DE-Vol. 15-1

TRENDS AND DEVELOPMENTS IN MECHANISMS, MACHINES, AND ROBOTICS — 1988

Volume One
Mechanisms
Global Motion • Expert Systems • Structural Design • Analysis and Synthesis

edited by
A. MIDHA

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Volume One

Mechanisms:
Global Motion
Expert Systems
Structural Design
Analysis and Synthesis

presented at

THE 1988 ASME DESIGN TECHNOLOGY CONFERENCES —
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edited by

A. MIDHA
SCHOOL OF MECHANICAL ENGINEERING
PURDUE UNIVERSITY

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FOREWORD

The nearly two hundred papers presented at the 20th Biennial ASME Mechanisms Conference, held in Kissimmee, Florida during September 25-28, 1988, are contained in these three volumes entitled "Trends and Developments in Mechanisms, Machines and Robotics." The conference has been organized by the Mechanisms Committee of the Design Engineering Division, ASME, under the auspices of the Design Technology Conferences.

Volume one contains such topical areas as global motion properties of mechanisms, expert systems, structural design, and analysis and synthesis of mechanisms. The contents of volume two include theoretical kinematics, automated design, simulation of mechanical systems, machine systems and elements, and vibration and compliant systems. Finally, in volume three are contained dynamics of mechanisms and manipulators, mechanical design aspects related to robots and vehicles, and workspace, kinematics, geometry, task planning and control of robots.

Professor Crossley's plenary presentation, and the opening paper in volume one under "Milestones," is entitled "Recollections from Forty Years of Teaching Mechanisms." This excellent personal and historical narration should help us understand our origins as a professional community, our present health, and the bright future that lies ahead. Approprliately so, in the latter vein, this article is followed by one enlightening us to the research needs and opportunities in machine dynamics, by Professor Soni et al. This important and timely study is a result of sponsorship by the National Science Foundation and ASME.

A significant increase in participation, as measured by the number of papers herein, has been in good part due to many a young researcher on the scene, meeting the challenges of the day. Classical concepts are integrating with newer notions, as is exemplified by the union of structural synthesis and expert systems. Increased attention is being given to automation in design through more efficient analytical, algorithmic and computational efforts, as well as use of geometric data bases. More sophisticated modeling and prediction techniques are now being implemented in the consideration of compliance and vibration in machine systems. Finally, as is evident in volume three, its entirety is devoted to the various issues confronting us in the area of robotics. This surge of research activity is evidence of our community's responsiveness to societal needs.

These proceedings, however, would have been impossible without the fine contributions, and the selfless reviewing and counseling activities of numerous colleagues,from far and near. I owe a great debt of gratitude to members of the Mechanisms Committee: Joseph Duffy, Chairman, E. Roland Maki, Secretary, and Stephen Derby, Papers Review Chairman, Outside North America, for their strong support throughout. I am grateful to Steven Dubowsky and Kenneth Waldron, the past and present Technical Editors of the *Journal of Mechanisms, Transmissions, and Automation in Design,* for making the interaction at the journal end a most pleasurable one. I gratefully acknowledge the staunch support in many a form over the years from members of the School of Mechanical Engineering at Purdue University: Dr. Winfred Phillips, former Head, members of the Design Faculty, and Patricia Booth and Kathey Freeman. Last but not least, the ASME staff members in New York are thankfully acknowledged for making this momentous event possible.

Ashok Midha
Papers Review Chairman
Mechanisms Committee
School of Mechanical Engineering
Purdue University
West Lafayette, Indiana

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F. E. Crossley
Professor Emeritus of Civil Engineering
University of Massachusetts
Amherst, Massachusetts

This record of former times and events and people, which I have been asked to gather together, is of course also a record of some of my own journey through life, from 1943 into the '70s. I feel wonderfully fortunate to have been one of a circle of congenial friends, who met perhaps annually, usually at conferences, and often to reveal some new scientific contribution, but who otherwise followed each his own bent in different parts of this country. Several of them have contributed their comments to improve this article. I owe very much to this group of friends, who lent me enthusiasms and stimulus, supplied competition, and offered good criticisms and sage advice; it has been for me a real privilege to have known and worked with: Doug Adams, Joe Beggs, Ferdinand Freudenstein, Tom Goodman, Al Hall, Dick Hartenberg, "Yappie" J.P. Den Hartog, Herb Kuenzel, Jack Kimbrell, Jerry Lowen, Chuck McLarnan, Nicholas Rosenauer, George Sandor, Joe Shigley, Merhyle Spotts, Deh Tao, Del Tesar, and others in Europe, Japan and Australia, as well as many younger men. Some of these are no longer living, but I thank them all for having been such a blessing. Many of them have surpassed me in the high quality of their technical contributions, while I have tried to work on organizations and journals.

Early Days: The Forties

In 1943 and 1944, when this story begins, it was war-time. There were two military programs, one Army and the other Navy, which utilized the faculty and facilities of engineering colleges; for of regular undergraduate students there were very few -- all were drafted into service. The Army's Special Training Program, called ASTP, was discontinued after about a year, but the several Navy college training programs persisted. I had been teaching at the University of Detroit, which had contracted for ASTP, and so in 1943 I moved to Yale, which had the Navy. In the Navy's Mechanical Engineering curriculum, Kinematics was designated as Course M.E. 1, and we who taught it prided ourselves that this inferred that it was of first importance. It was the first of a standard sequence of courses in Machine Design, being followed by a 4-hour course in Machine Design (Elements), and a 3-hour course in Dynamics of Machinery, and these were taken by all M.E. majors. The Kinematics course occupied three lectures and one 3-hour long "Computation" or "Lab" period (usually an afternoon of graphical constructions) each week.

Lecturers in these courses tended to follow closely what was presented in their chosen textbook, and in fact, students demanded this close adherence, with assigned reading and assigned homework problems. Favorite textbooks were those written by Guillet [1], Schwamb, Merrill and James [2], Keown and Faires [3], and Ham and Crane [4]. For the computation periods there were sets of prepared problems available, showing linkages for which velocity and acceleration diagrams were to be completed, a cam base and follower for the layout of a cam profile, or the axes on which a pair of bevel gears in mesh was to be drawn in detail, for examples. One of the best of these large manuals, "Problems in Kinematics" by L.M. Headley [5], had appeared in 1938; then the excellent "Mechanism Problems" by A.S. Hall and E.W.Azpell [6], together with another, "Kinematic Problems" by R.M. Wingren [7], first appeared in 1948. I well remember, as a young assistant prof, having to teach four sections of M.E. 1, which entailed repeating the same lecture at 8, 9, 10, and 11 a.m. to a total of 120 students three times a week, and supervising four afternoons of "graphical comp" at each of which usually two prepared problem sheets were completed by each student, so that I would take home 240 sheets to grade every week. We kept busy! And I also taught evening courses at the New Haven Junior College, to augment my meager salary. My wife kept house, looked after two babies, and spent long hours at a coastal lookout post watching for attacking enemy aircraft.

