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Special Article

# Placentas, Peccaries, and Pathologists: Reminiscences of Kurt Benirschke on his Career

## An Interview with Rebecca N. Baergen

Dr. Baergen: Where did you grow up and what was your childhood like?

Dr. Benirschke: I was born on May 26, 1924 in Glueckstadt, north of Hamburg, where the Holstein cows come from. I grew up there, in this very small town of about 5000 people. It was a farming community—one doctor in town, one dentist—and my father was employed as an organic chemist in a large paper factory. He originally came from what is now the Czech republic and had finished his chemical degree in East Prussia, which is now part of Russia. I had a wonderful childhood with my two sisters and comfortable surroundings. My father ultimately quit his job and started a laundry. My sisters and I worked every weekend in the laundry just to make ends meet. When the war broke out my father, having been one of the few businessmen in town who knew anything about chemistry, was asked to start a laboratory that could examine possible poisonous gas attacks. So he started a laboratory, which I was made to run and so I did a lot of organic chemistry as a kid in high school.

Dr. Baergen: How did the decision to go to medical school come about?

Dr. Benirschke: Actually, I wanted to study organic chemistry like my father but my sister's husband dissuaded me. He said, "You know, you'll never be able to go the University now if you become an organic chemist. If you go and study medicine then there is a chance you might be accepted." And so in 1942, I began medical school in Hamburg and did one semester before I was drafted into the Air Force.

Dr. Baergen: What was your experience in the Air Force?

Dr. Benirschke: I was in the parachute division and, after a year, they sent me back to medical school in Wuerzburg, in Bavaria, to become a medic in the Air Force. We were sent to the northeastern front, East Prussia, and then to what is now Alsace Lorraine and eventually to the Battle of the Bulge. That's where I contracted acute hepatitis. I was sent to the hospital and was supposed to go back to the front but I contracted diphtheria. There was no more anti-serum in Germany so I was given sulfonamides, which didn't jive with my liver too well, and so I was again in the hospital. When the war was over, the hospital closed and I just went home. I was discharged later on by the British Army, which occupied that area. I was able to go back to medical school in September 1945 and finished medical school in Hamburg in 1948.

Dr. Baergen: What did you do after completing medical school?

Dr. Benirschke: The currency reform had just hit Germany and the medical schools were all somewhat disorganized, and there was no way I was going to get an internship. So I volunteered in the University of Hamburg Hospital in the bone marrow pavilion. I did lots of bone marrow, spleen, and liver biopsies for about half a year, which helped me a lot later.

Dr. Baergen: How did you come to immigrate to the United States?

Dr. Benirschke: My father had died during the war and I had told my mother that I was not a businessman and was not going to run the business. I had planned on emigrating and I got to know a girl who worked in the American Embassy. One day she told me, "on Monday morning there will be an open emigration to America and if you want to stand in line you can register." So I stood in line for 4 hours and became number 435 or something like that. It turned out that only 50 visas were going to be given and because I knew her, she struck out the number "4"; I became number 35 and got a visa to immigrate to America.

Dr. Baergen: Tell me about your first few months in New York.

Dr. Benirschke: At that time, you had to have a sponsor in America and know English. I had found a sponsor

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in America, a very distant cousin. My mother had invited an English boy, an inductee in the British Army, for Christmas so that I could learn some English. He played the violin and I played the piano and so we played music together, but I didn't really learn any English and he didn't learn any German. When I was going to emigrate, he suggested I come to London first and learn some English. So I went to London by freighter and we walked through London for about 4 weeks—up and down, all the museums, everything. Eventually I found a freighter to take me to America and left in August or September of 1949. I became a newspaper boy in New York, learning English on the street more or less. I used to go to Columbia University Medical Center on my route. I would go there every Thursday afternoon (because that's when the AMA journal came out) and look for internships. There were lots of them advertised, but they were always too far—Miami, Toledo—and I didn't have the money. I just barely lived. One day I came by a huge skyscraper; it had a big bronze plaque on it that said, "The Office for the Placement of Displaced Physicians." I said, "That's me." So I went in and I overheard two men talking in the hall about an internship available in Teaneck, New Jersey. I said, "Teaneck, I know where that bus goes" so I went to Teaneck to the Holy Name Hospital and presented myself to the internship committee. They had an ancient, third-generation German physician, Dr. Blenkle, who translated what I had to say. And after awhile I told him that he didn't have to translate because I could understand and speak some English. They said, "If you speak English, you're hired." So on the first of January 1950 I started as a rotating intern at the Holy Name Hospital in Teaneck, New Jersey.

Dr. Baergen: What was your internship like?

Dr. Benirschke: I did 3 months of surgery, a month of ear, nose, and throat, etc., and a month in pathology. While I was in pathology I noticed that the pathologist, Dr. Markley, went to New York every time we had a bone marrow. I told him he didn't have to go to New York because I knew how to do bone marrows. So I did his bone marrows, then his transfusions, traveling in his car to give transfusions to patients in the neighborhood with aplastic anemia. Pretty soon I was doing his autopsies and then his surgicals. I rotated out but continued doing autopsies. Then, at the end of the year, he asked me what I wanted to do. I said I'm going to be an associate of the German internist, Dr. Blenkle. And he said, "That's ridiculous, you're already half a pathologist. You ought to do pathology. In fact I've made an appointment for you as a resident in the Doctors' Hospital in New York." But before that was to start, he told me that there was a new pathology department chairman going to Harvard (Clinton van Zandt Hawn) and he was looking for interns. I didn't even know how to spell Harvard at the time, but I met with the new chairman and started as a new resident on July 1, 1951, at the Peter Bent Brigham Hospital in Boston.

Dr. Baergen: Tell me about your residency training.

