CAREERS IN ANESTHESIOLOGY

An Autobiographical Memoir

VOLUME V

PETER J. SAFAR

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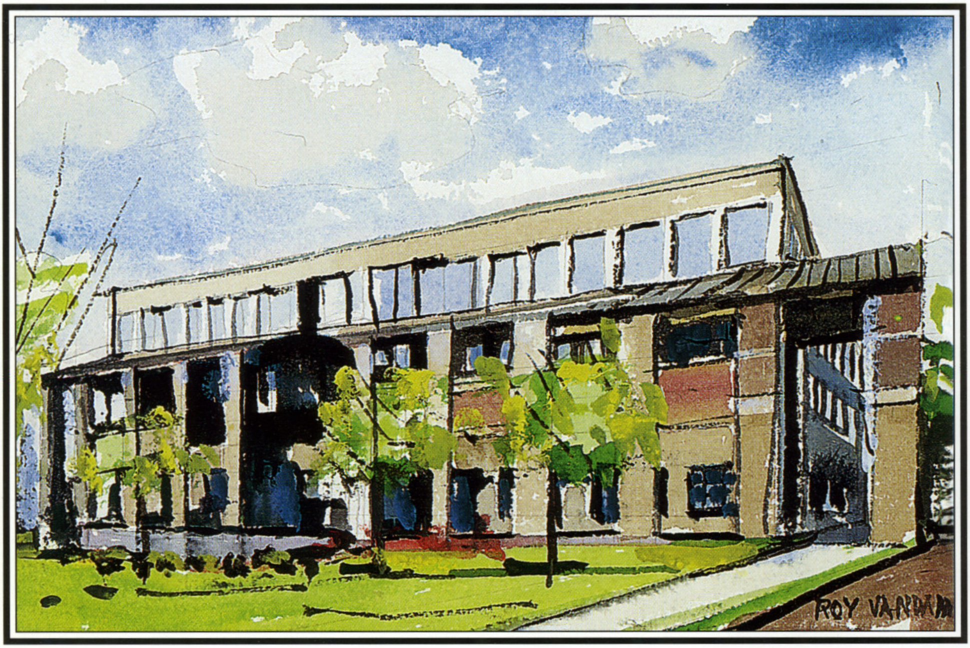
IN ANESTHESIOLOGY



Headquarters Building of the American Society of Anesthesiologists. Almost one third of the three spacious floors is devoted to the collections of the Wood Library-Museum. (From a painting by Professor Leroy Vandam)

CAREERS
IN ANESTHESIOLOGY

An Autobiographical Memoir

PETER J. SAFAR
From Vienna to Pittsburgh
For Anesthesiology and Acute Medicine

EDITED BY
B. Raymond Fink
Kathryn E. McGoldrick

VOLUME V

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TABLE OF CONTENTS

EDITORS' NOTE vii

PART ONE
VIENNA TO PITTSBURGH
1924 – 1961

Introduction 1
Vienna 11
New Haven 58
Philadelphia 68
Peru 95
Baltimore 109

PART TWO
PITTSBURGH
1961 – 2000

Introduction 159
The World 292
Conclusions 324

Acknowledgments 349
References 350
Abbreviations 377

EDITORS' NOTE

These memoirs in the "Careers" series inaugurate a new experiment in living history from the Wood Library-Museum of Anesthesiology. They present autobiographical panoramas of the protracted revolution which overtook anesthetic practice in the second half of the twentieth century. Principal participants here retell the motivations, actions, incidents and dominant events of their careers, virtually unburdened by limits of self-expression.

This volume pulsates with creative diversity, the throb of artisans busily weaving individual threads of their own making, into a tapestry more than a little reminiscent of a philosophy articulated by Chief Seattle at about the dawn of anesthesia.

This we know.

All things are connected
like the blood
which unites one family

Whatever befalls the earth
befalls the sons and daughters of the earth

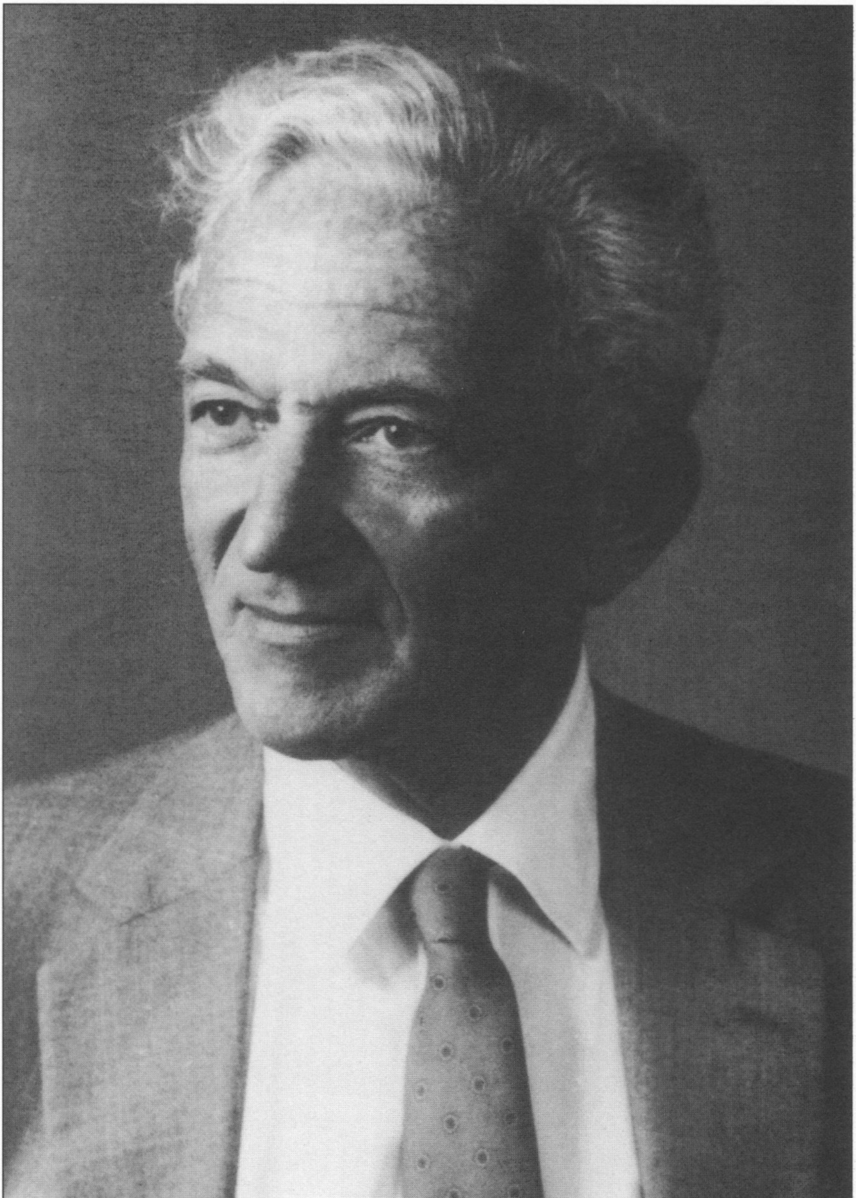
Man did not weave the web of life,
he is merely a strand in it.

Whatever he does to the web
he does to himself.

Someone has said that the unexamined life is not worth living. Well, this series presents a clutch of lives examined, found pre-eminently worth living—and most decidedly worth reading! As already indicated, they are distinguished invited cameos from the renascence of anesthesiology in World War II and its aftermath. Others awaiting the next turn of the press are delayed only by the incompressibility of time.

For the present volume of bedside reading, you and I are deeply indebted to WLM's Board of Trustees and Publications Committee, to prince of librarians Patrick Sim, to Karen Bieterman, stellar assistant librarian, to our production expert, Roz Pape, and to devoted staff working at the headquarters of the American Society of Anesthesiologists. Thanks to the exceptional vision of our Society, a large part of that comely building is Paul Wood Library-Museum territory. Inside and out, the entire edifice exudes pride and dedication to the ageless ideals of the physicians who planned, own, and use it. Leroy Vandam's evocative painting, frontispiece to this and subsequent volumes, reflects the subtle unifying role of the Society in the careers you are about to savor.

—B. Raymond Fink, M.D.
—Kathryn E. McGoldrick, M.D.



Peter Josef Safar, M.D.

FROM VIENNA TO PITTSBURGH FOR ANESTHESIOLOGY AND ACUTE MEDICINE

An
Autobiography

PETER J. SAFAR, M.D.

PART ONE

FROM VIENNA TO PITTSBURGH 1924 – 1961

...to find meaning in life...
— VIKTOR E. FRANKL
...make use of your talents and opportunities to help others...
— KARL SAFAR
...and help building on the edifice of mankind...
— VINCA SAFAR-LANDAUER

INTRODUCTION

Each generation views its own epoch as uniquely important. Those of us who had the opportunity to attend medical school in the 1940s and to learn anesthesiology and life support in the 1950s — whether in Europe or the United States — became a part of modern anesthesiology's birth and development.

My good colleague, Professor Raymond Fink, on behalf of the American Society of Anesthesiologists (ASA) and its Wood Library-Museum (WLM), asked me in 1997 to write an autobiography. It should be readable. It may include the diverse ramifications of my interests and involvements. It can be long. This autobiography was written before publication of this ASA series' volumes I-IV, which are concise and readable. Mine became long and detailed because it is meant for documentation, not to please the reader or entertain. I used it to document many names in order to acknowledge, thank, and honor my teachers, associates, colleagues, and students. The portraits (fig. 1-10) are to honor those individuals who had the greatest influence on my life as a physician. It proved impossible to separate all personal memories from the professional ones.

As a participant in the recent history of anesthesiology and acute medicine, my recollections are personal and, therefore, biased. Some of the relatives, colleagues, and friends I mention in this autobiography may disagree with my recollections. Such disagreements are reminiscent of the different "truths" seen by different observers of the same event — as in the Japanese movie *Rashamon* or the "Three blind men" touching the same elephant, each "seeing" something different. My recollections have been shaped by the following:

1. Historic perspectives. My roots in a family of Viennese physicians exposed me to conversations not only about politics, social issues, and the arts, but also about medicine, surgery, and even anesthesia during the first half of the twentieth century. During the second half of this century, my studies and experiences also were influenced by the histories of the universities of Vienna (1943-49), Yale (1949-50), and Pennsylvania (1950-52); the country of Peru (1953); the Johns Hopkins Hospital (1954-55); the Baltimore City Hospital (1955-61); and the University of Pittsburgh (1961-present) (see table 1, page 342).

2. International perspectives. My parents' multinational, multi-ethnic, multireligious (mostly agnostic) roots and circles — before, during, and after two world wars in Central Europe — influenced my values as a world citizen. In spite of Catholicism having been Austria's state religion, and my and my parents' birth and baptism certificates said "Catholic," we went to church

only occasionally for special events. These roots shaped what some have called an "ex-Viennese Yankee social democrat," a secular agnostic humanist (not an atheist).

3. Interdisciplinary perspectives. Having come into anesthesiology in 1950 after brief experiences in pathology, research, and surgery, I recognized early that the skills and concepts acquired by anesthetizing and life-supporting patients also would be valuable for critically ill or injured patients outside the operating room, recovery room, and hospital. That realization led me to team up with colleagues of other disciplines to help develop resuscitation research,[1-4] cardiopulmonary-cerebral resuscitation (CPCR),[5,6] emergency medical services (EMS),[7,8] respiratory therapy,[9] critical (intensive) care medicine (CCM),[9-16] biomedical ethics,[17] disaster reanimatology,[18] and "peace medicine."[19]

This autobiography was my first attempt to dig into the past, much of which, since coming to the United States (U.S.) in 1949, I tried to forget. Many of my professional involvements have been outside operating rooms and have been international. These memoirs are about people and events as I remember them now, in 2000 C.E. (Common Era), at age 76.