There were no graduate courses in Mechanisms offered in those days; in fact, in the war-time very few graduate engineering courses of any sort were taught anywhere, for lack

of students; such courses restarted in the 1950s. But in those days, few professors of engineering had doctor's degrees: the majority had either a Master's or the "terminal" degree of "Mechanical Engineer." Consequently there was extraordinarily little interest in extending the sophomore bounds of the subject of Mechanisms. It seems the subject was codified around 1900 or 1910, and had so remained in the U.S. It is interesting to read the Preface of the first edition (1904) of Schwamb and Merrill's "Elements of Mechanism" [2]; both authors were at M.I.T. They write that their book developed from course notes, but that "very little claim is made as to originality of the subject-matter which has been so fully covered by previous writers." These other authors they list as Reuleaux, Robinson, MacCord, Rankine, Goodeve and Unwin.

The stimulus to break out of this stagnant state came indirectly from Germany after the war. Several leading professors like Dick Hartenberg and Herb Kuenzel, through family background or diligent study, were able to read and discern the extensive German literature; Al Hall, too, presented several papers on this work, and undertook to translate even some Russian work; and Tom Goodman went over to Germany on a fellowship to learn first-hand of the work being developed there. After that, starting in 1957, three German kinematicians -- Beyer, Hain and Meyer zur Capellen -- were invited to come over here and tell us, in English of course, what they were working on. That's when the pot began to boil.

But it was Richard de Jonge of the Brooklyn Polytech who played the role of chief needler and whip!

A.E. Richard de Jonge.

If all the world were full of good, steady, average people, then I believe it would be a very dull place to live in. I thank the good Lord for the special ideosyncratic characters, who provide the spice.

Richard de Jonge was born in Berlin, Germany, about 1883. While at the Technical University there (Charlottenburg), he had become lecturer and lab assistant to Professor W. Hartmann, who made some valuable contributions to Kinematics. Thereafter, de Jonge spent fifteen years in Shanghai, China, as an engineer and naval architect, 1913-1928, then came to New York to work as a power-plant engineer for Babcock & Wilcox from 1931 until 1948 when he retired. But he also was an Adjunct Professor at the Brooklyn Polytech from 1932-1942 and again from 1952-1960. In 1942-1943 he contributed two important stimulant articles:

(a) "What is Wrong with 'Kinematics' and 'Mechanisms'?" [8]: In this he complained that practicing engineers were resorting too quickly to trial-and-error methods when laying out linkages to solve kinematic design problems, because the theory was not taught in colleges, nor available to them in books. Then he described the work of many famous 19th century authors -- Monge, Ampère, Poinsot, Bernouilli, Euler -- concerning kinematics, leading to the work of Reuleaux, Burmester, and Franke, who had developed the modern theories which he felt should be taught. He complained (largely truthfully) that none are mentioned by, or known to, current engineering professors or textbook writers in the U.S.A. Finally he described in this paper both "kinematic synthesis of mechanisms" and Grübler's "number synthesis" -- ideas which were entirely unknown to most of us in those days. A year later he published:

(b) "A Brief Account of Modern Kinematics" [9]: In this he elucidated the inflection circle and the Euler-Savary equation, some of Grübler's published work on constrained linkwork, and the basis of the linkage synthesis for three, four and five finitely separated positions of Burmester, Alt and Beyer. These articles should have been wonderful stimuli and guides to many of us.

Unfortunately, de Jonge had become soured because he felt that no one would listen to him. He also tried strongly to change our customary terminology, from using "instant centers of rotation" to "rotopoles" - a word he invented. At ASME meetings for years thereafter, he would always sit in the front row, and if any author dared to mention an instant center, he would stand and belabor him for his lack of good sense in not following his (de Jonge's) leadership. This exasperated many. Then, through the fifties, as many papers on coupler curve synthesis or position synthesis were presented by younger authors, this old dragon would rise and wipe the floor with them for not realizing it had all been done before: hadn't we read Alt's or someone else's papers? Well, no, frankly, we had not, because our knowledge of German was sparse, even though a working knowledge of one or even two foreign languages was required in those days for a doctor's degree.

He published two interesting lengthy articles on linkage varieties with the New York Academy of Sciences. In 1968, when he was 85 years of age, he came to Atlanta, Georgia, to attend the Tenth Mechanisms Conference; it was one of his last excursions from New York. He was still a distinguished, white-haired man of commanding mien, though a little stooped; but it was sad to note he needed help, in such things as to cross the street or to call a taxi. He still grumbled, though: ask those who lived nearer to him than I. He died on April 7, 1970, in New York, a few months after his wife's death.

The Fifties, When the Tide Began to Flow.

As early as 1943, E.S. Ault and Allen S. Hall, Jr., both of Purdue University, were publishing articles on new kinematic theory in *Machine Design* magazine. Most of the kinematic literature appeared in those days either in *Machine Design* or in *Product Engineering,* although a lesser number appeared in the *ASME Transactions* and *Design News.*

1953 became a banner year for Mechanisms, when the First Conference on Mechanisms was held at Purdue University. Al Hall writes me: "The original idea for the Purdue Conference came from Roger Bolz (of *Machine Design*), or perhaps Roger and Stan Ault (Professor at Purdue) together. My first memory is of being called into Ault's office one day, being introduced to Roger, then listening to the two of them explain what they had been talking about. I was asked whether I would take part. As a very junior rank, what could I say?! Roger was very active in planning the first 2 or 3 conferences. In fact, I believe he was "chairman" of the "planning committee" -- if we had that much formality at the time. He always pushed Colin Carmichael to the front when it was time for picture taking, or for a few words on behalf of *Machine Design.*

"I believe Ben Hummel was pulled into the action in about the same way that I was. In the years we worked

together his contributions to organizing, identifying possible speakers, (and finding) solutions to printing and publishing

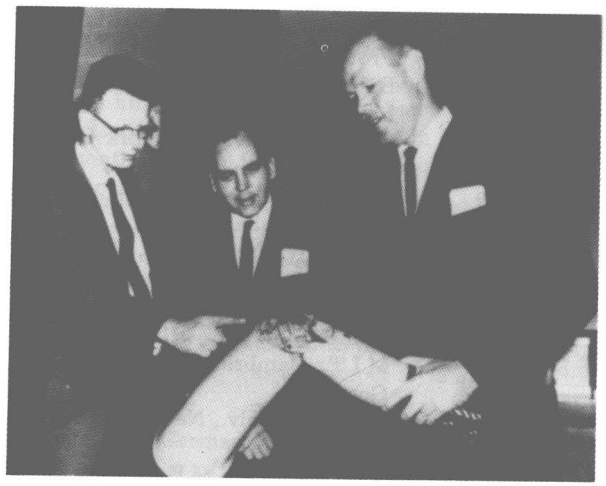

Fig. 1: At the Purdue Conference (left to right) Ben Hummel, Al Hall and Charles Radcliffe, about 1957.

problems, were really what made things go. I don't remember ever becoming chairman." In spite of this disclaimer of his, Al Hall was a very central figure in all the Purdue Conferences. The University Conference Center provided ideal "hotel" accommodations, and the first conference took place October 12-13, 1953. Seven papers were presented, of which two were by Purdue staff -- Al Hall on the types and classifications of mechanisms, and A.R. Holowenko on accelerations -- but all the rest were by industrial people -- two on non-circular gears, one on designing a linkage as a function generator, one on cams, and one on timing and correlating the motions in a loom -- several therefore on dynamic problems.

