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An array of scarlet and grey booklets: 65 years of the Symposium on Molecular Spectroscopy

Brenda P. Winnewisser

Department of Physics, The Ohio State University, Columbus, OH 43210, United States

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ABSTRACT

A history of the OSU International Symposium on Molecular Spectroscopy is presented with a broad brush, inspired by looking at the evolution of the program booklets of the meeting, and drawing upon a selection of abstracts, all of which are now accessible on-line, and on reminiscences. The important and enduring aspects of the meeting from the perspective of the author are identified, and a few of the changes traced. The essential contributions of the founders and successive official hosts of the meeting, Harald H. Nielsen, David Dennison, K. Narahari Rao, and Terry A. Miller are acknowledged.

1. Introduction

For 65 years, a throng of physicists and chemists, with a smattering of astronomers and the occasional engineer or biologist, has gathered annually for a week of intense information exchange at The Ohio State University (OSU). This meeting has been highly influential in the development of molecular physics and chemical physics. An overview is presented of the evolution of the meeting, aiming to give some idea of the impact of the meeting and its relationship with the two analogous though independent European meetings.

What pushed this meeting into being in 1946? Why not 1936, or 1956? It was not an accident; it was a unique situation. The United States in 1946 was recovering from the war effort. The industry, indeed the entire economy and population were undergoing a massive shift to a peace-time economy and to domestic and personal needs. A large portion of the physical scientists in academia had been drilling officer-candidates in basic science and engineering since 1942, in "accelerated programs" at various universities and many regional special schools. Scientific conferences, pure research and personal careers had remained a low priority all that time. Even women's colleges had been drained of technical teaching staff. At last a gigantic reboot of the whole country was going on.

Meanwhile, exciting advances in chemical physics theory in the late 1930s, such as the detailed treatment of the quantum-mechanical asymmetric rotor, and theories about the chemical bond, were sitting out there, waiting to be exploited. A host of theoretical questions and methods of approximation were available to be explored.

Even more stimulating to the scientific community, enormous technical advances had been made in radio, microwave and optical apparatus and applications, in the new radar technology, and in electronics in general, all driven by wartime military research. The first computers had been conceived. Not only that: much of this technical equipment was available literally dirt cheap, army surplus. Just as an example, 2K-33 Klystrons were one dollar per pound. Entire Dumont oscilloscopes were similarly, ridiculously, cheap. Right up into the 1960s, when the author was introduced to research, a pickup-truckload of such material for a few hundred dollars would almost have sufficed to set up a microwave laboratory.

In all these ways, 1946 was a very special time, calling for expanded communication between scientists. Molecular science had acquired, with the maturing of quantum mechanics, a decidedly interdisciplinary nature. The Journal of Chemical Physics was founded in 1933. All of this pointed to a meeting bringing together those interested in molecular structure and spectroscopy outside of the bandwidth of the traditional discipline associations, principally the American Physical Society or the American Chemical Society. There had been several isolated meetings of this sort

E-mail address: winnebp@physics.ohio-state.edu

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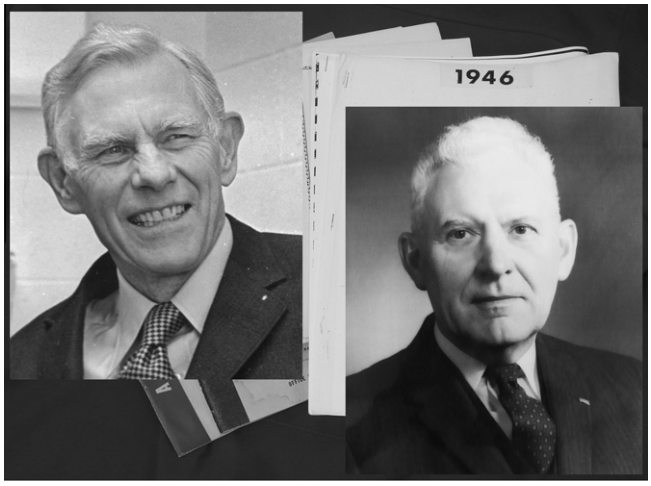

Fig. 1. Portraits of David Dennison and Harald H. Nielsen.

before the war, but the new meeting was to acquire a particular momentum, that propelled it into the future.

In close proximity in the Midwest, we find at that time David Dennison, of the University of Michigan (UM), and Harald H. Nielsen, of OSU, shown in Fig. 1 in portraits taken many years later. Both men were gifted theoretical physicists who were stimulated by problems arising from experimental work. Both had a strong Copenhagen connection; Dennison spent 3 years in Niels Bohr's Institute, with Nielsen overlapping him there one year. Discussion was constant in that hothouse of theory; Bohr himself always needed to talk his way to an understanding of quantum mechanical problems and their solutions. It is not surprising, then, that these two friends decided "Let's have a meeting." The time was ripe, they were in the middle of the country, and they both had by that time sufficient prestige to lend weight to invitations to participate.

In June of 1946, at the invitation of Nielsen and Dennison, 30 scientists gathered at the OSU campus for a small meeting which the hosts entitled "Symposium on Molecular Structure and Spectroscopy", to address experimental and theoretical aspects of the structure, dynamics and spectra of molecules, with an emphasis on small, isolated, gaseous molecules. The theoretical issues of concern were quantum-mechanical phenomena of molecular physics – quantum mechanics was only about 20 years old! The spectral regions considered ranged from radio-waves to the ultraviolet region, wherever molecules can emit or absorb photons. The discussion should transcend technique or discipline. The meeting had to be cheap; the war was just over. Funding for research was not as generous as it later became with respect to travel. It should also include as active participants students involved in research, which was another reason to keep it cheap. Therefore, it was decided to house the participants in dormitories, a mode of accommodation that had developed for academic gatherings on campuses even before the war.

The meeting was a success, and Dennison and Nielsen planned at that point, if not earlier, to hold it every year, alternating between OSU and UM as host universities. For logistical reasons, the first meeting took place in Columbus; for other logistical reasons it was repeated there, and never did move from Columbus. As the meeting grew, its growth continually reinforced the decision to keep it in Columbus. Skill in running a meeting was acquired, and scheduling at a large university is easier if an event has already claimed a space in the calendar. The result is that *I know of no meeting series that gives such good service and outstanding science for the equivalent expenditure of money, time and stress, and a comparable opportunity for young participants.* This summarizes some important reasons that a large number of participants have been coming for an unusually large number of years. The meeting thrived in symbiosis with the Department of Physics, and since 1992 continues to do so with the Department of Chemistry, at OSU.

