Recollections of the MTNS

Whats Up?

Margreta Kuijper suggested on the occasion of the 20th meeting of the MTNS we have some effort toward recovering the history of the organization (pardon the oxymoron). This occasion being the first effort along historical lines, appropriate now is gathering data, gathering stories, detecting some of the trends and then having a beer. Later someone can put a balance in these descriptions and distill accurate advice, so others can avoid our mistakes.

Our story starts with Bill Helton’s recollections of the liftoff of the MTNS from nothing to becoming one of the world’s most prominent theoretical systems meeting. Slides from Bill’s talk.\footnote{Beware, many early photos were not shot at the MTNS.} Chris Byrnes’ favorite maxim was “if your only tool is a hammer then every problem is a nail”, from which it follows that operator theory will loom large in Bill’s story and other important subjects will appear to be specs at the periphery.


These notes were assembled by Bill Helton, so the gross distortions you see should be attributed to him. There is a slot in this .tex file for those with their own accounts, so all observers are encouraged to contribute. Please send your tall tales to Bill.
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1. The Meetings

1973 : College Park, Maryland, USA,
1975 : Montreal, Canada,
1977 : Lubbock, Texas, USA,
1979 : Delft, Netherlands,
1981 : Santa Monica, California, USA,
1983 : Beer Sheva, Israel,
1985 : Stockholm, Sweden,
1987 : Phoenix, Arizona, USA,
1989 : Amsterdam, Netherlands,
1991 : Kobe, Japan,
1993 : Regensburg, Germany,
1996 : St. Louis, Missouri, USA,
1998 : Padova, Italy,
2000 : Perpignan, France,
2002 : Notre Dame, Indiana, USA,
2004 : Leuven, Belgium,
2006 : Kyoto, Japan,
2008 : Blacksburg, Virginia, USA,
2010 : Budapest, Hungary
2012 : Melbourne, Australia

Thanks are due to John Dougherty for the assembly of many of these notes.
2. Prehistory

In the beginning was Moshe Livsic who wanted to classify nonselfadjoint operators on Hilbert space up to unitary equivalence. For a restricted class, in 1954 (in Math Sb.) he wrote down an analytic function on the right half plane which was a faithful invariant under unitary transformations. The subject came to be called operator model theory. I. M. Gelfand, who at the time “ran” Soviet analysis, was always proud of having “discovered” Livsic’s work and promoting it. Another motivation for operator model theory which came later was solving the invariant subspace problem for operators on Hilbert space. deBranges (who much later solved the long standing Bieberbach conjecture) for example, published in the Bulletin of the AMS in 1964 a solution but it had a technical flaw. In the 1960s came Nagy and Foias who extended the theory and ideas in several basic directions. This was the most fashionable branch of operator theory through the 1960s. In hindsight we can say that the two main contributions of operator model theory were the model and something called the “commutant lifting theory”.

Also in the mid 1960s Lax and Phillips independently gave a scattering theory which mathematically sat in a projective space of functions (to many an engineer it would look sort of behavioral).

In a parallel universe far far away, Kalman, Youla, and names familiar throughout engineering households invented linear system theory. This I leave to others to describe.

In the early 1970s independently Dewilde, Fuhrmann and Helton discovered that these two universes were mostly the same. Also Arov in Odessa was seeing the correspondence. Figuring all this out led to excitement and much head scratching as to exactly how did the subjects match up and what could you do with the new wisdom.

Also there were two operator theoretic constructions in circuit theory. Each of these had a small following which showed up to the first MTNS, called the Operator Theory of Networks and Systems.

3. THE BIG BANG: OTNS 1973

The first meeting at U. Maryland in 1973 was sponsored by Bob Newcomb of Maryland and Armand Zemanian of SUNY Stony Brook. The meeting lasted one and half or two days and had short talks back to back with an occasional break. This standard engineering format puzzled the mathematicians who didn’t exactly know how to cope. At math conferences there were always breaks between talks. At one point Ralph Phillips raised his hand and ran out apologizing that he needed the restroom.

The MTNS 2012 audience might resonate with the introduction of Bob Newcomb as Brian Anderson’s thesis advisor.

They called the meeting because strong connections between system theory and operator theory had just emerged. Patrick Dewilde had discovered that Lax–Phillips scattering and parts of systems theory were closely related.
Paul Fuhrmann and Bill Helton discovered that Nagy–Foias operator model theory (in finite dimensions) and discrete time lossless systems theory were equivalent. Patrick, Paul and Bill were all Assistant Professors or postdocs.

Arov was far far away, near incommunicado with the west; indeed Brezhnev was in power. However, he and Adamyan (of the MG Krein school in Odessa) had already seen in the mid 1960s that model theory and the Lax–Phillips scattering were equivalent. This was certainly known to Phillips in 1966 since we were trying to figure out the connection in his seminar at Stanford. Also Livsic had a 1966 book giving a systems interpretation of his model theory but he did not seem very knowledgeable about the system theory as we know it. The book was not known in the West until after the bond between operator theory and systems was understood. Then the Livsic book was translated by the AMS in 1973 (at the request of Helton).

Patrick had done a circuits thesis at Stanford under Newcomb. Paul, though a Columbia PhD (under Lee Lorch), had spent some time with Peter Lax at NYU, Bill had learned operator model theory and some scattering theory as a graduate student in numerous seminars at Stanford run by Ralph Phillips. Zemanian his senior colleague at Stony Brook had introduced him to circuits and made the mathematics of them seem understandable and attractive.

Brockett, who was interested in infinite dimensional systems, had brought in Paul Fuhrmann, as a postdoc and put his graduate student, John Baras, on developing a continuous time theory. Jan Willems came down from Boston with Brockett, Baras and Fuhrmann.

Ralph Phillips and Peter Lax came to find out what this engineering was that corresponded to their scattering theory.

Duffin (of Bott–Duffin synthesis) and his students Anderson and Trapp were interested in “parallel sums of matrices” and operators and resistive circuits in parallel were the source of intuition for this.

Nhan Levan had discovered the Halmos dilation and that it corresponded to a theorem of Darlington in circuit theory.

Dick Saeks was interested in a rather general type of causality.

Dolezal from Stony Brook was interested in nonlinear passive circuits.

Ed Kamen came and impressed people as a bright new PhD; I forget what he lectured on, it might have been delay systems.

In all there were about 30–40 people and with the emphasis just describe you can well imagine why it was called OTNS, Operator Theory of Networks and Systems. Of the six senior people I mentioned Duffin was in the National Academy of Sciences, with Lax and Brockett becoming Academy members before too long. So it is fair to say that per capita it was a distinguished group. The younger speakers were no more than a few years past their PhD.

