels, no teacher who does not engage in research can even grasp the expanding knowledge and prepare the next generation to carry it forward. And, practically speaking, without direct contact with top-rate researchers, a young mathematician, no matter how brilliant, is doomed to a scientifically dull life far behind the frontiers.

You started your career in the sixties working intensively in convex analysis. Your book "Convex Analysis", Princeton University Press, 1970, became a landmark in the field. How exciting was that time and how do you see now the impact that the book had in the applied mathematical field?

C. Carathéodory, W. Fenchel, V. L. Klee, J.-J. Moreau, F. A. Valentine,... Who do you really think that set the ground for convex analysis? Werner Fenchel?

Was it A. W. Tucker himself who suggested the name "Convex Analysis"? What are your recollections of Professor Tucker and his influential activity?

Some of the history of "convex analysis" is recounted in the notes at the ends of the first two chapters of my book Variational Analysis, written with Roger Wets. Before the early 1960's, there was plenty of convexity, but almost entirely in geometric form with little that could be called "analysis". The geometry of convex sets had been studied by many excellent mathematicians, e.g. Minkowski, and had become important in functional analysis, specifically in Banach space theory and the study of norms. Convex functions other than norms began to attract much more attention once optimization started up in the early 1950's, and through the economic models that became popular in the same era, involving games, utility functions, and the like. Still, convex functions weren't handled in a way that was significantly different from that of other functions. That only came to be true later.

As a graduate student at Harvard, I got interested in convexity because I was amazed by linear programming duality and wanted to invent a "nonlinear programming duality". That was around 1961. The excitement then came from all the work going on in optimization, as represented in particular by the early volumes of collected papers being put together by Tucker and others at Princeton, and from the beginnings of what later become the sequence of Mathematical Programming Symposia. It didn't come from anything in convexity itself. At that time, I knew of no one else who was really much interested in trying to do "new" things with convexity. Indeed, nobody else at Harvard had much awareness of convexity, not to speak of optimization.

It was while I was writing up my dissertation—focused then on dual problems stated in terms of polar cones—that I came across Fenchel's conjugate convex functions, as described in Karlin's book on game theory. They turned out to be a wonderful vehicle expressing for "nonlinear programming duality", and I adopted them wholeheartedly. Around the time the thesis was nearly finished, I also found out about Moreau's efforts to apply convexity ideas, including duality, to problems in mechanics.

Moreau and I independently in those days at first, but soon in close exchanges with each other, made the crucial changes in outlook which, I believe, created "convex analysis" out of "convexity". For instance, he and I passed from the basic objects in Fenchel's work, which were pairs consisting of a convex set and a finite convex function on that set, to extended-real-valued functions implicitly having "effective domains", for which we moreover introduced set-valued subgradient mappings. Nevertheless, the idea that convex functions ought to be treated geometrically in terms of their epigraphs instead of their graphs was essentially something we had gotten from Fenchel.

Less than a year after completing my thesis, I went to Copenhagen to spend six months at the institute where Fenchel was working. He was no longer engaged then in convexity, so I had no scientific interaction with him in that respect, except that he arranged for Moreau to visit, so that we could talk.

Another year later, I went to Princeton for a whole academic year through an invitation from Tucker. I had kept contact with him as a student, even though I was at Harvard, not Princeton, and had never actually met him. (He had helped to convince my advisor that my research was promising.) He had me teach a course on convex functions, for which I wrote the lecture notes, and he then suggested that those notes be expanded to a book. And yes, it was he who suggested the title, Convex Analysis, thereby inventing the name for the new subject.

So, Tucker had a great effect on me, as he had had on others, such as his students Gale and Kuhn. He himself was not a very serious researcher, but he believed in the importance of the new theories growing out of optimization. With his personal contacts and influence, backed by Princeton's prestige, he acted as a major promoter of such developments, for example by arranging for "Convex Analysis" to be published by Princeton University Press. I wonder how the subject would have turned out if he hadn't moved me and my career in this way.

I think of Klee (a long-time colleague of mine in Seattle, who helped me get a job there), and Valentine (whom I once met but only briefly), as well as Caratheodory, as involved with "convexity" rather than "convex analysis". Their contributions can be seen as primarily geometric.