2. Superficial but symbolic changes

It took 10 years for the program and abstract book of the Symposium on Molecular Structure and Spectroscopy to mature from a few typewritten pages to a substantial booklet in the OSU colors as shown in Fig. 2. At first only a schedule with titles was printed. Starting in 1951, abstracts and an author index were included in a small booklet. Failings of size and color were corrected step by step, and by 1956 the meeting booklet was letter size, and had settled on the OSU colors. This was the first of what so far are 53 scarlet-and-grey booklets, thousands of them today gracing the shelves of many hundreds of spectroscopists. A significant change occurred in 1976, when the word *Structure* was dropped from the title of the conference. Up through perhaps the 1960s, structure questions provided much of the motivation for pursuing molecular spectroscopy. As the field expanded and the meeting grew, by the 1970s many of the questions posed went far beyond structure. The meeting had become very large, with up to four parallel sessions, and the study of structure in the form of crystallography and gas electron diffraction had in turn begun to be represented at other meetings, so that it was decided to no longer include these fields explicitly in the OSU meeting. Since then the cover has changed noticeably in one further respect: the word *International* was added in 1991, declaring what was already by then an established fact, but which was otherwise not obvious, since the meeting remained firmly anchored in the middle of the North American continent.

The first meetings were held in Mendenhall Laboratory on the central Oval, at that time the home of the Physics Department. It moved with the Department to the Alpheus E. Smith Laboratory in 1952, where free donuts were introduced at some point and henceforth, certainly since the 1960s, determined the topology of the trajectories of participants. The meeting registration desk and the donuts, as well as lecture halls, moved across the plaza to McPherson Hall in 2005, after the Physics Department moved to new quarters. The plaza itself remains a natural extension of the lecture halls for discussions. The subtle spatial move to the territory of the Chemistry Department reflects a shift that the center of gravity in the field has taken in the academic landscape, from

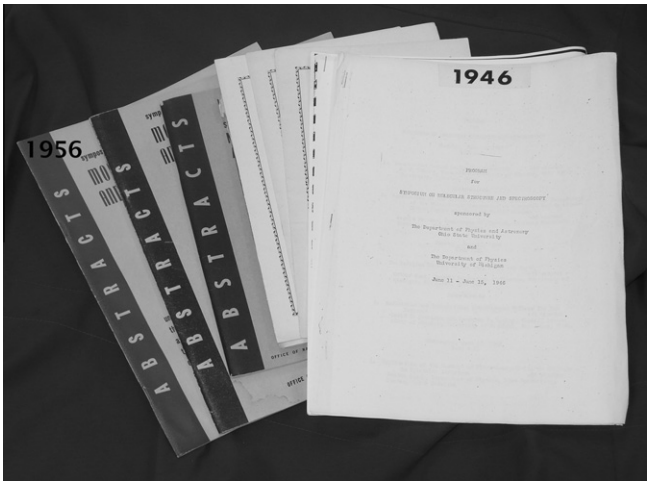

Fig. 2. Programs of the meetings of 1946, 1948, 1949, 1950, 1951, 1954, and 1956.

physics into chemistry. The perspectives, techniques and types of scientific questions posed in both disciplines have been vital to the exploration of atomic and molecular spectroscopy from the beginning, and this shift has had little effect on the nature of the science explored. One could perhaps also say that today, more chemists know a lot of physics and more physicists have learned a lot of chemistry than was the case 65 years ago.

Internationalization of the meeting began accidentally, due to foreign spectroscopists visiting in the US. Of course the Canadians were invited from the beginning. Foreign speakers began to be regularly invited by the 1960s. The various institutional and commercial sponsors have been important in financing this aspect of the meeting. In 1991 the name of the Symposium was changed to reflect the international nature of the event. That is also the year that the International Advisory Board was set up, a change that was more than superficial.

The names of most if not all of the pubs favored in the evenings by the participants have changed, but the value for communication of proximity to the establishments across High Street and Lane Avenue has remained unchanged. The older, segregated men's and women's dorms south of the Oval were abandoned in 1964 for new, air-conditioned, but still segregated North Campus dorms, and it was another year before the organaizers realized that one dorm should be available for those with spouses, and several more years before all such segregation was sensibly abandoned. Certainly hanging out on the steps of Taylor Tower or in one of the north campus dorm lobbies has been a good way to strike up new or renew old acquaintances.

3. Leafing through the programs

All the titles, and since 1951 all the abstracts except those of 1953, for which we have only titles, have been scanned, and are accessible via the meeting website [2]. There you will find a path to the OSU Library's Knowledge Bank [1]. Their page allows you to select a "community" entitled *OSU International Symposium on Molecular Spectroscopy*, which is divided into "collections", one for each decade. These can be searched by date, author or title. Each abstract is stored as an image and in addition has been keyed in, like the submitted abstracts since 1995, in TeX format. In addition, since the central uploading of Powerpoint presentations was implemented in 2003, and if the authors have agreed, these presentations are part of the material accessible through the website and the Knowledge Bank. Each talk can be addressed by an individual URI, as will be illustrated in the references to this paper. The full set of booklets is held by Terry Miller and a partial set currently by the OSU Archive. There are currently no abstracts for invited talks before 1990; the expectation for such abstracts, only indicated to authors starting in 1992, represented a shift in the speaker's obligations which did not take effect immediately.

What follows are some vignettes suggested by the program booklets, biased of course by my own perspective. I had to refrain from attempting a history of molecular spectroscopy, so much is omitted. Looking through the programs for the first time, I thought I would find a development from the study of the most obvious small molecules to larger and more complex systems, or from one or two spectral regions to all, from gas phase to liquids and solids. But my major impression upon perusing the programs was that the meeting has changed remarkably little since 1946. I was very surprised when I discovered this.

We look first at that initial meeting. The 30 talks varied in length from 15 to 50 min. They started Tuesday morning (Monday was for travel) and went through Saturday, with one last talk after lunch. Wednesday evening there was an excursion to the wonderful Perkins Observatory in Delaware, Ohio; a penciled comment on the program reads, "Nice try, but it rained". Of the 30 talks, 11 were from commercial entities, reporting on instrumentation and applications. The rest covered theory, experimental spectroscopy in the various regions of the spectrum, chemical physics, pedagogy and astronomy.

The next year, 1947, there were 53 talks: nearly double the number in the first year. They started Monday this time, running again through Saturday noon, to fit everyone in. There were traditional sessions as in the first year, but there was something that I had thought was a relatively recent feature: a special session. In this case it was on medical applications of spectroscopy. There was, in fact, a different one nearly every year, even in these early meetings. The scope of special sessions ranged from medical chemistry applications to astronomy and the Earth's atmosphere – all in the first 3 years, and that span is not much different today. The former subject, of course, is now more likely to be called biomolecular spectroscopy. The special sessions were given more emphasis eventually in "Seminars" with one or more 30-min talks, with increasing frequency through the 1980s and 90s. Mini-symposiums, organized by one or more of the people working directly in the given field, were formally introduced in 1996, assuring an insightful sampling of the given field. In all these forms, these flexible variations on the standard categories of spectroscopy broaden the range of people who could be interested in the meeting, and introduced the core of the spectroscopic community to a wide range of research in which spectroscopy is one component.