I am deeply grateful to family, friends, colleagues, and students who have taught and inspired me. Through my parents, Professor Karl Safar, M.D. (1892-1963) (fig. 1), and Vinzenzia (Vinca) Safar, nee Landauer, M.D. (1891-1983) (fig. 2), their circles, and my own busy life as a world citizen (table 1), I have formed impressions about the events that shaped the entire twentieth century. My mother, when in her eighties, and upon my request, wrote scholarly memoirs that are a mix of European history of the twentieth century and her personal life and views.[20]

Primary influences in my personal life have been my marriage to Eva Safar, nee Kyzivat in 1950 (also an ex-Viennese), our daughter Elizabeth (she was born in 1954 and died from status asthmaticus in 1966), our son Philip (born in 1960, he became a lawyer, mountaineer, and officer in the US Army), and our son Paul (born in 1969, he became a musician, composer, poet, and naturalist). My workaholic lifestyle as a clinician, teacher, researcher, and leader was strongly influenced by my roots and goals and my chance survival of World War II. World travels, music, piano playing, mountaineering, water sports, snow skiing, photography, philosophy, and world peace movements have been among other formative involvements.

I hope the following vignettes help young colleagues appreciate some of the foundations of their present practices and the labor pains of those who gave birth to modern anesthesiology. The times I will describe here were in many ways simpler than the 1990s. Communications were slower, but innovations were often implemented then more rapidly than now, because fewer committees and agencies meddled with the innovators. Defending anesthesiology half a century ago was difficult, but making careers in this new discipline was easier then.

Although inhalation analgesia began in the mid 1800s and local-regional anesthesia around 1900, modern general anesthesia, i.e., with a scientific basis and with the ability to control airway, ventilation, and circulation, was born in the 1930s in Britain and the United States. Modern local anesthesia began with Viennese ophthalmologist Carl Koller's use of cocaine for topical anesthesia (in 1884) and flourished soon thereafter with infiltration anesthesia for general surgery, as introduced by Schleich in Europe and Halsted in America. Modern anesthesiology as a specialty had its infancy during World War II, its childhood in the late 1940s, and its adolescence in the 1950s. That was followed by a maturity that now enables the most complex modern surgery with maximal anesthesia safety. Furthermore, anesthesiology has had ever-broadening benefits for humankind in general. Unfortunately, the public in the United States has lacked appreciation and understanding of our specialty. Some of our colleagues in other disciplines have appreciated and acknowledged individual anesthesiologists, but not anesthesiol-

Figure 1. Karl Safar, M.D. 1920s. My father. He was born in Vienna in 1892.

Figure 2. Vinca Safar-Landauer, M.D., 1920s. My mother. She was born in Salzburg, Austria, in 1891.

ogy as a specialty. I hope that the next generation will achieve public recognition of our specialty more successfully than our generation has.

A few definitions are appropriate here. "Anesthesia" is an artificially induced painless state that often requires life support, which includes resuscitation (the reversal of terminal states like severe shock or asphyxia, or clinical death). I have used the term "acute medicine" to encompass emergency medicine (EM) for critically ill or injured patients, plus CCM. EM plus CCM, which I have called "emergency and critical care medicine" (ECCM),[7,15] has to be multidisciplinary. CCM includes emergency resuscitation and long-term titrated life support of critically ill or injured (potentially salvageable) patients. CCM should apply not only to care in hospitals' intensive care units (ICU), but to the entire continuum,[15] from the prehospital arena through transportation with life support to in-hospital care in the emergency department (ED), operating room (OR), and ICU. Anesthetizing critically ill or injured patients is CCM.

I have always perceived anesthesiologists as specialists whose interests and expertise include the art and science of all of the above – plus pain control. "Reanimatology," a term coined by my friend Vladimir Negovsky of Moscow, or what we have termed "resuscitology," is the science of resuscitation, which in its broadest sense is the science of ECCM. Every "complete" anesthesiologist is, by definition, a reanimatologist and intensivist (or should be). After having promoted these views for almost half a century, I must admit that in their entirety they have been implemented more abroad than in the United States, where the modern concepts and methods of acute medicine were initiated.[1-5]

May these memoirs help younger colleagues appreciate that each of us is here on earth only for a brief visit, trying to contribute, to experience, and to enjoy life in an individualized mixture. My colleagues and I have been brought up with a commitment to help others, directly or indirectly, as our abilities allow.

Physicians throughout history have had unique opportunities to become strong links in one or more of the chains of human development. Each link is made up of circles within circles. Such global philosophic understanding might help presently active and struggling anesthesiologists-reanimatologists-intensivists to endure in the 1990s the harassment imposed on them by the "mismanaged care for managed profit by nonprofessional middlemen" in the U.S. It is time to reverse the focus "from profits to patients" (B. Lown). Leadership by clinicians with social conscience is needed.

Organized medicine and academic medicine in the 1970s and 1980s in the U.S. failed to initiate the changes needed to decrease escalating health care costs while achieving universal access to basic health care and steadily improving the quality of care. Medical practice should be a calling, not a business. The three-legged stool of patient care, teaching, and research that constitutes academic medicine should not be allowed to topple. If business medicine is allowed to destroy academic medicine in this country, America will lose its world leadership of medical specialties achieved after World War II. Changes are inevitable, but the good of the past must be preserved. Some basic values are timeless. Academicians and practitioners of medicine and nursing must defend the moral high ground, the many "Cs": commitment, competence, compassion, caring, common sense, confidentiality, collegiality, and community.

These memoirs are not a history book. My thoughts on the history of emergency resuscitation[1] and some of my personal involvement in the development of modern resuscitation[2-6] have been published. My interpretation of the histories of modern EMS,[7, 8] CCM,[9-16] brain resuscitation research,[6] ethical dilemmas of resuscitation medicine,[17] "disaster reanimatology,"[18] and peace medicine[19] currently exist only as sketches. I will touch on how anesthesiology fathered the above. I was interviewed by my friend, colleague, and successor as anesthesiology chairman at Pittsburgh, Peter Winter[21] for two ASA-WLM "living history" videotapes. "The Minute Men" club of the University of Pittsburgh School of Medicine, at its February 1997 meeting (George Washington's birthday celebration), invited me to summarize "My Life As a Physician" with a slide lecture.[22] Some of my early OR anesthesia teaching-coaching has been captured on videotapes, from which a teaching film on "The Art of Anesthetizing" may evolve later, to be donated to the ASA Wood Library-Museum.

VIENNA, 1924–1949

Austria Before the Nazis, 1924-38

Austria's First Republic lasted from 1918 to 1938. After the end of World War I (in 1918), the destruction of the Austrian-Hungarian empire by the victors and by nationalistic demons destroyed the balance of power in Europe. Old Austria, a counterforce to German-Prussian militarism, was abolished. The multiethnic Austrian-Hungarian empire of about 50 million people, ruled by the Hapsburgs for 600 years, could have initiated the democratic United States of Central Europe at the start of the twentieth century had Emperor Franz Joseph, before he became senile, turned the reins over to Crown Prince Rudolf (who committed suicide in 1889) or to Franz Ferdinand (who was assassinated in 1914 in Sarajevo). Almost a century later the dream is being realized in the European Union. Franz Joseph's naiveté and senility inadvertently triggered World War I. Generals of all six European superpowers, unchecked by senile rulers, ignited and perpetuated the slaughter of a generation of young men (see books by E.M. Remarque). Without World War I, there would have been no Hitler and no World War II. In 1918, the well-intentioned U.S. policy for the "self-determination of nations" might have given Germany, Austria, Bulgaria, and Turkey (the Central Powers) a chance to recover after they had lost World War I. That policy was not fully implemented after President Woodrow Wilson suffered a stroke. For example, new borders kept German-speaking people inside Czechoslovakia (the Sudetenland), Italy (the South-Tyrol), and France (Alsace-Lorraine). The end of old Austria, postwar starvation in the 1920s, and the worldwide depression of the 1930s led to Hitler and World War II.

Immediately after World War I, the small German-speaking remnant of Austria (population 7 million), became a republic, but suffered disastrous economic and health conditions. All old money was totally devaluated. The population of Vienna starved. After partial recovery in the 1920s, the worldwide economic crisis led to confusion, political polarization, widespread unemployment, and a yearning for better conditions. Solutions to these problems in Austria were seen across the border, where Hitler had begun to abolish unemployment in 1933. At the same time in the U.S., President Franklin D. Roosevelt accomplished the same by democratic means. The brief blossoming of Vienna under the social democratic local government around 1930 was squelched by the Austro-fascist government of Dollfuss and Schuschnigg. This was a relatively benevolent, right-wing dictatorship, linked with the Catholic Church. After Nazi thugs murdered Dollfuss in 1934, Schuschnigg became chancellor.

The Safár family (Czech for Schafer or Schaffer, i.e., "manager") originated in a small grass-thatched farmhouse of the 1600s in the village Ludovice near Josefstadt, in the Adler Mountains (the border between Bohemia and Silesia), now in the Czech Republic. My paternal grandfather, Josef Safar, was born and raised in Josefstadt. As a poor teenager, he came to Vienna in the 1870s to learn a trade. In the nineteenth century, many poor but courageous people sought better opportunities in Vienna, then the capital of an empire, or else in America. In Vienna, my grandfather first became a bookbinder's apprentice. By the turn of the century, as a self-made man and entrepreneur, he had created an international medical publishing house, "Der Medizinische Buchverlag Josef Safar." It supplied the multilingual Austrian empire and neighboring countries. The Safar publishing house (Verlag) went bankrupt in 1918 because of breakup of the multination state. In 1924, the Verlag was bought by the Springer Verlag of Germany and became "Minerva," Springer's branch in Vienna.

Karl Safar (see fig. 1), my father, was born in Vienna in 1892. He attended one of Vienna's gymnasiums (high schools). My father's choice to study medicine, which he did in Vienna in 1910-1917, was influenced by his father's association with physicians. My father became an internationally known professor in ophthalmology in Vienna. He pioneered eye surgery with electrocoagulation methods, primarily for retinal detachment.

Vinzenzia (Vinca) Safar-Landauer (see fig. 2), my mother,[20] was born in Salzburg, Austria, in 1891. Her parents were Viennese. Her father, Gustav Landauer (although baptized "Evangelisch," i.e., Protestant) was of Jewish ancestry. My mother's mother, Anna Scheuch, was of Austro-German (non-Jewish) middle class Viennese ancestry. Vinca went to grammar school in Celje (Slovenia) and to high school in Vienna's elite girls' gymnasium, which was pioneered by a Jewish-Viennese female educator, Dr. Schwarzwald. By her teens, Vinca had become a scholar of ancient Greek. She became one of very few female medical students in Vienna before World War I.

The pre-World War I Vienna of Stefan Zweig, Gustav Mahler, and Sigmund Freud; of the Zionist movement of Herzl; and of a world-famous medical school, was a haven for intellectuals, musicians, artists, scientists, and physicians — a place where anti-Semitism co-existed with tolerance. In this Vienna of the fin-de-siècle,[23] blond medical student Karl Safar met dark-haired medical student Vinca Landauer while dissecting bodies in anatomy classes. They fell in love. They and other "hippies" of the early 1900s pursued alpinism and other exploits of a non-Victorian lifestyle.

My parents were about to finish medical school when World War I broke out. Starting in the summer of 1914, Karl served with the Austrian army, first briefly at the front against Russia (where he acquired life-threatening dysentery), and later, at the front against Italy, accompanied by Vinca, who also volunteered. They helped defend Southern Tyrolia. Both parents worked there as "doctors" doing trauma surgery. They saw horrors in the most beautiful natural environment — all for the glory of generals and emperors. My parents became pacifists. In 1917, toward the end of World War I, they married and received their M.D. degrees in Vienna. After World War I ended, they cherished

American humanitarian aid to impoverished and starving Austrians. They supported the goals of the League of Nations.