Also in 1953 the first English-language book on the German work on linkages appeared: this was "Kinematics of Mechanisms" by N. Rosenauer and A.H. Willis [10], published in Sydney, Australia: this made a very strong impression, and was widely discussed in small meetings, because it was so drastically different from anything in other textbooks.

Nicholas Rosenauer.

Professor Nicholas Rosenauer had been for many years Professor of Applied Mechanics at the University of Latvia, in Riga. In the early years of the war, this Baltic country was overrun by the Russian Army, and still today remains a constituent of the Soviet Union. Rosenauer joined the refugees into West Germany (as it later became known). He then emigrated to Australia, where Professor Willis* helped him write, in English, of his long knowledge of the German literature in Kinematics. At age 65 he again emigrated, this time to Canada, and he taught in Windsor, Ontario; but his audience there was small, as his position was only temporary, filling in for someone on sabbatical leave. Thus his future was threatened by that severe sword of Damocles that is the lot of all elderly immigrant refugees -- no social security, no pension, and no savings -- for everything, his home and all his possessions, had been left behind in Latvia. He was then generously offered a professorship by Leighton Sissom at Tennessee Tech and use of an on-campus apartment, to which he and his wife moved, and there this quiet-mannered, soft spoken and genial man continued to teach until he died at age 79 of a heart attack on March 21, 1970.

He had a fine collection of books and articles, most of them in Russian, and a complete set of the Russian "Seminars on the Theory of Machines and Mechanisms" from the Academy of Sciences in Moscow, which should still be in the Cookeville, Tennessee, library. He also worked with me for many years as translator into Russian of the abstracts of the articles in the *Journal of Mechanisms.*

Other major events in the developments in the Fifties were the emergence of two young men, Tom Goodman and Ferdinand Freudenstein, who together were responsible for organizing the visits to the U.S. of both Professor Rudolf Beyer and Engineer Kurt Hain (pronounced Hine) from West Germany.

Tom Goodman.

Tom Goodman had graduated from Northwestern University in 1945, then earned an M.S. degree from the University of Pittsburgh in 1950. He won a Rhodes Scholarship and spent the next two years at Oxford, in England, then returned to the U.S. and earned a D.Sc. from M.I.T. From 1956 to 1960 he was an Assistant Professor at M.I.T. In 1956, however, he won a Fulbright Scholarship to the Technical University of Munich, and went there to study for a year under Rudolf Beyer. From this base he was able to assess the advanced work being done in Germany, and he traveled to meet many of the leading men in the kinematics field, even making a trip to Moscow to meet Professor Artobolevskii. Returning to M.I.T., he organized for September 2-12, 1958, a "Special Summer Program" of lectures entitled "Design and Dynamics of Mechanisms" by visiting lecturers Rudolf Beyer, Kurt Hain, with Douglas Adams, R.C. van Sickle, John Hrones, Ray Johnson and Herb Richardson and himself, stating his "aim has been to present material which is not readily available in American textbooks." It was the first of the summer schools or "alternate" conferences that supplemented the dissemination of ideas going on annually at the Purdue Conferences. Subsequent summer schools or conferences were held at Chicago, Yale University, University of Alabama, and then Oklahoma State University. One major result of that summer program was that he and Bruce Harding decided together to organize the translation of Hain's large book "Angewandte Getriebelehre": Doug Adams, Ferdinand Freudenstein, Don Raichel and I also each agreed to do a chapter or two.

I recall meeting Tom Goodman shortly after that, and he told me of his conviction that an engineering professor should never remain as purely a teacher; for he must get out to practice as a designer. Following this, he resigned from M.I.T. in 1960 to take a job with General Electric in Schenectady. Three years later he was called to Northwestern as a full professor, but in 1964 he died of cancer at age 38, tragically

* Professor Willis was at that time the head of the department at the University of New South Wales, and soon was Dean for a very long period.

leaving behind a lovely wife and little daughter. He will always be remembered by us who knew him as friend and colleague.

German Visitors, 1957-1961.

The first contacts made by Americans with German kinematicians after the war occurred when Dick Hartenberg and Joe Beggs attended the Getriebetagung (i.e., Mechanism Conference) in Bingen in 1955; Dick presented a paper. Then in 1957 Jacques Denavit and Dick went to the next Getriebetagung in Konstanz, the same year that Kurt Hain made his first trip to the U.S. Ferdinand Freudenstein, then Associate Professor at Columbia University in New York City, had organized a coast-to-coast trip for him (see the box below), and we can marvel at the number of schools that were interested then in hearing this foreign visitor. Ferdinand himself was young, had earned his Ph.D. at Columbia in 1954, but even before that had published ASME papers on linkage synthesis in 1953. Kurt Hain was a very practical designer, had developed typewriter mechanisms, and was famous as the originator of the "Three-point hitch" by which farm implements are hitched behind a field tractor. In his papers he had elaborated upon Franke's "Number Synthesis"* to render alternative designs for many linkage uses, and Alt's work on dimensioning a linkage to serve as a function generator or coupler path follower. He astounded those like myself, who attended the Fourth Conference on Mechanisms at Purdue, October 14-15, 1957, by presenting a bibliographic list of *five hundred and twenty* items (both books and articles) all in the German language and published in Germany since 1945. This came as appendix to a paper he presented there surveying the current work in Germany. But most importantly, he spoke in English.

Herr Hain was over here for a second time the following year, for the Special Summer Program at M.I.T., noted above; then between February and June 1961, he was at Yale University as Visiting Professor of Mechanical Engineering, and I had the privilege of attending his course of graduate lectures, of which I have preserved my notes.

Itinerary for Ing. Kurt Hain - Fall, 1957

October 6-7	Arrive Idlewild Airport, New York (now called Kennedy)
October 9	Columbia Univ., New York; Prof. F. Freudenstein
October 14-17	Fourth Mechanisms Conference, Purdue Univ.; Prof. A.S. Hall
October 18-21	Northwestern Univ., Evanston; Prof. R.S. Hartenberg
October 24-25	Los Angeles: Univ. of California, Agricultural Engineering Dept.; Prof. R.L. Perry
October 26-27	With friends, Lemon Grove, California
October 27-29	Univ. of California at Davis; Prof. Bainer
October 30-Nov. 1	Univ. of California at Berkeley; Prof. A.S. Levens
November 4-5	Univ. of Nebraska, Lincoln, Neb.; Prof. J.C. Wolford
November 6-8	Michigan State Univ., East Lansing; Prof. R. Hinkle
November 11-12	Case Inst. of Technology, Cleveland; Prof. D.K. Wright
November 13	Buffalo, N.Y. - A.S.M.E. meeting; Linde Co. Tonawanda
November 15	Columbia Univ., New York City; Prof. F. Freudenstein
November 18-19	College of the City of New York; Prof. Harold Rothbart
November 20	Yale Univ., New Haven, Conn.; Prof. E. Crossley
November 21	Univ. of Rhode Island, Kingston; Prof. Don Bradbury
November 22-23	M.I.T., Cambridge, Mass.; Prof. T.P. Goodman
November 25	Columbia Univ., New York; Prof. F. Freudenstein
December 1-7	New York: A.S.M.E. Annual Meeting (presented 2 papers)
December 7-14	Miscellaneous - Stevens Inst., Brooklyn Poly.
December 14	Return home.