**John Baras in 2012 recalls** that George Zames was at OTNS 1973 and that some of the operator theorists from the math department at Maryland came. He thinks George gave a talk there. Likely so, Bill does not recall. John says he talked to George and mentioned his work involving $H^\infty$ the
Corona Theorem pseudo meromorphic functions etc. George said he had looked at that sort of math and thought it would never go anywhere - it was too hard.

4. THE LIFT OFF

4.1. MONTREAL OTNS 1975. I figured OTNS was a one-off but Newcomb and Zemanian had in mind a conference series, so talked to De Santis who set one up a couple of years later in Montreal, 1975 at Concordia University. Dick Saeks had a lot of energy, talked very fast, was the kind of guy who did not worry much. He got things done. He was an MTNS enthusiast, worked with Nhan Levan, and they did much of the organizational work for all of the early US MTNSs: Lubbock '77, Santa Monica '81 Phoenix 87. By that time MTNS was well established.

I recall that about 60-80 people came. The engineers included Baras, Dewilde, Newcomb, Zemanian, Nhan Levan, Dick Saeks. Zames was there and talked a lot but there’s no record that he formally spoke. There was Bill Helton who fished in Joe Ball and Doug Clarke. The younger parallel summers Anderson and Trapp came back.

New was Pesi Masani, who came looking as if he had stumbled into an oasis. He had been a student of Norbert Wiener and wrote the famous Masani–Wiener paper which proved that one could do spectral factorization (in math circles called Wiener-Hopf factorization) for matrix-valued functions. Wiener originated $H^2$ engineering as opposed to $H^\infty$ through a subject he invented called “Prediction Theory”. What I always found strange was that Wiener doing functional analysis in connection with engineering was an interface between the two subjects which died out. Masani, seemed to me like its lone artifact. There was still research on prediction theory, but it seemed sequestered inside of math departments. Possibly if the Wiener school had set up something like the MTNS, cross fertilization would have continued.

As I recall Doug Clarke had a generalization of the Darlington dilation (I think it was). During his lecture some engineer quite aggressively asked Doug, “Why are you doing this? What are the applications?” Doug was irritated since that was what he came to find out from engineers; he never came to another MTNS. At my talk Zames stood up and asked, “What are the engineering advantages of state space over Input Output formulations?” Goofy to me that an engineer would ask a mathematician, wasn’t that their

\textsuperscript{2} If one looks at the “first??” book written on control theory “Theory of Servomechanisms” — coming out of MIT Radiation labs in 1947, by James, Nichols and Phillips (here is Ralph again) — Wiener’s book is heavily cited. Ralph told me that in practice it was little-used in their lab (which designed fire control radar for ships). What was used all over the place was a book on amplifier design. They adopted these techniques to design their control systems. From Ralph’s description I’m pretty sure this must have been Bode’s famous book. As digressions go, amusing is that the famous Bode Integral Theorem actually is just a rewrite in engineering language of a famous Paley-Wiener Theorem.
job? But I was not upset. Among the mathematicians Joe Ball was a new
PhD and got off light in his talk without being asked to explain the meaning
of life.

4.1.1. The first MTNS Proceedings. Montreal had the first MTNS conference proceedings

OTNS vol 1

Dick Saeks knew a guy with Western Periodicals of North Hollywood California and this fellow was real happy to publish a Proceedings of the OTNS. He figured when the conference caught on libraries would need to stock up;\footnote{When preparing these notes I actually did find a copy of OTNS vol. 2 for sale on Amazon (about 10 bucks) which was a bound University of Michigan library edition.} Dick must have laid it on pretty thick.

I found it all strange, since math conferences then and to this day do not have conference proceedings in the sense of a volume containing all of the talks, which you get when walking in the door. Some math conferences (e.g. IWOTA, and MTNS satellite) have books called conference proceedings, but they are entirely different. I even naively asked at the first steering meeting if we could make the proceedings optional to reduce the reg fee, since MTNS has always been way more expensive than math conferences. This of course was totally impractical (live and learn).

The ONTS 1 proceedings lists 25 papers or abstracts.

Guess who wrote the first MTNS article, volume 1 paper 1? Answer: Brian Anderson and P. J. Moylan.

4.1.2. Birth of the Steering Committee. At the Montreal meeting 1975 Bob Newcomb collared a few people and we went into an empty lecture room. He said he thought OTNS should be an ongoing thing and needed a steering committee. I did not know what one was or why but steering was OK with me. Bob mentioned some other conference he supported and said they had steering committee which worked well. According to the Lubbock conference proceedings the first steering committee was

- C. A. Desoer — University of California, Berkeley
- J. W. Helton — University of California, San Diego
- N. Levan — University of California, Los Angeles
- W. A. Porter — Louisiana State University
- R. Saeks — Texas Tech University
- A. H. Zemanian — State University of New York
- R. W. Newcomb (Chairman) — University of Maryland

My guess is Dewilde met with us, but the official list was all US based.

4.2. LUBBOCK OTNS 1977. The next meeting was at Texas Tech in Lubbock, home of Dick Saeks. The conference proceedings show:

- A “Colloquium talk” by Charlie Desoer of Berkeley

8 plenary talks by
and 25 session talks. Various speakers were

- R. W. Newcomb — University of Maryland
- A. N. Zemanian — SUNY at Stony Brook
- R. M. DeSantis — Ecole Polytechnique de Montreal
- D. Hammers — ITT Gilfillan Inc.
- Zames

and picking up

- Avi Feintuch — Ben Gurion University
- W. M. Wonham — University of Toronto (in the proceedings)
- Clyde Martin — NASA/AMES Research Center

I would guess there were about 70 people.

George Zames had no paper in the proceedings, but is listed in the index. I recall vaguely (with amusement) my negative reaction to his talk as being too diffuse with long passages spent on how the $H_\infty$ norm was wonderful because it was submultiplicative. To his credit he also thought its robustness properties were appealing. Desoer would go out of his way to tell me to pay attention to Zames; he said Zames had done important work (sector nonlinearities) earlier and even though he asked diffuse philosophical questions all the time, Desoer said one should take Zames very seriously. This was valuable advice, since as a young math macho guy I judged nearly everything by its mathematical substance, so it was Desoer who induced me to strain to see what I could learn from George. Now looking back, the Lubbock MTNS lay at the foundation of $H_\infty$ engineering, see §A.2.

4.3. DELFT MTNS 1979. There were over 300 registered participants which was shocking to us Americans. This was the conference which propelled MTNS to being a pillar of the establishment.

This is where many of our European regulars came first, so many that I leave it to others to track how these subjects flowed in and out. Also there was major heft in operator theory. Gohberg had escaped the Ukraine and he, with Kaashoek, Bart, and company, joined in. Foias had just escaped Romania and he came. Harry Dym had started to visit Patrick Dewilde in the late 70s and so Harry was sucked into the MTNS maelstrom, see A.1. Kailath came for the first time and many times thereafter.
Here is a list of all 140 authors in the proceedings. They are inaccurately subdivided by field by Bill who just recognized names and put them in categories. It would be good if some one looked up the long list of Engineers in the proceedings because surely some actually came from math departments.