My father decided to become an ophthalmologist because he had eczema of the hands and, therefore, could not consider performing major surgery, which required rubber gloves. Eye surgery then was done without gloves, using "no touch" techniques, i.e., touching tissues only with sterile instrument parts. My mother decided on pediatrics. They moved into the small four-story apartment house that my grandfather had built in the 1890s (and still owned), near Vienna's university hospital (Schloesselgasse 22, Vienna 8). In that house my parents initiated and modernized their Biedermeier apartment, which included father's private-practice office. By the 1990s, five generations of my Viennese family had lived and worked in that house. That type of city apartment living means that children see greenery only in city parks and on trips into the country, which is why my family often visited the second house my grandfather had built, in Vienna's suburb of Grinzing, a historic village on the fringes of the Vienna Woods.

Between the early 1920s and 1938, my father was first a resident and then an assistant professor in the Ophthalmology Klinik (department) #1, at Vienna's University Hospital (Allgemeines Krankenhaus, AKH). They operated mostly under recently discovered local anesthesia.

In the 1920s and 1930s, my mother worked as a pediatrician for the poor in the outpatient clinics of Vienna's blue-collar district, "Ottakring."[20] Vinca was a beautiful, dynamic, realistic idealist and a woman with a social conscience. She domineered in daily life, while my father led in ethical decisions. Vinca was a liberal, sympathetic to the accomplishments of Vienna's Social Democrats, led by physician Viktor Adler.

I was born on April 12, 1924. My mother delivered me in the Department of Obstetrics of Vienna's University Hospital, with a whiff of open-drop ether for analgesia, aided by Professor Kahr, an obstetrician and family friend who emigrated in

1938 (for "racial" reasons) and returned to Vienna in 1945 to again teach obstetrics. The baby he delivered in 1924, he was later to quiz in medical school in 1948.

My sister Hanni was born in 1927. She and I shared a good childhood and a confusing youth. She was beautiful, kind, and artistic, with a learned soprano. In 1948, Hanni married Josef Stepanik, who became an internationally recognized glaucoma expert, professor, and my father's successor as chief ophthalmologist at Vienna's large city hospital "Lainz." Stepanik had trained and researched at the university hospitals of Vienna and Cincinnati as well as Columbia University in New York. Hanni died in 1978 of sudden cardiac arrest, secondary to chronic asthma-generated emphysema and heart disease. Asthma-bronchitis-eczema (chronic "atopic" disease) plagued my grandfather Josef, my father Karl, and me, and killed my sister Hanni and my daughter Elizabeth.

My first contact with anesthesia was as a pediatric patient at the University Hospital of Vienna, probably around 1930: white coats, the ugly smell of antiseptics, and the pleasant smell of ether during open-drop induction via a gauze mask for a then very common tonsillectomy. I have no memory of fighting the ether induction. The surgeon was another family friend, Professor Schlander (who also emigrated when the Nazis came, and then later returned).

My first contact with what later became intensive care was in the mid-1930s. My father told me and my sister Hanni that our mother was dying from acute tuberculous pleurisy. He said that she had already chosen our new mother. Vinca was always "in charge." Father took us to see our mother in the University Hospital's medical ward. There she was, semiconscious, receiving what at that time were extraordinary measures. This included nasogastric tube feeding and a flow of oxygen into the throat, delivered via another rubber nasal tube. Professor and Chairman of the Internal Medicine Klinik #2, Nicholas Jagic, a family friend and tenant in the Safar apartment house, was "babysitting" her. To prevent pleural scarring, he performed daily pleural punctures to drain pus, using steel and glass syringes. *Ubi pus ibi evacua* (where pus, drain it) was, and should still be, the most important measure to prevent infections from becoming lethal. Vinca survived without respiratory insufficiency and enjoyed good health and mind until her sudden death at age 92 from a massive myocardial infarction. Her treatment and survival in the 1930s were unusual. In Europe and the U.S. until the 1960s, the usual management by physicians was intermittent observation (by rounding in hospitals, or by house visits, or by patients visiting doctors' offices), prescribing "cookbook" therapies (to be carried out by nurses), and leaving the patient.

At about age 10, I was allowed to look through my father's microscope: What a wonder to imagine what was going on in these retinal cells to make us see in such detail. As a teenager, I was permitted to hold retractors occasionally during Karl's eye operations. The first time I saw a knife going through the cornea, I fainted into the arms of a pretty OR nurse. After studying the anatomy involved in the surgery, I became interested and remained conscious during operations. When Karl dropped cocaine into the eye sac, he told me that this technique was discovered by Viennese ophthalmologist Carl Koller, prompted by the world's first psychoanalyst, Sigmund Freud. Both had influenced pre-World War I medical students, including my parents. When Carl injected Novocain (procaine) behind the bulbus, he told me that this local anesthetic, synthesized by Einhorn in Germany, was safer than cocaine, but that it did not work topically.

Doctors from abroad, including "head doctors" from America (general practitioners with eye, ear, nose, and throat interests) came to learn eye surgery from my father. He was a role model of a sensitive and surgically delicate physician and a highly principled man who generously shared his expertise with others. Although he never joined a political party, he remained committed to human rights and liberal causes and showed considerable courage during Nazi time, as described later.

During World War I, my parents later told me, there were no concepts of traumatic-hemorrhagic shock, no blood transfusions (although Vienna's Landsteiner had recently discovered the blood groups), no intravenous saline infusions (although the concept existed in animal labs, and necessary supplies were available), no antibiotics, no sulfonamides, no airway tubes, no airway suction (only wiping the pharynx with gauze), and no method for artificial ventilation except manual chest pressure. At their field hospital, horrible deaths occurred from unpreventable and untreatable tetanus. Penetrating abdominal and thoracic injuries had nearly 100% mortality. Anesthesia with open-drop chloroform in their field hospital turned out to be too "deadly" (apnea meant death) and was replaced with open-drop ether, which had a wider margin of safety, although long excitement (stage II), secretions, laryngospasm, and vomiting (often with unrecognized aspiration into the trachea) made induction of full (surgical) anesthesia a challenge. Young doctors, nurses, or orderlies, untrained in anesthesia, were assigned to drop ether on gauze masks, without oxygen enrichment. Ether-induced vomiting was often followed by "pneumonia." Therefore, topical anesthesia with cocaine and local infiltration anesthesia with Novocain were the preferred forms of anesthesia. They were used most commonly (and skillfully) by Austrian and German physicians during both world wars.

Between the world wars, almost all families in central Europe experienced medical tragedies, followed by breakthroughs. For example, my mother's father, a diabetic, died from a phlegmon of the face that led to cavernous sinus thrombosis—two years before insulin was discovered and rapidly introduced. My father's father (Josef) died of pneumococcal pneumonia, two years before the introduction of the first effective sulfonamide, Prontosil. One of my boyhood friends died of hepatic necrosis due to an experimental bismuth drug given for streptococcal sore throat. Many people died of tuberculosis. For central Europeans, penicillin and streptomycin arrived only after the liberation at the end of World War II in 1945.

In the 1930s, I could not predict that the Safar medical family would extend to four generations before the end of the twentieth century. My own sons chose not to become physicians but pursued their parents' avocations (alpinism, justice, internationalism, and music) and their own goals and dreams. I admire them for their spirit of independence.

In the 1930s, we were sheltered children. In grammar school (Albertgasse, Vienna 8) I had the same good teacher, Dr. Hellmann for all four years (1930-34). He was a kind man and also a musician. In grammar school, the city government distributed a bottle of milk every day to every child, free of charge, to combat rampant malnutrition. The classrooms were lit by gaslight and heated with coals in iron stoves. Poverty surrounded us and even made my physician parents live on very modest means. The great luxury was the family car, which took us children on summer vacations to nearby mountains, Austria's lakes near Salzburg, Czechoslovakia, Italy, or Yugoslavia. I enjoyed private piano lessons, ice skating, skiing, swimming, and photography since age six. Gymnasium in 1934-42 was tough. I enjoyed languages, arts, sports, and geometry, but had difficulty with mathematics. My brain's difficulty with abstract thinking and my lack of will to learn mathematics later became handicaps in my research, which I pursued without knowledge of statistics and computerism.

In the 1930s, on vacation trips to Yugoslavia and Italy, my parents took me to World War I cemeteries in the mountains. I could not understand these thousands of crosses and plaques with multinational names of teenagers and young men. Who had the right to order them to kill and to be killed? What for? A few years later, their homelands were hosting each other as vacationers. People then generally believed that war would never happen again. This proved to be wishful thinking. By the late 1930s, however, many Austrians saw hope for employment and prosperity in the governments across the borders — in fascist Italy and Nazi Germany. These systems at that time promised the people to keep peace. By then, however, Italy had already invaded Ethiopia, and Spain's new democracy became the victim of fascist armies, openly supported by Hitler and Mussolini.[24]

Austria During the Nazis, 1938-1945

This chapter is about a time of confusion leading to one of horrors. I apologize for its complexity, which is unavoidable as it reflects facts the way I see them now. My story is about luck, unimportant compared to the fates of many others caused by the crimes of the system. I was saved and taught by anti-Nazi physicians of Vienna.[25-28]

I have been asked why I wanted to become a physician and how I survived World War II. The answers to these two questions are inseparable.

Why did I decide to become a physician?

For at least three reasons: 1. My physician parents were role models. 2. The wonders of nature turned me on. 3. When I was 18 (in 1942) an influential "uncle," Clemens Wildner, who was an anti-Nazi friend of my parents and a former Austrian diplomat, said, "Peter, medicine is the only profession in which you can avoid being forced to become a servant of the devil."

How did I survive World War II?

I survived through luck, determination, and the courageous help of my father and his friends and colleagues of Austria's passive resistance who risked their own lives. I was not a hero. My father was. Do I feel guilty for having survived? Not guilty, but deeply grateful. My privileged survival of World War II motivated me into a life of workaholism in medicine, starting at the time of my conscription into the German Army in the fall of 1942 and my escape from military service.

My childhood and youth were full of dichotomies. I was brought up in Vienna with Austro-German language and culture. My name is of Czech origin, which was ridiculed by some of my German-named classmates.

After grammar school (elementary school), from age 10 to age 18 (1934-1942), I attended the Piaristengymnasium (in Vienna's 8th district), within walking distance from the Safar house, one of the more prestigious gymnasiums for boys. My first four years of gymnasium were before the Nazis; years five through eight, during the Nazis. I had ancient Greek for two years, then the Nazis stopped it in 1938. Latin continued for eight years and English for six. English lessons consisted mostly of reading and little talking. The Piaristengymnasium was run by the state but influenced by the Catholic church, which had founded that school 200 years earlier. The adjacent Piaristen church is a historic baroque beauty in which the famous Austrian composer Anton Bruckner had played the organ. A negative aspect of the pre-Nazi time was that clergymen forced us students to attend church on Sundays, which I did not like. I preferred to go into nature with my parents and friends. Also, the church made us go to confession and communion, to sing in the choir, and to push bellows for the school's church organ. Alienation caused by the church-controlled Austro-fascism helped drive some of us kids into the arms of German fascism.

In the so-called "free Austria" of the 1930s, the atmosphere in Vienna was most depressing, undoubtedly similar to, or worse than, the Great Depression in the United States. Although my parents had a fine apartment, inherited in my grandfather's house, and even a car (because of my father's income from private-practice surgery), they could not afford a meal with meat more often than once a week. Still, relatively well-off people like us frequently invited hungry school friends to share our meals. Each day several beggars knocked at our door. The streets were full of beggars; sidewalks were contaminated by tuberculous sputum.

We school boys in junior gymnasium (1934-38), young and impressionable, were confused by the symbols imposed on us.

