GARTNER PEDIATRIC HISTORY CENTER

ORAL HISTORY PROJECT

Nicholas Nelson, MD

Interviewed by James W. Kendig, MD

July 15, 2000
Hershey, Pennsylvania

http://aap.org/pediatrichistorycenter

Friends of Children fund
A charitable fund of the American Academy of Pediatrics

This project made possible by donations through the Friends of Children Fund, a philanthropic fund of the American Academy of Pediatrics.

American Academy of Pediatrics
DEDICATED TO THE HEALTH OF ALL CHILDREN®

Nicholas Nelson, MD

Interviewed by James W. Kendig, MD

PREFACE

Oral history has its roots in the sharing of stories which has occurred throughout the centuries. It is a primary source of historical data, gathering information from living individuals via recorded interviews. Outstanding pediatricians and other leaders in child health care are being interviewed as part of the Oral History Project at the Pediatric History Center of the American Academy of Pediatrics. Under the direction of the Historical Archives Advisory Committee, its purpose is to record and preserve the recollections of those who have made important contributions to the advancement of the health care of children through the collection of spoken memories and personal narrations.

This volume is the written record of one oral history interview. The reader is reminded that this is a verbatim transcript of spoken rather than written prose. It is intended to supplement other available sources of information about the individuals, organizations, institutions, and events that are discussed. The use of face-to-face interviews provides a unique opportunity to capture a firsthand, eyewitness account of events in an interactive session. Its importance lies less in the recitation of facts, names, and dates than in the interpretation of these by the speaker.

ABOUT THE INTERVIEWER

James W. Kendig, MD

Dr Kendig graduated from Lehigh University and from Thomas Jefferson Medical University, having served his residency training at Madigan Army Hospital. He was a professor of neonatology at the Milton S. Hershey Medical Center and the director of the neonatal fellowship program. In addition, he served as a Family Practitioner at the Norlanco Family Health Center and had worked for a period of 17 years at the University of Rochester Strong Memorial Hospital. He had a longstanding interest in medical history.

Interview of Nicholas M. Nelson, MD

DR. KENDIG: This is Dr. James Kendig. The date is July 5th, 2000. The place is Hershey, Pennsylvania. And we are here to conduct an interview of Dr. Nicholas Nelson for the American Academy of Pediatrics.

Good afternoon, Dr. Nelson. Thank you for coming over this afternoon to give us this interview. Could you start out by talking a little bit about your own childhood and your early education?

DR. NELSON: I am the third of 4 sons of a professor of mathematics, Cyril A. Nelson, the majority of whose career was at the women's division of Rutgers University, then called New Jersey College for Women, now Douglass College. Took his PhD in mathematics, University of Chicago. I was educated in my hometown of New Brunswick, New Jersey. I went on to complete high school at Deerfield Academy in Massachusetts. Yale College, from which I graduated in 1950. Cornell University Medical College, 1954. One year as a medical intern at the Cornell division of Bellevue [Hospital]. However, within 3 months I had decided that pediatrics was in fact the career I wanted, believing erroneously as a medical student that pediatrics had insufficient intellectual content. I changed that view. So, a year in pediatrics with Dr. [L. Emmett] Holt [Jr.] at Bellevue, 2 years in the Army, which was not the fashion but the demand of that day; the doctor draft was still on.

So from 1954 to 1956 I was a captain at the US Army Medical Corps 34th Evacuation Hospital in France, which was the supply center for the entire European theater at the time. But because that was also at the time that the French governments were falling approximately every 10 days, on the rare occasion when we had to evacuate a patient back to United States, they were in fact sent out through the front lines. It was a delectable 2 years. We were, at taxpayer's expense, permitted to tour Europe in a fashion that nobody is wealthy enough to do these days.

I finished my service time and completed my residency at a Children's Hospital Boston under Charles [A.] Janeway. And during my career in the Army, I happened to buy Clement [A.] Smith's third edition of *The Physiology of the Newborn Infant*, fell in love with the same. I became aware during my residency at a Children's Hospital in Boston that right across Longwood Avenue and up the street towards Huntington Avenue was the Boston Lying-in Hospital. But I had never met Dr. Smith. Somewhere during that. I became aware of a thing called a fellowship which was just beginning to be funded in that day by the NIH [US National Institutes of Health]. Dr. Smith's grants I subsequently learned were all under the Association for the Aid to Crippled Children. I'm not sure whether that association still exists, but it funded Clem for many years.

In any case at just about this time, now this is the fall of 1958, my first child was born at the Boston Lying-in Hospital, as were 2 others of my children. But our initial experience as new parents was quite unrewarding at the Boston Lying-in Hospital. Specifically, there was no such thing as a lactation consultant. My wife was determined to nurse her children. Unfortunately, after 3 days postpartum, she developed mastitis. I learned that from her very expert obstetrician. I must say until you have seen mastitis, it's scary as hell. I was sure she was going to die of some massive sepsis, which of course mastitis can become if untreated. In any case, the support of the hospital for that event did not match the support of our obstetrician.

Now fast forward a few weeks and on a particular Sunday, I was enjoying a Sunday lunch with my fellow residents who had been stuck with that particular weekend duty. This of course was when house rotations were 3 to 4 days a week. But Sunday tended to be a little bit of a slower day and I was waxing on rhapsodic to my fellow residents as to what a bitter experience we had had at the Boston Lying-in Hospital. And I was so engrossed that I became unaware until too late of being kicked under the table by 1 of the residents. I also became aware finally that behind us was rather large, imposing figure, which turned out to be Clement Smith, just as I was saying what a terrible experience we had had. Dr. Smith, who is a master at droll humor (I didn't know that the time), said, "I understand there is someone at this table who didn't appreciate the Lying-in Hospital." And everybody pointed to me, and I'm feeling about 3 microns tall. And Dr. Smith, again a master of timing, said, "I just want you to know," in a very severe voice, "that I agree with you."