Dr. Benirschke: You spent a year at the Brigham, half a year at the Boston Lying-In Hospital, a year at the Free Hospital for Women, a year at the Children's Hospital, and then the last year back at the Brigham. It was all anatomic pathology. Clinical pathology at the time wasn't really invented, because it was still done by the department of internal medicine. Our salary was 45 bucks a month and so I translated for money, $5 an article—lots of articles—for the budding new physicians there in medicine. I learned a lot of interesting new medicine and biology, particularly about the hypothalamus, transplant biology, and immunology, which were just then coming into fruition.

Dr. Baergen: Tell me some of your most vivid memories from your residency and the people who influenced you.

Dr. Benirschke: One thing that was really excellent at the Brigham was Grand Rounds, every Thursday morning from 8 to 12. Everybody was in attendance, all the professors, and lots of people from town came. Cases were discussed in great detail with very, very good teaching and reviewing sessions. At the Free Hospital for Women, I remember one wonderful thing was that, every Saturday morning, all physicians who did surgery on women came together for at least 2 or 3 hours. There was a bank of 25 to 30 microscopes, and the slides were handed from left to right and everybody discussed the cases out loud; it was a tremendous learning experience. I was there when Arthur Hertig hunted for the early implantation of eggs into the uterus—a very interesting experience. At Children's Hospital, Sidney Farber was Chairman of Pathology and the autopsies were extraordinarily well worked-up and presented in much greater detail than things are now, and there was much more participation of senior staff. Saturday mornings for instance, at Children's Hospital, Dave Hsia (a pediatrician on staff at Children's Hospital) had free-for-all case reviews. It was an interesting time. I remember that inborn errors of metabolism were discussed in detail, and chromosomes were for the first time being discovered. I remember well going down the hall one day in medical school and Guido Majno came excited down the hall and said, "You know it is possible now to tell boy from girl cells. There is an article in *Nature* this week." And so we learned about Barr bodies, and that was just the beginning (1). There was an enormous amount of cohesion

among the residents and interns. Most of us were not married; in fact I think being married was very exceptional. And all of us, more or less, lived in the hospital and so every evening at 9 or 10 o'clock we'd all come to the dining room (basically we were allowed to eat whatever the patient's didn't eat) and had extensive discussions about politics and medicine.

Dr. Baergen: What was your first position after you completed your residency?

Dr. Benirschke: After my training was over I desperately wanted to be the pathologist at the Boston Lying-In Hospital. I did become the assistant pathologist and no sooner did that happen than the chief pathologist, Don McKay, was drafted into the Korean War. Suddenly I became the chief pathologist at Lying-In Hospital knowing very little about neonatal mortality. But we learned a lot in those days because we needed to know. We always had clinical residents that had to do a month of pathology and they expected me to tell them the answers. At the same time, Hazel Mansell (later, Hazel Gore) was at the Free Hospital for Women, and she became a very good friend. She was an Australian immigrant and knew gynecologic pathology well. I stayed as pathologist at the Lying-In until 1960 when I went to Dartmouth.

Dr. Baergen: How did you initially get interested in obstetric, perinatal, and placental pathology?

Dr. Benirschke: I became interested in the pathology of the placenta, because it became obvious at the time that most of the perinatal problems really arose from placental problems. Because nobody really had done any placental pathology, I began to do placental and perinatal pathology. No one was interested in the reasons for perinatal mortality and so there was a void in any diagnostic work-up. I decided that we needed to have weekly conferences, which we had with the pediatricians and with Clement Smith (Chief of Neonatal Medicine). It was just one upmanship of trying to persuade them that we needed to find out why all these babies are born prematurely and die from hyaline membrane disease. Another reason was that in all this translation of German articles, particularly the stuff from Kiel on the then just developing notion of neurosecretion in the hypothalamus, the raison d'être of the fetal adrenal zone became a challenge (2). It was absent in anencephalics, and anencephalics didn't have a hypothalamus (3). I worked on the hypothalamus of newborns and how the neurosecretion developed, and then I decided I needed to find out what the fetal zone of the adrenal actually does. I knew that there was an Institute for Biology in Worcester, Massachusetts run by Ralph Dorfman. I went to him and told him he had to help me find out what steroids are made in the fetal adrenals. He said he had a young man, Eric Bloch (later a member of the Department of Biochemistry at Albert Einstein in New York), who could be my colleague. So we started working out what the fetal adrenal zone made (4,5).

Dr. Baergen: How did you go about becoming proficient in placental pathology?

Dr. Benirschke: Well, actually that wasn't so difficult. I was the pathologist at the Boston Lying-In and did about 150 to 180 baby autopsies a year and all the surgicals. As I said, I was interested in why the adrenals of anencephalics are so small and because we had very little money and there wasn't much else to do, every evening my wife, Marion, and I went to the hospital to look up all of the cases from the past on anencephalics. And one day she said, "Well you know, it's amazing but there are many that have only a single umbilical artery." I said, "Isn't that interesting; I wonder why? Somebody ought to look at the placentas." So I asked them to give me every placenta from one year and I examined them—5,000 or so—and found that about 1% had a single umbilical artery and the babies may or may not die (6–8). There was nothing really known or written about the pathology of the placenta, and it turned out that there were lots of lesions that had not really been described except in the German literature. Shirley Driscoll later became my associate pathologist and then began the famous Collaborative Perinatal Study involving 13 to 14 hospitals to look at 40,000 pregnancies prospectively, what happens to the babies, and to understand the etiology of cerebral palsy. I wrote the protocol for the examination of the placenta (9). Then, in the early to middle 1960s, Arthur Hertig was asked to write a volume on placental pathology for the *Henke-Lubarsch*, the famous German handbook of general pathology. He said, "I don't have the time, but I'll give you the name of someone who does." I was writing the protocol for the examination of the placenta for the collaborative study anyway, so I asked Shirley. So, she and I wrote the volume together (10) (Fig. 1).

