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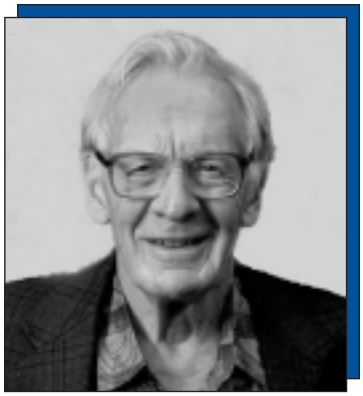
An Interview With John W. Severinghaus, M.D.


John W. Severinghaus, M.D.

W. Andrew Kofke, M.D., Editor
AUA Update

Dr. Severinghaus, could you provide some basic demographic information regarding your family and upbringing?

I was born May 6, 1922, in Madison Wisconsin, the eldest of three children of Elmer L. Severinghaus, professor of medicine at the University of Wisconsin, and Grace Colby Severinghaus. I had an early fascination with electricity, radio, furniture and boat-building, youth hostelling and fixing things. Close family friends included Ralph M. Waters, M.D., Philip D. Woodbridge, M.D., an anesthesiologist classmate of Elmer's at Harvard, and Chauncey D. Leake, Ph.D., who was responsible for attracting Dr. Waters to Madison. They made anesthesia attractive.

Where was your education and residency?

I attended Haverford College near Philadelphia, majoring in physics and electronics and deferred from military service to work at the Massachusetts Institute of Technology on radar during World War II, where I developed radar test equipment. When the atom bomb news was revealed, I decided to change from physics to biophysics and attend medical school. I was able to enter the University of Wisconsin School of Medicine without paperwork in September 1945 because the military class had just been canceled. I transferred to the College of Physicians and Surgeons at Columbia University in 1947 and graduated in 1949. After a two-year rotating internship in Cooperstown, New York, I began anesthesia residency with Robert D. Dripps, M.D., at the University of Pennsylvania in January 1952. After six months, Dr. Dripps assigned me to work as a research fellow with Julius H. Comroe, M.D., chair of the physiology and pharmacology departments of the Graduate School of Medicine at the University of Pennsylvania. My projects included respiratory physiology of lung dead space with Robert Forster, M.D., and carotid body pharmacology with Dr. Comroe. The draft forced me to interrupt this postdoctoral work and residency in mid-1953. I was assigned to the U.S. Public Health Service at the National Institutes of Health's (NIH's) new Clinical Center Anesthesia Department as director of research. After three years of work on hypothermia and blood gas analysis, I completed my residency with Stuart Cullen, M.D., at the University of Iowa from July 1956 to June 1957. At the American Physiological Society meeting in Iowa City in August 1957, Dr. Comroe persuaded Dr. Cullen and me to join him in a move to the University of California-San Francisco (UCSF) to found a new independent anesthesia department, with me nearly full-time in Dr. Comroe's new Cardiovascular Research Institute.

Who influenced you the most to pick a career in medicine, a career in anesthesiology and, finally, a career in academics and research?

I assumed I wanted an academic career from childhood, having been brought up in the University of Wisconsin environment, living a block from Dr. Waters, whose office was adjacent to my father's. When the atom bomb drove me out of physics, my father's career as an endocrinologist and nutritionist, and his background in physiological chemistry, were important to my decision. He had persuaded the Madison High School system to add a course in physiology for 11th graders, taught by one of his students,

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An Interview With John W. Severinghaus, M.D.

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which I had enjoyed. During medical school at Wisconsin, I participated in several physiology research programs. At Columbia I built the first electrophrenic respirator and sold copies to several anesthesia departments, winning the Borden Award. After internship I visited five centers to consider biophysics research, the last being at Pennsylvania, where within a few minutes, Dr. Dripps persuaded me that anesthesiology was the best field in which I could develop biophysical instrumentation. Dr. Comroe was the crucial influence attracting me to physiology research.

What motivated you to research the areas you have developed?