OPERATOR THEORY - 19
- I. C. Gohberg — University of Tel Aviv
- M. S. Livsic — University of the Negev
- J. W. Helton — University of California, San Diego
- C. Foias — Universite de Paris-Sud
- P. Fuhrmann — University of the Negev
- J. A. Ball — Virginia Polytechnic Institute and State University
- H. Wimmer — University of Wurzburg
- P. Lancaster — University of Calgary
- H. Dym — The Weizmann Institute of Science
- T. Nomuru — Tokyo Institute of Technology
- K. Furuta — Tokyo Institute of Technology
- H. Bart — VU Amsterdam
- F. van Schagen — VU Amsterdam
- Van der Monde — VU Amsterdam
- M. A. Kaashoek — VU Amsterdam
- J. P. Aubin — University of Paris IX
- A. J. Pritchard — University of Warwick
- L. Rodman — The University of Calgary
- C. van der Mee — VU Amsterdam

MATH DEPTS -6
- J. Murray — math — Texas Tech University
- A. Bultheel — math — Katholieke Universiteit Leuven
- M. Hazewinkel — Erasmus University
- L. R. Hunt — math Texas Tech University
- R. Triggiani — math Iowa State University
- A. H. Zemanian — applied math Stony Brook

ENGINEERS and some CS - 114
- Th. Kailath — Stanford University
- Ch. Desoer — University of California, Berkeley
- Y. Genin — M.B.L.E. Res. Laboratory
- T. A. C. M. Claasen — Philips Research Laboratories
- W. F. G. Mechlenbranker — Philips Research Laboratories
- G. P. Barabino — C. N. R.
- G. S. Barabino — C. N. R.
- F. H. Moss — IBM Corp.
- P. M. Merlin — Technion
- T. S. Zemanian — unaffiliated
• O. W. Marcus — Forensic Institute Wiesbaden
• B. Dziurla — Politechnika Gdanska
• R. W. Newcomb — University of Maryland
• E. A. Trachtenberg — University of South Africa
• Ph. Delsarte — Philips Research Laboratory
• Y. Kamp — Philips Research Laboratory
• F. M. Callier — Facultes Universitaire de Namur
• F. van Schagen — VU Amsterdam
• Van der Monde — VU Amsterdam
• M. Fliess — Universite Paris VIII
• D. Aeyels — Rijksuniversiteit Gent
• V. Dolezal — State University of New York at Stony Brook
• A. Blanc-Lapierre — Laboratoire des Signaux et Systemes
• D. Cyrot — Laboratoire des Signaux et Systemes
• M. Vidyasagar — Concordia University
• Y. Schiffmann — University of Dundee
• R. J. Wets — University of Kentucky
• M. Pavon — University of Kentucky
• J. P. Aubin — University of Paris IX
• A. Bensoussan — University of Paris IX
• C. van Putten — Stichting Mathematisch Centrum
• J. H. van Schuppen — Stichting Mathematisch Centrum
• J. Walrand — University of California, Berkeley
• P. Varaiya — University of California, Berkeley
• B. Gopinath — Bell Labs
• P. R. Kumar — University of Maryland, Baltimore County
• D. Q. Mayne — Imperial College of Science and Technology
• M. Clerget — IRIA-LABORIA
• A. S. Morse — Yale University
• M. P. Ekstron — University Erlangen
• M. Sendaula — Youngstown State University
• H. Korezlioglu — Ecole National Superieure des Telecommunications
• A. J. Berkhout — Delft University of Technology
• M. J. Chapman — University of Warwick
• J. S. Baras — University of Maryland
• L. M. Silverman — University of Southern California
• A. J. David — The Johns Hopkins University
• G. G. L. Meyer — The Johns Hopkins University
• N. Levan — University of California, Los Angeles
• W. Rupprecht — Technische Universitat Kaiserslautern
• P. Amstutz — Center National d’Etudes des Telecommunications
• M. J. R. Khan — Aligarh Muslim University
• K. A. Khan — Aligarh Muslim University
• L. S. Czarnecki — Silesian Technical University
4.3.1. Joe Ball’s recollections. So my first MTNS, as Bill said, was Montreal.\textsuperscript{4} I was just a young guy and didn’t know anybody, and there were all these big shots, and I was somewhat intimidated. So then I passed on Lubbock, but then by ’78 I got together with Bill\textsuperscript{5} and there was this community, so in ’79 I went to Delft, and that was very influential for me, because that’s where I came upon Israel Gohberg and Harry Dym and Rien Kaashoek and Lieba Rodman and Andre Ran and all this group. So what impressed me there was the coordinated talks. They had the Gohberg-Lancaster-Rodman book “Matrix Polynomials” just coming out, and we had the “Minimal Factorization of Rational Matrix Functions” by Bart-Gohberg-Kaashoek coming out. So each of these had 3 authors. So we had coordinated sessions where author 1 would come out, and talk about chapter 1, followed by author 2 who’d talk about chapter 2, and author 3 would talk about chapter 3. They were associate authors, so it was very well coordinated: all the talks were on this or that chapter of the book, referred to the previous talk and advertised the next talk, all the while being its own talk and promoting the book from all possible angles. After Israel Gohberg promoted his book one last time, I called out and asked if he had order forms, provoking laughter from everyone else. This was the beginning of the systems theory coming into the work of mathematicians — the state-space method using a realization technique as a method of solving function theory problems. So Delft was very influential in terms of, as Bill said, it was the biggest at the time, and you had a lot of mathematicians coming in and meeting engineers. This was my first contact with a host of important people with whom I had continuing contact (including collaborations) for years afterward.

This was probably the first time I was out of the States on July 4. July 4 in Delft was memorable for large dinner party that by happenstance I wound

\textsuperscript{4}Based on his talk at MTNS2012, 59:40 on the tape.
\textsuperscript{5}Joe visited UCSD for a year — ed.
up being a part of. At the table were Ciprian Foias, Patrick Dewilde, Harry Dym and some others whom I cannot recall. These people provided a lively, cultured, wide-ranging conversation on all kinds of topics (e.g., the origins of the Romanian language). I was meeting most of these people for the first time and I have had continuing contact and engagement with most of them over the many years since. In short, this was a very influential and important meeting for me personally at an early stage in my career.