Dr. Baergen: How did your collaboration with Peter Kaufmann develop?

Dr. Benirschke: Shirley Driscoll and I wrote the first edition of the *Pathology of the Human Placenta* (published in 1967) (10). This edition sold out and the editor of Springer-Verlag wanted to publish a second edition. Shirley and I had felt that we really needed an anatomist interested in placental pathology when we wrote the first edition but she was not interested in writing a second edition. I had met Peter Kaufmann at an anatomical meeting and discussed with him the possibility of collaborating on the new edition. He had done research on the placenta and was enthusiastic, so we starting writ-

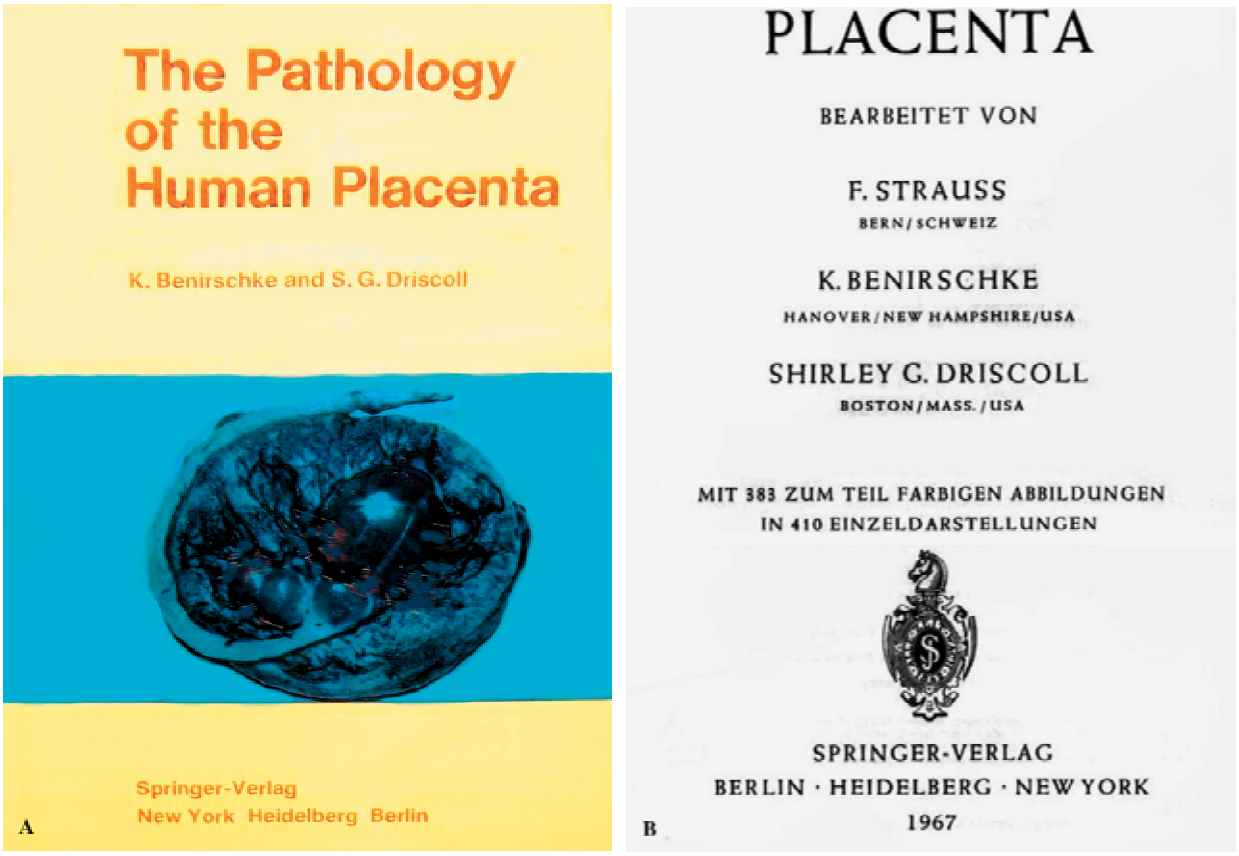


**FIG. 1.** Book cover (A) and frontispiece (B) of the first edition of *Pathology of the Placenta* by Kurt Benirschke and Shirley Driscoll published by Springer-Verlag in 1967. Reprinted with permission from Springer-Verlag.

ing the book together, which was first published in 1990 (11).

Dr. Baergen: Would you recount some of your most memorable experiences while you were at the Boston Lying-In Hospital?

Dr. Benirschke: There was a time when it was totally obscure why there were never any mitoses in syncytiotrophoblast and it was thought that the syncytium was really a derivative of the maternal corpus luteum cells. I came back from showing Arthur Hertig some slides and told two fellows, Ralph Richart and Michael Galton, an obstetrician from London who became a resident at the Boston Lying-In Hospital, that we needed to work out how the syncytium is formed. Each one worked on it separately and they went at it tooth and nail, competing with each other vigorously. Michael did it with cytology and with microspectrophotometry. Ralph did it with tritiated thymidine. He gave placentas tritiated thymidine *in vitro* and interrupted the cultures at different times and found that the grains eventually ended up in the syncytium. Michael showed by cytomicrospectrophotometry that syncytium was always diploid but that cytotrophoblast was diploid and/or quadriploid and concluded that the syncytium comes from cytotrophoblast. Both of them did this independently and published their findings at the same time (12,13).

