EDITORIAL

An Interview With Professor Ed Kollar

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Professor Dr. Edward J. Kollar, better known as Ed by his colleagues and collaborators, is one of the most important figures in the field of dental research. His work on epithelial/mesenchymal interactions for the initiation and formation of teeth, had and still has, an enormous impact on younger investigators of odontogenesis. We could say that he is one of a few pioneers persons who inspired the recent research on tooth engineering and regeneration. Ed was born in 1934, Forest City, PA. He started his professional career in the University of Chicago as an Instructor and a Research Associate in Zoology during 1963. Thereafter he became an Assistant Professor and an Associate Professor of Anatomy. During 1971, he moved to the University of Connecticut Health Center and became a Professor of Oral Biology after several years. Later, he became the Associate Dean in Academic Affairs and in 1997 Professor Emeritus. During his long career he published numerous and important scientific papers. Science apart, Ed is a well-balanced person appreciating family and sharing activities with friends. Ed was present during the last ''Tooth Morphogenesis and Differentiation'' meeting (TMD 2007 meeting) and we were very pleased to share his memories, philosophy for science and life and advice.

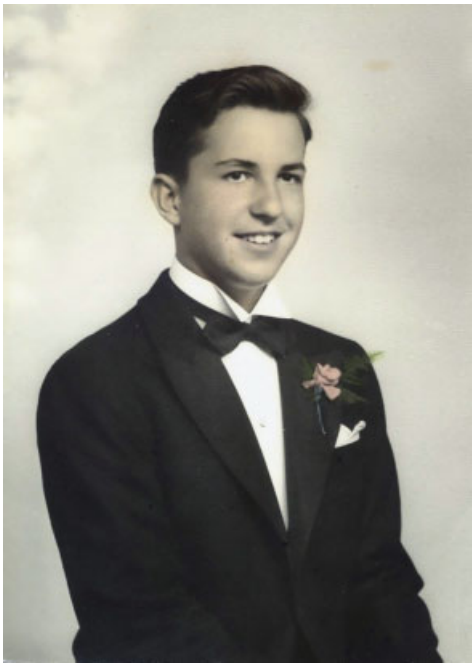

Fig. 1. Ed Kollar, 1950. Before science. USA.

How many of the ''TMD'' conferences have you attended?

E. K.: I think that I missed only one (York, England) in the series starting in Strasbourg in 1978. As Tim (Mitsiadis) noted in his opening introduction, the size of the conferences has grown dramatically. That indicates the quality of the conferences and the attractiveness of the research topics to young and aspiring investigators. Of course, the intimacy, depth and impact of small group discussions have been lost as the number of participants has grown. Maybe a round table format might be built into the program with a small group of the major researchers discussing the broader and cutting edge issues, which could be held at the end of the general sessions. Questions from the audience could be entertained at that time.

How did you become interested in a career as a research scientist?

E. K.: That is one of the strangest journeys I have ever taken, now that I consider it from this distance. I am amazed that it turned out so dramatically well!

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Fig. 2. Ed Kollar, 1978. On Sabbatical. London, UK.

I was born in a small village in northeastern New England, USA, during the middle of the great world depression (1929–1939). There were limited opportunities for my future, but I had very earnest teachers who taught me Latin, mathematics and biology and supported me to be accepted as a student at a nearby Jesuit university. I commuted for 4 years by hitch-hiking and car-pooling and ended with a B.S. in Biology with a major in a premedical degree, cum laude, at the top of my class. I had hoped to go to medical school.

In the USA, one must get a baccalaureate degree before enrolling in a medical or graduate school for a higher degree. My university was considered widely as a ''feeder'' school offering highly qualified candidates for enrollment in medical schools in the region. I was mystified that I had not been accepted by any of the schools to which I had applied—until about 40 years later I learned that my uncle asked the Chairman of the Biology Department of my undergraduate school why I had not been accepted. The answer was that my father had been killed in an automobile accident in my second year of undergraduate school and that according to the Department Chairman I could not afford to go to medical school. What a revelation after all of those years! I had carried around that rejection for years. What a relief to be rid of that burden. It could not possibly happen now in this country as part of an application from one university to another to summarily reject a student on financial status instead of merit.