4.4. SANTA MONICA MTNS 1981. With the Delft conference being spectacular the question was: would this continue? The 1981 conference proceedings show 61 offerings. But the dominant influence was Ronald Reagan, who fired all of the aircraft controllers the week before. MTNS attendance was low, maybe about 70. Two of the plenary speakers, Kalman and Jury, canceled at the last minute because of the air traffic controllers strike. I recall Saeks manfully stepped up to give Kalman’s talk and said, “if you were wondering how to get a large audience, just announce that Kalman is speaking but then do it yourself.”

Arriving at MTNS for the first time were Chris Byrnes and Art Krener, who became MTNS mainstays, with Chris organizing several MTNS meetings in the future; Sweden with Anders Lindquist and St Louis with Clyde Martin, David Gilliam and Biswa Datta. All in all MTNS 1981 was a solid group with many strong young researchers, though it was less dazzling than Delft.

The organizers in 1981 were Levan, Saeks and Helton.

5. Launching a Satellite: IWOTA BEGINS 1981 Santa Monica

My thought was to run a conference in operator theory next to the MTNS. The goal was to broaden the types of mathematicians attending the MTNS. Pure or applied operator theorists could give a talk at IWOTA, that would bring them to the neighborhood and then they would go to the MTNS, despite not feel comfortable talking at the MTNS. They would learn some systems theory from this and maybe find some open questions to work on. I set the meeting, the International Workshop on Operator Theory and Applications, in the same hotel as the MTNS in Santa Monica. It was the first conference I ever organized. Gohberg, Kaashoek, Bart came. They had already become MTNSers at Delft ’79. There was Joe Ball of course and various analysts; for example, Paul Berg, Alice Chang, Robert Gundy, Harold Widom, and Tom Wolf. I recall the attendance being about 40. Joe Ball was visiting UCSD for the year so helped. Alice Chang (UCLA) helped me get people to come.

The idea caught on to a significant extent because Gohberg believed in it and pushed it, so since 1981 there has been a satellite IWOTA conference to every MTNS. Gohberg was our president, and Kaashoek and I were vice presidents, and, yes, we have a steering committee patterned after MTNS. In the early 1990s IWOTA took on another mission. With the walls of the
Soviet Union coming down there was a desire to unite operator theorists from the FSU with the rest of us, and this desire burned very strongly in Israel Gohberg. He knew both sides of the iron curtain and he effectively used IWOTA as one of the main venues for bring the operator family together.

Around 2008 we broadened the vice presidency by adding Joe Ball, Leiba Rodman and Victor Vinnikov. When Israel died Kaashoek became our chief. For example, with MTNS in Melbourne we have IWOTA in Sydney, having about 140 enrolled. In 2010 IWOTA was in Berlin the week after MTNS and had 350 people. Since the year 2000 often meets in odd-numbered years as well. Adding it all up, the 2014 meeting in Amsterdam will be the 25th IWOTA. While the size of meetings vary, of the standing operator theory conferences in the world, IWOTA is often the biggest.

5.1. Joe Ball’s recollections. The first IWOTA was held in 1981 in Santa Monica, California as a satellite to the fourth MTNS (then called OTNS) symposium. Running IWOTA 1981 was made all the more challenging due to President Reagan’s decision to fire the air traffic controllers, causing a growing fear of flying in the summer of 1981 leading to last-minute cancellations and consequent adjustments in the projected program for the meeting. While we did have electricity in those days (ha!), by other contemporary measures technological conditions in those days were quite primitive. I was visiting with Bill Helton in the early summer of 1979 and saw first hand how he was grappling with this situation. I mention two instances of how he handled the technological challenges. (1) As there was no computerized word-processing at this time, everything was written out on paper. The constant shifts in the conference schedule could not be handled via a cut/copy-and-paste procedure as is done today. The solution was manual application of white-out fluid to erase an entry in the schedule table: apply the fluid, blow on it to help it dry, and then write the correction on top of the new surface. (2) There was no e-mail; communication over long distances was either writing a letter, putting in an envelope and trusting it to the post office, or long-distance phone calls. At UCSD in those days, there was required a long prefix to the phone number in order to get the cost of the phone call charged to the appropriate account. Bill ran out of patience dialing (I believe we were still rotary then) this long prefix for every phone call. He therefore took the initiative to go to Radio Shack (or some equivalent) and purchase a device which would automate the dialing of this long prefix. This provided considerable added efficiency in the flurry of phone calls occurring as the conference dates rapidly approached.

6. BEER SHEVA 1983

Paul Fuhrmann organized it with help from others around Israel, especially Avi Feintuch. One can look at Harry Dym’s recollections A.1.

One point is that $H^\infty$ was now becoming the hottest topic in the control community. Since the early $H^\infty$ crowd came to the MTNS it was becoming
cemented for the next decade as trendy. At this meeting was Zames - Helton (by now collaborators). I do not recall if Francis came probably yes.

An engineering counterpart to the operator theorists was the infinite dimensional systems folk. Ruth Curtain came (I think for the first time).

Also the geometric control crowd, e.g. Byrnes, Krener, Isidori were solidly on board now.

Jan Willems came for the first time since the OTNS 0 and thereafter played an active role. This was Victor Vinnikov’s first MTNS. There he spoke when he was 16 years old.

The point is that by this point the MTNS, with at least a half dozen strong constituent groups, was off and running strong.

I recall the conference dinner in a tent in the dessert. Chris Byrnes played an active role in the public discussion, and in planning and steering thereafter.

Goodbye to Hollywood; our publisher became Springer.

A chilling note to those who would like to dredge up history: Zames was not in the proceedings and I talked on something very different (putting rolloff constraints in $H^\infty$) than the proceedings paper with Ball. The moral of the story is that the most accurate records float around in notoriously inaccurate places, namely, peoples heads.

7. SIAG on control: from odd to even

The SIAM activity group (SIAG) in control started in the late 1980s. Art Krener started it. They throw conferences and have a newsletter and small dues. The first meeting was in San Francisco about 1989. It was called something like “Control in the 1990s”. The program committee was Chris Byrnes, Art, Bill Helton, and a couple of others.

A difficulty was conflict with MTNS every 6 years since SIAM met every 3 years and MTNS every 2. At MTNS in Regensburg the MTNS steering committee put MTNS off for 3 years to avoid a 1995 conflict with SIAM. This shifted MTNS from being on odd years to being on even years. This of course was a temporary fix. Which went kaplooie when the SIAG comet came around again in 1998, but neither organization blinked. This did not help the Padova meeting (which still prospered) but it was a disaster for the control SIAG. In 2005, at the business meeting, the SIAG voted to have their conference every other year. The leadership of the SIAG had met that morning and leaned against this change, with some members for and some against. However, at the business meeting open to the full membership (with free wine no less) some wag (OK it was me) suggested from the floor that the conference be held every 2 years. This carried the business meeting with little opposition. So now MTNS meets in even years and the SIAG on control meets in odd years.
8. STOCKHOLM 1985

8.1. Anders Lindquist’s recollections. Now, let me say something first here, that I haven’t really prepared such a nice presentation as Bill, here.\textsuperscript{6} The reason for that is that, if you look at the program, my name is not there, so therefore I didn’t think that I should prepare. But I’m going to just improvise here, you know? And if I forget something, please remind me.