Subsequent to that I asked him, "What about this new thing called a fellowship?" And he was most supportive and so I went to work for him, not as his first fellow, there were several others, but I don't believe they had been funded by the NIH. The people who had been there previous to my arrival are all very well-known names in the field of neonatal technology. Mel [Mary Ellen] Avery, who was still at the Harvard School of Public Health working with Jere Mead, producing a famous paper published in the *American Journal of Diseases of Children* in 1959; but the abstract they had previously submitted was judged by the leaders of the American pediatrics of the day as of insufficient importance to be presented in any form at the Atlantic City [New Jersey] meetings [American Pediatric Society]. Also, in Dr. Smith's lab preceding me were Peter [A. M.] Auld himself, Dav [C. Davenport] Cook. I won't go on with their histories, they should be well known to anybody who is interested in this sort of history.

DR. KENDIG: Were you officially called 'fellows' at that time?

DR. NELSON: I can't remember.

Another was [Arnold] Jack Rudolph. And across the street at the same time, his brother Abe [Abraham M.] Rudolph, along with Julien [I. E.] Hoffman. Abe and Julian were perhaps the only, I can't recall, fellows of Dr. Alex [Alexander S.] Nadas. I'm not sure of the exact chronology here. I should also point out that Dr. Smith's right-hand man, a lady named Ruth Cherry was the older sister of James Cherry, a leading light of American infectious disease currently and actually for some years in Los Angeles. Ruth Cherry raised up all of us Smith fellows at the Boston Lying-in Hospital where we weren't altogether well received.

The Boston Lying-in the time, I suppose typical of many obstetrical hospitals even today, had a relatively small university service where babies were delivered by residents and their full-time teachers. Rather more of those children were delivered by private practitioners only a few of whom were supportive of the concept of people learning on their patients. It wasn't common but it wasn't rare either to find a little note on the bassinet saying under no circumstances allow Dr. Smith or his fellows near this infant.

DR. KENDIG: From the private pediatricians who also came in?

DR. NELSON: These were private pediatricians but there were several very major exceptions, including James Drorbaugh, who had been educated the University of Rochester. In fact, there was a respiratory fellowship with Herman Ron, I think. Also in that group, in addition to Jim Drorbaugh, was John Hubbell. They were the principal ones. And those 2, there were a couple others in that group but they were the major ones, they were the 2 pediatricians assigned to care for all the pregnant diabetic patients of the Joslin [Clinic] group. The infants of diabetic mothers, exclusively Joslin patients, and I'm forgetting the name of the obstetrician who delivered all of those ladies, usually by caesarean section, because although they were remarkably fecund their cervixes apparently did not loosen sufficiently so many of them came to have a cesarean section at 36 weeks or so. And of course, 36 weeks premature was in severe distress at that time; this was when respirators were just beginning to be used, in 1959.

In fact, 1959 was the year that one of the international pediatric meetings was in Montreal. And by virtue of Dr. Smith's physiology text, now in the third edition, and also Stuart Clifford who was the, if you will, clinical leader of the pediatric forces at the Lying-in hospital, many foreign visitors came to visit this famous nursery on their way back to their home countries from Montreal. And most of them stood at the door and laughed in an uproar because this famous unit had virtually nothing other than an occasional Gordon incubator. A feature of its care at the time was that infants were not fed by mouth until approximately 72 hours, a position that Dr. Smith gave up after considerable clinical pressure to the contrary. He was not the only one to hold that point of view, of course. But this famous nursery contained a jumble of babies in bassinets pushed so close together that you could barely insert a

knife blade between them. Nonetheless, inquiry was, under Dr. Smith, accepted and fostered to a remarkable degree. But it would not pass muster these days, nor would many others to be sure. Nurseries of that period would not be confused with a modern neonatal intensive care unit.

DR. KENDIG: Were there some more centers in the United States at that time?

DR. NELSON: Oh, yes. Here are the ones we became aware of, in fact easily conversant with. [Mildred T.] Stahlman at Vanderbilt [University], Paul Swyer in Toronto, Bob [Robert] Usher in Montreal, Bill [William A.] Silverman at Columbia [University College of Physicians and Surgeons], Baltimore, that is to say [Johns] Hopkins [Hospital], sort of faceless. Onn the other hand the author of the first book addressed to the problems of the newborn before the word neonatology was invented. You probably have it. The book continues of course, [Alexander J.] Schaffer! I had never met him, but he was known. I did meet him some years later. Bill [William H.] Tooley, I think at this time was in the general practice of pediatrics in Berkeley. He was a native of Berkeley. Stan [L. Stanley] James, who had been Dr. Holt's chief resident, defied Dr. Holt's advice that he would be ruining his career by going uptown to work with Virginia Apgar. Several of these people had been at Bellevue just before I got there. In fact, our contacts in this period, during my so-called fellowship from 1959 to 1961, were much more with James and Apgar than with Silverman as I recall. I have no idea the reasons for that other than I think Dr. Smith and Dr. Apgar had been associates for a long time. So during that 2-year period which was after Jack Rudolph, and others of that illustrious group had moved on, Sam [L. Samuel] Prod'hom from Switzerland joined us that year. A Mexican gentleman whose name has escaped me and Dimitri [Demetrios A.] Nicolopoulos. Dimitri is still active in Athens. The Mexican gentleman is probably retired now in Mexico City, but they began to make measurements of nitrogen balance and he was focused mainly on acid base balance.

And, who else did I mention? Oh, Nicolopoulos, nitrogen balance. And Dimitri's measurements of nitrogen balance in these babies who were thirsty and unfed for 3 days was collected and shown to Dr. Franny [Francis D.] Moore, chief of surgery at the Peter Bent Brigham [Hospital] and an authority in metabolic balance, and he remarked that these infants were showing more spectacularly negative nitrogen balance in the second or third day of life than he had seen in US Navy fliers existing in life rafts in the middle of the Pacific for 80 days with no food. And so, the scientific evidence in addition to the general clinical pressure from around the world began to change the Lying-in attitude towards preventing aspiration by denying newborn infant the privilege of eating. I should also point out that Mel Avery finished her fellowship around 1959 or 1960 and returned to Baltimore. She of course had indoctrinated, that's too severe a word, but told us about surfactant. She had allied herself with, I'm forgetting the name of the engineer, and he was responsible for building the Drinker respirator with Philip Drinker. This engineer

continued his interest in building respiratory assistant equipment, which continues to, well up until the last 10 years. Mind you, this is before positive pressure respiration really got going because it did not at the Lying-in hospital until much later, but it was beginning in Toronto and Vanderbilt and particularly at Columbia. In any case, Mel conceived the idea that it would be helpful and said, "Why don;t you,: and she had begun also, I believe, to put babies in a tank that was constantly evacuated by a vacuum cleaner that this gentleman, the engineer whose name is escaping me.