We could choose Austro-fascism's cross, which we rejected because it represented the status quo. The three arrows (symbol of the social democrats) and the hammer-and-sickle (symbol of the communists) religious people slandered, often (incorrectly) fearing that all "leftists" are "anti-Christ." According to our loving house helper (church-going, partially crippled, Irma Weigel), socialists and communists were bad "because they killed the czar." (The social democrats, of course, had nothing to do with communist dictatorships.) Irma was Hanni's and my loving nanny, who lived with us. In 1938, with poverty and diseases still rampant in Austria, the swastika looked attractive; it represented Germany's new Nazi movement, which, since 1933, was reported to have abolished unemployment and reversed the nation's malaise after World War I. In addition to being confused, at age 13, I became subtly rebellious, although I loved my parents dearly. They let me find out the truth for myself.

On March 11, 1938, my father took me to Vienna's opera house. They played Tschaikowsky's *Eugen Onegin*, with Jarmila Novotna singing Tatjana. Novotna was a young opera star from Prague whom my father admired. When we left the opera house on this historic night in 1938, the Ringstrasse (Vienna's great boulevard encircling the city center) was filled with jubilating people. They waved banners bearing swastikas and yelled slogans such as "communists…socialists…Jews — verrecke (perish)." My father turned pale. He said to me, "This looks evil." I did not understand what he meant for a few months. Then he said, "This will lead to war, which will not end until we again will be fed by the Americans, like in 1918." What vision!

The next day (March 12) Hitler and German troops entered Linz (West of Vienna), and on March 13 they came to Vienna. There was no armed resistance and many people rejoiced in the streets. On March 13, the "Anschluss" (annexation) of Austria by Nazi Germany was complete. The April 10 plebiscite of 99.7% for annexation was evidently rigged. Even Catholic Cardinal Innitzer (of Sudeten-German background) recommended voting "yes." After October 1938, however, he and other clergymen increasingly supported the anti-Nazi resistance.

I temporarily forgot what my father said on March 11. Most of us 13-year-old boys were enthusiastic about the idea that all German-speaking people should live in one country. I also was temporarily attracted by National Socialism's emphasis on fitness, athletics, the ideals of "strength and beauty," and peace (we kids knew nothing about Hitler's weapons industry) — all embellished with songs and uniforms. Those who rejoiced in the streets included many decent, idealistic adults. The Depression was still on. Many adults became Nazi Party members to get or retain their jobs. With the annexation of Austria by Germany, there was near-full employment overnight. The murderous intentions of the regime were not then known, since few people read Hitler's published statement *Mein Kampf*. I never did. Western democracies' statesmen flirted with the Hitler regime throughout the 1930s, at first not suspecting that it would lead to war and other evils. Among Austrian teenagers, only those of Jewish or politically active anti-Nazi families easily refrained from embracing the Nazi movement in 1938.

The anti-Semitic propaganda of the regime had spread from Germany to Austria before 1938, which made Jewish friends of my family emigrate after the Anschluss as rapidly as they could, provided they got visas and sponsors. They had to leave their possessions behind. My parents helped some Jewish colleagues with contacts and money. Right after the Anschluss, adult thugs in brown shirts forced "Jews" to scrub by hand earlier anti-Nazi propaganda from the city streets, as an act of humiliation. Though this disgusted me and decent adult "Aryans," we were told that this harassment and humiliation of Jews and some other minorities represented excesses by fanatics, which should and would be outlawed. This was wishful thinking. The majority of Jews in Vienna could not get visas and soon were required to wear the yellow Star of David.

In the Spring of 1938, an estimated 50% of Austria's population, including my parents, cried privately because they knew that this was the end of Austria, and because they feared and objected to the Nazis' hateful slogans. We learned much later that the Nazis in Germany had, since 1933, committed isolated, secret murders of some leading political opponents – not because they were Jewish, but rather because they were socialist, communist, clergy, or other leaders whom Hitler feared. Until the war started I knew nothing of any killings committed by the regime.

Throughout Nazi time in Austria (17 months of peace followed by 5-1/2 years of war) my father refused to answer the obligatory Hitler salute. After the start of the war, with the beginning of martial law, his refusal to do so could have led to concentration camp and worse. We risked the same fate if caught when we listened (routinely) to the British Broadcasting Corporation (BBC) on the radio, or when my father cared medically for Jews (which was outlawed).

In 1938, although "Aryan," my father was dismissed from his position as docent (associate professor) because he refused to join the Nazi Party and because my mother was a "Mischling" (partially non-Aryan).[25] Most Viennese intellectuals of ethnically Hebrew ancestry were not religious. Some had been baptized, to "assimilate" into their communities, but were actually agnostics. Racial physical characteristics of most Central Europeans were mixed. However, the Nazis' Nuremberg laws determined race on the basis of religion over several generations. Under Hitler, according to the "large" Aryan certificate, which included the religion recorded for great-grandparents, my mother was half Jewish and I was one-quarter Jewish. But we were only one-quarter and one-eighth Jewish, respectively, if they asked for the "small" Aryan certificate (only grandparents' religions). As a Mischling, my mother also lost her job.[27] Throughout Nazi time, she helped my father in his private practice, which he was allowed to continue. She nurtured us, and hosted friends and acquaintances with a mixture of backgrounds, religions, and philosophies. Others in my father's position divorced, sending the "contaminated" spouse into emigration. Father remained loyal. I was allowed to finish high school (gymnasium) but, because I was a Mischling, would not have been allowed to register for the university. I was, however, "qualified" to become cannon fodder in the war if I was one-quarter Jewish. Boys who were half Jewish and half Aryan and others who declared themselves ethnically not German (as did the family of my future wife, Eva, of Czech origin) had better chances for survival, since they were not conscripted into the German Army and not taken to concentration camps. They were not, however, allowed to study in universities. Instead, these 18-year-old draftees were forced to work with war prisoners in war-related industries. Some were taken, together with Gypsies and other people of "inferior race," into the "technische Nothilfe," a paramilitary unit without arms.

Younger boys with "ethnic defects," e.g., of Czech or Polish ancestry (as opposed to "racial defects"), were not considered worthy of even a high school education and had to be laborers (Hilfsarbeiter = unskilled worker) for the duration of the Nazi regime. Because of his Czech ancestry, my brother-in-law Hans Kyzivat was expelled from the gymnasium at age 15. Having been interrogated by the Gestapo for a full day at their headquarters on Morzinplatz in Vienna's Inner City (for his connection with an anti-Nazi church group), Hans became a forced laborer at a war-related business (kriegswichtiger Betrieb) fitting and supplying eyeglasses for new recruits of the German Army. His older sister Olga was refused University enrollment by the regime and was also work-assigned.

From 1936 to 1938, before the Nazis, I belonged to a nonpolitical Austrian Boy Scout-like youth group called the "Neulaender." I have memories of romantic weekend bicycle trips and a mid-winter ski camp, sleeping in an open barn (with a bonfire next to dry hay). As teenagers we felt indestructible.

When Germany annexed Austria, all such youth groups were outlawed and their members under 15 were transferred into the "Jungvolk," a Nazi youth movement for kids, who were subsequently enticed to join the Hitler Youth, which was meant for boys aged 15-18. In the spring of 1938, I joined the Jungvolk at age 13 and wore their uniform off-and-on. I am ashamed that it took me awhile to become disenchanted. It can be explained (not excused) by the confusion of the time, and an ignorant boy being impressionable. I went to two camps, but quickly became disillusioned by their Prussian militaristic drills. By the summer of 1938, I escaped peer pressure, disobeying their order to join summer camp. Instead, I went on a private bicycle trip with my best grammar-school friend, Joseph (Pepperl) Trappel. We escaped the political confusion by bicycling through Austria, over many high mountain passes, including the famous Grossglockner Road.

Pepperl Trappel was a kind, mild-mannered and loyal friend who spent much time at our home. He was the only son of a very poor couple. The family lived in a small basement room. His father was dying of tuberculosis. Pepperl attended regular high school from ages 10-14. He did not join the Hitler Youth. He became a student dental technician, and was conscripted in 1942 into the Wehrmacht (German Army) like all of us. He was sent to the Russian front, where he survived the German retreat in the winter of 1943/44. When he was on a brief furlough in the spring of 1944 in Vienna, my father and I offered to help him fake illness to avoid returning to the front. He refused, partly out of fear of being caught, and in part out of loyalty to his comrades. After D-Day, he was ordered into the senseless defense at the Western Front and was never heard of again.

In late summer 1938, I was ordered to the local party (Gestapo) office to be scolded for not having come to the summer camp of the Jungvolk, and to make me join the Hitler Youth, now being age 14. I refused. They let me go when I said I was neither motivated nor fully Aryan.

Also in the summer of 1938, my father took me along on a drive to Italy to search for papers of my mother's ancestry, which the authorities had requested to document whether our family included non-Aryans. I was intrigued and excited when we discovered, in Ancona, Italy, that my mother's grandmother, Elizabeth Perugia, indeed had "Hebrew" on her birth certificate. "So what?," I thought, still oblivious to the fact that the regime's anti-Semitism might become more than merely an attitude and harassment.

In the autumn of 1938, on the evening of what later became known as the "Crystal Night," shop windows were being smashed by young adults. At Vienna's city center, thugs broke into the archbishop's palace and burned books. After it was over I learned that these shops were owned by Jews and also that the near-by synagogue was burnt. The government portrayed the window-smashing as spontaneous excesses of the public, who were "justifiably" angry about Jews' professional and financial successes. After the war I learned that from among the over 100,000 "Jews" in metropolitan Vienna in the 1930s (over 5% of the population of 2 million), about 5,000 were arrested during the Crystal Night. Thugs committed crimes unchecked by law.

Between the spring of 1938 and September 1939, almost all of our family's friends and colleagues with "Jewish" on their birth certificates, and Jewish gymnasium students, managed to emigrate with their families. In the summer of 1939, my father packed up the family car with me, my sister, and my mother, to go to Yugoslavia for "vacation." He had considered leaving Europe for good, perhaps with the help of a friend and colleague living in Cairo, Egypt, Ernest Hess, a Swiss internist (whose patients included Egypt's Khedive). Years earlier my father had saved Hess's eyesight. At the end of August, in Dubrovnik, radio messages with rumors about impending war were unclear, only rumors. On September 1, 1939, as we drove through Sarajevo across the bridge where Austria's crown prince Franz-

Ferdinand and his wife had been assassinated in June 1914 (igniting World War I), we heard Hitler's voice from a radio through an open window across the river, announcing Germany's invasion of Poland – igniting World War II! (This, unimaginable then, would not end until 55 million people were murdered over 5-1/2 years).[24]

From Sarajevo on to Belgrade. There my parents and their Yugoslav colleagues pondered how long the Nazi regime might last. I did not like that the Serbs sounded fanatically and insultingly anti-German and anti-Austrian, not just anti-Nazi. Father seriously considered not returning to Vienna but (I suspected later) hesitated because his professional future in exile was uncertain (he was a successful middle-aged eye surgeon who would have to repeat exams and might be considered a threat to private-practice specialists abroad) and because mother did not want to leave Vienna. With emotions concerning other family members in Vienna and wishful thinking that the war in Poland would soon be over and that the regime would mellow, my parents (hesitatingly) decided to drive back to Vienna. When we crossed the border from Yugoslavia back into Austria (Germany), the border guards gave us food-ration tickets. The curtain fell. I remember a gloomy atmosphere; none of the "hurrah patriotism" we were told occurred in 1914. We were all trapped in an unsafe land at war. And now martial law prevailed.

When German troops conquered Poland in September 1939 and France in the spring of 1940, many people rejoiced. The controlled news media showed only "clean" victories, without death and destruction. My father said, "Never before had such beautiful youth (the conscripted young men of my generation) been abused for such evil deeds." I was too young to understand all this. Germany's conquest of Poland and other European countries, and the bombing of Warsaw, Rotterdam, and London, convinced me that the system truly was evil.