Fig. 2: (left to right) Dick Hartenberg, Kurt Hain and Tom Goodman at Purdue, 1957 or 1958.

Just as in the sixth century Irish saints brought Christianity to the Germans, so now the Germans brought their kinematic science to the U.S. The second missionary was Rudolf Beyer, a Saxon, who had been a teacher in the engineering school at Zwickau before World War II. Thereafter, because Saxony became occupied by Russian troops, he had moved to Munich, where he became an adjunct (ausserordentliche) professor. Prof. Freudenstein invited him to visit and lecture in the U.S., and he first came to M.I.T. in September, 1958, and the Fifth Purdue Mechanisms Conference in October 1958, where he spoke on Space Mechanisms, and methods for the analysis of their motions including screw axes. It was in 1953 that he had published, jointly with Schörner, the little book on spatial mechanism fundamentals, illustrated with stereoscopic figures, to be viewed through red and green spectacles.

* Number synthesis is the direct translation of Zahlsynthese, but Hain told me this was an abbreviation of the earlier term Zahlensynthese, meaning "counting synthesis" or "census building."

In 1960, Professor Beyer came a second time to the U.S. We had invited him to come to Yale as a Visiting Professor for the fall semester, and there was a goodly graduate class: it was our first graduate course offered in Kinematics. The course started off well, but it soon became apparent that Dr. Beyer was very ill; he and I had lunch together almost daily, and I had to urge him to eat, for he could find no appetite. He left us for his home on medical advice early in November, and died about a month later in Munich of cancer of the liver.

Everybody loved him, for his friendliness and for his very courtly old-world manners. He was a careful and meticulous lecturer. I recollect also one occasion, after he had been with us about a week, he arrived one morning in my office dressed formally in striped trousers and black morning coat, and announced: "Today we must go to present our greetings to the Dean." Such, of course, was the proper style in the Germany of his day. So we went.

The third German missionary was Professor Walther Meyer zur Capellen of Aachen: he first came to the U.S. in March 1961 to attend the Yale "International Conference for Teachers of Mechanisms" [11], together with Professor Oene Bottema from Delft, Netherlands, whose name had been proposed to me by Ferdinand. Professor Bottema came again, as one of the lecturers at the Yale summer school in 1964: he was Professor of Mathematics and for many years the Rector (that is, equivalent to President) of Delft Technical University, and a kinder, nicer, more thoughtful and unassuming man could hardly be found anywhere.

Meyer zur Capellen came again in, I believe, 1966, and made a coast-to-coast lecture tour, which I arranged for him.

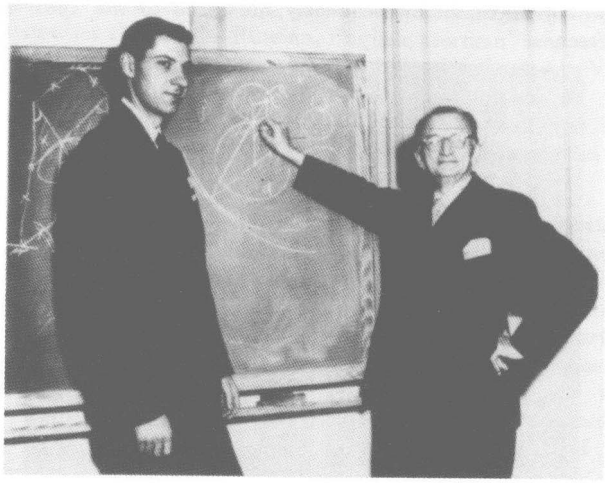

Fig. 3: Jacques Denavit (left) and Rudolf Beyer (right) at the Purdue Conference 1958.

From the Russian côterie, the first visitor was Professor Ivan Artobolevskii, who came for a visit sponsored by the Society of Friends (the Quakers) in, to the best recollection, January 1959. He visited Freudenstein at Columbia, and Evanston, Ill. where he stayed several days at the home of the Hartenberg's. But he gave no lectures. The roster of the 1964 Purdue Conference indicates that he and several other Russians attended, but we believe that is false. He came again to an ASME meeting in New York City in October 1974. Meanwhile, in 1968, his associate, Professor Arkady Bessonov from the same Institute of the Academy of Sciences in Moscow, came to take part in the Tenth Mechanisms Conference in Atlanta. But there were no lecture tours as with the Germans. Travel in the U.S.A. by Russians was greatly restricted by the State Department.

New Books and New Theories.

The '50s and '60s formed an exciting period to live in. The great sign of renaissance was the continual series of new books and new ideas that were appearing.

Let's start with Hrones and Nelson -- that hefty volume of huge dimensions, full of four-bar coupler curves, which appeared in 1951 [12]. It was not a textbook for teaching, of course, but presented a collection of curves very useful to the designer for matching his needs. Then, in 1953, contemporary with the First Purdue Conference, there appeared Rosenauer and Willis' book [10] (already mentioned), that introduced us to such novelties as the inflexion circle and curvature theory, Gruebler's census of four-, six- and eight-bar linkages and linkage synthesis. In the same year Hinkle's good new textbook [13] appeared, but it contained none of the new material. Then in 1955, Joe Begg's first book "Mechanism" [14] was published: it was the first textbook written for a graduate course and included many concepts of direct value to practicing engineers. The work was highly regarded in Russia, but I think it was ahead of its time here in U.S.A.

A most valuable and striking idea -- using complex numbers as vectors to describe a linkage -- was introduced by Dick Hartenberg in 1957 to the Fourth Mechanism Conference [15], and this was succeeded by many papers together with Jacques Denavit, culminating in their book, "Kinematic Synthesis of Linkages" [16] in 1964. Vectors on the complex plane were not new, of course, but their application to a four-bar linkage was. A French-language book by Deaux -- "Introduction à la Géometrie des Nombres Complexes" [17] -- had preceded these in 1947 (though this included nothing on quadrilaterals), and in 1950 an excellent book from Holland: "Advanced Plane Geometry" by Zwikker [18], dealt with the same, even applying the idea to gear teeth in mesh.