From Foil Miller, who was there in 1946, we learned at the 60th meeting that the long, dense program creates a misconception. The overlap of involvement in different wavelength regions or types of molecules was not large in those days, the sessions were sorted according to such categories, there were no parallel sessions, and some participants put their golf bags in their cars when they headed for Columbus. This is harder to do today! But at least one evening was frequently used for scientific discussions at the early meetings. In 1948 there was an evening MEETING OF ONR CONTRACTORS for those supported by the Office of Naval Research. In 1954 the evening meeting was an INFORMAL DISCUSSION OF SPECTROSCOPY SYMBOLS AND NOMENCLATURE.

Participation, looked at geographically, spanned the nation the very first year, from Pasadena to Schenectady. The first visitor from abroad came in 1947, from Belgium, Marcel Migeotte, who was a guest professor the following year at OSU, working with Harald Nielsen. He gave three talks over the four years of this collaboration:

> 1947: RECENT OBSERVATIONS IN FRANCE AND BELGIUM ON THE TRANSMISSION OF INFRARED RADIATION BY THE EARTH ATMOSPHERE [3]
>
> 1948: THE SOLAR SPECTRUM FROM 2.8 MU TO 5.1 MU [4]
>
> 1950: INTRODUCTORY PAPER ON SPECTROSCOPY OF THE EARTH'S ATMOSPHERE [5]

Migeotte's talk in 1950 was part of that year's special session on atmospheric and solar applications of spectroscopy. This talk was an invited tutorial, the 40 min opener for a full session on that topic. This format was used systematically in the first years, as the opening talk in nearly every session, providing an introduction or survey of the status of the subject of the session. It proved to be a durable format, probably more necessary at that time than today, when travel and fast communication of information are so much easier, but still sometimes encountered in today's sessions, especially in mini-symposia. These survey talks are and were valuable particularly for students.

If we look now for technical developments reported at the Symposium, we come to a talk by Peter Fellgett, presenting in 1952 what immediately became known as the Fellgett or multiplex

advantage of an interferometer, with a round aperture, over a scanned grating with ever-narrower slits, particularly important in the infrared region. The problem and solution are presented so generally and succinctly that I quote the abstract in full:

MULTICHANNEL SPECTROMETRY

The sensitivity of infrared detectors is already so close to fundamental limits that further large increases seem unlikely. Improvements in the performance of spectrometers are possible by increasing the area of the source that is used, either by increased dispersion or by multislit techniques. It is theoretically possible to increase the intensity of the source almost without limit if the random thermal processes of radiation can be replaced by a coherent mechanism such as occurs in a radio antenna. This principle is the basis of modern microwave spectrometry, but there is no immediate prospect of it being extended to infrared frequencies. Semicoherent spark techniques perhaps deserve more attention than they have been given in recent years. Further improvements must depend on using more efficient methods of observation. A scanning spectrometer is very inefficient in that at every instant it rejects all but one element of the incident spectrum. This loss is avoided in a spectrograph by having effectively a separate radiation detector for each spectral element. Sensitive infrared image detectors are not at present available, but the spectral elements scan be measured simultaneously by a single detector if mutually orthogonal modulation patterns are impressed on the separate elements. A convenient method of producing the required modulation is by varying the path difference in a two-beam interferometer. No dispersing system, such as prism or grating, is then required.

The theory of this method, which may be termed multichannel spectrometry, has been investigated in detail, and it has been confirmed experimentally that the method gives spectra with the expected resolution and that the theoretical increase in sensitivity is realized.

The multichannel method is especially appropriate to astronomical spectrometry, where no improvement in intensity or area of the source is possible [6].

This is one example of conceptual innovations to which the community was introduced at these meetings. Note the comment about microwave spectroscopy, in Fellgett's summary of the basic challenges of spectroscopic measurement. Microwaves provided at the time the only coherent spectroscopy, allowing Doppler resolution at low frequency and thus revealing all kinds of fine and hyperfine structure, and driving some of the theoretical developments.

Moving along roughly chronologically (and aiming deliberately to include all the speakers at the history session of the 2010 Symposium, other than the author), one landmark is the first talk given at Columbus by a frequent and eminent attendee, Bob Curl, in 1958. He reported on

THE MICROWAVE SPECTRUM, BARRIER TO INTERNAL ROTATION AND STRUCTURE OF METHYL FORMATE [7].

This molecule has now attained notoriety as a prominent interstellar weed, and modeling its internal rotation fine structure is still not trivial. Note that Talk TJ04 at the 2010 Symposium was about the rotational spectrum of an isotopolog of methyl formate [8]. This shows that many of the molecular systems selected for study decades ago remain relevant, if for different reasons, as methods of investigating them offer new options: Water, ozone, carbon dioxide, ammonia and methane are of course the most prominent of such species, and are found in the Symposium programs almost every year.

The next year, 1959, brought another important first talk: Jon Hougen debuted here with a talk on

THE FARADAY DISPERSION OF O_2 [9].

Already Jon had found an anomalous and for most of us obscure effect which he managed to mathematically tame. Most of his numerous contributions to the Symposium have been anything but obscure, and have enlivened and illuminated many corners of our science.

That same year, we find Marilyn Jacox in the program for the first time. Her talk on

THE VIBRATIONAL SPECTRA OF THE CRYSTALLINE METHYL HALIDES [10]

is an indicator that spectra of molecules in the solid phase have long been a small, but robust component of the program.

As a second point, her debut in Columbus allows me to touch upon the subject of the participation of women. They were there from the beginning. There were more than I realized before studying the programs. There were two women, Frances Bell and Eleanor L. Saier, listed as second authors on two talks among the 30 contributions in 1946, and I wager that there were at least that many among the audience of students and members of the Midwest scientific community who came to listen and learn, as they do today. At the second meeting, Hertha Sponer, Professor of Physics at Duke University, chaired a session, returning the next year to give an invited talk, on

ELECTRONIC SPECTRA OF POLYATOMIC MOLECULES [11],

the opening tutorial talk of the relevant session. She also gave talks at three later meetings, the last time in 1957 [12].

