The Seventh MTNS Symposium

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1. The conference and the city

The Seventh International Symposium on the Mathematical Theory of Networks and Systems (MTNS) was held in Stockholm, from Monday the 10th to Friday the 14th of June, 1985. The conference sessions took place in the Royal Institute of Technology in the Swedish capital. The renowned institute is located somewhat north of the center of the city, forming a large complex of red brick buildings which offered the MTNS symposium a variety of lecture rooms for the parallel sessions (of which there were often five or six), as well as a simple but effective auditorium for the plenary lectures.

The MTNS conference takes place once every two years. It started out as an informal meeting (now counted as the zeroth MTNS) organized by R. W. Newcomb in 1972 at the University of Maryland, College Park. The first two symposia were in fact held under the heading OTNS (Operator Theory of Networks and Systems), but the name was changed to reflect the importance of various other mathematical disciplines in network and system theory. The MTNS symposium is now the leading conference in a field that might be described as 'Mathematical System Theory' to distinguish it from its larger neighbor, the more applications-oriented area known as 'Systems and Control'. The systems and control field itself is covered by many conferences, such as the ones organized by the IEEE, INRIA, and IFAC.

One of the interesting features of MTNS is that it is a truly international undertaking. There is no particular organization which is responsible for the conference; rather, its continuity is ensured by an international committee of scientists, chaired by Paul A. Fuhrmann of the Ben Gurion University of the Negev in Israel. The site of the conference is different each time, and the preceding years have seen meetings at various locations in the United States, Canada, the Netherlands, and Israel. For 1985, Anders Lindquist of the

Division of Optimization and Systems Theory of the Royal Institute of Technology volunteered to organize the symposium. LINDQUIST did the job together with CHRISTOPHER I. BYRNES, whose home base is Arizona State University at Tempe, Arizona. The Swedish-Arizonan cooperation resulted in a smoothly-running conference.

For myself, it was the first visit to the Swedish capital. Stockholm is a remarkable city, built on a conglomerate of islands, some larger, some smaller. The situation called for a comparison with my home town, Amsterdam, which is also a pretty watery place. I would formulate as a conjecture that the number of bridges is smaller in Stockholm than it is in Amsterdam, but that their total length is larger. On the other hand, I don't hesitate to formulate as a theorem (rigorously proven) that there are more places in Amsterdam than there are in Stockholm where a person can have a beer after a hard day's work.

The seventh MTNS was the largest ever held. In the List of Participants, I counted 285 attendants from 26 countries. The researchers from the United States were most numerous (74), followed by those from host nation Sweden. The third largest party came, and this is no surprise for those who know the MTNS conference, from the Netherlands (25 participants). Groups of ten or more researchers also came from France (18), Italy (18), the United Kingdom (14), Canada (12), Poland (11), Israel (11), and the Federal Republic of Germany (10). The List of Participants also mentions six Belgians, four Chinese, four Japanese, and four Soviet citizens. The large size of the Dutch delegation is an indication of the high level of activity that the small country by the sea maintains in the field of system theory — or, at least, in those aspects of the field that are traditionally emphasized at the MTNS symposia.

2. MAIN LECTURES

There were nine plenary speakers at the symposium, and the organization had put the lecturers simply in alphabetical order. As a result, it was J. ACKERMANN from the Institut für Dynamik der Flugsysteme in Oberpfaffenhofen, West-Germany, who gave the opening talk. He discussed the problem of 'simultaneous stabilization': how to construct a single controller which will stabilize several different systems (say, an airplane under different flight conditions). Ackermann's solution essentially came down to defining a parametrized class of controllers, and using a heuristic scheme to look for a feasible solution. There has also been some theoretical work in this area using heavy mathematical equipment. However, results of BYRNES and GHOSH giving a criterion for a 'generic' class of systems were put aside by ACKERMANN, who noted: 'practical cases are not generic'. What we have to learn from this remarkable statement is, I presume, that one has to be very careful in assuming that sets looking 'fat' or 'thin' from a certain mathematical point of view correspond to situations that 'almost always' or 'almost never' occur in practice.