Fig. 3. Ed Kollar, 1980. Teaching duties. USA.

Therefore, without any knowledge of research (there was none in my undergraduate school) I somehow learned (perhaps from the Chairman who prevented me from going to medical school) that there was this other possibility, that I might get a Ph.D.! This was the first seeming disaster/change of course in a series of serendipitous events that would shape my life and scientific career.

I applied to Syracuse University for a Teaching Assistant because their prospectus/catalog mentioned a possibility of research in embryology. I had been excited by histology and embryology as an undergraduate student. And, I was pleased that I was accepted to their Graduate School. Unfortunately, the faculty member interested in embryology was no longer active in research. I then turned to problems of growth and regeneration as an alternative to embryology. Remember this is 1955!

I eventually received a Ph.D., and in the process serendipity reared its gracious head again and I met Catherine, a fellow graduate student, who has been the love of my life and the mother of our five children and the most joyous and supportive part of my life. Thank you mister Chairman of the Biology Department; in the end you did me the greatest favor.

Of course, then the issue was what to do with this Ph.D. I applied, as is now common practice, for a postdoctoral position with several universities that by this time I knew could respond to my interests in developmental biology into which embryology had now morphed. Serendipity was by my side again and I was accepted by Dr. Aaron Moscona at the University of Chicago as a postdoctoral fellow and as an Instructor in the college of the university where I would also teach undergraduates.

It is my contention that the University of Chicago gives you one free idea by osmosis when you step into the campus and after that it is up to you to nurture and embellish that gift. In a few months of my being there at a laboratory meeting I was giving a report about what I was doing and a good friend at Syracuse who also followed me to Chicago asked one of the most decisive questions of my life: Why are you doing this? I had no answer! After the embarrassment of not having a comprehensive overview of my interests, I spent 1 or 2 days of hard thought and I came up with a whole life's plan that has guided me throughout my research career. I have made certain that every student I have trained knows why the problems they are working on are important and in what global context their research will be viewed.

The next serendipitous surprise presented itself in the next few years. I had begun my tooth development research and published the first few seminal papers with the help of Dr. Dahlberg of the Zoller Dental Clinic at the University of Chicago who introduced me to Dr. Shirley Glasstone, the founder of tooth tissue culture. In addition, what is even more important, Dr. Dahlberg provided me with a Zoller technician to assist with my experiments.

In addition, serendipity was still beside me: a Professor from the School of Dentistry of the University of Illinois was also in Chicago; he was part of the planning committee to start the opening of the University of Connecticut School of Dental Medicine. Through him I was given a position in Connecticut and it has been my home ever since.

What brought you to your interest in the development of teeth?

E. K.: As a young postdoctoral fellow at the University of Chicago in 1963, I was looking for a model system to work on for myself. My postdoctoral laboratory (i.e. Dr. Moscona's laboratory) was investigating feather development. That was a field of investigation in many laboratories around the world whose basic interest was the study of epithelial–mesenchymal interactions. I was working on feather development with Bea Garber, a former student of Paul Weiss, one of the founding fathers of American Embryology. Bea and I became close friends; I am grateful for her generous friendship through my early days as a scientist. Sadly, she is gone now and I miss her to this day.

Learning the basics of tissue culture, microdissection and tissue recombination techniques

Fig. 4. Ed Kollar, 1997. Retirement party. USA.

with her, I began to branch out and moved to another object of research. Soon, it occurred to me that no one had looked at pelage hair and whiskers in mammals. Independently, I began to study the development of pelage hairs and vibrissae in mouse embryos. Luckily, my laboratory was down the hall from a large laboratory whose major interest was animal behavior in mice. I had a free and constant source of embryos. How much luck can one person have? Subsequently, I demonstrated that, similar to the feather, the hair follicle was dependant on an inductive signal from the mesenchymal papilla at the base of the hair follicle.

I was then interested by the idea that there are clusters of neurons on the brain, which are called ''barrels'' that mimic the same pattern and position as the whiskers on the animal snout. However, I realized that I needed more markers than just hair keratin to be able to pursue this intriguing correlation. The question of the patterning of the vibrissae still intrigues me!