Here I have to diverge further: Sponer was part of another reason that this meeting was initiated and thrived in the US. She was one of the many European physicists and some chemists who left Germany and Europe before World War II, and who, after beginning to understand the quantum mechanics of atoms, had already been turning to the challenges posed by molecules – various many-body problems, some of which are still troublesome today. Besides Sponer, there were others, including Herzberg, Teller, Heitler, London, and Franck, who made significant contributions to molecular quantum physics and spectroscopy, and who were by then in North America. Sponer was not Jewish: she left because under the Nazis, as a woman, she had no hope of employment in the German academic world. During the years immediately after the war, scientists in Europe were literally digging their institutes out of the rubble. Meanwhile the American physicists and chemists, the immigrants as well as American-born talents, many of whom like Dennison, Nielsen, and Pauling had studied in Europe, were making great strides in molecular science. The strength of the Columbus Symposium was derived in part from the lead that the US took through the work of these people and their students in chemical physics and spectroscopy after the war.

To return to women at the meeting and the year 1948, there were three other women on the program besides Hertha Sponer. That is 4 out of 83 participants: nearly 5% of the talks! This is statistics with small numbers, but small was all the numbers we had back then. The proportion of women among Ph.D.s granted in physics did not rise above 3% in the US between 1935 and 1970, except for a pathological spike during WWII, when it hit a staggering 15% in 1945. There were, however, somewhat more women in chemistry.

From 1946 to 1960, between 0 and 6 women contributed to the Symposium each year, usually not the same ones. Two nuns, from Catholic teaching orders, were among them. It was easy to count the women in the early years, because up through 1956 the women

are listed in the author index in the correct British manner: men's initials only are given, but women's first names are written out. (In 1970, when I shared authorship of a book with my husband and his brother I had to protest when the proofs came back from an English publisher showing our names on the cover as G. Winnewisser, M. Winnewisser and Brenda P. Winnewisser, which was not how we had submitted my name.) It is noticeable that women in industry showed up as authors in larger numbers than women academics in the early years. Many of the women in industry who came to the meeting probably did not have doctorates, but the talks are evidence that they were engaged in research. France had far more women in spectroscopy, and in physics in general, than any other country. This is illustrated by two abstracts submitted in 1951 [13] and 1958 [14] by Jean Lecomte, a leading figure in spectroscopy in France who organized a meeting on molecular spectroscopy in Paris in 1953. His two very long abstracts each covered a wide range of experiments. The abstract for 1951 mentions six women and one man among his co-workers who were worth mentioning. The 1958 abstract includes the following references to his students or co-workers: ... *With the contributions of Mme. Vincent-Geisse, we have improved ... particularly suitable, according to M.F. Gans and Mme. G. Wagner ... as concerns solids, Mlle. Jeromec successfully ... Mme. Cameo-Bosco started working out the well-known method* ... [14]. I believe that even in France these ratios must have been anomalous.

1967 was the first year I attended the Symposium, where I was among 21 women, of whom 7 were contributing authors. That was 4% of the participants, and less than 2% of the authors, or 3% of the talks – however you reckon it, not much change from the 1940s. Starting about that time, a modest but steady increase in the number of women in physics and chemistry, and at the meeting, became noticeable. It became a subject of discussion. There was a long transitional period. When I first chaired a session, a good-natured Frenchman grinned and addressed me during the session with a bow and a chuckle as "Madame Chairman"; there were giggles in the audience. I learned to smile if I must. We all survived that period, and I will admit that I sometimes enjoyed the mixed blessing of being frequently the token woman. Today the women are an established fraction of the spectroscopic community. Out of the roughly 520 registered participants in 2010, 115 are women. We are very happy to be no longer considered oddities to be remarked upon. This is truly a tremendous change.

At that meeting in 1967, I expressed spontaneously to K. Narahari Rao, whose lab at the time was one of the two or three leading sources of high-quality, high-resolution infrared spectra, how wonderful it would be to see rotationally resolved IR spectra of the two molecules about which my husband Manfred and I were reporting in a microwave session. He equally spontaneously said, "Well, why don't you come here and measure them, then?" This led to my coming to Columbus the following spring for a semester, and seeing the next Symposium from inside. Rao contributed directly to the recognition of women in the profession. I can count six women whom he encouraged and mentored as graduate students, or ostdocs or otherwise: Linda Brown, Mary Ann Smith, Brenda Winnewisser, Malathi Devi, Romola d'Cunha, Sister Noel Dreska, and Karen Keppler Albert as graduate. One of them, Sister Noel Dreska, was required – not by Rao – to wear her classic black wool nun's habit even in the basement of Smith Lab, messing around otherwise happily and competently with pumps and big vacuum chambers and molecules. All of these women were or continue to be frequent participants in the Symposium. Professionally competent women were a fact of life for Rao. I can assure you that not all physics professors of his generation were so comfortable with women in their labs.

The full statistics of the meeting in 1967, taken as typical of that period, reveal something about the role of the meeting in the regional scientific community. That year, there were 247 talks, and 495 registered participants, or only 450 if you exclude OSU students and staff, in either case about twice the number of talks. The number of authors listed was in the same ballpark, 424, not surprising since a majority of the talks had two authors. But the number of participants whose names appear in the list of authors is only about 40–50%! Roughly half of the participants came just to see and hear what was going on and to meet people.

The talk that I remember most vividly from 1967 was an invited talk presented by Pierre and Jannine Connes, of which there is unfortunately no abstract:

> HIGH RESOLUTION PLANETARY AND LABORATORY SPECTRA IN THE NEAR INFRARED BY FOURIER SPECTROSCOPY [15].

For nearly all of us, this was our introduction to the possibilities of high resolution Fourier transform spectroscopy. Their project was considered so revolutionary, that they had not been able to get financial support for it in France. They were just returning from a few months' stay in California, where the people at CalTech had built a step-scanned interferometer ahead of time from Pierre Connes' plans. The Connes had gotten the system up and running, hardware and software, and promptly and dramatically demonstrated that this technique was a viable tool for measuring the IR spectrum of Venus with a telescope, an incredibly important advance in the study of planetary atmospheres. He presented the experimental aspects, she the information processing. Their two contributions were both spectacular. We grating-high-resolution folk all just looked at each other and knew, most of us for the first time, that gratings were doomed, except for certain narrow-band applications, particularly in astronomy. Rao saw it too, probably sooner than we had (he invited them), but he had just committed his last possible major sources of funding for an instrument to use the largest gratings ever ruled, 12 by 16 in., one of which he had just acquired for an evacuated 10 m spectrograph. Its competitiveness turned out to be more short-lived than we all had expected. Those talks provided one of the electrifying moments that such a meeting can bring.

New ways of generating exotic molecular systems led to a talk which probably has the shortest title in the history of the meeting:

C_{60},

by Rick Smalley, given in 1988, 3 years after the original experiments identifying the molecule. This was part of a multi-themed Seminar on "hot topics". It was followed by a Seminar dedicated to buckyballs and related fullerenes in 1991, shortly after the infrared spectrum of C_{60} had been observed, but when a large number of chemists and physicists still did not believe it existed. It was another 5 years before Curl, Kroto and Smalley were awarded the Nobel Prize for that work.