Now, I can tell you a bit: the first meeting I went to was in Delft. Nahn Levan suggested I ought to come to Santa Monica, so I got there despite the fact of the lockout of the air traffic controllers — the firing of them, actually, that Reagan did then after the strike. That was a rather small meeting, then, because of that, so a lot of people fell out. Then there was Beer Sheva, and Beer Sheva also was marred. This was the time of the Lebanon invasion, so a lot of people didn’t come to Israel at that time, and I think it was down to 80; I don’t know if anybody of that time was here. This was the time when they picked me into the steering committee and they asked me to do the thing in Stockholm in ’85, and at that time, also, Chris Byrnes was there, and he offered also to help, so it was I and Chris Byrnes together that organized this meeting.

We had a program like this [Figure 1], and we actually promised that it would be sunny and the best time for weather ever in June in Stockholm, and the program said that Stockholm is beautiful in June. It wasn’t quite as beautiful as we had promised, but there was nothing to do about it. Now we really thought like this: we wanted to make this meeting into a little bit more systems theory oriented than it had been before. There were (I didn’t know Delft had so many) quite a bit over 300 at that time, in Stockholm.

We worked for a long time on this thing, and we even got a member of the royal family to be a patron of the MTNS. We thought of asking the King, but on the other hand if he said “no” then we couldn’t really go to the Vice King, Prince Bertil, and say, “you are our second choice.” So we played the safer game, to take the second man to the throne. So he says “yes,” and we invited him here, and you can see my signature and Chris’s signature there on the invitation [Figure 2]. Now, there were several things here, you know, around this. This is Chris, [we want a picture of Chris] actually at MTNS ’85.

Now, there was one thing where we were sitting there one day and we were thinking, you know, discussing what to do. We had great plans about everything. We were young and foolish in those days; today I’m just old and foolish. You see the thing was we said, “what are we going to have, here, for interesting things?” And my oldest son said, “look, there’ll be the Olympic track team of the United States has Coca-Cola as the sponsor, so whose sponsor do you have?” And we said, “well, it has to be beer,” so I called up, so we got an official beer of MTNS, right? So I was sitting

\textsuperscript{6}Based on his talk at MTNS2012, 39:34 on the tape
MTNS -85
7th International Symposium on the Mathematical Theory of Networks and Systems
Royal Institute of Technology
Stockholm
June 10-14 1985
INVITATION PROGRAM

FIGURE 1. MTNS '85 program
there, and he says “look, there’s this upstart Spendrups company, run by two brothers, I’ll try to call him.” So I got him in those, you know in ’85 you didn’t usually have a telephone in your car, but this guy Spendrup had a phone like this. You try to get him at a red light. So I got this Spendrup
at a red light, I said “look, if you send up as much beer as we can drink to MTNS, we will make you into the official beer of the MTNS ’85.” It was all silent at the other end, and then he says, “can you tell me just one reason why I would do anything like this?” And I said, “look, this is the first time a math meeting has an official beer. And when people see this, they will remember your beer forever.” And it was all silent for a long time, and then he says, “okay, let’s go for it!” So they sent up much more beer than we could drink at the meeting: in fact, for half a year I had beer in my office, under my desk, behind the sofa, and everywhere, everywhere was beer for half a year. Pretty good beer, actually. And then the market share of Spendrups boomed to 25% now, so I think it was MTNS that did that. So that was the official beer.

We also had an official airline, which was SAS. I was young and energetic in those days, so I said I’d try to get some money from the industry. So I started calling up companies and I said, “will you give money?” and they said “no, no, why would we give you money for a conference like this?” And then I succeeded (I don’t know what the trick was) to get two big companies to give us 25 thousand crowns. That was a lot of money in those days. I mean, it wasn’t big for them. Then I could call up and say “these have already given,” and in that way it came in. And we had 20 industrial sponsors. And we used this money in a way I will explain in just a moment.

Now, of course, Chris and I, we thought, “people are going to complain, nobody’s going to be happy here,” so we wanted to pre-empt all of the complaints. So we sat down and we wrote down all of the complaints that people could possibly have. Then we had a long complaint form 1040 here — Americans know what a 1040 is, it’s the regular tax return long form — and the idea is that we would print this. And then, of course, we were silly guys in those days (even in those days), and so what we did was that we put a box where you were supposed to put your complaint form. This box didn’t have a bottom, but it had a wastepaper basket under it. Anyway, we did get some of these complaints in, so let’s look at them.
LONG COMPLAINT FORM (1040)

- The mats arrived too late
- The hotel accommodation is:
  - too expensive
  - not sufficiently lavish
  - other (explain: ________________________ )
- Stockholm is not beautiful in June
- Stockholm is too expensive
- Too few MacDonalds
- I was not a plenary speaker

I had to speak on:
- Monday which I didn't like because ________________________
- Tuesday which I didn't like because ________________________
- Wednesday which I didn't like because ________________________
- Thursday which I didn't like because ________________________
- Friday which I didn't like because ________________________

I had to speak
- before lunch, when everybody was hungry
- after lunch, when everybody was sleepy
- I didn't like my time because ________________________
The registration fee
was too high
included too few extras

The lunches
did not include martinis
were not gourmet
were better at CDC

Too many sessions (in which I was not involved)
Not enough sessions (in which I was involved)
There was no limousine to meet me at the airport

The entertainment was
insufficient
too expensive
taken by somebody else before me

The weather was
too hot
too cold
there wasn't enough snow for skiing

The organization
was too laid-back
was too aggressive
did not include me
other (explain: _________________________)
The talks

- started too early
- ended too late
- I was not given enough time to talk
- Nobody came to my talk
- The chairs were uncomfortable
- The chairs were too comfortable
- No free telephone service

The conference banquet would have been better if held

- at a 2-star restaurant
- at a 2-arch restaurant
- in the cafeteria
- closer to the royal palace

Because this form is processed by an optical scan, additional complaints are limited to 12 letters (this is equivalent to 3 four-letter words)
These had most of the complaints that you could imagine on it. This was just the first page, you know, and this was the second page, I don’t know if you missed any, but this is the way it was, you know? And then we had the end, here, and I remember especially that this was Chris’s idea, the complaint was that it was not in a two-star restaurant or a two-arch restaurant (which was of course McDonald’s), and then we also had these silly things at the end.