At that time I wrote a chapter that listed birds and mammals together with all the appendages that the epithelium can make (feathers, hair, horns, nails, claws, mammary glands, etc. and yes teeth!). The epithelium is capable of great plasticity as it can express many structures and organize them in very precise arrays and positions. The question for me was how the epithelium and dermis could to do all of that in such an organized way.

The skin was quickly acknowledged to be a versatile model for studying tissue interactions in other laboratories around the world as Irma

Thesleff demonstrated at this ''TMD'' conference with her amusing cartoon of a fictitious chimera containing all of the ectodermal derivatives of all species in one drawing (the design was from a previous article of Dr. Chong on ectodemal derivatives). What signals were necessary for the development of the position, morphology and subsequent characteristic biochemical expression in the derivatives?

I realized that this research program had limitations in that the biochemical marker for hair follicles was keratin. Therefore, I looked more carefully at the ectoderm and realized that the oral epithelium not only keratinized in specific areas but also gave rise to the teeth with enamel and dentin as distinct expression of the epithelium and the mesenchymal components. The teeth also had to specialize into highly detailed shapes and be specifically positioned so that mastication could take place. The diastemal area in rodents, for example, is an evolutionary adaptation that allows the jaw to elongate to accommodate to a gnawing mode of eating.

There was one thing missing in this almost perfect model. There are few mutations of the teeth. The few that do occur (tabby, etc.) did not seem sufficiently interesting to me. It occurred to me that if the mutation disrupted the dentition sufficiently, mastication would not be possible and the mutant animals would die soon after weaning and the mutant gene would not enter the gene pool. A variation on this question was resolved years later while I was working in Salzburg.

There were some papers from the 1930s implying that tooth development requires epithelial–mesenchymal interactions. Decades earlier Dr. Albert Dahlberg and Dr. Shirley Glasstone did some experiments that made the teeth as a model all the more interesting.

In 1968, I published the first paper in a series that demonstrated that the shape of the teeth was controlled by the dental papilla and followed that with another demonstrating that the dental papilla could induce teeth in nondental areas such as the diastemal area and even outside the oral mucosa! One of the control recombinations of enamel organ epithelium placed on the nondental mesenchyme produced an intriguing result. The dental epithelium branched profusely in a pattern resembling an ameloblastoma! At this conference (i.e. the ninth TMD conference) it was reported that by manipulating the signaling pathways the same branching occurred and in addition multiple teeth were formed if the signaling mechanisms were manipulated.

I remember my very first presentations at a meeting of the American Association of Anatomists in the late 1960s. After my presentation was done a lone unidentified voice from the audience (with a British accent, I think) asked: Do you expect us to believe these results? I was able to regain my composure and invite the person to my laboratory to review the raw data. No one ever appeared.

By this time in the early 1970s and into the 1980s other laboratories had begun to publish their work, Harold Slavkin in Los Angeles, Irma Thesleff in Helsinki, Jean-Victor Ruch in Strasbourg and others. Later there would be many more.

They were only a few big groups in the tooth field during your early years of research. How did you interact?

E. K.: Actually for the most part the interactions were completely pleasant and cooperative. Irma (Thesleff), Jean-Victor (Ruch) and myself were on our own research lines that may have intersected and then veered in directions that sent us in different pathways. Attendance at meetings was always a pleasure because we would see each other. Hal (Slavkin) took a decidedly more

Fig. 5. Ed Kollar. Ed's house. New England, USA.

Fig. 6. Ed Kollar. Other accomplishments. USA.

reductionist biochemical pathway veering off into enamel biochemistry and his interest in epithelio-mesenchymal interactions did not necessarily have a impact on me.

Can you describe how you felt when you had some of the great "scoops" in the field?

E. K.: There were several that gave me great pleasure! I went to Salzburg on a sabbatical with Klaus Kratochwil at the Institute of Molecular Biology in 1987. I was always (and still am) intrigued by the difference between glandular vs. adnexal (feathers, hair, teeth, nails, claws) as outgrowths of the epithelium. I wanted to see if we could reverse the glandular expression by the strong inductive influence of a papilla.

We labored intensively to retrieve enough mammary-bud epithelium to recombine the mammary epithelium with isolated dental papillae. After months it looked as if it would be an even more Herculean effort than we had anticipated. And, THEN!, Klaus mentioned that he had a colony of MOV-13 mice that were defective because they could not make collagen I and die around age 13 days of embryonic development because of hemorrhage in the great vessels!