This brings us into what has indeed, changed about the techniques of spectroscopy. Options of what we can study, and how we can study it, have changed radically, as well they must, in 65 years. The biggest revolution in spectroscopy was that introduced by coherent light in the IR, vis and UV: lasers. Interferometers, as I just indicated, were a simultaneous revolution, and we seem to be well into another revolution with digital frequency combs. (This, by the way, is one of the directions identified in a list put together by Kevin Lehmann with input from the Symposium community for the program book of the 50th Symposium. You would enjoy looking at the whole list, to see which subjects they mentioned have and have not been explored in the last 15 years.) We have seen, in one of the 2010 mini-symposiums, results so far from the combination of these two concepts. Each of these new techniques allows us to measure systems that were not accessible to measurement before. With double resonance, lamb-dips, cavity

ringdown, beams or jets and femto-second time resolution we have moved step by step into molecular systems at the boundaries of the phases, and at the boundaries of classifications of systems, including molecules close to dissociation, high Rydberg states, quasi-linear and van der Waals molecules, clusters, and droplets. We have moved into measurements giving us parameters of time-dependent Hamiltonians. The boundaries between our subdisciplines have shifted and collapsed, like the separation of variables that is no longer a necessary criterion for a system to be accessible to study.

One example of the dissolving of boundaries is the booming field of molecules in stellar environments and the interstellar medium, a category unknown to all but a few astronomers in 1946. Charlie Townes and his group threw the subject open by observing microwave transitions of ammonia from the galactic center in 1968. Radio-astronomical observations of molecules were swept into the established category of microwave sessions at Columbus for years. As time went on, the relevant talks contained as much or more astronomy and astrophysics as spectroscopy. Finally the first mini-symposium on the subject was included in 1999, with the suitably ambitious title "Spectroscopy of the Heavens", immediately occupying two sessions. Since 2003 it has been a regular feature of the Symposium, filling 3–5 sessions to accommodate the talks submitted on the subject, labeled in 2010 "Astronomical Species and Processes".

Concerning authorship, you will find that the papers at the earliest meetings had usually only one or two authors. You can see that students shared the authorship with their advisors. Later on, collaborations between laboratories began to show up and to represent an ever larger portion of the talks. This can be seen as a feedback loop: Meetings inspire collaborations. Travel and communication are necessary to carry them out. In those 65 years, travel became an order of magnitude easier and cheaper, while phone, fax and email made possible communication on a scale not dreamt of in 1946, so that collaborations stimulated by a meeting today can move forward in a way which simply would not have been possible at that time. By the 1990s, the combination Bangalore, Pisa, and Giessen, Germany was not unusual for the locales of authors on a single paper.

The role of computing is a category of change all by itself, above and beyond other technological changes. In 1948 the first hint of what was to come was given in a talk entitled

> USE OF IBM PUNCH CARDS IN THE COMPUTATION OF THERMODYNAMIC QUANTITIES FROM SPECTRA,

from The Cryogenic Laboratory, Ohio State University [16]. This may have involved using automatic card-sorting machines to make complex sequential sorts. A year later, in 1949, an evening discussion was put on the program to present a

> REPORT BY THE COMMITTEE ON PUNCH CARD SYSTEMS FOR FILING INFRA-RED SPECTROSCOPIC DATA [17].

The next contributed paper on the subject was in 1951, with the title

> PUNCHED CARD IDENTIFICATION OF MIXTURES BY COMBINED USE OF INFRARED POSITIONS AND N-O BAND REGIONS, from the Dow Chemical Company [18],

and the next year a contribution from Arthur D. Little Inc. on

> AUTOMATED RECORDING OF INFRARED SPECTRA ON PUNCHED CARDS [19].

Indeed, I remember engineering applications of card-sorting ingenuity still in use in 1960, when I had a summer job at Bell Labs. A few talks in the late 1950s reported on digital calculations, for example identifying "finger-prints" in low-resolution infra-red bands [20], and representing electronic wave functions of N_2 [21]. In 1962, in the Duke Microwave Laboratory, a least squares fit requiring the diagonalization of the Hamiltonian of an asymmetric rotor, for perhaps 20 transitions, meant using pencil and paper and a Monroe electromechanical calculator which had 4 operations and 9 or 10 digits, for 8 h, for a week or two or more, to go up to $J = 16$. Soon after that "mainframe" computers were installed in most of the major research institutions. But not every university had a computer. At the Technical University of Karlsruhe, Germany in 1965, we mailed key-punching lists for programs and parameters and data, hand-written character by character, to a regional computing center in Darmstadt, hoping we had not made a mistake with maybe just a column or a comma, waiting for a result in the return post 2 or 3 days later. Then the spread of computers and the development of computer analysis of data took off. By 1968, such a matrix diagonalization was done on a local "mainframe" computer – but still using punch cards to enter information. There were entire cabinets designed to hold nothing but punch-cards. Meanwhile in Rao's lab at OSU in 1968 we used a foot-peddle to trigger a dedicated card-punch to record the position of each line in a spectrum. The infrared detector signal was recorded on a large roll of precision chart paper over usually 12 h of continuous measurement, during which the grating in a vacuum chamber was slowly rotated. The next day, the chart paper was slowly scrolled manually over an illuminated glass plate marked with a centering grid flanked by cylinders with teeth fitting the holes in the top and bottom of the paper. Each line center was determined by eye, and when a foot hit the peddle, the card punch recorded the line in units of paper position and gave it an index on a separate card for each line. That year the Symposium had its first special session about spectroscopic applications of computers.

Today, as you know, we can control a measurement, predict, model (a term which has acquired a whole new meaning), and fit on a laptop spectra of a range of molecular systems so wide that we did not even know some of them existed 65 years ago. We have energy levels and wave-functions literally at our finger-tips. The few remaining punch cards are antiques hoarded by old experimentalists for precision shimming of table-top experimental setups.

One of the constants in this meeting has been the format of invited talks, generally of 30 or 40 min, and contributed talks of 10 or 15 min. When posters began to crop up at various meetings in the late 1970s, there was occasional debate about whether this should happen also at the Symposium. It has been generally agreed among the participants and the organizers that if we want to perceive the meeting as contributing to the training of young scientists, it is important for them to learn how to present a short, focused, professionally illustrated, convincing oral presentation, and there is no better way to do this than in the oral format of this meeting. For the audience, the efficacy of communication in the two cases, poster or talk, depends on the circumstances. I will come back to this subject again in the context of the European meetings.

The scientific scope, the essence of the subject matter, the format, the modest setting, the encouragement of young people: all the priorities were set very early, in the first few years. In many ways, this meeting sprang, rather like Athena, full-grown upon the scientific public.