Now, I have been thinking a bit about all of this, and when I heard the previous talk here, one thing I would like to talk about are steering committee lunches. The first steering committee lunch that I was invited to was in Beer Sheva, and that was in a kosher Chinese restaurant in Beer Sheva. There was a small group of us. So the second time we thought we’d gotten so much money from these companies that we would have a really nice lunch for the steering group. So we got it down at ??Grashella?? that is one of the fancy restaurants down next to the royal palace. And it has a star in the Michelin. And on the way out we had to add a few people to the steering committee, so there was Israel Gohberg, and we said “Israel, why don’t you join us in the steering committee?” So we took him in, informally in those days, without votes and things like that, since there were fewer people. And we went down there and Israel Gohberg was really really very impressed, he said “it’s good to be a member of the steering committee, this is the thing to be.”

The MTNS came to Amsterdam, and here Gohberg still remembered about the steering committee lunches, and at that time we got (as I told the other day) salad and plastic, and a piece of bread, so he was almost about to resign. So until then we had had the best steering committee lunch so far.

Now, some of the proceedings that Chris and I gave out, there are still quite a bit of references to them. So, there are a lot of good people at this meeting. George Zames was giving a plenary, though I don’t remember if he was in the program.

Somebody mentioned Louis de Branges, and that fits also into this history in a sense, because the first time I met Louis de Branges was precisely at one of these meetings that were satellite meetings (IWOTA). He was also at MTNS in Beer Sheva. So anyway, it all came down to that he wanted to come, and he had some people coming to MTNS ’85, and he wanted to come sometime before and visit. So I invited him to come, I think he was there for a month before the meeting and in the just some maybe something like a month before he came, he wrote to me and he says, “I’ve proved the Bieberbach conjecture.” Now, if you don’t know what the Bieberbach conjecture is, well, it was an unsolved problem in mathematics people hadn’t been able to do, so there was a conjecture concerning it like this. So he proved the Bieberbach conjecture, and I remember that that time I went out to lunch and I met Leonard Karlisov, and I said “oh, Louis de Branges, he has proven the Bieberbach conjecture.” And Leonard said “I don’t believe
it” — Louis de Branges had a lot against him because he claimed many things before that didn’t turn out correct. But, de Branges went to Russia, and in then Leningrad he worked with the Nikolski group, and they went through the proof, and it all came out correct, and so then he went back to the West and the first place — the first place in the West — where Louis de Branges talked about the Bieberbach conjecture was in my seminar in Stockholm. By accident, of course, because I’d already invited him. So that was interesting and he gave a whole series of talks on this; however, in the middle of it he also claimed that he had done the Riemann hypothesis, and that has gone on for some time after that. Also I remember that he was very upset that there was a short proof of the Bieberbach conjecture in —; he had an operator theory proof that he says was systems theory, I remember he was standing there in the halls, you know, and all of these people were there, and he says “this is systems theory.” Which was very good for us, in systems theory. And there’s some point to it, because it was a type of operator theory with analytic functions that we like in the system theory community. Or do we? Did you say “noncommittal”? Okay. So, then there was a proof of it by complex analysis of 12 pages, and Louis was very upset, because now they wouldn’t read all this theory. And of course now it’s down to two pages, maybe.

Okay, um. What else can I say here? Are there any trivia questions? Well, one trivia question: who was giving a plenary talk both at the MTNS ’79 and ’89 — both Delft and Amsterdam? Well, it was Kailath, and it was exactly the same talk, and it was about something that I was quite familiar with, in fact. So, that’s that. Any more trivia question? What does the MTNS steering committee really do? Well, they sure have a nice lunch, that’s for sure. Any questions for this? Because I haven’t really prepared anything, I’m just talking right out of my heart. Is there anything I’ve forgotten? Unfortunately Chris Byrnes is not with us any longer. He would have had some colorful details more concerning this, if he’d been here, I’m sure.

8.2. Questions: Mostly inaudible. [question]

There were some really nice papers in these proceedings, because a lot of people came, and they actually contributed to that. Though, George Zames did not contribute a paper. I did try to get him to do that.

[question]
Well I certainly think it helped to put us on the map, right? Because, you see, I came — this was my second year back from Kentucky, so I was trying to build up this optimization and systems theory group. So I think it was good to have that as a first project. Because we worked on it for two years, so it was a good project for establishing. Because I was a pretty young man in those days, even already then I was young.

[question]
Were there many errors? We planted some errors in there, so that we would test you, to see whether you really — this is what I do in my papers, sometimes: I put some errors there. Otherwise, you don’t get any reaction, right?

Okay, that’s it.

9. KOBE 1991

9.1. Yutaka Yamamoto’s recollections. The MTNS in ’91 was held from June 17 through June 21. It was quite customary to have MTNS in June, as is unlike the case later on. And it was held in the Kobe International Conference Center and Kobe Hotel. Oh, and by the way, this Kobe hotel was later used as a conference site also for CDC ’96. This was before the Hanshin big earthquake, and the CDC was after the earthquake, so we were really worried, but fortunately, this hotel was fairly resilient.

[Inaudible question about earthquake]

But… after the earthquake, we wanted to see and the road was just jumping like this. So that somebody, you know, we had to approach the island by ship, and somebody kindly came over to pick us up, and I was sitting in the tiny back seat, and every now and then because of this road, I bumped my head on the ceiling. So that was… you can guess how bad it was.

In any case, these are the people who organized it [please add this picture] There were two general chairs. Hizo Kudama, who retired and Hideki Kimura, and a bunch of these guys. I was in charge of foreign affairs, and I don’t know what the function was, because I thought I did publicity, but the thing is that Akiro Sozumi seems to be in charge of publicity, so I really don’t know. Maybe I was just writing letters and corresponding with the appearing speakers, I don’t know.

Statistics shows that the registration fee was ¥30 thousand, which is approximately, at the time, $300. Now, maybe $350 or something. 332 accepted papers, only 273 were presented. Large number of attendees. Only 236 from Japan, and the rest are from overseas. 5 plenaries, 2 mini-courses, 10 special topics lectures. Oh by the way, at the time, the same plenary was referred to as special topics lectures and so that’s what it was. And the memorable thing is that it was pre-internet age, of course, so we printed a large number of brochures, posters, and took a lot of money to send them out over the world.

Fortunately, when we started out, which would be ’88-’89, Japan was in the middle of a real estate bubble, so we were lucky enough to be able to collect a huge amount of donations from companies. The bubble crashed in the middle of 1990, and the MTNS was ’91, but by that time we just ran away with a lot of money and a lot of donations already paid into our bank accounts, so we were well-off.