Description of spatial linkage motions by matrices and even dual numbers and quaternions was beginning. Jacques Denavit's milestone thesis was completed in 1956. The digital computer had appeared and was being used by some for analysis, and the analog computer for simulation. These were strong influences weaning us all away from the traditional graphical methods of displacement, velocity and acceleration analysis of linkages. But until the mid-50s we did not have Fortran; I remember taking a graduate course in writing programs in "machine language." Perhaps I should have stuck with it, but instead I started trying to make the analog computer simulate the motion of a four-bar linkage. In 1962-1963, I got a Fulbright lectureship to the Technological University of Munich, Germany, and the first article on the four-bar simulation was published there in 1963. This analog computer development was one of the courses offered at the Yale summer school in 1963, and in the summer of 1964 I was invited again to Germany to lecture at five different universities and demonstrate the technique on the new Telefunken portable computer. It was an exciting experience: we had developed two or three different circuits for the four-bar and its coupler curves, also some for the slider-crank inversions and function

curves. This research continued until 1970, when Lou Torfason while at Georgia Tech put together many remarkable circuits for three-dimensional linkages, and to show various geometric solids, as hyperboloids and toruses with their curves of intersection, shown stereoscopically.

Attention was also being given to mechanisms other than linkages. One of these was cams, and Harold Rothbart's outstanding book on cam design [19] came out in 1956. Thirteen years later, Neklutin's book [20] also appeared. A reprint of Buckingham's famous book on gears [21] appeared in 1963, and an interesting book for designers was written by Tuttle [22].

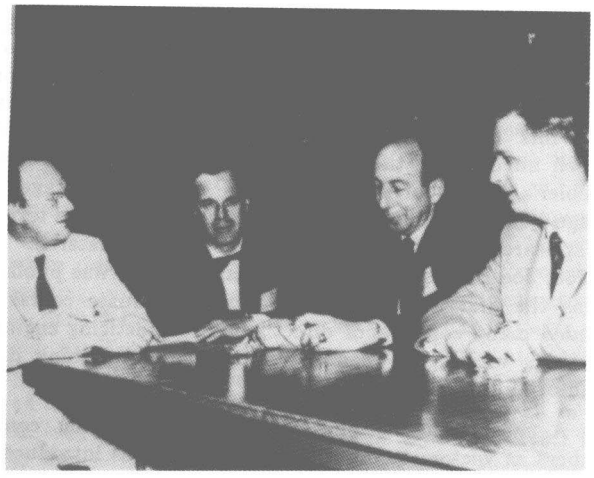

Fig. 4: (left to right) A. Holowenko, M. Percival, Harold Rothbart and Ray Johnson, 1958.

Summer Schools and Special Conferences.

Quite a spate of summer schools and conferences, other than the regular Purdue conferences, were organized with a view to spreading the information on new topics in Kinematics. After the "Special Summer Program" at M.I.T. in September, 1958, mentioned already, there were six within the next ten years:

November 1960:	"Synthesis of Mechanisms" Conference, Univ. of Alabama, with Prof. Herb Kuenzel (chairman); Speakers: Doug Adams, E. Crossley, G. Holt, S. Okcuoglu, L.R. Koenig, S.E. Rose.
March 1961:	First International Conference for Teachers of Mechanisms, Yale University, with E. Crossley (chairman); Speakers: K. Hain, W. Meyer zur Capellen, O. Bottema, K.H. Hunt, F. Freudenstein, H.A. Rothbart, J.S. Beggs, A.S. Hall, S. Rappaport, J. Denavit, E.P. Bullard.
July 1962 (six weeks)	Advanced Science Seminar in Kinematics, Illinois Inst. of Technology, with A. Fejer (chairman); Staff: R.S. Hartenberg, J. Denavit, A. Cowie.
July 1963 (four weeks)	Advanced Science Seminar in Mechanisms, Yale University, with E. Crossley (chairman); Staff: O. Bottema, F. Freudenstein, A.S. Hall, G.N. Sandor (F.M. Dimentberg was invited too, but could not travel).
July 1966 (two weeks)	Recent Developments in Kinematics, Massachusetts Inst. of Technology. Staff: D. Adams, F. Freudenstein, K. Hain, A.S. Hall, and B. Roth.
July 1969	(First of the long series) Summer School for Kinematics, Oklahoma State University.

As a consequence of these activities, the demand grew for graduate courses in Mechanisms, but of course first we had to have textbooks. So four landmark books appeared: the first had been Rosenauer and Willis's in 1953; next came Al Hall's [23] in 1961; then Dick Hartenberg and J. Denavit's [16] in 1964; and Joe Beggs' second book "Advanced Mechanism" [24] in 1966. But copies of papers from conferences and course notes also supplemented the support for graduate courses. And it was in this same time-frame that a thin but stimulating little book appeared from England, written by an Australian, Ken Hunt [25] in 1959. Subsequently, he came to the 1961 Yale Conference.

Fig. 5: (left to right) Ed Schmidt, unknown, Herb Kuenzel, Fan Chen and Lee Harrisberger inspect a function generation linkage, 1958.

Meanwhile, there had been a series of new books on the Dynamics of Machines: I had the good luck to have been the first to write one [26], in 1954, from notes I developed for a

senior course. In the next three years three others appeared as my competition: in 1955 Holowenko [27], in 1956 Hartman [28] and in 1957 Mabie and Ocvirk [29]. Later, in 1960, came Maxwell [30], in 1961 Shigley's second book [31], and in 1968 Hirschhorn [32] (from Australia). There was also a good lab. exercise book of problems by Leutwiler [33] (1956).

In the early sixties and even before that, interest in the work of German and Russian authors had led quite a few to make translations. In 1959 Al Hall's co-workers published a translation of some of Dimentberg's work on screws [34], while Del Tesar as a graduate student translated some of Mueller [35]; these were both published as University Reports. In 1963 Herb Kuenzel's translation [36] of Beyer's "Kinematic Synthesis of Linkages" was published as a book; and Tom Goodman gathered a group of volunteers who translated Kurt Hain's large volume on Applied Kinematics [37], which appeared in 1967, but Tom had died before the project was completed. By other efforts, both the Air Force and N.A.S.A. were persuaded to translate and publish several volumes of Russian work, for example [38,39]. In those days, too, Dick de Jonge began to talk glowingly of the work of an Italian, Allievi, which he was translating; when he died I inherited this manuscript, only to find he had only half completed it. If anyone would like to finish it, please contact me.

This strong interest in translations led many to realize that there were no good English equivalents to a number of foreign terms. Dick Hartenberg and Tom Goodman published in 1960 (as an ASME paper) "Kinematics: A German-English Glossary," which made a profound impression, so much so that three years later an echo came from Moscow, when the Soviet Academy of Sciences published a German-English-Russian glossary. Then in 1964 a second expanded ASME paper, "Kinematics Glossary -- Russian, English, German" was written by the same authors, along with Artobolevskii, Levitskii, Vil'dt and Grodzenskaya of the USSR Academy. Then in 1965 French and Bulgarian equivalent terms were included in a booklet "Concise Terminological Dictionary on Kinematics and Dynamics of Mashines (sic)" published in Sofia.