Other really interesting aspects of those days were that twins died so much more frequently than singletons. I read a lot about twins, particularly because of the transplantation of kidneys in identical twins. Joseph Murray transplanted kidneys successfully in a set of twins and I told him that they were identical twins. But he said, "No, they are fraternal because they had separate placentas." They published it in the *New England Journal of Medicine* (14) as fraternal twins but about 5 years ago, they retracted it and said that they had been identical twins. They knew nothing about placentation then. The twins looked different and one had kidney disease while the other did not. Anyway, at that time I wanted to know more about twins, and it wasn't very well studied then anywhere. But I became interested in armadillos by then because of their monozygotic quadruplets (15,16). Why armadillos always have identical quadruplets is totally obscure. There are many theories, but none has been

proven. So I imported armadillos to the Boston Lying-In Hospital—where they escaped.

Dr. Baergen: Would you elaborate on that story?

Dr. Benirschke: There was no department of animal care or a chief veterinarian at Harvard. Everybody had their own little mice cages or whatever. I had bought armadillos from a guy in Texas and put them in the autopsy room, which was in the basement next to the kitchen. They escaped that night and turned over a vat of formalin, which spilled into the hallway. The kitchen department became very upset, so they opened the door, put a big fan in there and the armadillos ran out. Fortunately, they were not hard to catch!

Dr. Baergen: You also had a colony of marmoset monkeys in Boston. What interested you in marmosets?

Dr. Benirschke: I started a colony of marmoset monkeys at home in my basement. We had up to 35 monkeys. They always have fraternal twins, they always have blood vessel anastomoses, and they are always chimeric (17,18). We found out later that the female twin isn't sterile in marmoset monkeys, which is what happens to fraternal twins in cattle (19). This is probably because of poor placental handling of androgens in the cow placenta. So we had lots of armadillos and marmosets and all kinds of animals in the cellar and they made a big impression. My wife, obviously, could almost never leave the house because we had to look after the animals at home, so we were basically housebound, but travel was rare in those days. Airplane travel was just beginning and so the senior faculty was in town all the time.

Dr. Baergen: You are often given credit for raising the consciousness of people in the medical community about the importance of the placenta. Is there any thing in particular that you think enabled you to do that?

Dr. Benirschke: Well, basically, to pathology the placenta is a new organ and it isn't very seriously considered because it's done its duty, it has raised a baby, and might as well be discarded. So it had never really been looked at from the point of view of its contribution *in utero.* The Collaborative Study first propelled the possibility that there was more to it than that because it could be looked upon as an organ that may be responsible for growth retardation or abortion or premature birth. The notion that infarcts are correlated with preeclampsia and with prematurity, growth retardation, and so on was accepted but the mechanism by which this came about really was not further elaborated on. I think it was the Collaborative Study and the fact that I wrote this first book on placental pathology that brought to mind that perhaps one can identify a few things in the placenta that make it easier to understand prenatal growth. Then came sonography, chromosomes, and imprinting. Perhaps also the fact that placental pathology findings helped defend obstetricians in lawsuits added to its acceptance (20). One could learn a few things that were heretofore obscure. So I think it was a natural progression, beginning perhaps with the single umbilical artery, which was a shock to everybody that such a thing existed and that it had a correlation with perinatal mortality and anomalies.

Dr. Baergen: Tell me about your first case of maternal floor infarction.

Dr. Benirschke: Maternal floor infarction is a really interesting condition that I hope in the next 10 years or so will become understood from a genetic point of view because, clearly, one thing we don't understand at all as yet is the regulation of trophoblast in fetal development. Maternal floor infarction clearly is misdirected trophoblastic activity that produces excessive amounts of fibrinoid (Fig. 2). Inasmuch as it is frequently recurrent in subsequent pregnancies, the possibility exists that this lesion reflects genetic misinformation in the placenta. With the cloning of genes and so on it ought to be possible soon to take a family with this condition and look for the genes. Mrs. Kirk, I remember her well, had seven pregnancies with maternal floor infarction; that's why we described it because it was so striking that one pregnancy after another suffered from the same, theretofore totally unrecognized abnormality of the placenta (10,21).

Dr. Baergen: What other aspects of placental pathology are you particularly interested in?

Dr. Benirschke: I think the most interesting cases are the mosaics and chimeras (18,22). I'm mostly interested in comparative placentation, as you know, and in the twinning phenomenon. I think one real puzzle is how arteriovenous anastomoses form in the first place in monozygotic twins (23–26). Why is it that armadillos never have anastomoses in monozygotic twins, but human monochorionic twins always do have anastomoses?

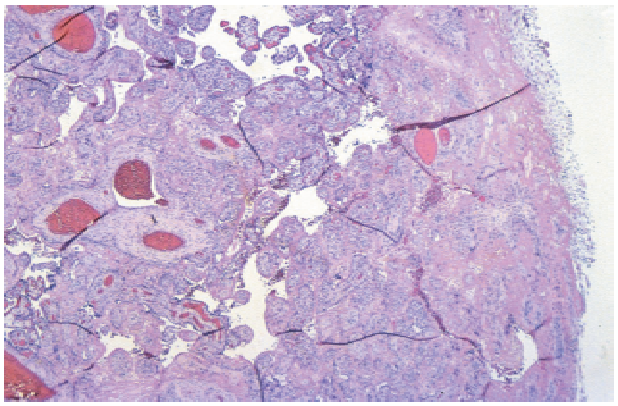

**FIG. 2.** Photomicrograph of a typical maternal floor infarction as first described by Dr. Benirschke (10), showing deposition of perivillous fibrinoid particularly in the maternal floor (extreme right).