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7Based on his talk at MTNS2012, 1:02:00 on the tape
Also, this is one of the last MTNSs where you also had printed proceedings. That was probably the last time, I guess, maybe also St. Louis. In any case, we had to print the proceedings, and had a bit of trouble of choosing a publisher. And there was a company called ??Mitta Koppi?? industries or and they sell a copy of the Xerox machine, so to speak, and they also had a publication department. We were lucky enough to be able to talk them into publishing these 2 or 3 volumes (huge volumes) by saying that “look, this is the first applied mathematics at a conference in Asia, so culturally, scientifically significant,” so they should do it. So, finally, we convinced them, fortunately. But unfortunately the whole company ran out of business after that, hopefully not because of us. Obviously, the book did sell very well. So that’s a memorable occasion for the Japanese control and systems and even applied math community, to be really internationalized, and lots of important people came over.

10. NOTRE DAME 2002

10.1. Joachim Rosenthal’s recollections. So, thanks, welcome to everybody.\textsuperscript{8} I am supposed to speak mainly about MTNS 2002 which I organized at the university of Notre Dame. Let me start with a little bit of background, and the background started in 1993: there was an influential meeting in Mansfield, Massachusetts, so outside of Boston, where some of the people present here in Melbourne (Margreta Kuijper, Frank Kschischang) were there. The meeting was organized by Dave Forney, who realized that problems we studied in linear systems theory and behavior theory were studied in different context and with different language in coding theory and symbolic dynamics. So Forney’s idea was to invite people from the three communities and tell each other their language for the same topic. It was a really fun meeting. Researchers in systems theory were given a blue ribbon, the ones in coding theory a red ribbon and people in symbolic dynamics a green ribbon. In this way it was immediately apparent what language the person was speaking and that created a lot of interaction.

At MTNS 1998 in Padova Andi Loeliger and myself decided to organize a series of sessions with some tutorials to further the collaboration between coding theorists and systems theorists. Dave Forney was at MTNS 1998 plenary speaker and he explained in his plenary talk the coding theory point of view to linear behaviors.

So while the Padova meeting was going on, some people from the steering committee approached me, and said “hmm, there are two issues. One is that MTNS should be again in the US, and the other thing is that we’d like to have some expansion, you know, there is mathematical theory of network and systems where’s the network part? Coding would fit very well, so if you could expand that a bit and organize, that would be great. So would you be willing to do it?” And I was thinking a little and said “well sure, yeah, I

\textsuperscript{8}Based on his talk at MTNS2012, 1:24:00 on the tape
would”, and so the steering committee meeting had a vote, and the people who talked to me were probably in support and others felt MTNS should first go to France. So MTNS 2000 ended up in Perpignan, and I couldn’t go there to this Perpignan meeting because at the same week I was traveling with my family back from sabbatical in Europe back to the United States. Two days before I left for the States I received a phone call from Giorgio Picci who asked “Are you still willing to do MTNS 2002?” and I said “well, sure. I’m not prepared for a proposal, so...” and he said “doesn’t matter, I will let you know,” and the day after I was back in the States, I get again a phone call from Giorgio who told me “so they decided to have it in 2002 in Notre Dame, is it still okay?” “sure, it is.”

One of the first things you have to get settled as an organizer are the dates for the meeting. I picked a date in August trying to minimize the potential conflicts with other meetings. The responses to these dates were quite funny indeed. The American people on the steering committee responded “oh, great. This is during our semester break, and no problem we can come and it’s a good date.” And the Europeans responded “terrible date! This is during our semester break and we want to go on vacation.” Different attitudes.

As you may know Notre Dame is also famous for football. During MTNS 2002 at Notre Dame there was also a special training week in football and the large dining hall was divided in two parts, one for the MTNS participants and the other part for the football players. - Here you see a funny picture depicting this division of the dining hall. There are other pictures you can still find on the MTNS 2002 conference website which is still up and running.

As an organizer you have to come up with the program committee and the list of invited speakers. I heard in the late ’90s some criticism that the steering committee selects the organizers and then the organizers select members of the steering committee as plenary speakers. In part for this reason I tried to do 3 disjoint sets consisting of the program committee, the steering committee and the invited plenary and semi-plenary speakers. The only person who was in two two sets was Bill Helton, but for good reason. I asked the operator theory people, Gohberg and others, whom they suggest as plenary speaker? And everybody said “Bill, of course, because he is organizing IWOTA 2002 and is not giving a talk there and he’s just doing the labor, and he’s a great speaker.”

At the time of MTNS 2002 I had the feeling that mathematical systems theory was drawn into two different directions. What was happening is that some people studied more and more theoretical problems with no relation to applications. They had almost a tendency to move or gravitate completely to pure mathematics. And then there were lots of people who for better or worse, felt they should go really towards applications. It was about the time I stopped to go to CDC, because CDC became extremely applied for my taste. As a mathematician CDC was almost disconnected to what I’m interested in. In the same time there were good mathematicians like Hans
Schumacher who told me that they would like to enter areas of mathematical systems theory where real applications are happening and where students potentially have an easier time to obtain jobs.

This was a little the guidance to have several special topics at MTNS 2002 covered. So I asked Hans Schumacher if he could organize something about systems theory in finance. Similarly I asked Mark Alber and Raimund Ober to organize a Mini-Symposium about Biological systems, I asked David Forney and Brian Marcus to organize a Mini-Symposium on Communication systems and Paul Van Dooren, Uwe Helmke and Volker Mehrmann were asked to organize a Mini-Symposium on Control and Computation.

One of the difficulties for the organization of MTNS 2002 were the unfortunate events of 9/11 in September 2001. As a result there were still a lot of people hesitant to sign up for a conference in the US in summer of 2002. Nonetheless we had about 420 people attending. We tried to do the conference as low-budget as possible. Thanks to the generous support from different sources from within the University of Notre Dame and from the National Science foundation we were able to cover about half of the actual expenses and as a result MTNS 2002 was one of the most affordable ones. The conference fee was $260 and the student fee was $70. But this is easier to do on a university campus than if you just do it at a conference center.

11. KYOTO 2006

11.1. Yutaka Yamamoto’s recollections. MTNS 2006 was after the one in Leuven in 2004, and of course after the one in Notre Dame in ’02, and we had — sorry, I forgot to bring the conference center sign, it’s really a very nice building in the Kobe International Conference center. It was held rather late: July 24th through 28th. The reason being that June is rainy season for Japan, so I followed it. Actually June is much worse than hot summer because the humidity is up to 90 to 100%, so you have high temperature plus humidity. By this time my calculation was the rainy season would have been over, but actually it was not this year, so I had to worry about whether or not I can have a banquet outside. Fortunately, just in the middle of the conference, the rainy season was gone and we could have a banquet outside. The total attendees for this was 481, or 483, which one was correct I don’t know now, but more or less that figure, so it’s one of the largest so far. Probably the one in Leuven in some trivia question was larger, but I never knew the statistics.