Respiratory physiology involved physical measuring apparatus, and my design of the electrophrenic respirator stimulated that interest. My first research project at Pennsylvania as a new resident was to use a spirometer and a Pauling oxygen analyzer to determine the uptake of nitrous oxide during anesthesia. My research year with Drs. Comroe and Forster at Pennsylvania included working with mass spectrometers, constructing recording spirometers and dog experiments about the carotid body's role in the regulation of breathing. Dr. Dripps asked me to co-author a physiologic reviews paper on regulation of breathing during my first year at NIH. Hypothermia was in vogue for both cardiac and neurosurgery at NIH in 1953, and a paper by John J. Osborn, M.D., had just reported that CO_2 excretion was blocked by hypothermia. My first big project was to test and falsify that hypothesis. For this I had to develop very accurate blood pH and Pco_2 measurements and redetermine the effects of temperature on pK, the dissociation constant of carbonic acid and on the solubility of CO_2. It turned out that Dr. Osborn's error was failure to correct the laboratory blood Pco_2 values to actual body temperature.

How was the concept of MAC determined? Did you have a role in that?

My laboratory group at UCSF, about 1960, shortly after Ted Eger, M.D., arrived, had weekly lunch meetings to discuss our separate projects. Dr. Eger was interested in the relative potency of anesthetics. He wanted a way to compare them numerically in terms of their alveolar concentrations at the time of establishment of a minimal level of anesthesia to permit surgery. It was clear to all that for each patient or animal, there was a critical alveolar (and thus arterial and ultimately brain) pressure of an agent that just prevented a motor response to pain. He believed this would be a relatively invariant number between patients. This would be the minimal alveolar anesthetic concentration. I recalled that in aviation, a similar index, Mach, was the ratio of an aircraft's speed to the speed of sound. A hypersonic flight was defined, for example, as Mach 2, twice the speed of sound. I suggested the same symbol be used for the ratio of concentration of the anesthetic in the alveoli (as determined in the airway at end expiration) to that critical no-movement level, which would be defined as 1 MAC, originally MAAC. It still should be MAAC since we can't agree on whether the single "A" refers to alveolar or anesthetic or both.

I get blood gases on my patients routinely. Can you briefly recount the story of the development of this technology in your early research?

To follow from my answer to question 4, above, while developing blood Pco_2 measurement using the laborious Van Slyke analysis of plasma CO_2 content and pH, with the Henderson Hasselbalch equation, in August 1954, I heard Richard Stow, M.D., describe his invention of a CO_2 electrode and his prediction that it couldn't ever be made stable. I immediately realized how to make it stable (by adding bicarbonate to the inner electrolyte), and within a week had built the first successful, stable Pco_2 electrode. Over the next years, while developing and testing it and using it in the laboratory, I was still frustrated by inability to measure blood PO_2. I tried polarographic electrodes unsuccessfully. In April 1956, I organized a meeting of scientists interested in oxygen analysis at which Leland Clark, M.D., revealed his invention of a polarographic oxygen electrode using a polyethylene membrane to separate the blood from the platinum cathode, a concept that had escaped me and many other physiologists. His invention changed my life, and we became close friends. His electrode consumed so much oxygen that it required a stirred cuvette for accuracte blood PO_2 analysis. I mounted my stable version of Stow's Pco_2 electrode and Dr. Clark's PO_2 electrode in a 37-degree-thermostated water bath with a tiny stirring paddle in front of Dr. Clark's electrode and with a small tonometer in which the blood sample could be equilibrated with a known gas PO_2 and used to calibrate the PO_2 electrode's blood/gas ratio. This was the first blood/gas analysis system, which I demonstrated at the ASA Annual Meeting in 1957 and published in 1958. Shortly thereafter I added a pH electrode immersed in the water.

When the atom bomb news was revealed, I decided to change from physics to biophysics and attend medical school. I was able to enter the University of Wisconsin School of Medicine without paperwork in September 1945 because the military class had just been canceled.

Why did altitude physiology interest you? How did you collect data on altitude physiology? On hypoxia?