Why is it that some make arteriovenous and others only artery to artery anastomoses? So this must relate, to me at least, to very early embryonic blood vessel formation when the developing embryo sends out its blood vessels to populate the villi. Interesting to me is how a totally new form of placentation develops in evolution, placentas that are so different in structure. How can one animal develop a new placenta? All the spleens of animals, all the livers, are all the same. It's the placentas that are so vastly different, and that to me is a major puzzle. Why is it that the South American primates have such a different placenta from all the other primates? And how does this lead to our comprehension of what develops? And, how do you go from an epitheliochorial placenta of the lemur to one that becomes invasive in African and South American primates?

But I'm most disappointed by the fact that the overriding problem in obstetrics, as I see it now, is prematurity and ascending infection (27–29), and it is not addressed. Ascending infection clearly is a problem of the endocervix and it isn't really taken seriously from a therapeutic or preventative point of view by the obstetricians. The "incompetent cervix" is basically only a sequel to chronic endocervicitis, and it is striking that almost all of the babies who are born between 20 and 30 weeks are born because they have chorioamnionitis. They are the only babies that now die; all other babies make it because hyaline membrane has been eliminated.

Dr. Baergen: How did you become interested in comparative pathology?

Dr. Benirschke: Chromosomes had just been "invented" in the late 1950s. Very shortly after I came to New Hampshire, I was asked to speak to the New Hampshire Society of Pediatricians, a very small club. I told them about chromosomes and I told them I was interested in finding a mule to find out why mules are sterile. "Oh, I know where there is one," said one of them, and so we did chromosomes on mules (30). And then there was a picture in *Life* magazine of a zebronkey (a zebra-donkey hybrid) in Manila, the Philippines, and I said, "I've got to get a piece of skin off that animal. I've got to find out what that's all about." My last resident in Boston was from the Philippines so I wrote her, Josephine DeVenecia, and said "how about a piece of skin from that zebronkey?" Her husband was on the board of trustees of the zoo in Manila and sure enough I got a piece of zebronkey. It had the weirdest chromosomes. I published it in *Chromosoma* in 1964 with a picture of the parents (31). They had misidentified the species of zebra and I got all kinds of protests—telephone calls from taxonomists telling me that's nonsense. So, then I needed pieces of zebra from different species and that's how I got to the Catskill game farm and began doing comparative cytogenetics because it was of interest with respect to sterility. Some very unusual findings came up very quickly such as the fact that there were very similar looking animals such as the muntjac and barking deer from India and China that had very different chromosomes. One has only 6 chromosomes; the other one has 46 (32). I did a lot of chromosome work and began working with T. C. Hsu (a geneticist, later at M.D. Anderson in Texas) on writing the *Atlas of Mammalian Chromosomes*, which took 10 years (33–42). One year I decided to hold an international conference on reproductive failure and one on comparative cytogenetics, and in those informal meetings David Carr (a geneticist from Canada) and I decided to "tackle abortions" (Fig. 3). So we found out that abortuses were largely due to chromosomal errors (43,44), and later Andre Boué and his wife in Paris looked at a large number of them to support this (45). But those were innocent days when nobody had a clue of why abortions happened.

Dr. Baergen: You also have an interest in Freemartin cattle.

Dr. Benirschke: Well, they are named Freemartins because they are dud cows. They never become fertile and never give milk. They were considered basically useless and traditionally they were slaughtered on St. Martin's Day in England. Cows occasionally have twins and Frank Lillie, professor of zoology in Chicago in 1916, wrote a paper saying that the putative cause of Freemartinism was the presence of anastomoses between the placentas of boy and girl twins (18,46). At the same time, in 1916 to 1917, Viennese endocrinologists Keller and Tandler (47) also wrote that, because of connections between the twins' circulation in the placenta, the females were sterile. If there were no connections then the females would be okay. So, Freemartins are sterile female cows, which are twins to male cows. They are also only blood

**FIG. 3.** WHO meeting on abortions in Geneva. From left to right: Bob Edwards (UK), Kurt Benirschke, Alfred Gropp (Germany), J. Schwarzacher (Vienna).

chimeras and not connective tissue chimeras. The interesting thing of course is that marmoset monkeys also are all blood chimeras, but the females are not sterile. Nor are the few human blood chimeras, such as Mrs. McKay in London, a very well known case, who was one of the first fraternal twins with anastomoses (18,22). Her twin brother had died when he was 3 years old or so. She was pregnant when she was studied by the transplantation immunologists, and they could determine from her lymphocytes that she still had circulating male lymphocytes. They determined the genotype of the twin who died. This type of blood chimerism was an anatomic curiosity, and it had to be worked out endocrinologically why this was so different in primates.

Dr. Baergen: What were your first experiences with twin-to-twin transfusion syndrome (23)?

Dr. Benirschke: I decided the only way to cure the transfusion syndrome in twins was to interrupt the circulation between the twins (Fig. 4). The means of getting at the fetuses was difficult at the time but I persuaded an obstetrician to actually try to ligate the vessels and we did this. He performed a hysterotomy to locate the umbilical vessels. But at the time you couldn't identify the location of the placenta because ultrasonography hadn't been invented yet, so he cut through the placenta to get to the umbilical vessels and the babies basically exsanguinated (unpublished data). But they clearly had the transfusion syndrome and that is ultimately why Julian DeLia started to do laser therapy and coagulate the vessels.

Dr. Baergen: Tell me about your experiences at Dartmouth.