Many Austrians were in favor of German-speaking Austria (in the spring of 1938) and Sudetenland (in the fall of 1938) becoming part of Germany. Some, however, lost their initial enthusiasm for Hitler already before the outbreak of World War II, owing to the arrests and disappearances of some political opponents, maltreatment of Jews (nothing known then about murder), and the occupation of Czechoslovakia in the spring of 1939. With the unprovoked, armed aggression against Poland in September 1939, and other brutal conquests of Europe to follow, and the subsequent retreats of the German armies, more and more initially enthusiastic Austrians became disillusioned, and recognized what the regime had been trying to hide. Most Austrians felt caught in a dictatorship that conscripted their young men into the German Army as cannon fodder. I learned later that deserters were executed on the spot. Among Austrians who resisted actively, after 1945 we learned that 2,700 had been executed.[24] Could we have done more to help others? Yes, but active resistance would not have altered the course of the war, and we would not have survived. Among the young heroes of the active Austrian resistance who were caught and executed was Ludwig Stepanik, the brother of my brother-in-law. Ludwig reportedly lost his life in a most brutal manner on June 21, 1944 in the concentration camp Mauthausen.

After the war began in September 1939, the regime ruled by fear and secrecy, skillfully hiding its brutality, at least vis à vis the civilian population. The brutality of the regime became known to some members of my parents' circles sooner than to the general population. My family and I remained partially informed throughout the war by regularly listening to the BBC, either in our apartment or in that of our family's private English teacher, Mrs. Connel, an elderly English widow of an Austrian man. Whenever the doorbell rang, we turned pale and changed the station. Concentration camps were officially labelled "holding camps," and the BBC reported only vague suspicions about killings in the camps. I learned only after the war's end that, starting in 1943, all 100% Jews in Vienna were taken to concentration camps. Then I was already gone, as a conscript, trying to save my own neck. The official explanation of these arrests was "only for the duration of the war, because they are enemies of the German people." It sounded like what was done to Japanese-Americans in the U.S. The mass murder of Jews and others in concentration camps from 1943 to the end of the war, we learned about only at the time of liberation in the spring of 1945.

Throughout the war, my father was assigned by the government to work as an ophthalmologist in various hospitals in Vienna. In the afternoons he could pursue private practice at home and operate in private clinics. Both parents helped others who were harassed or endangered by the regime. All members of my immediate family survived. After World War II, my father was reinstated as a professor. While my sister's gymnasium teachers harassed her because of her not-fully-Aryan background, my professors and the principal of the Piaristengymnasium remained more liberal and tolerant than those in other schools. The Piaristen professors remained kind to the three or four of us who were "Mischlings."

During the war years before conscription, I escaped into music, went to concerts and operas, played much piano, and took lessons in dancing and horseback riding. Whenever possible, during and after the war, I made chamber music at home. Four players on two pianos would learn about the symphonic literature, including Mendelsohn and Mahler, whose works were outlawed as Jewish. I accompanied singers. Later I learned to appreciate piano trios. Looking back, our obliviousness to the events at the fronts seems selfish. In the summer of 1941, oblivious to the invasion of Russia, I bicycled with school friends Weichinger and Herndl via Austria's highest Alpine passes to the Bayreuth Wagner Music Festival and back, over 2500 km. Introduced to the Bayreuth musicians by laryngologist Wiethe of Vienna (partially Jewish), we heard fabulous music without political innuendos. I quickly abandoned a brief flirtation with the idea of becoming a professional musician, realizing that this would require exceptional talent, which I did not have. Musical talent, based on several generations of amateur musicians from Eva's and my ancestries, flourished in our son Paul. I personally liked geometry, I briefly entertained the idea of becoming an engineer or architect, but around the time of graduation from gymnasium I had settled on becoming a physician if that was possible.

In 1939-42, although I tried not to think about the war, which was "somewhere else," I empathized with the freezing victims of torpedoed ships and with soldiers on both sides, killed or maimed in battles fought in hot and cold climates. After the costly, heroic, and successful defenses of Moscow and Leningrad in 1941-42 and the landing of American forces in North Africa in the fall of 1942, more and more people realized that Germany would (and should) lose the war. Still, there was no uncensored news, which might have shown the realism of murder and destruction caused by this war. The weekly newsreels in movie houses never showed injured or dead people, as television does now.

A doomsday atmosphere pervaded when we gymnasium students finished Matura (graduation exams) in spring of 1942. The realists were depressed — soon to be taken to the killing fields with a high risk of not returning. Optimists behaved as if they were indestructible. My feelings were somewhere in between. At that time, we avoided talking politics to each other as one could not trust anyone. After the war I learned that the majority of my graduating class felt as victims, not as champions, of a cause. From the fall of 1942 on, each of us was on his own, trying to survive this madness.

In the spring of 1942, immediately after graduation from gymnasium (Matura), we 18-year-olds were conscripted into labor camps (Arbeitsdienst). Mine was in Bavaria. There the other "inmates" were mostly anti-Nazi boys from the German-speaking part of France (Alsace-Lorraine), which Hitler had conquered in June 1940. These boys sided with France rather than Germany. We were treated worse than in a boot camp, ordered to crawl in mud, and stepped on by the nailed boots of our drill sergeants, who were semi-intellectual brutes. We were made to dig ditches around an airport near Munich. Brief assignment to help a farmer was more pleasant. (I had earlier summer experiences as a volunteer farm worker in Austria.) We had no clue that not far away from our labor camp was the concentration camp of Dachau. In 1945, one of my Alsacian co-inmates of 1942 entered Austria with the French Army. We brought the most sadistic sergeant to trial.

When we were discharged from that labor camp in the fall of 1942, we were immediately conscripted into the German Army. Our generation was trapped. Active resistance was suicidal. Determined to try to survive and not to kill anyone, I decided to risk arrest (even execution by the Gestapo) rather than go to the front, where I would be forced to kill and be killed for the wrong cause. Naively, I plotted how, if sent to the front, I would desert to the other side, imagining the SS police behind me and the Russians not understanding me.

By the time I was conscripted into the German Army in the fall of 1942, any thinking Nazi or anti-Nazi could sense that Germany was losing the war — in Stalingrad, Moscow, Leningrad, and North Africa. We had no idea that Hitler and his officers' corps would continue sacrificing millions of additional casualties on all sides, just to delay the inevitable for another two and one-half years.

I could select the type of service. My father and his friends advised me to ask for the artillery, because that meant basic training in a camp in Bruenn (Brno in occupied Czechoslovakia), run by officers of the old Austrian Army who had been drafted into the German Army. These officers had formed a passive resistance network of loyal friends.

In Bruenn, I was trained for two months as a telegrapher. I became skilled with the Morse code. Had I ended up at the front, I would not even have known how to use a rifle. During this boot camp in the artillery unit, I remember seeing the firing of a heavy gun only once; we telegrapher recruits merely observed. When we were occasionally allowed into the city of Brno, I sometimes changed into civilian clothes and wandered over a hill to a small house owned by distant relatives of my father. They introduced me to some Czech men, apparently with the active anti-Nazi resistance. They did not talk much. I did not understand what they were up to. I did not know then that in May 1942 the SS governor of Bohemia, Heydrich, had been assassinated by people linked with a Czech exile group in England. In retaliation, the Nazis had murdered all men of Lidice and destroyed the village.

Fritz Kohler was the only gymnasium collegue conscripted into the same labor camp and army unit. He had just lost his only brother at the Russian front. Kohler was wounded in Monte Cassino and by bombs in a military hospital near Vienna. he survived severe wound sepsis with small-volume fresh-blood transfusion, polyvinylpyrrolidone and sulfonamide. After the war, he became a distinguished professor of chemical physics in Vienna and Bochum and a supporter of international peace movements.

In December 1942, at the Army training camp in Brno, those of us recruits who were gymnasium graduates, then still with lowest rank (private) in the artillery, were individually interviewed and asked to volunteer for officers' school. There, we knew, we would be able to avoid the front for another year. Looking into the eyes of the interviewing officer, Captain Kuhlang, who asked me to volunteer for officers' school, I instinctively suspected that he was "one of us." I said, "I will never become an officer of this regime; and, by the way, I am not fully Aryan." Although he could have had me arrested immediately, he responded only that the lack of a fully-Aryan certificate would not matter anymore because officers were needed to replace the many who had been killed. But — since I refuse officer's training — he would be forced to have me sent as a private to the front now. We suspected that this would be Stalingrad.

Risky actions by friends and colleagues of my parents, and my father, who informally belonged to Austria's passive resistance, saved the lives of several young conscripts. For me this began on Christmas Eve of 1942. Major Maric, who was in charge of the German Army's training camp in Bruenn in 1942, was an Austrian officer in World War I. He was tipped off by another former Austrian officer, Major Morawek, then assigned to duty in occupied Poland. Morawek had been tipped off by his and my parents' World War I buddy, Max Block, (a former x-ray technologist of probably Jewish background in semi-hiding in Vienna). Major Maric allowed several of us 18-year-old boys from Vienna a three-day furlough, starting December 24, 1942, "before going to the front." Only later did I suspect that Maric intended this furlough as an opportunity for us to go into semi-hiding, by whatever means possible, to avoid the front.

On arrival in Vienna, in field uniform, not expected by my family, I encountered a doomsday atmosphere. There was grieving about the continued killings, particularly in Stalingrad, and the expectation that I might be on the way there. My parents and I plotted but saw no way out. On Christmas Day, I was invited to the opera by my "aunt," Ella Jagic, the wife of the professor who had saved my mother in 1936. They played Richard Strauss's *Rosenkavalier*. The opera's plot, an eighteenth-century sensitive and joyous Viennese love affair, stood in stark contrast to the suspected events on multiple fronts. Despite the war, the opera house was overheated, and I was wearing an Army uniform with a woolen field shirt. Having had eczema off and on throughout my young years, I (unexpectedly) broke out with fulminating eczema over the entire skin of my trunk and arms. Another member of my father's physician clique (also partly non-Aryan) was Hans Temple, a dermatologist and civilian employee in a dermatology army hospital (lazarette) in Vienna. Temple told me to seek admission to "his" army hospital and alerted the admitting army medical officer. The chief army medical officer of the hospital was another Viennese dermatologist, Geiger. Although he was not a member of the Block-Temple clique, I suspect that he knew but ignored what went on.

I became a patient-soldier in the hospital, where most of the soldier-patients had skin or venereal diseases. Many had syphilis, perhaps deliberately acquired. Some were treated for complications with the only available therapy at the time, salvarsan, a potential killer itself. Gonorrhea was effectively treated with the reasonably safe sulfonamide prontosil. Starting in January 1943, as a partly real and partly fake patient in the army hospital, I delved into day-and-night studies of medicine. Throughout 1943, I pored over a one-volume medical encyclopedia and began to study the famous anatomy books by Viennese professors *Hochstaedter* and *Tandler*, books my parents had used before World War I.

In the spring of 1943, I was not yet a registered medical student when the eczema disappeared. An SS inspection was imminent, either to arrest us or to send us to the front. I smeared my whole body with tuberculin test ointment, knowing that it would cause a reaction. (I had suffered a mild bout of TB infection as a child.) I consciously risked provoking miliary TB and subconsciously denied the real risk of being detected and executed for desertion. A wild rash broke out all over my body. The doctors, including Geiger, were puzzled but apparently sufficiently impressed to sustain my status as a patient for a while.