Professor Freudenstein at Columbia had a doctoral student, George Sandor, who in 1959 presented an outstanding thesis on using complex numbers as vectors for 3, 4 and 5-position synthesis of a four-bar, thus finding new aspects of Ball's synthesis. This work was preceded by the earlier article of K.H. Sieker in Germany. But as early as 1961, Freudenstein was listing 37 different digital computer programs [40] that were available that had been written by himself or others at Purdue, Northwestern, Michigan, Ohio State or by IBM and Bendix. Running times of two to four minutes on an IBM 650 are mentioned. I remember at Georgia Tech in 1964 if we wished to use the computer, we prepared decks of punch cards, handed them in to an operator, and came back next day for any results, if it didn't abort. There was no quick interaction to get rid of grammatical or syntax errors. In 1962, George Sandor joined me on the faculty at Yale as an Associate Professor.

One result of my listening to Hain's course of lectures in 1961 was a fascination with the "Number Synthesis." Teaching the same course the following year, by chance I assigned to my students as homework to find out the number of ten-bar chains with pin joints only; I started hastily to try to find out the answer myself, but to my chagrin, this took me months of work. I had lunch very frequently with a Yale professor of Maths, Oystein Ore, who was a specialist in "graph theory" and had written a book on it. I guess I described to him one day my dilemma of the ten-bar linkages, and he suggested that I try inverting the linkage. It worked famously and so I had a proof of how many eight-bars exist. Then I learned from a chance remark of Freudenstein that he also was working on something similar, and this stimulated me to compete. The earliest opportunity for me to present my work was a Conference on Theoretical and Applied Mechanics in Atlanta, Georgia in 1964, (though the proceedings were not published till the following year) [41]. The next year, Freudenstein and his doctoral student Dobrjanskyj presented their work [42] at the International Congress of Applied Mechanics in Europe. In the summer of 1964 I was also in Europe lecturing in Berlin, and discovered that Professor Alt had made a study of ten-bar linkages, and his unpublished manuscript was still in the departmental files [43]; this collection showed me that I had missed a few groups, but that Alt had a few duplications. Thereafter Dr. Woo [44] described an entirely different method to determine the correct total of distinct varieties that exist.

Changes in the Purdue Conference

By the early 1960's, the Mechanisms Conference had grown too big for its sponsors -- *Machine Design* magazine. And the editors could no longer guarantee that all the papers presented would be published. This of course was a serious hindrance to young professors who depended upon published work for advancement. But there was another, more subtle effect taking place -- the new fascination with mathematical, graphical or electronic methods of analyzing or synthesizing the four-bar chain. The first conferences had featured articles by engineers in industry solving actual design problems; this is what the magazine wanted, for their readers were draftsmen and designers working at the board. The more abstract ideas of a "function generator" or "curved path synthesis" were outside their ken.

To illustrate this, I remember writing to Ben Hummel, the editor of *Machine Design,* asking him if he would be interested in a series of translations of recent writings of Hain, for I had just attended a short course given by him in Germany. This is part of his reply, dated 6/26/61: "Here I go again: the précis is not too appealing to us. Value to the reader vs. space occupied is the problem. We have not played the Purdue Conference, co-sponsored by us, as heavily as we could have....Recently we've selected only some of (the papers to publish.) The criterion has been: Direct job help." "Development of mechanisms is not supported commercially by anyone -- Nobody sells a 'mechanism' in the sense that bearings, motors, are sold."

The organizers of the conference began to look around for a new sponsor, and the Machine Design Division of the A.S.M.E. came forward, for most of the faculty people attending the Purdue conferences were members, and some of them officers, of this division anyway. Thus in October 1964, the Eighth Mechanism Conference became the first ASME Mechanisms Conference; it was still held at Purdue, and the first elected chairman was Ferdinand Freudenstein. Two years later, Dick Hartenberg became chairman of the Ninth Conference, still held at Purdue, in 1966. But by then sentiment favored moving the conference to different university

sites, and for the Tenth Conference, several places were considered. I put in a bid for Georgia Tech, and this was accepted: Bernard Roth was appointed Papers chairman; the terms of office ran from about November 1966 until November 1968, so we had two years for preparation. That's when my wife and I first made contact and went to Russia, to attend their conference. But before that, we started The Journal.

Beginning of the "Journal of Mechanisms."

In 1963 during the summer school at Yale, the complaint was frequently heard that it was too difficult to get ordinary research papers published, since *Machine Design* had curbed its welcome. Many people requested a new, high-standard publishing vehicle, and from this the *Journal of Mechanisms* evolved. In the Fall of 1964, I was invited by Bill Johnson, who was then Professor of Mechanical Engineering at the Manchester Institute of Science and Technology in England, to give a graduate course on Mechanisms over there. I jumped at the opportunity to carry some of the new work being done back to my homeland. Prof. Johnson was editor of the *International Journal of Mechanical Sciences,* published by Pergamon Press; he introduced me to Robert Maxwell, chairman of this publishing house, and so our Journal was born, which later changed its name to *Mechanism and Machine Theory,* when it was adopted as the official voice of IFToMM. To begin, I undertook the two tasks of obtaining articles for the first (and subsequent) issue, and of inviting supporters to become members of an "Honorary Editorial Advisory Board"; everyone I asked, from Russia, Germany, U.S.A. or Japan agreed to join. The first issue came out in the spring of 1966. In this enterprise, as in the organizing of the two conferences at Yale, I was extraordinarily fortunate to enlist the collaboration and support of my wife, May, who became editorial secretary and general factotum for the Journal. There was entailed a lot of detailed work and record keeping: to acknowledge receipt of work from an author, assign it for review, chivvy the reviewer to get it back, etc., and to my formal business letters, May would add little postscripts of friendly contact or news. These letters went all over the world, and in this way we felt we achieved a familial community of the brethren of kinematics. It was good.

Contacting the Russians and East Europeans.

By 1966, many of us were well aware that there was a large body of scientists actively developing kinematic theory in East Europe, and that this was built with great pride upon the work of P.L. Chebyshev. As early as 1961, Prof. Freudenstein was including the Chebyshev approximation theory in his graduate courses in Mechanisms as he stated at the Yale conference [45]. For those others of us who found reading Russian difficult, at least a French translation of his collected works [46] was available in U.S., and some other Russian work had been translated into German and published; for example, S.S. Bloch. And in 1966 and 1967, in the first two years of the *Journal of Mechanisms,* articles in English by the Russian authors Dimentberg, Yoslovich, Tavkhelidze, Artobolevskii and others were appearing.

In 1965 the Russian hierarchy made the first move to create an international conference where specialists from many countries could meet: a group in Bulgaria, headed by Professors Balevski and Konstantinov, organized a conference at Varna held September 27-30, 1965, to which, however, no American went, though I believe one or two were invited. At that conference, an "International Coordinating Committee" was established to form a Union to call future conferences, and from U.S.A. Professor Sandor was, I believe, invited to join. The proceedings of this conference are listed by abstracts in the *Journal of Mechanisms* [47].