Dr. Benirschke: Well Dartmouth is, I think, the third oldest medical school in this country. In 1959 to 1960, they hired a new dean, Marsh Tenney, an old Dartmouth alumnus, who wanted to revitalize the school. He sought new faculty and new pathologists, because his pathologist had died in an airplane accident. They wanted me and so I moved to Dartmouth in July 1960 to start a new Department of Pathology. I was appointed as chairman and I got a big NIH grant to develop a new department with emphasis on reproductive pathology (Fig. 5). I

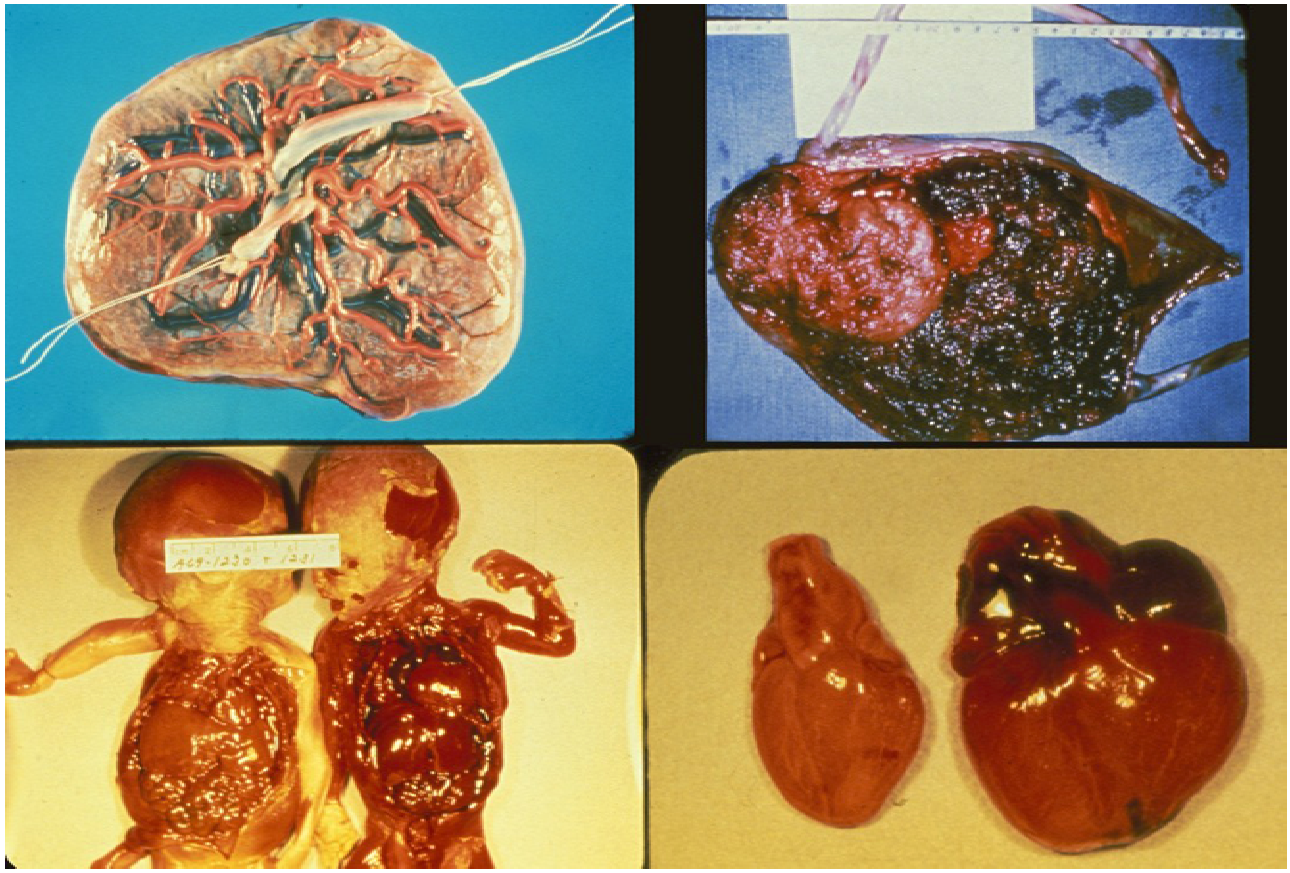

**FIG. 4.** Gross photograph of the twin-to-twin transfusion syndrome. The fused diamniotic-monochorionic twin placenta (upper two photographs) show congestion of the recipient twin's placenta and pallor or the donor twin's placenta. The lower pictures show the twins with cardiac enlargement and visceral congestion of the recipient twin.

**FIG. 5.** Dartmouth Medical School in July 1966, meeting on Comparative Aspects of Reproductive Failure. Front row from the left: Peter Gruenwald, T.C. Jone, A.T. Hertig, R.L. Snyder, J. Warkany, T.C. Hsu. Second row: Ken Ryan, E.S.E. Hafez, Kurt Benirschke, Shirley Driscoll, B. Boeving, C.H. Bridges. Third row: David Carr, Art Silverstein, John King, Jim Metcalfe, David Hsia. Last row: John Josimovich, Peter Kennedy, A.B. Hoerlein, F.B. Hutt, Jack Frenkel, Vergil Ferm, J.L. Hancock, Richard Blandau, Michael Galton.

taught the entire pathology course myself with some 20 students who became very good friends. It was a wonderful atmosphere, except it was very lonely academically speaking. There wasn't much medical investigation going on then as yet.

Actually, that is an interesting chapter because I had been asked by the State of New Hampshire to testify on behalf of a woman, a student at Dartmouth, whose roommate had found a dead baby in her closet. This girl was found passed out in the bathroom, had bled all over, and the allegation was that she had tried to drown the baby. I was asked to do the autopsy on the baby. Someone also gave me the placenta, and it was very clear that she had abruptio placentae with large infarcts. So the baby had not been drowned. I came to court and as I came up the stairs to the courthouse, the lawyers came out and said, "It's been settled." They told me the girl had been acquitted because they knew the substance of my testimony. That was my first law experience in this country.

Dr. Baergen: Why did you decide to leave Dartmouth?