I shared a large laboratory with Robert Mitchell, M.D., at UCSF in the Cardiovascular Research Institute. Bob's research field was the relation of spinal fluid acid-base balance to the regulation of breathing. I invited Dr. Hans Loeschcke from Göttingen, Germany, to join us because he had shown respiratory responses in cats to acid applied in the 4th ventricle. Dr. Mitchell eventually discovered the specific ventral medullary surface pH chemosensitive respiratory control areas shortly after Dr. Loeschcke returned to Germany. Since these areas were pH sensors, and since cerebrospinal fluid (CSF) bicarbonate ion concentration determines the relation between blood Pco_2 and CSF pH, we realized that spinal fluid bicarbonate was the answer to a 20-year-old altitude ventilatory regulation enigma. Dick Riley, M.D. (Johns Hopkins) and Charles Houston, M.D.

(University of Vermont), while in the Navy, had shown that subjects acclimatized for weeks in a chamber to the pressure of Mt. Everest continued to hyperventilate when they returned to normal pressure, although they had alkaline blood and high PO_2. We had access to the relatively new University of California White Mountain laboratories at 3,810 meters and 4,340 meters altitude, and decided to test whether CSF bicarbonate explains the curious changes of respiration during and after altitude acclimatization. Dr. Mitchell, two anesthesia postdoctoral fellows and I chose to be the subjects for serial lumbar spinal fluid sampling before, during and after a five-day trip to the Barcroft laboratory (3,810 meters). My new blood/gas apparatus was ideally suited for this study, being small, portable and able to measure both blood and spinal fluid values. In retrospect we should have recruited more subjects, stayed longer and had better standard pH buffers. I published some erroneous physiologic conclusions as a result, claiming that some active transport process of the blood/brain barrier was regulating spinal fluid pH back toward normal. The concept turned out to be correct, but the outcome is a stable but higher CSF pH with a falling bicarbonate concentration over the first week or so at altitude. It did solve the Riley-Houston enigma.

Is There a Patient in the House?

Dr. Severinghaus, top right, observes Edwin S. Munson, M.D., as he prepares Thomas F. Hornbein, M.D., for a blood/gas monitoring procedure. Lawrence J. Saidman, M.D., far left, looks on. Such self-experimentation was common in the 1950s.

Can you relate what the research culture was like 40 to 50 years ago? I understand you and your colleagues did things like examining the effects of altitude by cannulating yourselves and making observations. Any truth to that? Any comments from fellows' spouses about their research activities?

The "research culture" was not really different 50 years ago, except that we had no peer-review or committee on human research to satisfy. Self-experimention in physiology has been the honored tradition for centuries. As anesthesiologists we had no fear of lumbar punctures and later also volunteered for arterial and jugular bulb punctures and cardiac catheterization in studies of pulmonary pressure and cerebral blood flow at altitude. None of my studies would be denied even now by regulatory groups. As for spouse opinions, my wife, Elinor, has never opposed my research work, albeit often expressing her concern for my safety.

Can you recount any stories about what it was like to be a research subject in your own studies? Any recollections on focused electrodes to determine lung water in yourself or studies on the effects of hypoxia?

Spinal taps usually leave me with a bad headache for a few days, which are treated by staying supine. Our first drive to the Barcroft laboratory came immediately after sea-level control spinal taps, and I was miserable, so Freeman Bradley, my technician, drove the university station wagon, loaded with equipment, while I tried to stay flat in the right front seat. Freeman was used to driving a very small and narrow car. Partway up the mountain, in an early July snow storm and perhaps not aware of the width of the station wagon, we slid off the right road edge, ending up on a 45-degree slope, almost a tip-over. The excitement overcame my headache enough to commandeer and drive the next car, which was being driven by an ancient photographer who was also too frightened to drive further. We three proceeded successfully to the Crooked Creek laboratory at 3,200 meters altitude to get help. It eventually required a mammoth tow truck to come from Bishop (1,200 meters).