Since the mid seventies you have been working on stochastic optimization, mainly with Roger Wets. It seems that it took a long while to see stochastic optimization receiving proper attention from the optimization community. Do you agree?

I owe my involvement in stochastic programming to Roger Wets. This was his subject when we first became friends around 1965. He has always been motivated by its many applications, whereas for me the theoretical implications, in particular the ones revolving around, or making use of duality, provided the most intriguing aspects. We have been good partners from that perspective, and the partnership has lasted for a long time.

Stochastic programming has been slow to gain ground among practitioners for several reasons, despite its obvious relevance to numerous problems. For many years, the lack of adequate computing power was a handicap. An equal obstacle, however, has been the extra mental machinery required in treating problems in this area and even in formulating them properly. I have seen that over and over, not just in the optimization community but also in working with engineers and trying to teach the subject to students. A different way of thinking is often needed, and people tend to resist that, or to feel lost and retreat to ground they regard as safer. I'm confident, though, that stochastic programming will increasingly be accepted as an indispensable tool for many purposes.

Your recent book "Variational Analysis", Springer-Verlag, 1998, with Roger Wets, emerges as an overwhelming life-time project. You say in the first paragraph of the Preface: "In this book we aim to present, in a unified framework, a broad spectrum of mathematical theory that has grown in connection with the study of problems of optimization, equilibrium, control, and stability of linear and nonlinear systems. The title Variational Analysis reflects this breadth." How do you feel about the book a few years after its publication? Has the purpose of forming a "coherent branch of analysis" been well digested by the book audience?

That book took over 10 years to write—if one includes the fact that at least twice we decided to start the job from the beginning again, totally reorganizing what we had. In that period I had the feeling of an enormous responsibility, but a joyful burden one even if involved with pain, somewhat like a woman carrying a baby within her and finally giving birth. I am very happy with the book (although it would be nice to have an opportunity to make a few little corrections), and Wets and I have heard many heart-warming comments about it. Also, it has won a prize[1].

Still, I have to confess that I have gone through a bit of "*post partum* depression" since it was finished. It's clear—and we knew it always —that such a massive amount of theory can't be digested very quickly, even by those who could benefit from it the most. Another feature of the situation, equally predictable, is that some of the colleagues who could most readily understand what we have tried to do often have their own philosophies and paradigms to sell. It's discouraging to run into circumstances where developments we were especially proud of, and which we regarded as very helpful and definitive, appear simply to be ignored.

But in all this I have a very long view. We now take for granted that "convex analysis" is a good subject with worthwhile ideas, yet it was not always that way. There was actually a lot of resistance to it in the early days, from individuals who preferred a geometric presentation to one targeting concepts of analysis. Even on the practical plane, it's fair to say that little respect was paid to convex analysis in numerical optimization until around 1990, say. Having seen how ideas that are vital, and sound, can slowly win new converts over many years, I can well dream that the same will happen with variational analysis.

Of course, in the meantime there are many projects to work on, whether directly based on variational analysis or aimed in a different direction, and such matters are keeping me thoroughly busy.

Nonlinear optimization has been also part of your research interests, in particular duality and Lagrange multiplier methods. Nonlinear optimization has been recently enriching its classical methodology with new techniques especially tailored to simulation models that are expensive, ill-posed or that require high performance computing. Would you like to elaborate your thoughts on this new trend?

The growth of numerical methodology based on duality

[1] *Frederick W. Manchester Prize (INFORMS, 1997).*

and new ways of working with, or conceiving of, Lagrange multipliers has been thrilling. Semi-definite programming fits that description, but so too do the many decomposition schemes in large-scale optimization, including optimal control and stochastic programming. Also in this mix, at least as close cousins, are schemes for solving variational inequality problems.

I've been active myself in some of this, but on a more basic level of theory a bigger goal has been to establish a better understanding of how solutions to optimization problems, both of convex and nonconvex types, depend on data parameters. That's essential not only to numerical efficacy and simulation, but also to the stability of mathematical models. I find it to be a tough but fascinating area of research with broad connections to other things. It requires us to look at problems in different ways than in the past, and that's always valuable. Otherwise it won't be possible to bring optimization to the difficult tasks for which it is greatly needed in economics and technology.