Dr. Benirschke: I think it must have been in December 1969, Ken Ryan (who later became Chairman of Obstetrics and Gynecology at the University of California at San Diego and later Harvard) asked if five of us wouldn't get together on a weekend at the Sheraton Hotel in Boston to talk about how to start a modern Department of Obstetrics. So we spent the weekend with the blackboard and fantasized of how one would start a really first-rate new Department of Obstetrics and Gynecology. It was Fred Naftolin (who ultimately became chairman of Obstetrics and Gynecology at Yale), Ken Ryan, John Davies (an obstetrician and gynecologist), an endocrinologist from Montreal (Sam Solomon), and myself. And then Ken told us that he'd been asked to go to La Jolla because they were starting a new medical school there, and he had just wanted to get some direction. I think it was in January 1970, when we all went to La Jolla and it was wonderful. I knew La Jolla from a site visit for the NIH a few months earlier, so I showed these guys around. A month later, Cliff Grobstein who was the dean then asked us to start a new Department of Obstetrics and Gynecology. Averill Liebow had previously asked me to be in the pathology department and I had turned him down. We started a new department and got a big Rockefeller grant to make progress in the radioimmunoassay of hormones, which was brand new at the time and for which much hope was held in understanding reproductive physiology. And so I came to San Diego as professor in pathology and reproductive medicine. I did the obstetric and gynecologic teaching in the pathology course and started pla-

cental examinations at the hospital here. We were located in the Muir Biology Department and had our labs there until many years later when we moved to the hospital.

Dr. Baergen: Later you worked with the San Diego Zoo and started your long-time collaboration with them. How did that all come about?

Dr. Benirschke: At the time of my site visit to San Diego for the NIH, I brought a movie made at Dartmouth that I decided I would show to the zoo veterinarians. At one of the international congresses that I held in New Hampshire, I brought in John King, a Kenyan veterinarian. I had persuaded him to bring along from Africa some M99, a morphine analog with which you can immobilize many animals with impunity and which you can counteract with Nalorphine quite readily. It was a marvel to the veterinary profession at the time who lost a lot of animals from anesthesia. So I had made that movie and showed it here at the zoo when I made the site visit. The zoo veterinarians at first laughed, but now they are all using it. After coming to San Diego, I was asked to be on the research committee of the zoo because I knew Charlie Schroeder, the director of the zoo. Pretty soon I was chairman of the research committee. The research done at the zoo had nothing to do with maintenance of the animal collection or doing honest to goodness research on animals for their benefit. So I persuaded my committee to write a "white letter" to the Board of Trustees, pleading with them to start their own research department. In 1975 they made the decision to start a research department (48). After 3 or 4 months nothing happened, so I went to the director and he said that the Board of Trustees really didn't know what to do and they asked if I would start it. I told them I was a full-time employee of the university, but they asked me to start it in my "spare time." They said they would pay me $20,000. So in 1976 I started the research department there and did both jobs together. Then I became chairman of the Department of Pathology here for 2 years and did that at the same time. I got grants, hired Bill Lasley and Oliver Ryder and others to start the research department, which then became the Center of Reproduction of Endangered Species at the zoo, CRES as it is now. Those years—doing both jobs together—were challenging. I became a member of the Board of Trustees of the Zoo in 1987 or 1988 or so (Fig. 6 and 7).

Dr. Baergen: Tell me about your peccary farm and how that got started.

Dr. Benirschke: Peccaries are pig relatives. They are interesting animals that developed only in the Americas. There are three species now, but at the time when I learned about them there were two known species. A medical student, Mary Hufty, came to me and suggested

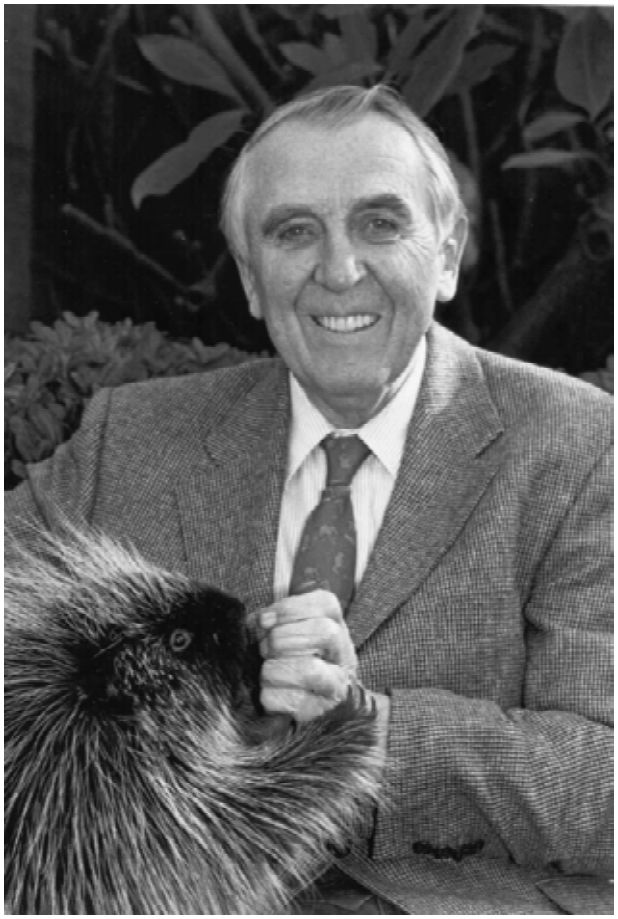

**FIG. 6.** Dr. Benirschke at the San Diego Zoo with a North American porcupine (*Erethizon dorsatum*).

working out the chromosomes of the peccaries. She did and wrote a paper on the chromosomes of the North American species (the collared peccary) and the South American (the white-lipped peccary) (49). Then I met Ralph Wetzel at an armadillo meeting in Texas. He was interested in recording the fauna of Paraguay because it was poorly studied and Paraguay was being deforested so that many of the animals would vanish before they would be discovered. He and his student had found a peccary skull that didn't really quite fit with what they knew of the two other two species. I thought that it must be a derivative of the migration of these animals to South America from North America, a descendent rather than an ancestral species. We made a bet for $2,000. So it was incumbent on me to find out more about it. I analyzed the chromosomes on an animal in captivity and its chromosome number of 20 was very different from the other two species (30 and 26) (50–51). I was ready to cash in on my $2,000, but Ralph had died. However, while I was in Paraguay I found out that this species was virtually ex-