4. The Dijon and prague series of meetings

One sign of the importance of the OSU Symposium is its relationship to the European spectroscopy meetings. Let me drop back to some earlier abstracts to introduce their founders.

Ian Mills, from England, first came to Columbus in 1955 while he was a post-doc with Bryce Crawford in Minnesota, as one of the authors of the following talk:

THE INFRARED ABSORPTION INTENSITIES OF ETHYLENE AND SOME DEUTERATED ETHYLENES [22].

Gilbert Amat came from Paris to work that year with Nielsen, and although he and Ian Mills had met before, two somewhat shy young men at a European meeting, it was during a visit of Amat in Minnesota, following their meeting again in Columbus, that they paddled about on a lake and became friends. Amat's first talk here was as follows:

SOME ASPECTS OF THE THEORY OF *l*-TYPE DOUBLING AND *l*-TYPE RESONANCE [23].

Then, 10 years later, came Dušan Papoušek, a protegé of Joe Pliva at the Czechoslovak Academy of Sciences. Papoušek spent the academic year 1967–1968 in Columbus as a visiting professor, as had Pliva some years earlier. He presented a talk in 1968:

THE HIGH RESOLUTION INFRARED SPECTRA OF DIMETHYLACETYLENE AND THE BARRIER TO INTERNAL ROTATION [24].

Shortly before the Columbus meeting, which was in September at that time, the molecular spectroscopy group at the National Research Council of Canada had invited a small group of spectroscopists to go to Ottawa for a small, informal meeting about some problems in the spectroscopy of small molecules. This meeting included, among others, Ian Mills, Gilbert Amat, and Joe Pliva. The subject of a possible meeting in Europe came up. In his thank-you note after the meeting, Gilbert Amat wrote, "We certainly have to do it again ... perhaps in Europe next time" [25]. Perhaps it was a courtesy or a dream, but a seed had definitely been planted [26,27].

Papoušek was not at the meeting in Ottawa. He returned home in September of that year, immediately after the Soviet suppression of the Prague Spring. Since he had been in Columbus for a year, he had a clean slate, politically, at home. Pliva, in Canada, could not return for political reasons, and Dušan Papoušek became the head of the spectroscopy group in the Academy of Sciences.

There had been several broadly based meetings hosted in various cities in Western Europe in the 1950s and 60s covering molecular spectroscopy. These events were becoming uncomfortably large; they included the rough equivalent of PittCon (Pittsburg Conference, with a large commercial analytical component) in the US, which had started in 1950. The organizers of the European meeting announced in 1968 that they would exclude rotationally resolved spectroscopy in 1969. This made Gilbert very unhappy, so he called up Ian, across the channel, who was similarly distressed. By January 1969 Ian wrote to Phil Bunker: "Since I last wrote to you the Amat-Mills conference has suddenly been reactivated for this coming summer! This action is prompted by the fact that the European Molecular Spectroscopy Group is apparently holding a meeting in Liége in September which will be confined to the solid state. Thus we feel that a small informal conference on high resolution rovibronic spectroscopy would be in order, somewhat along the lines of yours in Ottawa" [27]. The plans went forward very fast. We must give credit to the Ottawa group, whose "small informal meeting" led to something ultimately much, much bigger. The meeting was intended to be, indeed, the first of a series, to take place, alternating with the existing, broader European meeting, now called EUCMOS, every 2 years. It should *not* be held in a large European city. It should be cheap. The students should be housed in dormitories. The first meeting took place in 1969 at the Université de Bourgogne in Dijon.

Meanwhile, Dušan was frustrated because his group and others in Eastern Europe, after the exhilarating but brief liberality of the Prague Spring, were, yet again, largely cut off from contact with Western colleagues. Restrictions imposed by the Soviets made it even hard for Eastern European groups to contact each other. He was not sure the group in Prague could survive, especially in such isolation. It was only rarely possible, even later, for Russian or Eastern European scientists to attend the Dijon meetings. Dušan proposed to the Czechoslovak Academy of Sciences to initiate a series of meetings, alternating in years with the Dijon series, in Prague. This series would be open to participants from all the Eastern European nations as well as to Western guests. The idea met with the approval of the Academy, in particular due to the support of the internationally respected Prof. Rudolf Brdička, at that time head of the J. Heyrovsky Institute for Physical Chemistry of the Academy [28]. This "semi-permeable membrane" concept for dealing with the iron curtain led to a unique opportunity for participants from both sides. The first meeting took place in 1970 in Prague.

I am telling you all this, because these meetings were consciously set up to emulate important aspects of the Columbus Symposium, aspects that the organizers had learned to appreciate as participants here: International participation, modest accommodations and other, informal measures to encourage young scientists and students, as well as to maximize communication among all the participants, a minimum of hierarchy and ritual (this itself is now a time-honored tradition), and what I will call a total absence of institutionalized arrogance. There was also an emphasis, slightly more focused than in Columbus, on new developments and fundamental advances in high resolution molecular spectroscopy. Underlying the immediate motivations to launch those two series of meetings was the fact that not only did the Europeans have discipline boundaries to overcome, just as we did in the US, they had national and most important, linguistic boundaries to deal with. The dominant divide, the iron curtain, remained a serious obstacle until 1990. Up to that year, Westerners could go and did go to Prague, but this tunneling was highly asymmetric. Eastern Europeans could only with great difficulty go to Dijon or Tours. Mamet Aliev, when he finally made it from Moscow to Tours, after several failed attempts to get permission, announced triumphantly and recklessly in his talk that "some forbidden transitions have become allowed".

The Columbus Symposium was not only a role model, it gave direct help, making the Symposium mailing list available, and for the Prague meeting, some years later, making available the ingenious meeting-management software developed here by Terry Miller's group at OSU. This spared the Prague group the cost, particularly heavy for them, of mailed invitations and correspondence. Since we were living in Germany, we attended many of both of these series of meetings, and the influence of this Columbus Symposium, as opposed to the influence of the meetings of the traditional national scientific societies, was very obvious. If you have ever been to a meeting of the Faraday Society, especially 20 or 30 years ago, or the Bunsengesellschaft in Germany, fine institutions as they truly are, you will know what I mean. The influence of the Columbus Symposium was freely confirmed to me by Ian Mills, Gilbert Amat, and Dušan Papoušek during conversations and emails during the past year and a half.