Eureka! Serendipity! Again! How can you make a tooth without first making dentin, which is composed of collagen type I? The experiments went into high gear. The article came out and the conclusion was that the MOV-13 teeth could be made outside of the embryo, which dies at day 13 because the genome regulates the collagen gene differently in the dental papilla in teeth and in bone despite the viral block in all other fibroblasts.

I later heard in 1990 that at the National Institutes of Health and the National Dental Institute someone walked into a study section reviewing grant applications and said that they could ignore a pending application on this subject because the project was already done. Timing, timing, timing!

There was also a great stir because Dr. Jan Kronmiller, a doctoral student in my laboratory, devised and executed a project that also scooped another unidentified laboratory, who apparently—from the grapevine—was hysterical upon seeing the published articles. Dr. Kronmiller investigated the relationship between the epidermal growth factor (EGF) and retinol. The last paper in 1991 showed that EGF antisense oligonucleotides could block odontogenesis. It is reassuring to learn that you are ahead of the pack!

What about the hen's tooth?

E. K.: In 1980 we used mouse dental papilla to interact with chick epithelium to form teeth. That has been a problem ever since because we published a tooth with enamel. Yes, that did create more of a commotion than I thought it would. But the debate was mostly about whether the crown contained true enamel implying amelogenin secretion and subsequent calcification. Unfortunately, we didn't have at the time many markers to definitively show if amelogenin was being synthesized.

However, what is ignored was that we reported that we got other structures that were devoid of enamel, but were dentin in the shape of tooth crowns and roots. The fact that dentin can only be expressed with an interaction with an enamel organ was one of our arguments for teeth being formed in these explants. Over 10 or more years I talked to people working on enamel formation and the conclusion was that amelogenin might not be the only protein to support enamel mineralization. It is certainly expressed later in evolution but there may have been earlier proteins expressed at earlier stages. By 1987 Dr. Mina did a paper showing that early epithelium of the mouse (9–11 days) can induce teeth in the chick second mandibular
arch. There was an early signal coming from the epithelium that set up the dentition, the pattern and the inductive epithelial–mesenchymal interactions.

By the time I moved to become the Academic Associate Dean in Connecticut my research activity was moving more to the background; however, there was an explosion of new ideas and techniques: Hox genes, transgenic mice, knockouts, signaling molecules and all the new issues from molecular biology. We know so much of the molecular biology now. We know where the signaling pathway is, we can manipulate it, but we don't know yet how the tooth bud initiates the cascade and starts the signaling pathway. That's the link

that is missing. It occurs to me that the signaling might become activated in the epithelium at the time of the fusion of the palate and the mandibular processes.

Several years ago, a paper from John Fallon's laboratory reported initiation of tooth formation in talpid mutant chick embryos. Whether those immature ''teeth'' would form an enameled crown is not known to my knowledge.

Many colleagues suggest that a contamination had occurred during the realization of your recombinations between epithelium and mesenchyme. This is an actual question as we are concerned about recombinations between stem cells and tissues of different origins. Which is your opinion?

E. K.: There is some evidence—some at this meeting—that the gene for amelogenin cannot be found in the chick. That being true, the conclusion has to be that the teeth we saw that did express enamel could have done so because there was contamination with mouse epithelium in the tissue separations and some mouse epithelial cells were part of the grafted material. I have to emphasize that the combination of mouse dental papilla and first-arch chick epithelium did produce a larger number of enamel-free tooth-like structures. It has been known from the beginning of tooth development studies that during the first stages of odontogenesis the epithelium elicits the secretion of a small layer of dentin and then that interaction evokes ameloblast maturation. That seems to be the sine qua non requisite interaction and signal for enamel deposition. I still firmly believe that the conclusion that there was a cross-class interaction was correct. The kind of enamel protein obviously can't be amelogenin, whether it is some other molecule is not known. Whether or not an enamel organ can initiate crown dentin formation and then fail to initiate ameloblast maturation is not known. Clearly in rodent incisors there is not a noticeable enamel layer on the lingual surface as opposed to a thick layer on the labial aspect. This arrangement is absolutely necessary for the rodent to grind the incisor down to a sharp incisal edge. We all know what happens if the opposing occlusal incisor is not there. There are still some mysteries left.