No longer able to keep me at his hospital, Temple arranged for my transfer to another dermatology army hospital in Vienna. The director of that hospital was Gustav Riehl, Jr., professor and former Austrian medical officer. (His father had initiated dermatology as a specialty at the University of Vienna.) Risking his own life, Riehl harbored several semideserters like me. Not all "protected patients" were Austrians. For example, a German, Kurt Althaus (who after the war became a public health physician leader in Muenster), was anti-Nazi for religious reasons. We became friends and — as soldier-patients — studied together as medical students, frequently hiding from inspections in my family's vineyard in the suburb of Grinzing.

Riehl's hospital also cared for burn victims who came from the front. They included the most ghastly injuries of face and hands from flame throwers. Though my papers said I was a patient, I worked part-time as a paramedic, and caring for these burn victims was my first lesson in critical care nursing. There was no skin grafting, no intravenous fluid therapy, and no antibiotics (only topical sulfonamide).

Whenever an SS inspection was announced through the grapevine, my hospital patient records disappeared, and I was transferred temporarily to another army hospital. My papers in Brno most likely had "disappeared," since my unit there never inquired about me. To avoid the discovery of my ruse, when my skin was intact, I found by chance that I could produce orthostatic albuminuria by leaning backward for long periods over the hard edge of a table. This probably caused reflex renal vasospasm or kinking of vessels. Protective physicians added the diagnosis of "nephritis." I also had off-and-on purulent sinusitis; my hospital record was further embellished by Wilfonseder, professor of otolaryngology and anti-Nazi, at another army hospital in the "Rudolfspital."

In the fall of 1943, Riehl honored my and Althaus's wishes to attend the Vienna Medical School while being soldiers-patients-paramedics at his hospital. As a patient on paper, and working part-time as a paramedic in the hospital, I attended medical school daily in the uniform of an army private. I commuted by bicycle to avoid the trolleys, where I would have been stopped, interrogated, and detected by police. (Throughout, I studied feverishly, often with female medical student Milus Kalbac, also of Vienna's Czech minority. We had a platonic friendship. In 1947, she introduced me to her cousin, Eva, who became my wife).

According to a recent report by the historical and prestigious journal *Wiener Klinische Worchenschrift*,[25-28] almost 50%

of the faculty of Vienna's medical school were dismissed or emigrated at the start of the Nazi era.[25] Some were later murdered in concentration camps.[26] The recently published lists include among the dismissed faculty, my father,[25] and among the dismissed pediatricians, my mother.[27] In the same journal I recently thanked my lifesavers and teachers by name,[28] showing that not all non-Jewish faculty members who remained in their positions were Nazis.

How did I get into medical school given my not-fully-Aryan status? Another member of the passive resistance, with the name of Mr. Hans Hermann, was in charge of medical school admissions and registered me, ignoring the need for the major Aryan certificate. Althaus and I were the only medical students in the uniform of the lowest army rank. All other male medical students were army medical officers, as, for example (I recently learned) our friend Otto (Teddy) Mayrhofer, a future pioneer of European anesthesiology and now chairman emeritus of anesthesiology in Vienna.

In 1943/44, while I studied first-year medical school courses, several of us "medics" at Riehl's army hospital helped others feign disease or injury to avoid front duty. One of our co-conspirators was Beutel, a young Viennese oral surgeon who, I believe, was also there as a "patient."

In summer 1944, I had been with Riehl one year when he considered our remaining in the army hospital to be too risky. This was right after the failed attempt to assassinate Hitler, when many resistance people were executed. Again risking his own life, Riehl decided to recommend that Althaus and I (and probably a few other "patients" he trusted and protected) be discharged from the army as "physically unfit for service." At the discharge examination, my skin looked good. (I did not dare take another tuberculin test.) Although there were some close shaves (near-detection) during the discharge procedures, Riehl's plan succeeded with the help of sergeants in various army offices, who were loyal Austrians.

After my discharge from the German Army in the summer of 1944, I disappeared for a while into the mountains, in civilian clothes, with my sister Hanni, hiding from Nazi civilians who might denounce me. Then, back in medical school in civilian clothes, I lived at home but mostly studied in classrooms, laboratories, the university hospital, or my family's vineyard in the Viennese suburb of Grinzing. I tried to pay back for still being alive by studying especially diligently. In 1944/45, some of us youngsters in semi-hiding formed a literary club. Amid bombings and during the battle of Vienna, we read *Gone With the Wind,* trying to defy the insanity of the times and identifying with the story of the American Civil War.

During the last year of the war, there were air raids on Vienna. Frequent air alarms, which "cleared" the streets, made it difficult for the Gestapo to find people. In contrast to carpet bombing in Germany, in Austria the Allied Forces officially aimed only at military targets. Nevertheless, many bombs went awry, killing an estimated 20,000 civilians in Vienna.[24] Near-hits of our bomb shelters were common, as was helping bombed-out people to survive. Once, when running to a shelter with my father, I felt bomb fragments hit very close. My father barely escaped another bombing, that of the Triester Hospital in Vienna, where he was "duty assigned." They had aimed for the nearby railroad terminal and killed many, including forced laborers hiding in open trenches. An air raid by American bombers from Foggia, Italy, on March 10, 1945, destroyed Gestapo headquarters — the former Hotel Metropol at the Morzinplatz, on the rim of Vienna's first district. On the way, they accidentally also destroyed our beloved opera house. In 1943, the Nazi governor of Austria stated the intention to "deport" also Czechs and other Slavs among Austro-German citizens, but this was not carried out.

In summary, my pre-clinical studies of medicine (years 1 and 2) took place in the Third Reich, from the spring of 1943 to the spring of 1945, while I was declared "unfit for military service." I felt destined to survive and obligated to help save others, as a medical student and, later, as a physician.

My feverish preclinical studies benefited from the positive teaching of fine professors.[28] Only chairmen of physiology Plattner, of biochemistry Barenscheen (who had a lady scientist who had been dismissed for racial reasons, teach us), and of anatomy Pernkopf, had the reputations as enthusiastic Party members. Physiology (not Plattner) had turned me on. When the front approached, Plattner offered me a teaching-assistant position "next year." I declined, saying, "I doubt that you will be here next year." Shortly thereafter, he, like most other Nazis, escaped to the West.

The survival and end of World War II I experienced as a medical student. The last winter of the war, I finished my preclinical examinations, studying by candlelight because air raids had interrupted electric power. After the physiology exam, I volunteered as a medical student clinical clerk in surgery to be of service during the expected street fighting. I was assigned to the Division of Orthopedics under Chiari.

The clinical disciplines at the University of Vienna traditionally had been taught by two competing departments. Students registered for one or the other. In the late nineteenth century, the Surgery Klinik (Department) #1 was led by the legendary Professor Theodor Billroth, whose spirit is still felt there now. Billroth had invented operations, promoted anesthesia with chloroform induction followed by ether (both by open drop), tried a chloroform-ether mixture (1:2), discovered a bactericidal effect of molds (but did not pursue it), initiated one of the first ambulance services, started the first residency program in surgery, and made chamber music with Brahms.

The Battle of Vienna occurred in April 1945. My parents, sister, and other family members shuttled back and forth between our nearby apartment house's cellar (we slept on coals) and the nearby university hospital (for shelter and to help patients). Luckily, all family members survived the battle. We and our neighbors were starved and without water, gas, or electricity. The Red Army approached Vienna, first from Hungary and the south, but conquered Vienna from the south and west, guided by members of the Austrian resistance. Tanks rolled down from the Vienna Woods eastward toward our hospital. The retreating SS units did not expect them from the west. Some phone lines were open. As the front approached Vienna, my father could talk to a colleague in Yugoslavia already liberated by Tito's partisans. I could telephone medical student Milus Kalbac in the outskirts of Vienna while her district was being liberated by the Red Army. As the fighting approached, most of the political prisoners in Vienna's Landesgericht reportedly were freed.

During the fighting, early in April 1945, Professor of Surgery Schoenbauer, then a general in the German Army, saved our university hospital, the historic (huge) Allgemeines Krankenhaus (AKH).[29] As the Red Army approached our hospital, the SS troops tried to use the hospital as a fortress. Hospital staff had turned over a wrecked bus to block the entrance to the first and largest courtyard, which housed Surgery Klinik #1. In his Wehrmacht General's uniform, Schoenbauer positioned himself at the entrance door and firmly (with risk to his own life) ordered the SS officer to leave, quoting the Geneva Convention. After several tense exchanges, the SS troops left. The battle raged around the hospital for one week. Hospital personnel in white coats, including some of us, under sniper fire on the surrounding streets, pulled wounded or dead victims, some in uniform and some in civilian clothes, into the hospital "fortress."

Fritz Helmer (now professor emeritus of surgery in Vienna) eloquently (in the German language) described how the Austrian resistance group called "O-5," which included some physicians, helped save Vienna.[29] When the Red Army advanced, a Nazi ordered doctors and nurses at our university hospital (including me) into an amphitheater. He told us to take up guns and become part of the last resistance by the Volkswehr, which consisted of teenagers and old men. One courageous professor and an outspoken group of faculty-staff physicians demanded that the recruiting Nazi leave the room. No one joined the call to arms. There were no reprisals.

During the fighting outside, the physicians' resistance group at the university hospital selected Professor Schoenbauer as the new director of the hospital after the previous civilian Nazi director had escaped to the West. Although Schoenbauer was a member of the Nazi Party, he was a decent, honorable man. The four clinical department chairmen who were not Nazi Party members — Chairman of Surgery #2 Professor Wolfgang Denk; Chairman of Medicine #2 Nicholas Jagic (my "uncle"); Chairman of Ophthalmology #2 Karl Lindner; and Chairman of Pathology Hermann Chiari — declined. Schoenbauer accepted.[29]

My clinical experience there was to function as an orderly, helping surgeons and nurses, during and after that battle, in the operating rooms for orthopedic surgery and later in the "bunker" (a concrete-reinforced general operating room) of Schoenbauer's general surgery unit. I watched young surgical assistants give open-drop anesthesia — inductions with ethylchloride and maintenance with ether — sometimes by candlelight (both agents are flammable). Almost all patients snored, i.e., were partially obstructed. Ethylchloride sprayed on improvised gauze masks was considered tricky, being almost as potent as chloroform. When breathing ceased, after a few perfunctory pushes on the chest and an adrenaline injection into the heart, the patient was declared dead. Therefore, local infiltration anesthesia with Novocain (procaine) was always the first choice. Shell fragments in soft tissue often were removed without anesthesia. Morphine or pantopon was given intramuscularly, not intravenously.

The Soviet army did not bomb Vienna from the air. They caused some damage by artillery shells. They fought from street to street, mostly against SS troops, who eventually withdrew across the Danube to the north and then escaped to the west. Before leaving, however, following Hitler's "scorched earth" order, the SS gave Vienna a death blow, firing shells into the center of the city, which severely damaged St. Stephen's Cathedral (and destroyed my future wife Eva Kyzivat's family jewelry shop). There were heroic deeds by Austrians as officers in the regular German Army who opposed these orders of destruction. The SS murdered several of them before they withdrew and the Red Army moved in.

On my 21st birthday (April 12, 1945), a sunny morning, I shook hands with the first liberating Red Army soldier in the courtyard of our university hospital. Without radio, newspapers, or electricity, it was several days until we learned that Franklin D. Roosevelt had died on that day. We then grieved the death of our real liberator. On April 17, as fighting continued across the Danube Canal, new Austrian leaders (liberated from concentration camps) initiated the Second Republic in the "Red Salon" of Vienna's war-damaged City Hall. (By coincidence, half a century later, in 1997, the mayor of Vienna honored my life's medical work with the city's "Golden Man of City Hall Award" in that Red Salon, with its red velvet paneling and beautiful chandeliers.)