One of the aspects I like about the system theory field is the fact that there is often a philosophical touch which helps to bear the weight of the technicalities. Several of the plenary lectures exhibited this feature very clearly, and among those the talk given by JAN C. WILLEMS of the University of Groningen. WILLEMS presented some of his recent work under the title 'Modelling, complexity and approximation of linear systems'. This work addresses the field of system identification, which, roughly speaking, deals with the problem of how to obtain a (dynamic) system model from observed data. There are some basic issues here which, according to the speaker, still need a fair amount of clearing up; 'causality structure', 'complexity', and 'approximation' are a few of the keywords in this context. The lecture was followed by a lively discussion, and I think this is welcome in a field where perhaps too often the approach is: select a model class, pick an error criterion, and compute compute compute. This is not to say that one has to stop doing computations, but certainly a complement is needed in terms of a discussion on the basics of the field.

Notwithstanding the alphabetic order of the speakers, WILLEMS was followed by two more main lecturers. W. MURRAY WONHAM of the University of Toronto informed the audience about recent progress in the field of 'discrete event systems', an area in which he himself was the first to initiate a major research program, about five years ago. Discrete event systems, as defined by WONHAM, are intended to describe certain types of decision situations, characterized by a finite number of states which go 'on' or 'off' at irregularly spaced points in time, influenced by internal dynamics as well as by so-called 'supervisory control'. These are rather untraditional objects to look at in a discipline where people are used to work with rather neat differential or difference equations. Nevertheless, it seems that some analogies can be drawn, so that, perhaps, new applications (such as production planning and control) come into reach. It has taken some time before a firm structure developed in this new field, but now there are at least some algorithms which can solve certain types of problems, be it that the amount of computation time needed seems to easily become a hurdle. Recently, there has also been a group at INRIA in France which considers discrete event systems in a setting that is somewhat different from Wonham's. It may well be that here we have a research area of which we will hear a lot more in the future. One can, at least, recognize a trend away from the vector space or manifold structures which have pervaded the system theory field for so long.

As was to be expected, the final plenary lecture was given by GEORGE ZAMES of McGill University. ZAMES shocked his audience slightly by noting, near to the end of his lecture, that he had given almost the same talk already once before, at an IEEE conference in 1976. Indeed, what ZAMES said had a very philosophical touch, and was, therefore, not tied to a specific time and place. The lecture was followed by a heated debate about the relation of π and 22/7, or, what one has to watch out for when using rational approximations for things that are not rational. ZAMES had stated that any useful system representation should be able to incorporate the uncertainty about the 'real', 'physical' system, and had claimed that transfer matrix representations are the only ones that fulfill this criterion; obviously, there were some in the audience who disagreed.

3. Adaptive control

Among the very many subjects that were discussed in the parallel sessions, perhaps most attention was drawn by adaptive control. This is a field which has been very actively explored over quite a few years now, and by a large group of researchers. Roughly, what one tries to do in adaptive control is to define controller structures that include an 'intelligent' (if one may use this word) reaction to a perceived suboptimal behavior of the controlled system, due, perhaps, to incorrect modelling or to drift of the parameters of the system. A precise definition of the problem would be difficult to give, and, in fact, the term 'adaptive control' should rather be understood as denoting a collection of disciplines, all working with the above idea in mind, but in several directions.

One branch tries to attack the problem by splitting it in two: first try to identify the parameters of the system to be controlled, then apply the control action that would be optimal for what you think that the system is. The 'adaptiveness' then comes from the combination of both aspects in one ongoing procedure. This is sometimes called a 'certainty equivalence' approach, because at each step one acts as if one is certain about the controlled system. One of the best-known results in system theory is that this approach is justified for linear systems if the uncertainty appears as additive Gaussian noise and if the cost criterion is of the quadratic integral type (the 'separation principle'). Of course, such a strong result is not expected for other types of uncertainty and other cost criteria, but, under suitable circumstances, the approach may still be feasible. Another direction is given by 'model reference adaptive control', which updates the controller parameters on the basis of the difference between the outputs of the actual system and of a reference model that one is trying to 'follow'. In these schemes, the control structure is still understood as basically consisting of a linear controller which contains, however, parameters that will vary along with the system dynamics. Looking at the control structure as a whole, one sees that this set-up actually defines a nonlinear controller (which, of course, explains much of the difficulty of the field).