**FIG. 7.** Dr. Benirschke showing the San Diego Zoo to Prince Philip.

tinct. So I decided it would be kind of fun to see if we couldn't do something about this species, which was listed as the most endangered animal in the world. So I bought a little place in the middle of the jungle, not far from the Mennonite colony in the jungle, and with my secretary, Mary Byrd, and a volunteer from the Peace Corp, started a peccary colony (52,53) (Fig. 8). We bought these animals basically for pennies from Indians who would have them either as pets or as food. We gradually learned how to breed peccaries and by 1986 had bred some 450 to 500 animals and released over 200 back into the wild. We're currently releasing animals with radiocollars to follow them into the wild and have, at the moment, in the vicinity of 60 to 70 animals that have been bred to be released. That's it! It'll make a good chapter in my new book on comparative placentation (54).

Dr. Baergen: You collaborated on a book about endangered species with Andy Warhol, *Vanishing Animals* (55). How did the two of you get together?

Dr. Benirschke: The chapter on the placenta for the *Handbook of Pathology*, which I wrote, was the first book that Springer-Verlag published in New York (9). Because I had published the first book with them, they asked me to be the after-dinner speaker for their 21st anniversary dinner. The owner, Dr. Goetze, who died last year, said that they'd love to hear about animals. He had known that Andy Warhol had made a series of 10 animal pictures in the past—zebras that are striped red and brown and yellow and so on—actually very beautiful animal pictures. So he invited Andy to be at the dinner and we sat together. While I was speaking, Goetze said that we ought to write a book together. And so after dinner we went to the bar and decided to write the book.

Dr. Baergen: Tell me about unusual placentas that you have examined, such as the Beluga whale placenta.

Dr. Benirschke: Well whale placentas are of interest, for different reasons: first, nobody gets to see them (56–58). Obviously whale placentas are shed into the ocean and are eaten by sharks and that's the end of them. It is very unusual to get a whale placenta unless a pregnant whale happens to be caught. And like with any other placenta, they are not being examined: the placentas are thrown out. The Beluga whale placenta came from the Navy Research Department because they knew that I would be interested (59). It's a biological problem to understand breeding of whales and dolphins in the wild. The age and previous fecundity of whales and dolphins have been investigated by sectioning the ovaries and finding corpora albicantia. The notion of whether you can tell reliably from an ovarian specimen how many babies that female has delivered in the past, or whether you also count corpora atretica really was unexamined, because the history is very rarely known. We studied this

**FIG. 8.** Dr. Benirschke with Chacoan peccary (*Catagonus wagneri*) at his farm in Paraguay.

but the question is really much more interesting (60). When a whale is in heat and ovulates—they're not necessarily traveling in packs and the blue whales are alone—how do they find each other to breed? Because in some of these species estrus is very short and because it is totally unknown whether they can smell each other, or whether they make specific sounds, how is a male to know that she is ready for copulation. It is also unknown why hybrids are so relatively common but the question is much, much larger. When you look at the chromosomes of gazelles, or sheep and cattle etc., they are all different. You look at whales: they almost always have either 44 or 42 chromosomes. And the same is true of seals. They almost all have the same chromosome number and structure. So how is one to understand speciation of whales? How do you become a new species of whale when you roam all over the ocean? I mean a rat, running around finds another rat and has babies and they become the same rats. But to speciate, in general, that requires moving onto a new island or to be cut off from the ancestral population. How this works in whales in unknown. So, those are interesting biological questions that would lead you to study the placenta of whales because they can interbreed, they have the same kind of placenta—a very superficial, very thin placenta as you know—and the whole sac is covered with villi.

Dr. Baergen: Now that you are "retired," one of the projects that you have been working on is comparative placentation and you've just made a CD-ROM on this subject and placed it on the UCSD website (54). What else are you working on?

Dr. Benirschke: Well this is volume 1. I'm going to continue this and I'm working on a bunch of cytogenetic problems in the zoo. For instance we have three different species of armadillo that look very much the same and we are trying to figure out what their characteristics are. I'm obviously very concerned over cloning of animals, which is what I'm hoping will develop further from the specimens that we have collected in the frozen zoo, that is to intentionally clone animals that are really in need of reproduction (61). We have some hundreds of different species' cells frozen, a large number of them are endangered.

Dr. Baergen: Have you found your career in pathology to be a satisfying experience?

Dr. Benirschke: Well I made it satisfying for myself. I was asked once by a German woman writing for Northern German TV station whether it was more satisfying for me here than it would have been in Germany, and she asked me to compare the two countries. I can say that none of the things that I've been able to do here I could have possibly done in Germany. Because the disciplines are virtually never mingled, you could not do veterinary work or zoological comparative studies as a medical doctor in Germany. A good friend of mine, Alfred Gropp, was a superb cytogeneticist and was also chairman of the Department of Pathology in Luebeck. He could never really make himself a member of the established elite of pathologists in Germany because he was working on something that nobody else does. You know, the placenta is not an organ that many pathologists have taken very seriously. If I were to classify tumors of the thyroid or of the breast, I'd become a better-known pathologist, but I don't mind. This is much more interesting, I think, at least to me.

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