On a later occasion in Copenhagen while on sabbatical, I was a subject for a study of the time-course of cerebral blood flow in response to a sudden reduction of alveolar Pco_2 to 20 mm Hg. Niels Lassen, M.D., my longtime friend and colleague, having done hundreds of jugular bulb punctures, managed to nick my hypoglossal nerve and instantly paralyze the muscles of the right side of my tongue, making me almost speechless. In his anxiety, he visited me every few hours for several days. It resolved on the 3rd day without sequellae. I assume the needle had caused an arterial hemorrhage within the nerve sheath.

As our studies of the physiology and pathology of high-altitudes progressed, we pursued the etiology of high-altitude pul-

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Point

I'm Unhappy Where I Am — Should I Send Out My CV?

W. Andrew Kofke, M.D.
Philadelphia, Pennsylvania

So the great academic career you signed up for seems to be amok. Clinical demands are increasing, academic perks are seemingly absent, you feel like you are in private practice and your chair has resigned/been fired or perhaps is consulting at places that are looking for a chair. Given the results of demographic forces and a lack of residents a few years ago, it seems like everyone is looking for anesthesiologists these days. Thinking about sending out the old curriculum vitae (CV) to get the job/career/mood back on track? Some thoughts on this:

If someone else invites you *de novo*, that is one thing. If, on the other hand, you become the dissatisfied job seeker, it is best done if all hope is gone at the current position. Given continued commitment to an academic career, I would suggest the following as indicators of such a hopeless situation.

Institutional Leadership Failure. Signs indicate the dean/CEO/VP or other such power person views anesthesiology/radiology/pathology/ED as "service" departments, like the laundry. If sure of this, scram.

Institutional Bureaucratic Failure. Does your IACUC reject your protocol for use of too high a barbiturate dose for euthanasia? Does your institutional review board believe anesthesia is too dangerous to study? Are armed people inspecting your institution's billing practices? If so read *Moo* by Jane Smiley (Ballantine Publishing Group) and assess your patience. Such places are not for the impatient.

Clinic-Research Dissociation. You are studying XXX but clinically doing ZZZ. Optimally your patients should reflect your research interests. If no such clinical materials are around or you are not assigned to them, evaluate the likelihood of a fix. If not, consider carefully the possibilities of your making progress in your academic area if you do not solve the problem.

Dissembling Chair Syndrome. Once you have bought a house and moved the family, you find clinical time up and academic resources down, in direct violation of your offer letter. Assess and weigh: A) stay and fight, B) stay and acquiesce or C) leave. Options A and C present the possibility of legal action, which would then become your primary nonclinical activity.

Dissonant Chair Phenomenon. Your chair, in a direct imitation of Darth Vader, calls you an amateur or otherwise indicates his/her disgust with your presence. Evaluate the criticisms introspectively and honestly and decide on their merit. If your chair is right, you should thank him/her and go into private practice (or perhaps one of the many university groups that resemble a private practice). If you disagree, go to another bona fide university position.


W. Andrew Kofke, M.D.

Diminishing Remuneration Effect. This could have several possible causes. If due to low clinical load, consider taking advantage of the time to get in those grants and papers. If due to billing or clinical inefficiency, help to fix it. If, however, there is a Robin Hood thing going on (rob from the hard-working rich and give to the never-here-at-night poor) or some other perfidy, you may find better smelling gas elsewhere. Of course there may be an internal distribution going on to promote some department goal. If you are unwillingly not on the receiving end of this, then it may also be a time to re-evaluate, although remember that such things can be temporary.

Crummy Clinical Practice. Your colleagues habitually call in sick on your academic days, unexpectedly pulling you into the operating room; the hospital will not buy any drugs that are still in patent; nursing support is abysmal. Get it fixed or get out.

In all these considerations, one must always keep in mind the risk of leaving an unacceptable place: The new one could be worse. If you change residence, you will necessarily incur an equity loss in real estate due to transaction costs, and it will take two to four years to get your academic program, if you had one, up and running again. By that time, the problems you left might be solved.

This is a time of workforce shortage, high chair turnover and fiscal squeeze on academia in all disciplines. Everyone seems to be seeking an anesthesiologist. Sending the CV may seem irresistible… just be careful.