The major difference of these two series of meetings from the Columbus Symposium is that they employ the format of invited lectures, sometimes short contributed papers, but primarily posters as contributions. The language of presentation, after brief initial efforts to include French and Russian, settled into being exclusively English. Both meetings started out small, like the Ottawa conference in 1968, with a format as in Columbus, but grew extremely rapidly, showing the need that they were filling. When they reached the size where one had to decide between parallel sessions

and posters, the posters won by popular demand at both European meetings. We witnessed the transition from our vantage point in Giessen, Germany and realized that for our students, and all those other youngsters whose English was marginal, in terms of both speaking and listening, the poster format has advantages. It was far less strenuous and less intimidating for them to talk until they were hoarse to a few people at a time in front of a poster than to struggle to perfect one all-or-nothing talk, or to risk asking a stupid question in bad English. Since there are no parallel sessions, all posters are accessible to all. Soon enough, the young people who stay in the game will be ripe for a trip to Columbus or elsewhere and for an oral presentation in English. Furthermore, at a poster, they have a good chance to talk directly to some famous spectroscopist who might take an interest in their work! This is of course one of the great aspects of all of these meetings.

5. Running the symposium introduction

The person primarily in charge of running the meeting spends a large part of the year with the meeting on his mind. I know it is true of Terry Miller, and I saw it with Rao. We, Manfred and I, ran the Dijon-series meeting in Giessen in 1989. It was a lot of work. There comes the day of the abstract deadline, for example. The Columbus Symposium has always had a rather hard deadline. (In contrast, for our Giessen meeting, it fell at the maximum of a Gaussian distribution of actual submissions, half-width two weeks.) When I was in Columbus in 1968, the setting up of feasible sessions began the next day. Rao took the submitted abstracts home, and sorted them into piles on the living room floor by topic. He then sorted those piles into new piles for possible sessions, and then waded about in the resulting labyrinth shifting and switching until every talk submitted up to then was suitably accommodated, every exclusion principle had been observed, and eventually every session had a chair. In those days abstracts were all typed on different machines, with different fonts and quirks. A special, seasonal secretary was mobilized, who typed the entire program more or less night and day at her home for about 2 weeks, until it was ready for the press, about a month after the deadline. The growth of the meeting, as documented in Fig. 3, made this an increasingly arduous process, even when Rao got help later on from Weldon Matthews and Terry Miller.

The growth in the number of talks was rapid and roughly steady up to 1976, after which a major dip occurred. This was due to a change in funding policies in the aftermath of the Viet Nam War. The Mansfield amendment to the Defense Procurement Authorization Act of 1970 abruptly ended direct support of basic research at universities by the Department of Defense. Since many spectroscopy laboratories were clients of the Office of Naval Research, the US Air Force, and the Army Research Office, the Mansfield amendment hit the spectroscopic community especially hard. It took several years for even established, highly reputable laboratories to find adequate alternate funding. The National Science Foundation budget was not expanded to take over the spectroscopy programs. Since then the growth has been less steady and less rapid than earlier, but substantial. Another measure of growth might be the number or registered participants, but this is padded by grad students and staff at OSU, and shows frequent large swings of 50 or more. In the last ten years it shows a clear overall rise from about 420 to 500 [29]. The all-time high was 600 who came to the 50th. Surprisingly, recent average attendance is not very far from the number of 450 cited above for 1967. The number of attendees has not expanded as much since then as the number of talks, but that means that the meeting has remained at a manageable size. This shift in the proportion of participants who are not presenting is probably related to the enormous changes in communication in the last few decades.

Such a large Symposium demands a lot of the person in charge. Almost every year, a new category of problem manages to turn up. Most of us do not notice this, because the organizers have dealt with bumps in the road, such as infrastructure breakdown, dormitory room disasters, microphone battery failures, donut delivery emergencies, etc., quietly and effectively. For example, there was always a back-up banquet speaker at the meeting, in the days of banquets, in case the scheduled speaker could not turn up. We have been extremely fortunate that OSU has provided three successive remarkable hosts of these meetings, each of whom in their own manner has made a significant commitment and contribution to the evolution of the meeting, and each has successfully run it for a commendable number of years.

Harald Nielsen was the first, a capable and gracious organizer who was chair of the Physics Department from 1946 to 1967, and shepherded the meeting even longer, for 25 years. Many of the durable aspects of the meeting that I have highlighted were established with him at the helm, though we should not forget the role of Dennison. Ian Mills writes of Nielsen: *Harald himself was a very kind and human person ... when I was very young and wet behind the ears, he paid everything (fare from UK, etc.). I had written to him asking him a question about his great Rev. Mod. Phys. paper (unreadable, and largely unread by anyone but Gilbert Amat, full of an*

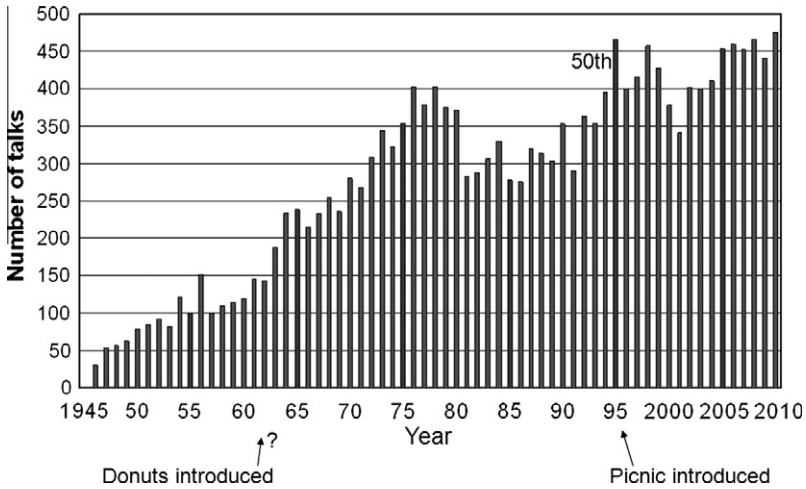

Fig. 3. Number of talks per year included in the book of abstracts, or presented, including since 1995 post-deadline papers, at the Symposium on Molecular Spectroscopy.