DR KENDIG: Emerson.

DR NELSON: Jack [John H.] Emerson, yes, thank you very much. Jack Emerson. So Peter and I bit on that. We had built a little plastic cylinder. My wife had suggested that maybe if you took the porthole from an Air-Shields isolette, you could make a pretty good neck seal around the baby and the baby's head, outside of course, and you could generate some negative pressure that way. Well, we could in fact. Now mind you this is before blood gases were sampled with modern electrodes. Although, somewhere in that period I began making measurements with Riley bubbles, which Tim [Thomas K.] Oliver [Jr.] was also doing; I hadn't met Tim at that point. But we both were beginning to struggle to make measurements of oxygen pressure. However, these babies in this little vacuum tank, to the naked eye were beginning to turn sort of pink. And a couple of them actually managed to survive at a time when by bedside analysis it seemed unlikely. These were mainly 36 weeks, fairly fat but struggling-to-breathe infants of diabetic mothers. A couple of them did survive, although they had significant excoriations around their neck from this isolette porthole. And God knows what kind of hurricane roars they must have heard and what body chills they must have sustained. I should point out here, I've forgotten so far that most of our study babies were Joslin infants of diabetic mothers. These would be well-infants of diabetic mothers, who were gone with the parents. This was before informed consent, our version of informed consent was to have the parents present as we collected gases from their little nostrils and so forth. I'm not convinced that the modern batteries of lawyers are any better. In any case, we became aware of the need to measure some gases and at that time instrumentational laboratories were just beginning. But we didn't have the money to buy one of their electrodes, besides which they used more blood than we thought babies could supply. The art of arterial sticks in babies had not been perfected, certainly not by us. But we did begin to do umbilical artery sampling under the tutelage of Stan James and Virginia Apgar and Millie Stahlman; she would get blood from the left atrium through an umbilical vein catheter, but that turned out to be relatively chancy. If you went into the umbilical artery you knew you were in the arterial system, so the business of threading a catheter through the umbilical vein up to the left atrium which was a significant event when it occurred. They knew, Apgar and Stahlman, what they were doing because they were sampling pressures and so forth at the same time. In any case, the first oxygen

measurements being made, we made with Riley bubbles. But then we built our own polarograph and I bet you have the electronic guts of that. But the instrumentation labs, I forget the guy who ran it, but he was excellent engineer and a great salesman, and he also had the fine platinum electrodes, much smaller than the Clark electrodes. And we rapidly learned in trying to make oxygen measurements in high oxygen breathing babies that it was a very difficult business because the polarograph consumes the oxygen that it is trying to measure. As soon as you get beyond full saturation you are dealing with oxygen that's dissolved in the blood and while this is familiar to those that make blood gas measurements, there weren't many such individuals at that time.

I'm working up to the Patrick Bouvier Kennedy story in a minute. John Severinghaus at UCSF [University of California, San Francisco] who, by the way, was the son of the onetime dean of medicine at Columbia, Aura Severinghaus. He had developed, it wasn't quite perfected then, the CO2 electrode, which was basically a pH electrode. And now, it would be nice if fellows of today went back over that because most of those techniques for blood gas had been known in the beer industry since the 19th century but just made a very slow transition into medicine. In any case, there was a period around, I would say 1960 to 1961, Sam Prod'hom has gone back to Switzerland, I think and Wes [Wesley] Boston and Florence Geller were in the lab at that time and the 3 of us became, at least in the Boston area community, the gurus of blood gas analysis. And we used to get taxicabs showing up at, not that they were particularly clever but we had instrumentation laboratory business and of course the concept of sampling blood gases hadn't really begun to take root in clinical medicine, although in physiology, in fact the polarigraph we built had been designed by John Pappenheimer in physiology something like 5 or 10 years before. But clinical medicine probably properly moves very much slower than, say, the beer industry or physiology. In any case, for a while there we were the blood gas gurus and taxicabs would arrive at the lying-in hospital with the samples duly preserved in ice from this or that baby.

At about this time I had begun to wonder whether I had really made a late decision for medicine to measure standard deviations and so forth and rattle test tubes and polarigraphs and not have much contact patients, to the extent that I decided to go into practice in my own hometown of New Brunswick New Jersey, which I did for 2 years, almost 3 in fact. First clearing it with Dr. Smith, I was aware of this indecision on my part. I was also aware that in his younger day it was quite common for people to have one foot in either camp, Dr. Smith had spent a brief time in practice. Dr [Alexander] Nadas had, they all had in generations just before us. In fact, the concept of full-time medical instruction was just beginning to really take root. Dr [Charles A.] Janeway himself, who was the son of Theodore [Caldwell] Janeway, once told us that his father who had been a prime moving force in the full-time teaching of medicine. Dr Janeway told us that towards the end of his father's life he had decided it was an error to have a faculty made up exclusively of full full-time

people because one needs to keep one's practice skills up. That argument will go on.

In any case, I went into practice and it was during that period that Wes Boston and Florence Geller, who continued in Dr Smith's lab, continued developing their expertise in the analysis the blood gases. And, mind you, this again was before respirators were used in any but highly experimental and usually unsuccessful circumstances. In fact, it was often a death knell, if you ever had a baby going on a respirator in the early 1960s. It was about this period, I had been in practice for a year or so.

DR KENDIG: Solo practice?