In their next move, I received a letter from Academician Artobolevskii in October 1966, announcing that the Fifth Soviet Conference on "Contemporary Problems in the Theory of Machines and Mechanisms" would take place between 2nd and 7th October 1967 in Sukhumi in Soviet Georgia and urging me to attend it. No American had been to any of the preceding Soviet conferences; so I decided to go, as a representative of the American Conference, of course, and since the host of the conference was Professor Tavkhelidze of the Georgian Institute of Technology in Tbilisi, I was able to make a play on our titles, that it was the Professor of Mechanisms from the (U.S.) Georgia Tech who was visiting his opposite number, the Professor of Mechanisms at the (Soviet) Georgia Tech. My wife accompanied me. Prof. Douglas Adams from M.I.T. also attended, for Prof. Den Hartog had also received an invitation, and passed it on. Professor Meyer zur Capellen and his wife from Aachen also took part. We five were the only westerners there.

During this conference Academician Artobolevskii spoke to me of his desire to establish a new international Federation for the Theory of Machines and Mechanisms, and I agreed to work with him on the idea. We called it "IFToMM". This meant that somehow I needed to organize a "national committee" to represent the U.S. on it. Meyer zur Capellen agreed to do the same for West Germany. And then it would need a Constitution: Artobolevskii and Bessonov offered to compose a draft, but as it turned out, it had to be written in English, so I wound up writing it, after studying the Constitutions of IUTAM and IFAC and one or two others, that I obtained from the National Academy of Sciences in Washington.

The Sukhumi conference was very well done; there was a marvelous performance of folk music and Cossack dancing one evening in the State Theatre, specially put on. And Georgian hospitality was abundant and overflowing: Soviet Georgia is famous for its wines and brandy. Afterward, my wife and I were invited to drive two days through the mountains with the Tavkhelidzes to Tbilisi and then to stay at their home.

The Tenth Mechanisms Conference.

This Russian conference was a valuable prelude to the ASME Conference to be held a year later in 1965, for I was able to send invitations to many I had met. I had learned that in communist countries the only way for anyone to go to a foreign conference is to be sent by some government bureau, officially as a delegate, and this requires that they hand in an official written invitation about twelve months ahead of time. Between this Russian conference and the *Journal of Mechanisms* file, I had accumulated a great number of names and addresses, and sent very many invitations. Artobolevskii himself, however, suffered a heart attack and was forbidden to fly, so the only Russian who eventually came was Bessonov, a friendly, ingenuous man with huge bushy eyebrows. There was however a good international group attending (see list).

Foreigners attending the Tenth Mechanisms Conference
Atlanta, Georgia
October 6-9, 1968.

Australia:	K.J. Waldron (Sydney)
Bulgaria:	M. Konstantinov (Sofia); M. Rashev (Sofia)
Canada:	J.R. Colbourne (Alberta); D. Scott and L.E. Torfason (New Brunswick)
Denmark:	P.W. Jensen (Copenhagen)
Germany (West):	K. Hain (Braunschweig); H.W. Mueller (Darmstadt); W. Thomas (Bocholt)
Netherlands:	E.A. Dijksman (Eindhoven)
Japan:	H. Iyoi (Chiba); K. Ogawa (Tokyo)
Rumania:	N.I. Manolescu, R. Bogdan and C. Pelecudi (Bucharest)
United Kingdom:	T. Davies (Swansea); J. Duffy (Liverpool); E. Primrose (Leicester)
U.S.S.R.:	A.P. Bessonov (Moscow)

My wife and I held a grand garden party at our home for all the authors and foreign guests; over two hundred came, liquor and food in quantity were consumed and I believe it was a great success.

A New Organization -- USCToMM.

Another of the sideline events of this Tenth Conference was a meeting to establish a "United States Council for T.M.M." and as officers Doug Adams, Phil Barkan, J.P. Den Hartog, Eric Ungar and I were elected. The basic reason for a new organization was that the A.S.M.E. by its charter was a purely American society, and had refused, because it was unable, to join IFToMM when I asked. A similar situation had obtained earlier with respect to I.F.A.C. -- the International Federation for Automatic Control: they formed a national committee for automatic control, ACAC which, to raise money for their international dues, put on the well-known Joint Automatic Control Congress; and ASME had agreed to be one of a half-dozen national societies that supported this JACC, and indirectly IFAC.

As soon as IFToMM was founded a year later and the United States delegation voted to join it, the United States Council (U.S.C.T.o.M.M.) assumed the obligation to pay the international dues of $2000 per year. The Council decided to incorporate itself as a non-profit educational entity in Massachusetts, and Prof. Doug Adams, being a lawyer, drew up and processed the needed papers. In the first two years we had good support from the General Electric Co. and from I.B.M., both of which sent representatives to several meetings we held in Boston. But raising this amount of money annually was a problem. There were several options: to get it from the Federal Government, from industrial foundations or companies, or by membership fees. I held several discussions with the staff at the National Academy of Sciences in Washington, to see if they would adopt us as one of the group of international Unions that the U.S. Congress has voted to join and pay dues for. I personally was unhappy about trying to obtain grants from a few wealthy persons or foundations, because I felt we needed to aim for widespread national participation; and this meant that we either had to run our own conference, a rival to the Mechanisms Conference of the ASME, or that we had to jointly sponsor some conferences and hope for a share in the profits, if any. The Board of Directors were all ASME members, and unwilling to compete with ASME. At our second Annual Meeting, Chuck McLarnan came as a delegate from the Machine Design Division, and asked the Council to become a permanent committee of the Division, promising that a surcharge would be added to the Registration Fees for future ASME Mechanisms Conferences enough to pay those international dues. With this promise, the Corporation voted to dissolve itself. The ASME, however, reneged on its commitment: a surcharge of, if I remember correctly, five dollars was added by vote of the Machine Design Division to the registration fee at the next ASME Mechanisms Conference, held at Columbus, Ohio; Chuck McLarnan being chairman. ASME however pocketed all this earmarked money on the grounds that the conference had not made a profit. I tried this a second time: in those days the ASME Gearing Committee was small and in trouble; at ASME meetings it could support only two or three sessions. I suggested that by working together we could convert their next national meeting into an international one. They were very skeptical, for in those days AGMA held the important meetings. I was then chairman of IFToMM's Conferences - planning Commission, and had been told by Europeans that the U.S. was five years behind in our gear manufacturing standards, testing procedures and quality. So again for an agreed five dollar surcharge on the ASME Gearing Conference registration fee, and for IFToMM co-sponsorship printed on the program, I undertook single-handedly to try to bring in foreign participants and enlarge their conference. These efforts were very successful, for a large group of Japanese attended along with others from England, France, Germany and Hungary. The Conference was held in San Francisco, but never a penny was passed afterward from ASME to USCToMM. Consequently USCToMM was not able to pay its dues to IFToMM that year.

ASME did, however, change its operating rules to permit it to belong to international organizations, and thus ASME became the American national member of IFToMM.

I.F.T.o.M.M. - The International Federation for the Theory of Machines and Mechanisms.