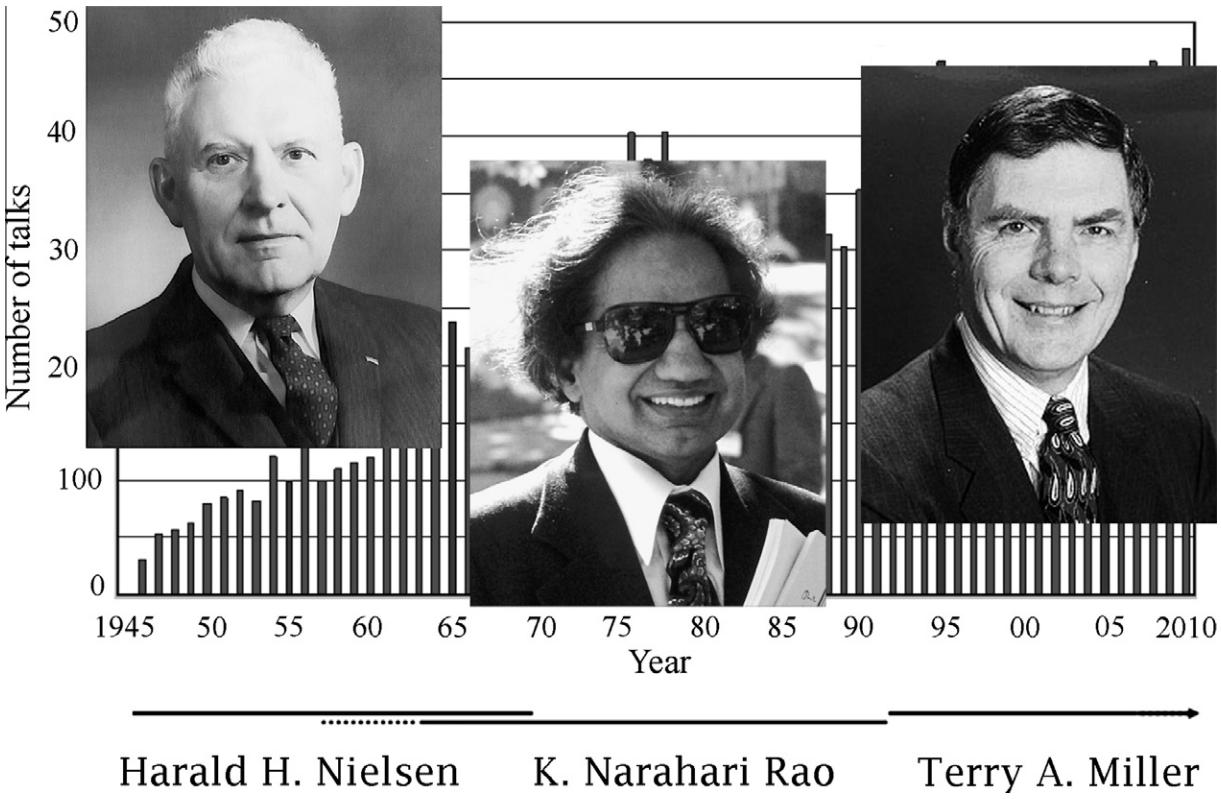

Fig. 4. The three spectroscopists who have hosted the OSU International Symposium on Molecular Spectroscopy through 65 meetings.

infinity of misprints and errors! But that paper was nonetheless a great step forwards). But my question was trivial, and he answered it at once, with good grace [30].

During the last six of Nielsen's years in charge, he officially shared the task with K. Narahari Rao, who worked with him starting in 1954 as a postdoc, rising in the department to become full professor at OSU in 1963. This is indicated in Fig. 4.

Rao, a cheerful and persistent pragmatist, automatically grew into the job of running the meeting, gradually taking on more and more of the work, particularly after Nielsen was afflicted with Parkinson's disease. Starting in 1964, Rao was listed with Nielsen as co-host. In 1971, Nielsen's name dropped from the inside cover, after 24 years – but so did Rao's: there was no name at all printed there! As Ian Mills observed on the basis of the half-year he spent at OSU that year, *. . . Rao's contribution was great; . . . he had a warm heart, and he worshipped Harald Nielsen, and did everything for him when he became old with Parkinson's. He essentially ran both JMS [Journal of Molecular Spectroscopy] and the Columbus Conference for many years, always leaving Harald's name at the top of the heading with his as assistant – long after Harald had ceased to do anything. Rao worked hard at it* [30]. It appears that Rao wanted to spare Nielsen the pain of public knowledge that he could no longer even nominally steer the fate of the meeting, which continued to thrive. Rao went to great pains to secure funding to keep it cheap, and yet select and pay travel for the best national and international speakers, a task that became difficult for a while, in the 1970s. He kept his ear to the ground and his eye on the Journal in order to know whom to invite. Including the 6 years he formally shared the job with Nielsen, Rao ran the show for 27 years, until his own health had begun to limit his activities. In 1992 it was time to pass the torch. Upon his retirement from his post that year, several spectroscopists who had made their first talks in Columbus decided to sponsor a prize for the best talks given at each meeting. They were very conscious of his successful efforts to encourage young spectroscopists, and also to more or less gently prod them about the professional way of doing things, from his perspective as editor of the Journal of Molecular Spectroscopy. This was the beginning of the Rao Prize Program.

By then high resolution spectroscopy was firmly established in the Chemistry Department at OSU. Terry A. Miller arrived from Bell Telephone Laboratories in 1986. He made the non-trivial decision to accept the task of taking over the Symposium in 1992, which allowed us all to continue to come Columbus. His first major step was to introduce the International Advisory Committee, thereby establishing a network for finding speakers and sharing responsibility and decisions. The meeting continued to grow. The Miller group also immediately began to bring almost every aspect of the organization of the meeting into the computer age, step by step: mailing lists, preregistration, dormitory reservations, abstract submission (online since 1995), checking for acceptable abstract format, and a major portion of the scheduling puzzle. The final step (so far), now that computer projection has almost universally replaced older methods, was to automate the uploading of entire Power Point presentations, up to midnight before the day of presentation, an exceedingly generous service. The use of computers has dramatically changed the nature of the task of preparing for the meeting and of running it. The meeting staff gets smarter and smarter, too, in preempting problems and simplifying procedures. The delegated planning of mini-symposia and international participation in the selection of plenary speakers eliminate any justification for suspicions of provincial Columbus bias about the planning of the program. Terry and the organizing committees, international and local, organized a spectacular 50th Symposium in 1995; that included introducing a picnic to replace the banquet, which had truly become too quaint for the spectroscopy crowd. It must be allowed that we heard some very interesting and unusual

banquet speakers over the preceding years. The picnic, however, reinforces the abiding goal of facilitating interactions between participants, young and old. Terry Miller has been able to deal with a robust conference with an ever larger and wider range of spectroscopy-related topics for 19 years so far.

We must take off our hats to these three eminent spectroscopists who have made it possible for us to come and share one week a year in Columbus indulging in spectroscopy. As a matter of fact, the field of molecular spectroscopy as a whole has been blessed with a succession of fine men, and a few fine women, who have set the standards and tone for our dealings with one another as well as with our science. This meeting is where many of us observed and absorbed these standards. The durability of the meeting, and the consistency of our rituals, few as they are, have something in common with medieval pilgrimages. With our tolerance of modest quarters, our community of questions and goals, our thirst for news and innovations, and for visiting and revisiting with like-minded souls on the same quest, for shared food and drink and fun, we have made OSU the center of a classic pilgrimage. That is part of the remarkable fact that the Symposium, with its format, scientific focus and scope, and human component, has remained as recognizable as it has.

Acknowledgements

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