Let me now increase my level of curiosity and ask you more personal questions. The George B. Dantzig Prize (SIAM and Mathematical Programming Society, 1982), the The John von Neumann Lecture (SIAM, 1992), and the John von Neumann Theory Prize (INFORMS, 1999) are impressive recognitions. However, it is clear that it is neither recognition nor any other oriented-career goal that keeps you moving on. What makes you so active at your age? Are you addicted to mathematics?

It's the excitement of discovering new properties and relationships—ones having the intellectual beauty that only mathematics seems able to bring—that keeps me going. I never get tired of it. This process builds its own momentum. New flashes of insight stimulate curiosity more and more.

Of course, a mathematician has to be in tune with some of the basics of a mathematical way of life, such as pleasure in spending hours in quiet contemplation, and in dedication to writing projects. But we all know that this somewhat solitary side of mathematical life also brings with it a kind of social life that few people outside of our professional world can even imagine. The frequent travel that's not just tied to a few laboratories, the network of friends and research collaborators in different cities and even different countries, the extended family of former students, and the interactions with current students—what fun, and what an opportunity to explore music, art, nature, and our many other interests. All these features keep me going too.

Recently, at the end of a live radio interview by telephone that was being broadcast nationally in Australia, I was asked whether I really liked mountain hiking and backpacking. The interviewer had seen that about me on a web site and appeared to be incredulous that someone with such outdoor activities could fit her mental picture of a mathematician. So little did she know about the lives we lead!

Have you ever felt that a result of yours was unfairly neglected? Which? Why?

Yes, I have often felt that certain results I had worked very hard to obtain, and which I regarded as deep and important, were neglected. That was the case in the early days and still goes on now. For instance, the duality theorems I developed in the 1960's, connecting duality with perturbations, were ignored for a long time while most people in optimization thought only about "Lagrangian duality". And in the last couple of years, I and several of my students have worked very hard at bringing variational analysis to bear on Hamilton-Jacobi theory, but despite strong theorems can't seem to get attention from the PDE people who work in that subject.

In most cases the trouble has come from the fact that new ideas have been involved which other people didn't have the time or energy to appreciate. That can be an unhappy state of affairs, but time can change it. I've never been seriously bothered by it and have simply operated on the principle that good ideas will come through eventually. This has in fact been my experience.

Anyway, there are always so many other exciting projects to work on that one can't be very distracted by such disappointments, which may after all only be temporary.

What would you like to prove or see proven that is still open?

Oh, this is a hard kind of question for me. I belong to the class of mathematicians who are theory-builders more than problem-solvers. I get my satisfaction from being able to put a subject into a robust new framework which yields many new insights, rather than from cracking a hard nut like Fermat's last theorem. Of course, I spend a lot of time proving a lot of things, but for me the main challenge ultimately is trying to get others to look at something in a different and better way. Of course, that can be frustrating! But, to tie it in with an earlier question, a key part is getting students to follow the desired thought patterns. That's good for them and also for the theoretical progress. Without having been so deeply engaged with teaching for many years, I don't think I could have gone as far with my research.

So, if I would state my own idea of an open challenge, it would be, for instance, on the grand scale of enhancing the appreciation and use of "variational analysis" (by which I don't just mean my book!). I do nonetheless have specific results that I would like to be able to prove in several areas, but they would take much more space to describe.

What was the most gratifying paper you ever wrote? Why?

Oh, again very hard to say. There are so many papers, and so many years have gone by. And I've worked on so many different topics, often in different directions. Anyway, for "gratification" it's hard to beat books. The two books that I'm most proud of are obviously Convex Analysis and Variational Analysis. Both have greatly gratified me both "externally" (recognition) and "internally" (personal feeling of accomplishment). So far, Convex Analysis has been the winner externally, but Variational Analysis is the winner internally.

Interview by Luís Nunes Vicente (University of Coimbra)

R. Tyrrell Rockafellar completed his undergraduate studies at Harvard University in 1957, and his PhD in 1963 at Harvard as well. He has been in the faculty of the Department of Mathematics of the University of Washington since 1966.

His research and teaching interests focus on convex and variational analysis, optimization, and control. He is well known in the field and his contributions can be found in several books and in more than one hundred papers.

Professor Rockafellar gave a plenary lecture in the conference Optimization 2001, held in Aveiro, Portugal, July 23-25, 2001.