The Federation was established during the World Congress which was held in Zakopane, Poland, from 23 through 27 September 1969. Thirteen national delegations voted then to become charter members: Australia, Bulgaria, West Germany, East Germany, Italy, Poland, Soviet Union, United Kingdom (Britain), United States, Yugoslavia, Hungary, India and Rumania. A further three, Czechoslovakia, The Netherlands and Norway, were provisionally accredited. (By 1975 it had enlarged to have 18 member countries.) The U.S. National Committee for T.M.M. was composed in 1968-69 of seventeen members: Messrs. Adams (MIT), Barkan (Gen. Elec.), Crossley (Georgia Tech.), Den Hartog (MIT), Gartner (Conn.), Hall (Purdue), Hartenberg (Northwestern), McLarnan (Ohio St.), Michalec (Stevens), Muster (Houston), Rothbart (F. Dickinson), Sandor (Rensselaer), Tao (IBM), Roth (Stanford), Uicker (Wisconsin), Ungar (Bolt, Beranek & Newman), and Yang (U. California Davis). Many of these got to Poland for the Congress, and Doug Muster was elected chairman of the U.S. delegation which signed the protocol to join IFToMM.

This inaugural meeting was called the Second Congress, strangely, out of courtesy to the Bulgarians, who had organized and held a previous "International Conference on Mechanisms and Machines" in Varna 27-30 September 1965, to which

scientists from East and West Germany, England, Australia, as well as from East European countries, had gone, although none from USA, as mentioned already.

The inaugural Congress in Poland had also been preceded by an executive conference in Varna, Bulgaria earlier the same summer (2-4 July 1969), to which George Sandor and I went representing the U.S.A. Plans for the program and procedures and simultaneous translation at the Plenary Session were approved, but the chief accomplishment was approval of a draft of a Constitution for IFToMM. The writing of this Constitution had been another of my jobs: I had obtained through our National Academy of Sciences' Foreign Secretary copies of the Constitutions of several other Unions and Federations, and so I composed one for us. This was passed back and forth to Dr. Michael Konstantinov, a vigorous able man, in Bulgaria many times and to Artobolevskii in Moscow, until I wrote a third version. Then, right at Varna, Konstantinov produced his Fourth Version, in a peculiar English which he did not speak and I could hardly understand. Oh boy! It had to be presented for approval the next day, so Konstantinov and I sat down to negotiate at an all-night session, and I remember waking up a dismayed secretary at 4 a.m. to get our Fifth agreed draft typed up and duplicated before the committee met at 11 a.m. In spite of that pressure, the text has stood up, with only minor changes, ever since. I have copies of all the early drafts.

There is so much history to IFToMM that we have little space for here. The Third Congress was a wonderful one, held at Kupari, just outside Dubrovnik, Yugoslavia, in 1971. Beside all the papers and committee sessions, there was a formal reception by the Mayor of Dubrovnik in the Castle; and a marvelous dance in the main hotel. Two neighboring hotels were involved, and next the smaller one lay the private villa of (former) President Tito and a guest house, which happened to be occupied just then by Richard Burton and Elizabeth Taylor, who were making a film of Tito. They had a chauffeur who had a Rolls Royce, who drove them half a mile each day to the square in front of our hotel, where they took off by helicopter.

At the Zakopane, Poland, IFToMM Congress, the convention had elected officers: Professor Artobolevskii as President, me as Vice President, Dr. Werner Thomas of West Germany as Treasurer and Dr. Michael Konstantinov of Bulgaria as Secretary General. At the Third (Dubrovnik) Congress, our terms of office were continued for four years (for a total of six), except that Dr. Thomas stepped down in favor of Professor Walther Meyer zur Capellen, and Dr. Konstantinov was replaced by Dr. Emil Stanchev (also of Bulgaria), a poor choice of the Party, because he could write and understand no German nor English, and consequently communications rather collapsed.

According to the Constitution, the Officers formed an executive council which had to meet annually between Congresses. The first such meeting was in Paris in 1970, at which we had talks with an officer of UNESCO to explore the possible affiliation of IFToMM with that body, but this did not succeed. In 1972, the meeting was in Cologne, and Chuck McLarnan came with me to participate. Then in 1973, we met in Split, Yugoslavia, and in 1974, Chuck McLarnan went in my place to the Council meeting in Italy. I was trying hard to get more people from here to undertake to play a more active role in the Federation. 1975 was the year for the Fourth World Congress, held in Newcastle, England; Professor Maunder was elected the second President of IFToMM, and a new team took over.

IFToMM has also a Standardization Commission, on which talented, modest Jerry Lowen worked patiently for many years; its job was to define mechanism terminology concisely.

Other Organizations - The Aerospace Mechanisms Symposium

In May 1966, the first Aerospace Mechanisms Conference was held at the University of Santa Clara, California. This started a series of annual conferences, co-sponsored by the University of Santa Clara (Richard K. Pefley and Stein Weissenberger being the chief organizers), Lockheed Missiles and Space Company (George G. Herzl and Alfred L. Rinaldo) and the Jet Propulsion Laboratory of the California Institute of Technology (Peter T. Lyman and William J. Schimandle): Dr. Herzl served as the editor of the Proceedings, which were prepared under contract with N.A.S.A. - the National Aeronautics and Space Administration. The objective of the Symposia was, in the words of Peter Lyman, "to provide a forum for the open discussion and interchange of real problems of interest to the designer of aerospace mechanisms" - such as a closed tubular extendible boom, a 1500 ft. long antenna array, a despinning device for a satellite, a solenoid-actuated camera shutter and shock absorbers for a landing vehicle.

I remember, sometime after I had been nominated by Dick Hartenberg in October 1966, to be the chairman of the next ASME Mechanisms Conference, saying to him that I wanted to try to work out some sort of better working arrangement between our two conferences; and he warned me that they wanted none of it. Nevertheless, I decided to attend one of their Symposia officially, and invite all of those attending to come to our next conference in Atlanta, so I went to the Third Aerospace Mechanisms Symposium, which took place at the Jet Propulsion Lab. in Pasadena, on May 23-24, 1968. I was sharply snubbed; in a subsequent letter of 5/31/68 to F. Freudenstein, I reported "I found myself unwelcome ... I (asked if I might) announce our conference and say all would be welcome to attend: this was vetoed, but then allowed during the final 'next-year-planning' session, provided I did not appear personally but rather handed in a written announcement to be read by the chairman. And so it was done." Later, however, I had a good talk with Dr. George Herzl; he was very interested in my direct contacts with Academician Artobolevskii, who had promised at that time to come to Atlanta to the Conference in October 1968. He urged me to propose to the Russians a joint symposium for open discussion of satellite design problems between experts of our two countries. Thus started an interesting story.

George Herzl was a slender black-haired man of medium height, who had a keen mind; his own specialty was the design and operation of extendible stabilization booms for satellites and gravity-gradient libration and nutation dampers [48].

I transmitted Herzl's proposal to Artobolevskii at once; the favorable response from the Soviet Academy of Sciences was brought to the Mechanisms Conference by Prof. Arkady Bessonov. I recall how with considerable agitation he asked for a private conference and an interpreter, for with so important a message to divulge, he was not satisfied with his knowledge of English; so we met and he transmitted to me the message that the Soviet Academy of Sciences had approved of Dr. Herzl's scheme. I had not known at the time that Artobolevskii's