… one must always keep in mind the risk of leaving an unacceptable place: The new one could be worse.

So You're Unhappy Where You Are, and You've Sent Me Your CV?

Donald S. Prough, M.D.
Galveston, Texas

The editorial by W. Andrew Kofke, M.D., on the facing page contains a number of important considerations for any faculty anesthesiologist who is thinking of relocating to another academic department to enhance the prospects of professional advancement. As a department chair, I receive curriculum vitaes (CVs) from dissatisfied faculty members from other departments. On several occasions, I have encouraged qualified applicants to remain in their present positions because doing so offers them the best chance of achieving their goals. Here are a few of the things that I think about (and also discuss with applicants) when I receive an unsolicited CV from an unhappy anesthesiologist.

With Dr. Kofke's permission, I will borrow his catalogue of reasons for unhappiness because it nicely covers the range of reasons that I have encountered.

Institutional Leadership Failure. At the current time, anesthesiology departments represent a major financial and operational problem for many (most? all?) medical center administrators. As the shortage of academic anesthesiologists has driven salaries and benefits well beyond the revenue that an anesthesiologist can generate in most hospitals, as the shortage has resulted in occasional inability to staff the desired number of operating rooms and as the interest of faculty anesthesiologists in academic activity has stagnated or declined, administrators have inevitably begun to discuss ways to increase the productivity or decrease the cost of anesthesiology departments. Be cautious in assuming that your institutional leadership is peculiarly unsympathetic.

Institutional Bureaucratic Failure. External political and regulatory pressures have pushed the most reasonable of institutions to greater bureaucratic lengths than many would have thought possible 10 years ago. Animal care and use committees or institutional review boards that "rubber stamp" submissions from investigators are now rare or vanishing. In an individual institution, learn the idiosyncracies of the committees by carefully considering their criticisms of your protocols and discussing your plans with members of the committees. Once you have figured out how to do that in your current institution, assume that you will need to completely repeat the process if you move.

Clinic-Research Dissociation. Dr. Kofke's comments are particularly pertinent on this issue. I have only one additional suggestion. If you can't do as much of your preferred clinical activity because the number of faculty who share your interests is too high, consider patience as a short-term strategy. Given the current level of faculty turnover, you are likely soon to be doing much more of what you prefer by simply being ready and present.


Donald S. Prough, M.D.

Dissembling Chair Syndrome. I would add one suggestion to Dr. Kofke's list. If you feel that you have been misled, discuss your goals and requirement clearly and nonconfrontationally with your chair. If your original negotiations were explicit, the odds are good that you were recruited to do what you want to do. Because chairs have highly variable experience and expertise, you may need to spend some time educating yours as to what is necessary to achieve your mutual goals. Don't assume that your chair completely understands or is unsympathetic to your needs.

Dissonant Chair Phenomenon. Dr. Kofke has nicely outlined the most likely scenarios. Keep in mind that a perfectly good faculty member and an equally good department may simply be ill-suited for each other. Another possibility to consider is that you can take your chair's critiques and modify your behavior. Most poorly adaptive behavior will be a handicap either in another academic program or in private practice.

Diminishing Remuneration Effect. Dr. Kofke's approach to this problem is logical and appropriate. Another option, which may not be feasible unless you have been at your present position for a few years, is to lobby for a formula-driven compensation approach that applies equally to all faculty. Of course if the Robin Hood phenomenon is at fault, expect little cooperation from the beneficiaries.

Crummy Clinical Practice. Realism may be essential in dealing with this situation. Many aspects of hospital practice are simply less satisfying than they were 10 years ago, and they are unlikely to get better soon. Only a few hospitals have managed to avoid deterioration in services and amenities as health care reimbursement has declined. There really isn't much green grass on the other side of any medical fences.

The risks of ill-advised relocation are substantial, as Dr. Kofke clearly points out. Although I may sound like a Pollyanna, try to be happy and productive where you are. Perhaps the least fulfilled anesthesiologists I know are those who have left multiple positions that failed to meet their expectations and transferred into another position that they found equally wanting.