DR NELSON: No, I joined quite a busy practice. And that's another story. No, I was very busy the first week because it was an established practice, a group of 4 of us. It was about this time that Patrick Bouvier Kennedy was born on the Cape [Cod] and whisked to Boston. And the papers erroneously said Jim Drorbaugh, who I spoke of earlier, was the chief resident. In fact he was not; he had been in a successful private practice of pediatrics for quite some time and he became the physician of record. However, given the prominence of the patient, he was rapidly pushed aside by armies of full-tenured professors who crowded the bedside of this famous patient. And this was at a time when hyperbaric oxygen was much in vogue, at least in Boston and specifically at 300 Longwood Avenue for patients with cardiac problems as well as tissue infection. And aware that hyaline membrane disease featured hypoxia, the word was amongst these tenured professors, I will leave them nameless for the moment, that it would be an awfully good idea to the Patrick Bouvier Kennedy in a hyperbaric chamber. Well, Patrick Bouvier Kennedy had also had some blood gases sampled by Wes Boston who was still in London, Ontario, I think, and Florence Geller, who tragically died a few years later of some carcinoma. They had noticed in their many blood gas retrospective analyses that approximately 24 hours before the baby, a large baby one must say, with hyaline membrane disease began to improve clinically without benefit of respirator, you could see the PO2 begin to rise from the depths of 25 or so. And when they saw that rising PO2, sometimes accompanied by a slight decrease in PCO2 which was frequently in the 60s or 80s, that was a good sign. You might look forward to a little bit of an improvement. And Patrick Bouvier Kennedy's PO2 had begun to rise at the time he was put into the hyperbaric chamber from which he did not exit alive. Many years subsequent to that at a cocktail party, I think in Stan James' apartment, is where I first met Dr. Apgar. This is in New York; the occasion is some meeting or another. And Millie Stallman was there, vivacious as always. And we got to talking about the hyperbaric chamber some years later now and Millie said, "Put their tail in it, put their arm in it, just don't put their lung in it." So, we enjoyed that look backwards and that is as much I know about the Bouvier Kennedy episode.

DR KENDIG: Were you in Boston at the time?

DR NELSON: No, I was in practice what happened. Now this story is so delectable, probably completely apocryphal. Not completely, but I can't vouch for the validity of it, but it's so delectable I'll tell it anyway and trust the editors to expunge it if they feel necessary. This story is that my first professor of pediatrics at Cornell, Sam [Samuel Z.] Levine of [Harry H.] Gordon, Levine, and [L. Emmett] Holt [Jr], one of the early major nutritionists of pediatrics was happily in retirement, sitting, watching the telephone, when the phone rang. And it was Bobby Kennedy who was, I can remember exactly, Attorney General at the time; but he was frequently his brother's hatchet man. Anyway, he was most gracious according to this story. And he said, "Dr. Levine, are you free? My new nephew is in Boston rather ill and we hoped you'd be willing to come and consult." Patrick Bouvier Kennedy. And of course Dr. Levine said yes. Bobby allegedly said, "Fine. There is a Secret Service car waiting at the door for you now. It will drive you, it will whisk you to Idlewild [Airport]." This was before Idlewild became JFK. "And you will be flown to Logan Airport." Which he was. Another Secret Service car drove him to the Boston Lying-in Hospital and he became one of the phalanxes of professors who tended this dying baby. And the epilogue of the story is that Dr Levine returned to New York by taxi to Logan and shuttle to JFK and airport bus to York Avenue.

Anyway, so I returned from my interlude of practice, a procedure by the way I would recommend to anybody. I'm not sure it requires 3 years in practice to get the full flavor but I would recommend all academicians to spend a period of time in practice and vice versa as a matter of fact. And many schools have done the latter but not the former. And I formed the concept that the world of practice, the reward system is based on the esteem of your patients. And one thing I think that most physicians have extreme difficulty in handling, certainly I did, was the relatively rare patients who do not hold you in esteem and are in fact quite the opposite. Whereas an academic life I think we have well-run residencies, it's the residents and maybe even the students who have that level of intimacy with the patient, with the parents. Our reward system is in the esteem of our colleagues.

In any case, so I returned to Dr. Smith's lab. Oh, and I forgot major business during the 1959 to 1961 period. A child became ill with hyaline membrane disease. This child was in fact Dr. Smith's grandchild. He was desperately ill. And by this time Abe Rudolph had in the process of cardiac catheterization, a fairly large number of Joslin's infants with diabetic mothers with respiratory distress discovered the wide-open ductus. Much wider than Geoffrey Dawes had found in non-respiratory distress. And the concept arose of tying the ductus. And long before, as we are all aware of course, Abe joined by Julien and particularly Mike [Michael] Heymann, the control of the ductus became one of their major works over the next 30 odd years. But at that time all that could be done was to tie it. And in this particular instance the ductus of this grandchild was tied and it was unavailing, the baby went on to

expire anyway.

DR KENDIG: Who did the surgery?

DR NELSON: It was probably Dr. [Robert E.] Gross himself. I don't recall. If not he, somebody of that stature. I was only a fellow at the time.

So, after this interlude in practice I returned to Dr. Smith's lab in 1961 and then the only significant event that I can recall by this time, of course, the role of surfactant was well established. And the Ross Conferences on Pediatric Research, which had been going on for some time. In fact, in my army days, the first exchange transfusion I ever did was on a child who was the color of the *American Journal of Surgery* and his bilirubin was 35 or so. And my only previous exposure to exchange transfusions had been in Bellevue where they were done as a professor's procedure by Alexander Wiener. And his version of a continuous exchange was the put a catheter in; polyethylene catheters were just becoming available. Another technological advance was the Rochester needle. We called them Rochester needles, others called them buffalo needles. It was a small gauge needle from which one amputated the hub and then threaded that onto a piece of polyethylene tubing, and of course this is now done by the manufacturer for us. But in those days, even to this day, for me to put a scalp vein needle I have to use a hemostat; I can't use those little wings. We had to do it with a hemostat. And it was cutting off the hub, of course, that allowed the needle to lie flat enough on the scalp that you could actually keep it in there. In any case, the Wiener exchange transfusion was a polyethylene tube in the saphenous vein and then he would make a nick in the radial artery, this is an operating room, and from the radial artery blood would drip into a sterile shot glass. So, by the time I got into the service that was the only exchange I had ever seen. My first exchange, which was by the way successful, was done with a baby in one hand and the appropriate copy of the Ross Laboratories discussion of exchange transfusions with Louis Diamond and so forth in the other hand.