So, from a certain point of view, adaptive control simply means that one is trying to find a nonlinear controller which will reach some specified design goal for a linear system containing some unknown parameters, that is, for a class of linear systems. It then becomes a natural question to ask, how big such a class of linear systems can be, in order that there exists a nonlinear controller which will reach a minimal design goal, say, asymptotic stability. Several sufficient conditions for this to happen have been around for a long time, but it was not clear how close these were to being necessary. In 1983,

A. S. MORSE explicitly stated a conjecture in which he formulated the problem in its simplest instance: does there exist a universal controller of the form $\dot{z}(t) = f(z(t), x(t))$, $u(t) = g(z(t), x(t))$, where $z(t)$ is in $\mathbb{R}^m$ (an 'm^{th} order controller') and f and g are differentiable functions, which will stabilize the class of systems of the form $\dot{x}(t) = x(t) + \lambda u(t)$ ($x(t) \in \mathbb{R}, \lambda \in \mathbb{R} \setminus \{0\}$ fixed but unknown)? A solution to this problem had been known for a long time for the case where λ is restricted to either the positive or the negative reals. MORSE conjectured that this condition — the sign of λ must be known — is also necessary, so that the answer to the above question would be negative for λ arbitrarily ranging over $\mathbb{R}$. The conjecture was explained by EDUARDO SONTAG to his colleague ROGER D. NUSSBAUM at Rutgers University. NUSSBAUM, not burdened by many years of study in adaptive control, was quick to show that Morse's conjecture is false, by producing a universal controller of the desired type. He even showed that it is sufficient to use a first-order controller ($m = 1$) and to let f and g be real-analytic functions. By way of consolation, NUSSBAUM also proved that Morse's conjecture is true when f and g are restricted to be polynomials. (The solution was published in *Systems and Control Letters, vol. 3* (1983), pp. 243-246.)

Nussbaum's result indicated that it should be possible to design universal controllers for much larger classes of systems than had been considered before. Research in this direction culminated recently in work by BENGT MÅRTENSSON, who is a Ph. D. student at Lund University in Sweden. The MTNS meeting in Stockholm was the first occasion for MÅRTENSSON to present his results at a major conference. He spoke in one of the parallel sessions. MÅRTENSSON showed how to construct a universal controller for any class of linear systems having the property that there is a uniform bound on the orders of the *linear* controllers that can be used to stabilize any *particular* element from the class. The order of the universal controller can be taken equal to this bound. This result encompasses all previous results on sufficient conditions for stabilizing adaptive control, including, of course, the one by NUSSBAUM.

In addition to the sufficiency result by MÅRTENSSON, CHRIS BYRNES, in his plenary talk, announced a proof of the necessity of the same condition. So, it seems that the problem has been completely solved. However, there are a few points that still call for discussion. In his presentation, MÅRTENSSON emphasized that his universal controller is useless from the applied point of view. The excursions that will take place in the controlled system before stabilization sets in are so large that any practical use is precluded. MÅRTENSSON even succeeded to obtain computer simulation results in the simplest cases, due to overflow problems. So it may be that the algorithm converges only on a cosmological time scale, which, although the requirements of the mathematical problem are still met, does not really represent a solution to the engineering problems that supposedly motivate the study of adaptive control.

Therefore, it remains to be seen what the real conclusion will be from the development that apparently reached its summit at the MTNS meeting. As a corollary to his result (which has now been published in *Systems and Control Letters, vol. 6* (1985), pp. 87-91), MÅRTENSSON showed that if one allows the order of the nonlinear controller to vary with time, then one can construct a universal controller for the whole class of linear systems that can be stabilized at all by a finite-order controller. In a sense, this is a disappointing result since it shows that no interesting conditions come out from the problem of constructing a universal stabilizing controller. Apparently, the requirement of asymptotic stability in itself is too weak to distinguish between systems that are 'easy' or 'difficult' to control; a distinction which, of course, is felt very clearly in practice. So, it appears that something stronger should be looked for, which brings us back (a little wiser, though) to the problem that has bothered the mathematical study of adaptive control all along: how to obtain a sharply defined and not too intractable mathematical question that properly reflects at least some of the aspects that one has to reckon with in actual control applications. Time will tell whether Mårtensson's result marks the beginning or the end of a development.