The conduct of Soviet soldiers in Vienna was variable. I know of no official policy of mass brutality, as committed by German (primarily SS) troops in the Soviet Union. Eva's family (Eva then age 14), living in the third district, encountered a not-joyful liberation when the battle raged at the nearby Danube Canal. As part of the self-declared Czech minority, her family was linked with an anti-Nazi resistance group based in the nearby university church of Vienna's first district. (That university church is adjacent to Schubert's music school and the original university building, which had become home of the Austrian Academy of Sciences.) Soldiers engaged in the furor of war can not evaluate the political opinions of individual civilians. The Red Army conquered the third district, and when rumors about rape reached the street, Eva's family found shelter in the university church. Girls disguised themselves as old women. Neither

Eva's father's ability to speak Russian, nor her Czech family's anti-Nazi stand protected them against indiscriminate acts of total war. All Kyzivats luckily survived. Eva's brother Hans and her father were ordered by Russian soldiers during heavy artillery fire to build bridges which the SS troops had blown up. Hans later also became a U.S. citizen.

The Red Army brought us peace and the U.S. Army brought us freedom. During the first weeks of Soviet occupation young ladies felt insecure. We could house ("hide") female friends in my parents' apartment, which was "protected" by a "hands-off" label on the door, provided to some known anti-Nazis by Austria's resistance group. In August 1945, the troops of the Western Allies entered Vienna. U.S. troops were elegantly dressed and well supplied. We were awed when a huge water reservoir near our city home, which had been dug by hand by German slave laborers during the past year, was transformed by U.S. bulldozers into a parking lot within a few hours. The Austrian provinces and Vienna were divided into four sectors to be occupied for the next 10 years by American, British, French, and Russian forces. However, civilians with Austrian identification cards (kind of a passport approved by the occupation forces) could move freely throughout the country. The democratic leaders of the Second Austrian Republic had considerable independence — quite different from the situation in Germany.

Each survivor of World War II has his or her own unique story to tell. My family and I were very lucky. All members of my immediate family survived — thanks to the relatively rapid end of the Hitler regime after Patton's, Ridgeway's, and Montgomery's armies crossed the Rhine. Collegiality among physicians saved many lives. The medical profession was privileged in having had a better chance than other professions to uphold humanitarian principles while surviving themselves.

The bombing of civilians was started by Nazi Germany — in Spain (1936/37) and then in World War II, first of Warsaw, Rotterdam, Coventry, London, Moscow, and Leningrad. Retaliating Allies destroyed German and Japanese cities with mass bombing of civilian populations. I believe that the deliberate mass bombing of civilians in Germany was also immoral and lengthened rather than shortened the war. It made some Germans side with fanatic nationalists. World War II ended with Hiroshima and Nagasaki. In the fall of 1945 the world sighed "never again."

Many current writers take an oversimplified approach in summarizing the Nazi time. This, at first confusing and ultimately most murderous, epoch in the history of the Western World, should be analyzed in four phases:

1. Before 1933–hope. The National Socialist (Nazi) movement and party in Germany, although outlawed, gained the popular support of economically deprived people. Germany, led by the democratic Weimar Republic, and remnant Austria, led by a pseudo-democratic government, would have endured, had World War I not ended with the harsh, unfair treaties of Versailles and St. Germain.

2. 1933-39–dictatorship in peacetime. This included the beginning of harassment and humiliation of Jews and other "opponents," but economic prosperity for the majority, the uniting of most German-speaking people in one country, and propaganda about keeping peace. Unless you were employed in war-related industry, you did not realize that much of the prosperity was due to armament. There was no sign of a holocaust. Some enthusiasm for the regime by the majority (particularly the young, unwise, or uninformed, like myself) was natural and understandable.

3. 1939-42–dictatorship at victorious warfare. In this phase, the reactions of people separated the stupid or fanatic from the humanists. Aggressions and invasions for "widened Lebensraum" (space for Germans to live) through the conquests of Poland, Luxembourg, Belgium, Holland, France, Denmark, Norway, the Balkans, and the vast spaces in the Soviet Union made many Germans jubilant, not considering the killing and

maiming of people on all sides. Atrocities occurred in secrecy, unknown to the public, but some regular soldiers at the fronts saw evidence of it. A minority (my family) considered any war of conquest as mass murder.

4. 1942-45–defeat. The war was lost, but German generals continued the killings on all fronts. Mass murders were committed at many fronts not only by the SS. Those in concentration camps (the holocaust) were still secret, but suspected by some. Most Germans and Austrians felt betrayed and victimized by their own leaders. Few remained stupidly fanatic until the end.

Only those who personally experienced a brutal dictatorial tyranny can fully understand what happened. Not until long after the war did I ponder how I might have felt if, in the fall of 1939, my father had turned south and made our family become emigrants. How would I have felt as a British or American soldier, ordered to shoot at Austrian or German conscripts, perhaps some of my own classmates?

After the war, we came to appreciate the enormous sacrifice of Allied soldiers, many of whom lost their lives so that we could survive. In contrast, the majority of conscripted Austrian and German boys of my generation eventually considered themselves victims of their country's leadership. Most Allied soldiers believed that they had fought for a noble cause — saving democracy and humanism.

Austria After the Nazis, 1945-49

The war ended for Vienna in April and for Germany in May 1945. In Vienna most uninjured survivors were jubilant. Spirited citizenry, immediate American help, and the Marshall Plan in 1948-53 helped to rebuild Austria and avoid the economic collapse and starvation that occurred after World War I.

In spring 1945, a free republic was born in eastern Austria, under occupation by the Red Army. There are several likely reasons why the Soviets did not impose a communist dictatorship in Austria as they did in the late 1940s in Hungary and Czechoslovakia: A coalition of conservative Catholic and Social Democratic leaders of prewar Austria, when released from Nazi prison, immediately formed a functioning free Austrian government in Vienna. Austria's new political leaders established good working relations with all four occupation forces. The Communist Party of Austria remained very small owing to a domineering Social Democratic Party tradition. Finally, perhaps the Soviets wanted Austria to remain neutral as a buffer zone between East and West, similar to Switzerland.

Right after the liberation, after only one or two months interruption, medical school activities resumed. I continued my intense medical studies toward graduation from the University of Vienna Medical School on March 19, 1948. Hermann Chiari was professor and chairman of pathology. His daily lectures for third- and fourth-year medical students were actually a comprehensive review of pathophysiology and therapeutics of the entire organism. I fondly remember my final pathology exam — an experience each student endured for two days. I had to conduct an almost-complete autopsy and identify numerous histologic and bacteriologic slides. My other key teachers or examiners I recognized in another publication.[28]

Right after liberation, my father, reduced to cachectic appearance from weeks of malnutrition, was reinstated as a professor and became acting chairman of the ophthalmology Klinik #1 in 1945/46. Soon thereafter he accepted the chairmanship of ophthalmology at the large city hospital of Vienna in Lainz, until he retired at age 70. In 1945 my father needed retinal detachment operations on himself. Others, including myself, needing minor operations or other health care, experienced shortages of supplies and drugs until the Marshall Plan came to the rescue of Europe. Lethal infectious diseases took their tolls even among the young and fit, as I experienced during a medical student clinical clerkship in internal medicine.

During these postwar years in the medical school, I focused on clinical courses that required very little hands-on patient work. I also indulged in hiking, mountain climbing on rock and ice, travel, and music making. My social life was rich with mixed circles of young people, survivors of World War II. Though Vienna was still severely damaged, concerts, theaters, and balls for waltzing were reopened already in summer and autumn of 1945. Living, loving, and learning exploded. CARE packages from America helped reverse previous semistarvation. The packaged American soft white bread we ate like delicious desserts. We had not seen white bread for six years.

In my personal life, in February 1947, I met 17-year-old Eva Kyzivat, my future wife, at a house ball. It was romantic love at first sight. Eva and I married in 1950.

After many tough exams, almost all of them oral, I wanted to become a surgeon. Radiology Professor Zdansky (an anti-Nazi friend of my parents, who later became a distinguished professor in Basel, Switzerland), already had influenced me during the war with "the crown of diagnosis is roentgenology, and that of therapy, surgery."

Upon the advice of my father, my "uncle" and forensic pathologist Werkgartner, and my professors in surgery, I joined the imaginative pathologist Professor Friedrich Feyrter (from March through December 1948) to learn from him gross and histologic pathology and principles of research. At that time, he had been released as chairman of pathology in Graz, as he had been a member of the Nazi Party. He was, however, a decent man with high academic principles. We never talked politics. In 1948, he was the pathologist in a university-affiliated community hospital in Vienna, the Hanusch Krankenhaus. Under his intense coaching, I helped him histologically examine numerous clinical specimens. He taught me the importance of honesty and accuracy in reporting observations (at autopsies), to have clinicians present at autopsies, and to show them where they erred, to teach, not to blame. (*Errare humanum est*: erring is human.) While some surgical errors had tragic results, lawsuits in medicine did not exist at that time and years to come.

Feyrter coached me in completing two research papers. He was actually the one who wrote them, as he dictated the text to me during many sessions in his apartment. His devoted wife and histology laboratory technician, whom he treated somewhat like a servant, in 19th century style, served black coffee beyond the limit of tolerance. Feyrter insisted that I be the sole author of two papers: One project described his silver staining of cells in the gastric mucosa, which he thought had endocrine function.[30] The other concerned oncology.[31] We reported that in specimens of intestinal polyposis, those polyps that showed malignant degeneration of the epithelium also had neuromas of autonomic nerve fibers in their bases. We thought that this suggested a neurogenic component in carcinogenesis. He made me present one of these studies at a pathology meeting in Vienna. This first scientific presentation of mine was with 3x4-inch glass slides and a gas-lamp projector. I believed that my talk was a disaster, because I was terribly shy and speechless. Hesitatingly, I barely got through it. Feyrter's beautiful female associate cheered me on. (My shyness did not change until — after Yale and Penn — I learned in Peru to present knowledge with confidence.)

After Feyrter, I became a surgical intern at the Surgery Klinik #1, under Schoenbauer, from January to September 1949, and again, after the academic year 1949/50 at Yale, from July to September 1950. In October 1950, I began in America with an anesthesiology residency in Philadelphia.

For some survivors of the war, the scars on body and spirit remained for life. My early postwar life was clouded by revelations of worse-than-suspected atrocities, particularly in concentration camps, by the knowledge that some of those jailed would not come back, and by futile waiting for my gymnasium classmates to return from the front or war prison. The majority of my gymnasium classmates who were killed in the war (16, i.e., one-third of my class) were not active in the Nazi movement, certainly not after our graduation in 1942. I will summarize some of their fates in personal memoirs. Up to 80% of some classes of older boys in Germany and Austria who had graduated earlier, in the late 1930s, never returned from the war. Rudolph Weichinger became U.S. prisoner of war in Kentucky. Peter Boehler, medical student, Army officer and son of orthopedic trauma surgery pioneer Boehler, was killed at the front south of Vienna on April 12, 1945. My boyhood friend Willi Hohm was permanently blinded by a sharpshooter on the eastern front during the last week of the war. He became an internationally famous leader in sports by the handicapped.

What about the "Nazi doctors"? Professor Pernkopf was chairman of anatomy during my studies in 1943-44, but I never met him. In 1938, when the authorities appointed him dean and rector (chancellor) of the university, he revealed himself as a committed Nazi; he immediately fired Jewish colleagues. He created a famous textbook of topographic anatomy, with unique illustrations, which enchanted physicians around the world, including the U.S., after the war. Only recently did I learn that some of the anatomic specimens used to illustrate Pernkopf's book — and for which some of us medical students had been asked to dissect (under the direction of a fine, seemingly anti-Nazi polio-crippled anatomist Ehmann) — came from executed political prisoners in Vienna. Students did not think this through; capital punishment had existed in Austria since before the Nazis. Bodies of executed "criminals" were often provided for anatomy-class dissection.