DR KENDIG: This was in France?

DR NELSON: In France. And I had the publication in one hand and the baby in the other and it worked fine. Now how did I get onto that? Somewhere in 1961 a Ross conference was held and Dr. Smith invited me to accompany him and I was happy to go. It was held in Kansas City. I'm not sure who arranged it. But the attendees included several of the famous leading lights pulmonary physiology. Herman Rahn, as I recall was there. André Cournand, by this time a Nobel laureate for cardiac catheterization. The Rudolphs, I think both Jack and Abe. The professor of physiology at Penn, his name is going to escape me. Known for his work in pulmonary transfer factor.

DR KENDIG: [R. E.] Forster?

DR NELSON: Forster. Thank you. There were a lot of leading lights there. And there were a few of us fellows. I'm not sure we were even called fellows then. I was one. A tall, Californian who spoke well and drank better, named Bill [William] Tooley was there. And that began our friendship of many years. And Bill had given up his practice of general pediatrics in Berkeley and become Julius [H.] Comroe's [Jr] first fellow, I think. He and Marshall Klaus were, I believe, Dr. Comroe's first fellows. And that was a fun meeting. He was particularly fun for Mel Avery, who of course was there; at this time surfactant was well known. And the keynote speaker was Dr Cournand. And I forget his exact words, I can't remember whether Mel's talk was before or after but it really made no difference because Dr. Cournand drew dramatic and immensely supportive attention to Mel Avery's work. And I'm sure she was on cloud 9 for quite some months after that. So I spent next 4 or 5 years with Clem. The date year by the way is 1964 to 1970. I was in practice from 1961 to 1964.

End of side one.

DR KENDIG: This is Jim Kendig conducting an interview for the American Academy of Pediatrics.

DR NELSON: And I just wanted to say regarding my years in practice in my own hometown, I mentioned that I think it is a good procedure for any academician to spend some time at that juncture. But there was an individual in that town who was, they were all good pediatricians, this guy was based dollars. I won't give his name because he has relatives still active in pediatrics, but this individual had not been blessed with the superior training that I thought I had had, but man, did he know pediatrics and did he read. This guy, now this was long before the modern full text online, he had full text in his head on almost any subject. He was miraculous. I felt quite small beside him.

In any case, I did not spend my life in pediatrics, I evidently have a personality that needs to be doing something relatively if not dramatically new every 4 or 5 years. So I returned to Dr. Smith's lab in 1964. I had a marvelous time. I'm not sure any particular breakthroughs were made but I enjoyed myself. One event I can recall was that, now this is 1964 so the idea of babies who go on respirators being given a death sentence had begun to fade. There were remarkable successes being achieved in the area of respiratory intervention. So much so that we began to, in the laboratory, look at a little machine, it was in fact made by the Harvard apparatus company, it was a volume respirator. And one particular night when Chet [Cheston M.] Berlin [Jr] was still a white suit, I'm not sure of the exact date, along came this baby who was fat and normal but zonked out with his mother's pain relief, just was not ready to breathe. This baby badly needed to have something to breathe for

him. So, Chet and I put him on the Harvard apparatus volume. It was an electrically driven syringe and we had to take a little piece of tape and obliterate on this machine where it said rodent respirator. The baby did just fine. Why did he do fine? Because he had perfectly normal lungs. He just, his motor was out until he got rid of whatever the pain relief was, the opiate.

We also began to use umbilical catheters, umbilical arterial catheters. At first, they were just used just for blood sampling. And then it occurred to us that you know it's a struggle to get the scalp vein in, keep it in. Why don't we begin to use it for injecting nutrients? And the nutrient at the time was just glucose water or saline. The TPN [total parenteral nutrition], I'm forgetting exactly when TPN came in but largely at the specific behest of Bill Hurd and his associate at Columbia. I think TPN is at least as major an event in neonatology as expertise in respirators. I don't think we were using it then. I'll back track, I remember during my residency at Bellevue under Dr. Holt who was a nutritionist of some repute and of course we had a lot of babies with diarrhea at that time and being Bellevue babies not all of them were very well nutrified. And amigen was the first protein hydrolysate that was available. And we began to use it at Dr. Holt's nudging as I recall, in babies who were very sick with diarrheal dehydration and poorly nourished. Although it regularly produced fever, it seemed to, again clinically, nobody was making measurements of nitrogen balance at the time, it did seem to help in their general improvement.

Anyway, at the Lying-in 1964 and thereafter, I don't recall exactly when TPN began to the used there; I'm sure after most of the rest of the country. And the fellows that I remember best who came through at that time were Chris North from England, Renato [Machado] Fiori from Brazil, Luis Prudent from Argentina. In fact, Luis came I think the year before I came to Hershey and he had hoped for, planned for a 2-year fellowship and with my departure, I'll come back to that in a minute, that was not to be so he finished his fellowship with Stan James in New York. About a year or so before coming to Hershey I became aware that I better look for a job because Dr. Smith was retiring. The year before his retirement, 2 years before his retirement he developed a severe pain in his hip and at his age at the time, which was younger than I am now, not too many people in their seventh decade come down with pains in the hips for benign reasons. However, it was proven that he had rheumatoid arthritis, which was very severe. In effect he was almost on total bed rest, certainly not at work, for the last 2 years or so before his retirement. In any case his retirement was due and his major support from the Association for the Aid of Crippled Children was due to retire with him. There was NIH support, a training grant for which I remember writing up a renewal application. But I was a relatively junior position and that application to continue Dr. Smith's training grant after his retirement was not supported, quite properly so. And I became aware that, since I had 4 young children, I had better start looking for another job. And I looked at quite a few. Rochester, I learned about from Bob [Robert] Haggerty. I had known

Bob for quite some time, in fact he had nominated me as his successor as an associate editor of the *New England Journal* [*of Medicine*], Dr. [Joseph] Garland, the editor at the time, accepted that suggestion at a marvelous time as one of the 3 or 4 associate editors of the journal during my Boston phase. That was due to Bob Haggerty. And he needed a neonatologist so I went to look into neonatology. But what I discovered, as you well know, was that when the Strong Memorial Hospital was built under [George Hoyt] Whipple's direction it included no nursery of any description and that made it difficult for Bob to recruit.