So, is mesenchyme, epithelium or both responsible for tooth induction? Do you have a crystallized opinion right now?

E. K.: The definitive answer to that is the data that Mina produced using the potent early 11–13-day embryonic mouse epithelium and the second-arch chick mesenchyme. That 1987 article changed odontogenic research forever and this meeting underscores the importance of that paper.

Fig. 7. Ed Kollar, his wife Catherine (middle), and Irma Thesleff (left) 2007. TMD meeting. Zurich, Switzerland.

Do you believe that somebody can create a root without having a crown?

E. K.: That remains to be seen. The epithelial diaphragm determines the shape of the dental root. There are many experiments that should be done and I eagerly await the appearance of the new data. When the last of cervical ameloblasts stop making the crown, the epithelium gives rise to a two or three cell layered diaphragm that controls the shape of the roots. As the diaphragm disintegrates, the remnants of the enamel organ reside in the emerging periodontal ligament. What are the interactions of those cells with the dental sac mesenchyme?

Can you start tooth formation in an inverse manner, starting from the root and finishing with the crown? This is what happens when the incisal part of the rodent incisors is sectioned. The root contributes to the regeneration of the lost part.

E. K.: That is not a fair question. The incisor has no true root! The embryonal part of the basal portion of the incisor is interesting in that the enamel organ does not completely regress into a protective ameloblast layer but remains highly regenerative. There was evidence of that at this meeting and, therein, may lay the answer. I have not seen any data using recombinations of the basal part of the mature incisor and embryonic mouse second-arch mandibular menenchyme. If these data exist, they would go a long way to answering your question.

Not having a background in dentistry, was it particularly difficult to join that discipline?

E. K.: No, not at all! My department was oral biology and the school was very much oriented toward an intense basic sciences background for the students. I had total freedom to conduct my research. Everybody was interested in teeth. Over the years I became what I call a ''closet dentist,'' I know what they do, they know what I do. I have come to be able to understand the language of dentistry; I only lack the clinical skills.

What can you tell from your day-to-day experience about the relationship between basic research and dental training?

E. K.: It depends on the school. Some dental schools just teach you to drill and fill. It's wholly an attitudinal viewpoint, the philosophy is different between schools. I have been lucky to work in institutions where there was a strong scientific-based curriculum for the dental students.

What makes a better dental researcher, a dentist reconverted into biological sciences or a biologist switching into dentistry? Who could make the breakthrough tomorrow in our field of research?

E. K.: All the tissue engineering that is going on is directed toward clinical problems. Dentists have direct access to patient material, information and treatment, whereas a biologist may not have all the confidence to deal with a clinical problem. As the future is moving toward tissue engineering and the clinical solutions that go with that, I would say then that a dentist with a Ph.D. is better than just a Ph.D. with no clinical training. My school has dedicated itself to cotraining and post-D.M.D. training to students who wish to engage in basic and clinical research and it seems to have worked very well. We may soon be getting to the point of actual clinical trials of repair modalities and the interaction of the clinician who understands the basic science and the basic science investigator who understands the clinical problem will be an amazingly productive alliance.

What have you learnt from colleagues that has contributed to your own success?

E. K.: I taught mainly orthodontic and periodontology students. I began to think more in terms of what a particular problem could mean for the patient that they were treating. There was a lot of interaction, I was always integrated and included, never put off.

You have been in this field for more than 40 years now. Which are to your opinion the most significant research features that have been integrated into dental studies and into the medical practice. Which ones do you believe would be the next ones to be accomplished in the future?

E. K.: I would say tooth regeneration and repair is a key issue, but is still a long way out, maybe another 20 years or so. We have to be careful about how much public information we give, because people with serious dental problems have poignant hopes. Stem cell research is trying to get a tissue model able to replace teeth. Bone marrow cells have already been used to sustain enamel. There are still all the issues concerning root formation, and this is just the beginning. We might be able to disconnect the root from the crown, this could be a possibility.

What do you think was the most important insight from your laboratory?