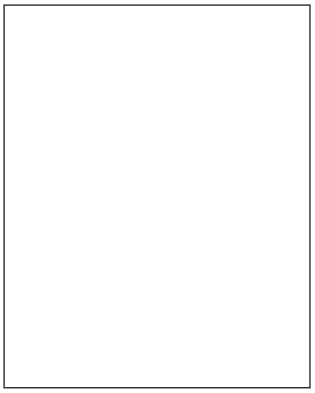
SAB Report *from AUA Annual Meeting*

C. Michael Crowder, M.D.
Frontenac, Missouri

My first official task as the newly appointed chair of the Scientific Advisory Board (SAB) is to summarize the SAB program at the AUA 2003 Annual Meeting. The quality and quantity of the presentations were particularly outstanding this year, and 105 abstracts were presented: 10 oral presentations, 13 poster-discussions and 82 posters. Each of the oral presenters discussed laboratory research on a broad range of topics, including pain mechanisms, mechanisms of anesthesia, behavioral neurobiology, protein structure, sepsis, Parkinson's disease, anesthetic toxicity and anesthetic renal protection. Similarly the poster-discussion and poster presentations emphasized the variety of clinical and basic research that is ongoing in anesthesiology.

The highlight of the SAB program was the inspirational presentation by Warren M. Zapol, M.D., titled "Scientist-Clinician: Going to the Extremes." Dr. Zapol highlighted his extraordinary research accomplishments over his 33-year career in anesthesiology. Besides covering the impressive list of accomplishments that include development of extracorporeal membrane oxygenation, or ECMO, one of the first clinical applications of nitric oxide, and the demonstration of the diving response in seals and humans, Dr. Zapol's talk managed to relate some of his enthusiasm and love for scientific inquiry. Dr. Zapol closed by exhorting all of us to have a similar passion for what we do and not to be afraid to tackle difficult but important problems.

The next AUA Annual Meeting will be held on May 13-15, 2004, in Sacramento, California. The SAB urges all members to submit their best science for presentation in Sacramento next year. In particular the SAB wants to encourage the presentation of clinical studies that are generally underrepresented at AUA meetings.



C. Michael Crowder, M.D.

Reminder!

The deadline to submit an abstract for presentation at the AUA 2004 Annual Meeting in Sacramento, California, is Monday, November 3, 2003. To download an abstract **Submission Form**, **Disclosure Form** and **Specifications for Abstract Submission**, visit the AUA Web site at <www.auahq.org> and click on "Annual Meeting."

Jeffrey Cooper, Ph.D., Receives National Award

The American Association of Nurse Anesthetists (AANA) recently announced that Jeffrey Cooper, Ph.D., corporate director of the Massachusetts General Hospital Biomedical Engineering, will receive the Public Interest in Anesthesia Award. The Public Interest in Anesthesia Award recognizes individuals who have made outstanding contributions in the area of anesthesia patient safety and the promotion of quality anesthesia care.

AANA recognized Dr. Cooper for his contributions in the prevention of adverse events and patient injury through research and education. As one of the founders of the Anesthesia Patient Safety Foundation, Dr. Cooper also organized the nonprofit Center for Medical Simulation, which focuses on training in the management of critical events, teamwork and other issues related to patient safety.

"It is the greatest honor so far in my professional life to receive this award," said Dr. Cooper. "Patient safety is a never-ending effort, and I encourage all anesthesia professionals to continue their commitment toward this important undertaking."

AANA presented Dr. Cooper with the award during its 70th annual meeting.

Some Comic Relief ...

'The Secret Room at the Surgicenter'

An Interview With John W. Severinghaus, M.D.