I also looked at Buffalo and St. Louis. Phil [Phillip] Dodge was the chair of St. Louis that time and he offered me more time, space and money to run his division of neonatology than subsequently was offered by our founding dean at Penn [Pennsylvania] State [University] for me to start a department. And you may wonder why I made the choice. The choice has to do with the fact that I'm an Easterner as was my wife. We got to St. Louis and at the time of the second visit my wife accompanied me. Phil thought he had us all signed up so much that he had a welcoming party and at this welcoming party was Jim [James] Keating who had gone to St. Louis from Mass [Massachusetts] General [Hospital] with Phil, I forget how many years before, and Jim was at this party but not his wife. I said Jim, "Where is your wife?" He says, "She's up in Chicago. Had to get out of St. Louis." Well, having to get out of St. Louis didn't worry me too much. I'm sure you and I have to get out of Hershey occasionally. So Jenny [Virginia] and I went back to the Queeny Towers and ripped opened the Esso map and tried to contemplate how long it would take to get to Chicago from St. Louis. We figured 8 hours of hard-driving might make Peoria but not Chicago. Easterners are used to covering 6 state lines in a rise and fall of 100 feet, so I didn't find the prospect of living in St. Louis fascinating. Midwesterners please accept my apologies.

And just at that time I got a call from Dr. [George T.] Harrell. And I found Hershey fascinating. What was fascinating about Hershey was that at the time, you may or may not recall, it was one of these weekly throw-aways, it was in black. And it was very well written like most of those things are in Dr. Harrell had had a couple of stories in there and it featured hearts and handbags, how Dr. Harrell was going to create a medical school that addressed science but also bedside science and at a certain, that rang a little bit for me you know I had spent almost three years in private practice and so I was intrigued and was happy to come visit and I met several of his very prominent basic science chairs, Howard Morgan, Fred Rapp, Gene [Eugene] Davidson, those were physiology, neurology and biochemistry. All very prominent, well-funded researchers. And I remember saying to myself, 'This guy Harrell has a double agenda going here. He's going to try to create good practitioners and good scientists simultaneously.' And I thought that was a further intrigue. And I will never forget Dr. Harrell took us, as I'm sure he did all of his potential recruits, to the front of the building and said, "Notice how the curvature of the crescent faces the community." You look out over all these cornfields and you

want to say, "Dr Harrell, what community? But I was intrigued, so much so that I raced back to Countway Library, grabbed all the demographic analyses that I could and was able to document that, just as he said, within 100 miles of here there are quite a few people. We just didn't happen to be at the center. And as I learned more, I discovered that outside of Philadelphia and Pittsburgh there was nothing in terms of subspecialty developmental pediatrics in this area.

So in fact I began to get bitten, but I was very worried that I had no administrative skills. I didn't even balance my own checkbook. My wife did all of that. But I was intrigued enough that I went to the then director of the Boston Lying-in Hospital. His name escapes me, but I admired his style and the way he seemed to have everything in hand. And I told him of my burgeoning interest in Penn State and should I be too worried that I didn't know a damn thing about administration. I'll never forget it, he said, "Administration is 90 percent judgment; if you don't have a knack for figures you can hire somebody to do the figures." And I think, you could argue over the percentages, but I think that's turned out to be largely true. Now back to Hershey it was pretty clear that given the demography and the fact that the birth population at Hershey was only 400 in a big year that if we were going to develop neonatology in this area it was certainly not going to be with newborns. I forgot to point out that during the 1964 to 1970 period a dominant fellow was Jeff [Michael Jeffrey] Maisels who went on to do his [US] Army tour of duty for 3 years at the Walter Reed [Army Medical Center]. And so here I am at Hershey and we needed someone to get neonatology going and so I gave Jeff a call. It was a fortunate group of planets in conjunction because he was ready to begin looking for job. So he started the unit here, opening it in April of 1973, something like that. At which time you were at Lancaster weren't you?

DR KENDIG: I was in the Army then.

DR NELSON: Army then. Well at that time, Jeff and I calculated that in our service area there were at least 40,000 deliveries annually at that time and no neonatal unit that would pass muster anywhere. And although I'm sure a fair number of babies eventually surfaced somewhere in Philadelphia or Pittsburgh, it was likely to be true that more of them were dying on the vine. So we just started out, this would be now 1973; personally going to virtually every hospital that we could identify in this clinical watershed of 40,000 odd deliveries and tried to identify those that might be receptive to the services we hoped we would begin to deliver. And initially, of course, he encountered a fair amount of resistance because most of these hospitals thought that they were rendering pretty damn good service as it was. Then we got geared up a bit more and we arranged for a transport. The initial effort at transport was the usual ambulance at that time, a low-slung Cadillac limousine. The business of the current box like ambulances didn't begin to take root until the Robert Wood Johnson Foundation made a big effort in the emergency medical services area. This would be somewhere in the early 1970s. But Hershey

didn't have one; certainly not this hospital. But Jeff made a pitch to the local Hershey ambulance, all volunteer, and they were instantly very enthusiastic, as far as I can recall. They signed up to be our deliverer of babies and did it for something like 4 or 5 years or more. They put on hours of unrecompensed miles, many sets of tires and so forth, picking up and delivering babies.