Also I tried to make a point that we should make our presentations more accessible to everybody, so I set up the webpage and asked the attendees, not only the printed speakers, and also asked the invited speakers to upload, which is still, by the way, on this webpage, so that if you go to this page, you can get the full proceedings and also most of the printed speakers’ slides, and some of the bases. Budget: I was lucky enough to be able to be supported

9Based on his talk at MTNS2012, 1:11:36 on the tape
by many agencies, not by private companies, but the whole budget was ¥27 million, and also the registration fee was something like ¥45 thousand, I think. I’m not so sure, but it was in the range of 350 Euros, and it was pretty reasonable.

Here is a picture of the organizers. **Yutaka, send us a picture.**

By the way, I recall one more thing. At the time, it was customary to send the photo-ready mat and the speakers and authors bring the photo-ready copy to the site, and then we publish the proceedings after the conference. And I still remember that Hideki Kimura came to me and said, you know MTNS is a wonderful conference, you don’t have to make your paper ready when you submit your paper. That sounds like a great and nice idea, I thought, and somehow, this beautiful tradition is lost, as the internet age comes along.

[Yutaka showed many pictures of people and gave nice verbal captions]

12. **Acknowledgments and Warnings**

Bill expresses gratitude to John Dougherty for finding and helping assemble information for these notes.

A flaw in MTNS data is that publishing in the proceedings and speaking are two different things. So a person might have been at a meeting and spoken with little record of it. An example is Zames who spoke at Lubbock (I recall) but did not publish; he is listed in the Lubbock index.
APPENDIX A. RECOLLECTIONS BY OTHERS

A.1. Harry Dym. I participated in the Delft MTNS that Patrick ran in 1979, I did not go to Santa Monica 81, but did participate partially in the Beer Sheva conference that Paul ran, and in every other one since then through Budapest.

The old conferences were small and relatively inexpensive, much of the cost and organization was absorbed by the department of the organizers. The Beersheva conference was particularly small because there was active lobbying against participation because of the Lebanese war.

Curiously enough the Texas conference was very profitable for me personally. I did not attend, but Abie Feintuch did and told me that Patrick Dewilde was very interested in prediction theory. At the time there was a Belgian-Israeli exchange program and I wrote to him at Louvaine suggesting that this would be a good way to finance a short visit to Israel (10 days I think), if he was interested. He was. That was in the autumn of 1977 and Patrick had already moved to Delft, but we did not let such minor details stand in the way and submitted an application. We met each other for the first time in the summer of 1978 at Stanford, where we were both guests of Tom Kailath. Then in the spring of 1979 the Belgian authorities actually approved our application and Patrick visited me in Israel. That was the beginning of an active collaboration that lasted many years.

I also met Dima Arov at the Kobe MTNS (Israel Gohberg acted as translator), but that’s past your point of interest. (Harry and Dima have collaborated steadily to this day- The Ed.)

A.2. History of $H_\infty$ Control. $H_\infty$ control was a major thrust of MTNS for a decade plus (1985–2000 or so). Actually its roots lay with the MTNS. Here pasted in is an account of primordial $H_\infty$.

Zames account in his historical article (June 1996 Control Systems Magazine) is:
“Nevertheless, in 1976 Zames [30] relying on abstract reasoning, . . . proposed that the objective of feedback robustness design could be captured in terms of the minimization of sensitivity in a weighted operator norm such as $H_\infty$ or $L^1$. At the time this was thought to be a speculative undertaking, requiring the creation of new mathematics and with no assurance of eventual success.

The outlook changed shortly afterwards following a chance meeting with Bill Helton at a conference. At that point the sensitivity minimization problem had been reduced to an interpolation problem, namely that of finding the smallest $H_\infty$ function satisfying a set of Right Half Plane interpolation constraints. Asked for ideas about how to solve such a problem, Helton pointed to the (then not well known) results of Pick and Nevanlinna, who had solved the scalar interpolation problem around World War I. This meant that the sensitivity minimization problem admitted explicit and even closed
form solutions! It opened the path to a practical theory of robustness optimization. The broad outlines of such a theory were presented in a 1979 paper [31] which attracted unprecedented attention and enthusiasm, among others from Bruce Francis.

Helton’s account is quite similar: At the 1977 MTNS in Lubbock, after my talk on solving the multiport broadband impedance matching problem (using Nevanlinna -Pick , commutantant lifting, AAK), George came up and said wasn’t your talk set in $H^\infty$? Young and inexperienced I was kind of hurt that out of this long talk he was not asking about my beautiful details or the glories of AAK, but had merely gleaned what planet it was on. George went on to say he thought $H^\infty$ should also be used in control and (as usual) asked what do you think Bill? I said (as usual) to George who was always asking philosophical questions “how do I know George? You are the engineer.” George gave up and asked, “Bill can you solve this problem?” He showed me a problem where one approximates a function with an $H^\infty$ function. I pointed out that the problem was under-constrained and so a “perfect” approximating sequence could be chosen. In other words, this was a trivial problem, a disappointing situation, since no one thought control to be a trivial subject. Our next meeting was at a nonlinear circuits conference in Houston (my recollection we were invited by Rui de Figueirido who we met at the Lubbock MTNS). Then George asked me the same question, modified by adding an interpolation constraint. I told George this was a Nevanlinna–Pick problem and I recommended a book and sent George a broadbanding paper of mine with a few references circled. A few years later I went to a CDC, at that time a rare occurrence for me (who was more of a circuits buff), and was amazed and delighted to find the place abuzz with the control work of George Zames and Bruce Francis based on Nevanlinna–Pick interpolation. Their first $H^\infty$ paper, which appeared in a conference proceedings, had a sentence of thanks to Helton for pointing out Nevanlinna–Pick to them. At the CDC I asked George and Bruce if there was any interest in solving the multiport $H^\infty$ problem, since that would be rather straightforward. They were delighted, so we collaborated did it together.
APPENDIX B. CONFERENCE PROCEEDINGS
INTERNATIONAL SYMPOSIUM
ON
OPERATOR THEORY OF NETWORKS
AND SYSTEMS
Volume I
AUGUST 12-14, 1975
MONTREAL
<table>
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<td>&quot;Structure Result for Nonlinear Passive Systems&quot;</td>
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<td>B.D.O. Anderson and P.J. Moylan, Department of Electrical</td>
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<td>George E. Trapp, Department of Statistics and Computer Science,</td>
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Appendix C. EPILOGUE

Dear Reader,

We are compiling a history of the MTNS for presentation at MTNS 2012 and with the notion of having some archives. If you could send a picture of yourself or friends of good resolution preferably from around the they you first attended the MTNS, that would sure help. When did you first attend the MTNS? Any stories or tales or perspectives would be welcome. Please send me the pictures at

helton@ucsd.edu
Best, bill

QUESTIONS
A basic question is when did such and such a crowd start coming to the mtns?
How many math dept vs eng dept guys?
What trends are interesting to track? Conclusons to draw?