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monary edema, or HAPE. I tried three methods of measuring lung water during acclimatization. One was electrical measurement of lung resistance, which is normally very high compared with other nonaerated tissues. I designed and built a focusing electrode device to mount on the lateral chest wall. It worked and was used on Pike's Peak with an army group, but the data were not specific enough since lung resistance was much affected by both lung gas volume and pulmonary blood volume, both of which change in response to hypoxia. A second method used a mass spectrometer and inhalation of a mixture of traces of methyl ether and helium, with computer-controlled pneumatic breathing valves precisely timing several expiratory steps for gas concentration analysis. It showed that the army "volunteers" didn't get HAPE even though they were brought to a 4,300-meter altitude without acclimatization and immediately asked to maximally exercise on a bike. A third method was a double indicator dilution method in which a catheter had to be implanted in the aorta, not something for volunteers. It used a cold-concentrated saline injectate and measured temperature as the diffusible indicator (into all lung water) and conductivity as the nondiffusible indicator. Modifications of that method have been developed by many others. A similar aortic catheter single-indicator variation has recently been commercialized and used in critical care patients.

Niels Lassen, M.D., my longtime friend and colleague, having done hundreds of jugular bulb punctures, managed to nick my hypoglossal nerve and instantly paralyze the muscles of the right side of my tongue, making me almost speechless.

The studies of the effects of hypoxia include much of the last 40 years of my work. Three major groups include the regulation of breathing, high-altitude cerebral edema (HACE) and HAPE. The interactions of carotid body hypoxic stimulation with central CO_2 chemoreceptors is very complex, including the onset after five to 20 minutes of hypoxia of hypoxic ventilatory depression or decline, then over two to 10 days of carotid body hypersensitivity and over months to years of carotid body cellular hyperplasia and hypertrophy. The cause of HACE has been unknown, not due to high blood flow. We showed that it may be due to early stages of angiogenesis in which vascular endothelial growth factor (VEGF), a cytokine, breaks down capillary basement membranes in response to tissue hypoxia. We documented the increase in brain VEGF in hypoxic rats and recovered it from human nasal lavage at altitude as well. As for HAPE, I hope someday to prove that the leak in the lung is through the overdistended, hypertensive elastic arterial walls, not the capillaries downstream from the hypoxically constricted resistance vessels. It is not a widely accepted idea.

You probably have seen many changes over the years, some good and some bad. What would you consider the one or two biggest changes that have occurred that have affected you the most since you began practicing anesthesiology?

Fifty years ago, Dr. Dripps required each new resident to learn to use ether, open-drop, and with nitrous oxide for six months before using Pentothal. Patient comfort was of no concern, and postoperative vomiting was expected. Spinal anesthesia was widely avoided because some patients had become paralyzed (the source of trouble was never proven, but may have been phenol in which ampules were stored). Anesthesia machines had no ventilators. Doctors called the fire department for a pulmotor when a patient stopped breathing. Henry K. Beecher, M.D., had shown that the death rate was higher with use of curare. Drs. Comroe and Dripps had just shown that anesthetists could not detect cyanosis until saturation was below 80 percent. No oximeters were in clinical use. A blood/gas analysis took about a week at the University of Pennsylvania because it was routed through the head of the pulmonary medicine department for signature. But the biggest change for safety seems to have been pulse oximetry.

What are your thoughts about the future of anesthesia research and academic anesthesiology in general?

The anesthetics, narcotics, apparatus, methods and adjuvants now available are fine. The future could be bright but will require replacement of our broken health care system by a single-payer government medicare for all. California has 9,000 health insurance companies with 9,000 chief executives, which consume 30 percent of all health funds as overhead. The future for academic anesthesia and research depends on radical change in economics of medicine.

Did you have any influence helping to solve problems in the biosphere?

Yes. My son, Jeff Severinghaus, during his graduate work at Columbia University in geology, was asked by his professor, Wally Broecker, to help the Biosphere solve their problem. Oxygen was falling much faster than CO_2 was rising in the closed space. Jeff discussed the problems with me after his visit, wondering what kind of oxidation or metabolism had a very low RQ. When he told me that the new structure was built of steel, a neoprene pressure bellows and a concrete base, I suggested that new concrete was taking up the CO_2, changing its calcium hydroxide to calcium carbonate. I suggested he sample concrete at various depths and acidify it to see how much CO_2 was taken up in the concrete as a function of depth. That proved to be the correct solution. But the basic problem was that the planners used far too much compost in the soil, full of organisms, which used about 90 percent of the oxygen consumption of the system, far too much for the green plants to replace.