During this period Jeffrey would go back to all these hospitals with his crew, which was one of our 2 house officers or nurses or something and they would put on a show of picking up a baby. Stabilizing them first you know, and these sainted ladies who had run the intensive care unit of 'St. Elsewhere' said, "My God, is that what you are talking about?" And with regard to the intensive support, there was also a phase when the service transport was working well and we thought we could cover even more distance with helicopters. But we didn't have a helicopter here at that time. This was long before the current 2 that we have, but we were aware that at Fort Indiantown Gap there were quite a few Hueys that had just been freed up. This was obviously 1975 because Vietnam had come unglued then, and I guess it was Jeffrey who talked to someone at Fort Indiantown Gap. Sure enough since those helicopters were flown by Air Force reserve personnel and they'd be happy to participate. However, a Huey helicopter was military, they used Jet-A gas and after an hour and a half they come down like the stone because they've run out of gas and that during period of time you can't hear anything. Military helicopters look good on M.A.S.H. on television, but they were not very helpful in this instance. But the worst impediment was that at the time they were being flown by Air Force reserve personnel to get in their necessary logged hours. Apparently, at the time, the law said that if this reserve person is flying a helicopter he is not really in government service; he is a private citizen somehow or other. So we were afraid that if one of these reserve pilots picked up a patient in some 'St. Elsewhere' parking lot and a single piece of gravel went through some citizen's windshield, they would say, "Oh, I could sue Uncle Sam for decimation of my property." The law literally had to be changed before we could get Air Force Reserve people to fly helicopters. And some years after that we and virtually all hospitals started running their own helicopter service, which in my view has become much too easy. I know mine is a minority voice, but the idea of flying patients between here and let's say Lancaster, which is only 35 miles away over very good roads, strikes me as massive and possibly dangerous overkill. But mine is a thin voice on this, I think.

I think that brings me sort of up to date. What have I left that you want to hear about?

DR KENDIG: What advice would you give a young fellow today in neonatology in terms of planning their career?

DR NELSON: Well, before I give advice, let me give my own reaction. I don't know anyone in medicine who works harder than neonatologists. The ICU [intensive care

unit] guys and gals come pretty close. My own observation regarding those 2 fields, intensivism beyond the newborn area and neonatology are as follows. The neonatologist spends an awful lot of time contemplating maybe 4 major problems. One is jaundice, and jaundice bores me. I realize Jeffrey won't approve of that and Dave [David] Stevenson and thousands of others but jaundice bores me. Jaundice, sepsis, respiratory distress, occasional cardiac distress and nutrition, although nutrition is sort of routine now. I mean you plug them in and it is pretty constricted. Maybe once a while if a child, all of a sudden after 3 doses of milk, maybe goes flat out and you see metabolic disease or something different. So, it strikes me that a neonatologist has to struggle intellectually to keep excited about the field. Now I hope you can, because all of you guys seem to be still excited by the field. Certainly, you and Keith [Marks] do. I don't understand how you do it. To me it's intellectually restricting. But of course I haven't done it in 30 years so I hope I'm off base. So, when I went to cover the intensive care unit looking over the shoulder of Steve [Steven Wassner] and the other trained people, I found that much more expensive intellectually; but there was one aspect of it I couldn't stand. As soon as you can stop the drift and they're off the respirator, they are out of there and you have no idea what happened to them. There is utterly no continuity. Now you people have lots of continuity.

Emotionally I think I prefer the continuity; intellectually I think that I would find neonatology today stultifying and yet it isn't. I go to the meetings; I spend at least half of my time in the neonatal aspect of things it. But basically it is just to keep up; I'm not aware of big, new ideas. After respiratory support and nutritional support the huge problem remaining, as far as I'm concerned, is intraventricular hemorrhage. Yet I think we all know that if ever the problem of premature delivery were solved there wouldn't be such a thing as a neonatal unit as we know it. So, I would be interested in your rebuttal of that. Because it's clear that there are an awful lot of people who seem to be doing quite well and their intellects have not shriveled up despite quite a few years in neonatology. I do have the impression that after 5 or 10 years in the trenches, one has to have in an especial degree of stamina to keep it up with the previous enthusiasm. But that's a view from afar.

DR KENDIG: Where do you see neonatology going to the next 5 years?

DR NELSON: The biggest surprise to me in the last 4 or 5 years, I regularly go to Jerry [Jerold] Lucey's Hot Topics in Neonatology conferences and a couple of years ago he called me up to ask me to moderate a session on, I forget now what it's called.

DR KENDIG: Useless therapies?

DR NELSON: Yes, it was one of his first useless therapies. But this particular useless therapy, and you will know the name of the, I think it involves the nurses

heavily and you've got to do this and that with the babies..

DR KENDIG: Developmental [garbled].

DR NELSON: Is that the term? And the driving guru is some lady with a middle European accent. And I said, "Jerry, what is that? I've never heard of it." And he said, "You will. Go and talk to your nurses." And I did go and talk to our nurses. And they were very aware of it. And I sampled a couple of other units by telephone around the country and it was very clear this really turns these nurses on. Whether or not it is supported by science remains to be seen. But I believe firmly in the Hawthorne effect. And if nurses are turned on to something in the neonatal business, they are 80 percent of it; the rest of us could go fishing. So I don't think it's altogether bad to keep your nurses occupied and enthused. A neonatal unit is very much a team effort and if 1 group of the 2, whether it's the doctor or the nurse or whoever is feeling in a major way unfulfilled, that's not going to be healthy.

Notice how I enlarged that from neonatology. Let me come back to neonatology, all of pediatrics, of course, is applied developmental biology, but particularly neonatology. The grosser failures of developmental biology are first going to appear in these units, which is why there is so much concern, hardly inappropriately, over ethical concerns and here I'm not talking about the business of informed consent. I think a lot of the excesses that's brought informed consent into being were not at the behest of what I would consider real physicians, as opposed to mere MDs. So is this a way of saying that there is no scientific field left to be plowed in neonatology? Certainly not. All I can say is my vision is not that large. That's like saying we shouldn't be interested in molecular biology or something.

I still struggle to learn a little bit about molecular biology. Tom [Thomas] Hansen, distinguished ex-neonatologist and now chair of pediatrics at Ohio State [University], spent a couple of years learning molecular biology. He claimed he was well turned on. I think, at least in part, he and many others may be responding to the impression, especially for younger academicians, that you have to have some kind of shtick to get noticed and if you don't have the molecular shtick you have no future. I do not believe that at all. You know the molecular biologist is not at the bedside and his or her principals are not going to get to the bedside without a physician who is aware of what's going on in molecular biology.