E. K.: Mandibular epithelium from younger stages (9–11 days of development) was able to induce teeth in second-arch mesenchyme. Thus, the first signals for tooth development come from the epithelium and then the signal is transferred to the mesenchyme. There is no doubt that this finding as soon as the ramifications of the importance became known has provoked a wealth of new and exciting data from numerous laboratories around the world and has given all of us deeper insights into tooth development. These data were a turning point for research in tooth development!

Which has been your biggest professional achievement?

E. K.: In addition to my very basic first articles showing the interaction between the dental papilla

Fig. 8. Ed Kollar, with Tim Mitsiadis, 2007. Interview. Zurich, Switzerland.

and the oral epithelium, I think the 1987 article with Mina was the most important! She asked the question that was pivotal to opening up the work that was taken up by laboratories around the world. We knew from my work what the dental papilla could do. However, we did not know how that information was transmitted to the papilla. Her data provided the bridge to the next generation of studies of the signaling cascades.

I was in Salzburg and Mina was in my Connecticut laboratory doing a student laboratory project before becoming my Ph.D. student. She asked what was one of the long forgotten questions. There had been a question in 1968 that the dental papilla may not be the ultimate source of information. There were two other short reports that suggested that the epithelium might be important. But no definitive data were provided. There was the possibility that there were time-related differences in the embryonic age of the recombinant components in the various laboratories. Mina asked, as all of the previous experiments involved 14-embryonic day or later tissue, what information did the earlier epithelial tissue provide if any? She then recombined earlier first-mandibular-arch epithelium with second mandibular mesenchyme. The results, of course, opened up an entire field of research.

Soon after Mina's data appeared all of the signaling molecules and the ability to experimentally monitor them became available letting us know how the epithelium might be transmitting its signals. She has since recognized the potential of tissues in the lower jaw and tooth formation as a source of stem cells for tissue engineering of human teeth.

I want to mention here that Dr. Mina has been honored by the University of Connecticut by awarding her its Faculty Recognition Award from the Board of Directors. The award carries a prize of $10,000 and a laudatory recognition to be displayed permanently at the University of Connecticut School of Dental Medicine.

What was the best advice you received during your career?

E. K.: It came in the form of a question. I was discussing my intent to look at hair development during a laboratory group meeting. A friend was in the group and stopped me and asked: Why? What is special or interesting about hair? I didn't have a good answer and was quite chagrined. I went home and in a few days I had the answer. You read it in the opening paragraphs of this interview. I have been served very well by that one question for nearly five decades.

My advice to new researchers is to learn from others the techniques and ideas that will be the basis of your future work. Participate in the projects of your teachers and mentors. But here is the poorly understood truth. When you leave these wonderfully talented, gracious and nurturing people, you must stand on your own. Search for a new and unique problem to investigate that excites your imagination. Think about its significance and place it in a more global context. Make it a part of yourself. Find willing and worthy students and collaborators. In addition, enjoy a wonderful career.

Tell me about your students. You had many. How many of them were successful? Can you describe what they are doing now and if they consult you?

E. K.: Let me give you some perspective. My first graduate student who has had an amazingly productive, trail-blazing investigative career has retired. He left my laboratory not wishing ever to see an embryo or a test tube again. In a few years he recovered and began his enviable career.

Every student is different with different personalities, backgrounds, philosophies and personal needs. I have treated them as individuals with respect for their individualities and kept a professional relationship based on collegiality and respect. I have been blessed with very creative people and many of them already had professional credentials in the form of advanced clinical degrees. It was like having a postdoctoral student in the laboratory who needed only to be guided through academic and institutional requirements for another doctoral degree. All have been most rewarding and pleasurable experiences.

Several have gone off as independent investigators, others have then parlayed that into Deanships and other administrative positions. Some have returned to clinical practice both in and outside of academe. The depth of personal and social relationships has varied widely and that may be a result of the vast distances that separate us in the USA.

I see Mina very frequently and for the first few years of my retirement she welcomed my collaboration in her laboratory. However, osteoarthritis developed in my hands and impaired my ability to continue to do micro-dissections. We see each other on occasion. But as you know one's

life can get very complicated with laboratory, university and family life competing all the time with each other.