Is there anything you would like to add to this interview?

No.

The following letter from CMS was received by W. Andrew Kofke, M.D., in response to his inquiry regarding reimbursement to anesthesiologists for concurrent cases.

Department of Health & Human Services
Centers for Medicare & Medicaid Services

July 30, 2003

7500 Security Boulevard
Baltimore, MD 21244-1850

W. Andrew Kofke, M.D., M.B.A., F.C.C.M.
University of Pennsylvania Health System
Department of Anesthesia
3400 Spruce Street
Philadelphia, Pennsylvania 19104

Dear Dr. Kofke:

Administrator Scully asked me to thank you for your letter concerning the current Medicare payment rule for teaching anesthesiologists. You asked that we review our teaching physician payment policy and allow the teaching anesthesiologist to receive the full fee for each of two concurrent cases with residents.

In 1995, the Centers for Medicare & Medicaid Services (CMS) published in the Federal Register proposed and final regulations systematically addressing the payment policy for all teaching physicians. These regulations took effect for services furnished on or after January 1, 1996. Under the general teaching physician policy, a teaching physician must be present for the key portion of the service for which payment is sought.

In the case of surgery or a dangerous or complex procedure, the teaching physician must be present during all critical portions of the procedure and must be immediately available to furnish services during the entire service or procedure. The teaching physician may not bill for two concurrent cases but may bill for "overlapping procedures." In order to bill for two overlapping procedures, the teaching surgeon must be present during the key portions of both operations. Therefore the key portions may not take place at the same time. When all of the key portions of the initial procedure have been completed, the teaching surgeon may begin to become involved in a second procedure. If the teaching physician leaves the operating room after the key portions of the surgical procedure or during the closing of the surgical field to become involved in another surgical procedure, he or she must arrange for another physician to be immediately available to intervene in the original case should the need arise in order to bill for the original procedure.

With respect to anesthesia services, the teaching anesthesiologist must be present with the resident during all critical or key portions of the procedure, including induction and emergence. Generally, if a teaching physician is not sufficiently involved in a case with a resident or is involved in several services concurrently, then no payment is made under the physician fee schedule. Under these circumstances, the service of the teaching physician is considered a hospital service and is payable through the Medicare payment to the hospital for graduate medical education. However, there is a different policy that applies to teaching anesthesiologists who may be involved in several anesthesia cases concurrently.

Unlike other physicians, anesthesiologists may receive a "medical direction" payment, which is a partial payment for concurrent cases. Under this medical direction payment policy, the anesthesiologist can receive a partial payment if he/she performs certain components of the anesthesia service and directs another qualified individual (e.g., certified registered nurse anesthetist or resident) for the remainder of the service. This direction allows the anesthesiologist to cover several cases simultaneously.

In such cases, an anesthesiologist may medically direct the services of qualified individuals in two, three or four concurrent cases and receive a fee equal to 50 percent of the amount otherwise allowed for each case. The qualified individuals are usually certified registered nurse anesthetists (CRNAs) or residents. Thus if a teaching anesthesiologist is involved in two, three or four concurrent cases involving residents only or some combination of residents and CRNAs, partial payment can be made to the anesthesiologist for each of the concurrent cases.

The American Society of Anesthesiologists (ASA) recently sent us a letter requesting a meeting to discuss the teaching anesthesiologist payment issue. We intend to meet with them shortly to hear their concerns. If CMS does consider reviewing the teaching anesthesiologist payment policy, it would be appropriate to re-examine this policy in context of other anesthesia payment policies such as medical direction of concurrent anesthesia procedures. A proposal to revise the teaching anesthesiologist payment rules would have to be implemented through the rule-making process and allow for public comment.

Thank you again for your continued interest in the Medicare program.

Sincerely,

Thomas L. Grissom, Director
Center for Medicare Management