In many ways I wish medicine would catch up to, say, the physicist. The guy I was always in competition with throughout grade school, he always beat me, not by much, but he always beat me. And he went on to become a nuclear physicist. But he's a proud bench physicist. And there is another proud branch of physicists and that's the theoretical physicists who prides themselves on the fact that their most expensive equipment is a yellow legal pad and a couple of No. 2 pencils and their

brains. They have been at it much longer than medicine and I would like to see that happen in medicine. I remember being told by Dr [John] Waldhausen who was the founding head of our department of surgery some years ago, he had some big surgical meeting here at the hotel and they were addressed by George Cahill [Jr], distinguished investigator in medicine, particularly in insulin and its many actions, from the Joslin [Diabetes Center], so forth. And I'm sure George was a founding member of the Howard Hughes Medical Institute and he was a featured speaker at this particular meeting that I did not attend. But in this, according to Waldhausen, and Cahill made the point that if you can't do it full-time then there's no point in getting involved in research. And he's talking to a group of surgeons; a group of surgeons in a department of surgery that had an awful lot of NIH [US National Institutes of Health]-funded research. And John expressed to some passing irritation at being told that. And of course Cahill was correct in that if you're going to pursue competitive molecular biology you are not going to have time to make rounds every day of the week. And I don't see that changing all. We see it the training level. I'm sure if any of your fellows go over to the basic science division, the basic science guys get all irritated, "Well he's going to be on call 3 nights a week." I think that's a creative tension that should not be changed. I think it's necessary to both worlds and the tension is necessary to have continuing appreciation of each camp by the other. Some places do it well; some places do poorly if at all.

I notice, by the way, that the first few years that I was at Hershey we had more co-operative ventures between this medical center and the main campus which is 110 miles away than I ever saw in the Harvard quadrangle which is half a block away. But of course, they had had a longer period of time to get mad each at each other or at least decide on non-co-operation. So what to draw from that with neonatology, I'd say by all means maintain meaningful co-operation within the medical school. There are quite a few neonatologists who don't even relate to obstetrics let alone a medical school. Some years ago, there was an individual whose name is too well-known, I won't mention it, but you shouldn't have difficulty figuring it out who had the concept. He wasn't the only one who had this concept that neonatology should split off from pediatrics and join obstetrics. And obstetrics should split off from gynecology. And gynecology should return to surgery and the rest of pediatrics should return to medicine. Well, not only was this intellectually unpopular and emotionally assaultive, but it also denied certain basic principles of cash flow as to the support, now I'm referring back to the day when neonatology was a famous loser of money. That's changed relatively recently to quite the opposite. Given what happening in health care the moment, which will continue I think, neonatology generally could easily collapse in a matter of moments and all these people are out there might discover it all depends on how supportive American or world society is in general.

I've told the story, by the way, many times. I think one of the most fascinating

papers in the neonatal business I've seen in some years was 3 or 4 years ago there was a poster presentation related to Israel and they posed the question to 3 groups of Israelis. "What would you like to have done to your 450 gram, 22-weeker?" And the Russian émigrés, the recent immigrants from Russia to Israel, responded, "Whatever is God will." The native Israelis responded, "No heroics." The North American émigrés said, "Spare no expense." And of course, here we are, both parties in the process of recreating in my view fee-for-service medicine. That's the real meaning of these patient's bill of rights. We are on the verge of recreating fee-for-service medicine now with a 30 or 40 percent markup for regulation and profit. And I don't think our society, or any other society can support that level of investment. Nonetheless, whatever happens to the cash flow I think the concept of every birth being valued, as opposed to fold your hands and better luck next time, we are not going to go back to that. So all meaningful and hopeful, non-futile effort will, I think, continue to be expended in the name of the newborn.

I'm really quite happy with what has happened to neonatology. I'm quite unhappy as to sending, and I'm sure you are also, sending many of these babies back to an environment where they cannot possibly survive even as physically normal children. And that's beyond the issue of medicine alone. However, the second dean of the school, Harry Prystowsky, among the things that contributed to his reputation when he was at Gainesville, and I think I have this correctly, he was able to improve the perinatal outcome of a lot of Black ladies without an infusion of any particular money. He worked through the churches, the Black churches and I guess the white churches too, to encourage these ladies to register their pregnancies early and this, that and the other. It was a huge infusion of effort but I'm not sure it was a great infusion of new dollars. So I think that's a challenge for neonatology.

Among the things I'm proudest of neonatology having done in the relatively brief time I have been around, and certainly when we started, was the concept of co-operating between neonatology, those that took care of the newborn infant and those that delivered the newborn infant. That was unknown. Absolutely unknown. The idea of making rounds together, that's all changed and I don't think it's ever going to go back to that.

The intellectual challenge of what's to come. Well, we could talk about unsolved problems of course. Intraventricular hemorrhage, necrotizing enterocolitis. The answers to those are most likely going to be somewhere in developmental biology rather than clinical observations and arguments of bronchopulmonary dysplasia. I think those are all developmental diseases and I don't think 15 more studies of steroids yes or steroids no are going to make any difference in all. That's going to be a biological solution. But it's the answer to the question whether you can do anything with the biology we are presented with; I don't know. The clear answer of course, is the termination or absolute prevention of premature labor. I'd like to see some firepower addressed to that because almost everything else flows from it.

The business of genetic defects. The occasional Siamese twin gets lots of publicity, but they are really quite rare. And they wouldn't be the first on my list. But I won't be the least surprised and certainly delighted if some entirely new challenge comes up and if history repeats itself, and it always does sooner or later, the real answer to some of these things will come from somebody entirely outside of neonatology, probably outside of medicine altogether. Maybe working in the beer industry for all I know.

DR KENDIG: Well, Dr. Nelson, thank you very much for sharing your distinguished career with us today and on behalf of the academy of pediatrics, I thank you for coming over.

End of Recording