Are you more proud of your personal research achievement or for the achievements and successful careers of your students? Researcher, tutor or both?

E. K.: I am very comfortable in my place in the history of odontogenic research. I am disappointed that modern literature citation technology seems to begin with the 1980s and a search for references seems to have forgotten those of us who worked before that. I had the impression at this conference that when I was introduced most of the audience was under 35 years of age and thought I had long since departed! However, it was a wonderful moment!

Did you enjoy your period of Deanship? What have you done as a Dean for promoting science?

E. K.: Being the Associate Dean for Academic Affairs was one of the greatest experiences of my life. I wonder now if I should have started administrative duties earlier.

There is very little difference between administration and investigation. They both deal with problem solving. Different problems, yes! But the same demand for creative solutions. I learned how to effectively organize, run and even manage meetings. I organized a faculty to participate in major events in the school such as accreditation, self-study process and site visit from the Committee on Dental Accreditation from the American Dental Association that provides a statement of your institution's ranking amongst the nation's dental schools.

I wrote grant applications for funding for our D.M.D./Ph.D. Dental Scientists program that funded our students' stipends and research experiences. All of this again with the total and enthusiastic help of the entire faculty.

I think my position as a scholar and faculty member without any biases or connection to any administrative agendas helped elicit all of the help and confidence of my faculty colleagues. My Dean said when he thought about filling this position he realized that he needed an academic to be the Academic Dean and that is why he appointed me to the post. I am forever grateful for the experience and his insight and trust.

You are still, after so long, interested in tooth research, you follow the TMD conferences. Why?

E. K.: I know so many of you—you are like a scientific family to me. The quickest and laziest way to know what is happening is to attend these conferences. I promised Catherine that after all of those years of my travel and her taking care of the children that when I retired I would take her to all of the places I have been and to be interspersed some new destinations. For both of us Berlin is another must destination and we hope to be there!

Where do you think this field of research is going next?

E. K.: You can see it in this conference: regeneration, repair, stem cell technology, etc. Hopefully, with time and imagination, clinical applications will be possible. I thought that the discussion during the TMD meeting of how to proceed was wise and necessary. For many years, I have told my students in the laboratory and in classroom lectures that we were especially lucky that we are carrying our own tissue banks in our uninterrupted third molars that some day would be used for rejection-free dental tissue repair. I suggested that extracted teeth would not be discarded but processed for stem cell banks. However, we don't know if it will happen. Experiments will tell us if and where we can go.

How could you describe dental research compared with other medical-related disciplines? Why does it remain so far in the background of public recognition?

E. K.: There is something mystifying about why people tend to ignore dental problems. Even research investigators for years had not recognized the potential of tooth development as a model for so much insight into problems of cellular organization and differentiation. In 1963, there was so little interest, or even a current literature, that I thought I was alone being interested in studying these questions.

It may be a medical insurance problem as well. There are other more pressing problems such as cancer, heart disease, heart, liver and kidney replacement and so forth. You can but rarely do die from a dental disease and it is not perceived as a life-threatening condition. Yet the importance of a healthy dentition to our overall health and our emotional and psychological well-being is obvious. The repercussions of craniofacial and dental defects can be devastating.

Do you regret not doing science anymore?

E. K.: The same colleague I mentioned above who taught me such a basic lesson is 75 years old. He has

just been awarded another 5-year grant to continue his research. As he will most likely never see this interview, I can tell you that I feel very sorry for him. There is so much to do and so little time to do it. He does not know what he is missing.

The answer is I still do science but not hands on.

Apart from teeth, which other interests, hobbies do you have?

E. K.: We have a large family. We had five children and altogether nine grandchildren. We all like very much to cook and have parties. I also do some gardening, although now I don't have a big garden. Traveling is of course another of my hobbies. All of this is shared with my wonderful companion and friend, Catherine.

So, a balanced life is the secret that allows having good ideas and being successful in research?

E. K.: A balanced life is the secret to almost everything in life: personal integrity, intellectual stimulation, professional achievement, weight loss, marital happiness, family enjoyment, party conversation and peaceful sleep to mention only a few!

Thank you very much for your kindness!

E. K.: Thank you, it was my pleasure. See you in Berlin (i.e. where the next TMD meeting will take place)!