It became known long after 1945 that a few Nazi doctors had committed terrible crimes. Some doctors had participated secretly, even before the war, in involuntary euthanasia of brain-damaged children. Some had experimented on political prisoners, sometimes leading to the subject's death. Neither my father nor I knew about that at the time. This came into the open in Vienna in the summer of 1945, when Professor Eppinger committed suicide. This internationally famous hepatologist, then chairman of the Internal Medicine Klinik #1, who had been a committed Nazi, was discovered to have been involved in some of the lethal experiments on prisoners. In Pittsburgh in the 1980s, I received copies of documents from these crimes. The documents, hidden by the Nazis, had been discovered by Allied troops at the end of the war. These documents came from Constanze Smith of Washington, DC, formerly a curator at the New York Academy of Medicine, where these documents had been taken. She thought that our Resuscitation Research Center in Pittsburgh could determine whether anything could be learned for future patients, particularly regarding hypothermia. This "gift" inevitably posed ethical dilemmas: whether to reveal evidence about such ghastly deeds at all, and weighing the memory of the victims against potential benefits to future patients. I found nothing in these experiments to illuminate contemporary resuscitation medicine. They were not only unethical, criminal, and ghastly, but also stupidly done from a scientific perspective. Had they been conducted on animals, reporting their results would have offered us now no new knowledge.

What about anesthesia in Vienna?

Before, during, and for a few years after the end of World War II, the majority of surgical procedures in Austria were conducted under skillfully applied local infiltration or nerve-block anesthesia, using Novocain (procaine). Its short action necessitated rapid surgery or intraoperative re-injection, all performed by the surgeon. Rarely did a nurse stay at the patient's head to talk to the patient and to monitor vital signs. Blood-pressure monitoring by cuff method had been known since Riva Rocci, but was applied more in medicine than in surgery, except for neurosurgery.

We had received some Red Army soldiers for surgical treatment during the Battle of Vienna. Their officers insisted on regional anesthesia because they refused to let them become unconscious. I remember, for example, appendectomies done under local infiltration anesthesia with the abdomen turned 45 degrees to the left, so that the appendix could be accessed without relaxation.

While I was a surgeon's assistant in the spring of 1945, mostly in the bunker operating room of Surgery Klinik #1, we treated both civilian and military trauma victims. Many died of wound sepsis. Penicillin became available that summer — next to food, the greatest gift from the Allies. In 1948, as surgical interns, we had to inject intramuscular penicillin twice a day into many patients. Nurses were not allowed to give injections. These "penicillin injection rounds" took hours.

I was among the last medical students to have the privilege of assisting the legendary abdominal surgeon Finsterer. He performed major laparotomies under elegantly conducted regional anesthesia. This consisted of a field block of the abdominal wall plus the anterior celiac plexus block (after laparotomy), which he had introduced using large volumes of dilute Novocain, infiltrated along his finger, which was inserted between the aorta and vena cava. The patient was conscious, just sedated with morphine as needed. (An alternative to Finsterer's intra-operative celiac block from the front was Kappis's pre-operative celiac block from the back.) Finsterer was a kind, artistic, and religious man, an outspoken anti-Nazi who lost his son in the war.

General anesthesia was avoided as much as possible until 1945. The value of premedication with morphine and/or pentobarbital before general anesthesia was appreciated, as was the absolutely essential drying agent, atropine or scopolamine, before the use of ether. General anesthesia consisted of dropping ether on a gauze mask from the start (slow and with a wild excitement stage), or, more elegantly (and fast), ethyl chloride from a pressurized glass vial, followed by ether. Untrained part-time anesthetists (medical students like myself, orderlies, nurses, or surgical assistants, i.e., young doctors), were "self-trained" on the job. They picked up various manual tricks to provide an open air passage and to avoid movements or apnea. I observed and used jaw thrust (the Esmarch-Heiberg maneuver). Although some instinctively practiced backward tilt of the head, which was simpler, this technique was not appreciated. I do not remember having seen a pharyngeal tube.

Endotracheal anesthesia was not practiced in Vienna before 1945. I did not see the first anesthesia machine and intubation until after the end of World War II in 1946. Before then, there was no oxygen, no breathing bags, bellows, or ventilators. An electric suction machine was only occasionally available and usually weak. Under induction with open-drop ether, excitement was prolonged, common, and violent. Vomiting and aspiration during induction and emergence were common, and not infrequently led to postoperative death from "pneumonia." Although open-chest cardiopulmonary resuscitation (CPR) occasionally had been practiced in ORs of other institutions throughout Europe since the turn of the century, I never saw this procedure during my years at the university hospital of Vienna in the 1940s.

Asepsis was rigorous, since there were no antibiotics except sulfonamides, which were only partially effective in some cases. For infected tissues, "septic surgery," i.e., draining pus, was our daily routine. There were no blood transfusions, except for the occasional small-volume person-to-person transfusion of type O universal-donor blood, using rubber tubes, steel needles, and clamps. With such small-volume blood transfusions, a person in what is now understood to be hypovolemic shock occasionally rallied and survived.

During the war, the Allies had blood banking. On the German side, volume replacement apparently was poorly understood. A German-made synthetic colloid, polyvinylpyrrolidone (PVP), was sometimes used and was effective. Intravenous infusions of isotonic saline for blood loss were used rarely and not in large volumes. Blood banking was learned from the Western forces after World War II. After the war, we were also astonished to learn about the development, since the 1930s, of modern endotracheal anesthesia in Britain and the United States. The Allied forces used N_2O-O_2-ether anesthesia delivered from various machines. They used uncuffed rubber tubes. Leakage was sometimes blocked by pharyngeal packing.

Schoenbauer could perform a gastrectomy in 45 minutes, under local anesthesia. Through a prewar visit to the United States — with Walter Dandy, Sr. (of Johns Hopkins, who died in the 1940s) and influenced by Harvey Cushing (of the Johns Hopkins, Harvard, and Yale medical schools, who died in 1939) — Schoenbauer had become the first neurosurgeon in Austria. He and his assistant Kraus did almost all cranial operations under local infiltration anesthesia, believing that ether causes brain swelling. Kraus was gentle to brain tissue. There was no tracheal intubation. As a medical student or intern in the operating room, predominantly assigned to neurosurgery, I was required to "monitor" some of their patients under the drapes, and was frightened when patients in the suboccipital prone position sometimes lost consciousness and began snoring. I then had little skill and understanding to do anything about it (except to push the jaw forward). Diagnosing was aided by air-encephalography or ventriculography. One assistant treated brain swelling with intermittent ventricular punctures. It was not appreciated that (partial) upper airway obstruction in coma, not the general anesthetic per se, causes brain swelling. Did Cushing appreciate this? He brought the Riva-Rocci blood pressure cuff method from Europe to the U.S.; introduced a recovery room where blood pressure, heart rate, and breathing were monitored and recorded on the postanesthesia record; and detected that increased intracranial pressure causes hypertension and bradycardia. Walter Dandy, Sr., the subsequent U.S. giant of neurosurgery, considered tracheotomy for coma important; he introduced what we now would call a small specialty nursing ICU at Johns Hopkins.

After the war, as a surgical intern, I experienced Schoenbauer's endless so-called "visits" (the equivalent of American "ward rounds"). All white-coated assistants and students, in rank order, had to follow Schoenbauer, forming a long snake through several huge, open wards with 30 beds per room, lined up in a row a city block long. Nothing of educational value came from these rounds. I was dedicated to the bedside, and being a rebellious youngster, I stayed with my patients when this snake of rounding doctors approached. The first time I did this, Schoenbauer's stare said "How dare you!," but he said nothing. I believe that, to his credit, he accepted my misbehavior and ultimately even respected me for it (however silently).

Markus, an assistant professor, was a highly motivated and skilled surgeon in charge of the emergency department (ED) and traumatology in the Surgery Klinik #1. This unit included operating rooms and a short-term ward. He had a peculiar mix of patients, their problems ranging from barbiturate overdose to hip fractures. I learned much from Markus, particularly his approach to patients. We managed fractures by direct bone setting, using our bare hands under the fluoroscope. His unit and the designated trauma hospitals of Boehler were unique features in Austria. I later realized that such EDs, including those of so-called "trauma centers," should be part of an advanced general hospital program with all subspecialties available. Boehler hospitals now include all supporting departments. Although they also treat minor injuries, critical care efforts needed for the severe polytrauma patients are now advanced.

Denk was chairman of the Surgery Klinik #2, and an internationally recognized thoracic surgeon. Before Otto (Teddy) Mayrhofer (see later) introduced modern (endotracheal) anesthesia to Austria, Denk mostly operated on the thorax for patients requiring thoracoplastics under local infiltration anesthesia and spontaneous breathing of oxygen via nasopharyngeal catheter. Lung collapse was prevented by a scarred pleura (due to tuberculosis) or it was tolerated because of intentional pneumothorax two days before the operation. If the chest was opened inadvertently and the lung collapsed, spontaneous positive-pressure breathing via mask could be improvised. With Mayrhofer's arrival, all this changed. Thanks to Mayrhofer, the Surgery Klinik

#2 under Denk acquired modern anesthesia before the Surgery Klinik #1 under Schoenbauer.

At the Surgery Klinik #1, soon after 1945, Schoenbauer invited Kurz, a visiting American anesthesiologist, to demonstrate intratracheal anesthesia with an anesthesia machine Kurz had brought along. This first modern anesthesia demonstration that I saw, for an esophagectomy, did not impress the surgeons. Schoenbauer accepted general endotracheal anesthesia by machine only later, when his assistants Rudolf Kucher and Karl Steinbereithner, initially influenced by Kurz, devoted themselves full-time to anesthesiology. Mayrhofer, Kucher, and Steinbereithner became copioneers of anesthesiology in Austria and beyond.

Strongly supported by Denk, Mayrhofer quickly convinced his chief of the importance of modern anesthesia to enable advances in surgery. In the summer of 1947, Stuart Cullen of Iowa, who was doing early work with curare, visited Vienna's Surgery Klinik #2, accompanied by surgeon Brunswick. I did not meet them. In 1947/48, Mayrhofer visited several anesthesia departments in England and Scotland. When he returned to Vienna in 1948, I heard him lecture on oxygen therapy at the Billroth house of Vienna's Medical Society. He was ridiculed: "Why is that needed; is 21% oxygen in air not good enough?" In 1948, Mayrhofer gave the first endotracheal anesthesia for a lobectomy, with ether, without cautery.

Mayrhofer was not deterred, and he assumed leadership with vision and vigor. In 1949/50, when I was at Yale in surgery, Mayrhofer was in New York City for 16 months to study anesthesiology with Emanuel Papper and Virginia Apgar at the Columbia Presbyterian Hospital. Mayrhofer and I met there. He returned to Vienna in November 1950, to begin the development of an academic anesthesiology department, which trained more than 100 anesthesiologists during his tenure from 1950 to 1992. When Mayrhofer became chief anesthesiologist in Vienna in 1950, I was starting my residency with Robert Dripps in Philadelphia. In 1951, Mayrhofer was the first in the world to introduce succinylcholine into anesthesia practice. He became the uniquely successful anesthesiology department chairman at the University of Vienna, an organizational pioneer of our specialty in Europe. In Austria's anesthesiology, Mayrhofer of Vienna and Haid of Innsbruck (trained in Iowa) were followed by Feurstein of Salzburg, Bergmann of Linz, and others. Mayrhofer and I maintained collegial contact. Then Eva and I developed a friendship with him and his wife Elly — lasting to this day.

Thiopental (pentothal) was introduced in the United States during World War II, starting with its disastrous use after the attack on Pearl Harbor. Doses usually used for normovolemic patients, when given to bled-out victims, stopped breathing and the heart. Thiopental was introduced in Austria and Germany only after 1945, having been preceded by another not-quite-as-short-acting barbiturate, Evipal. Before there was an anesthesia machine at our Klinik, probably in 1946, knowing nothing about Evipal, I was ordered to inject it intravenously into a patient undergoing an operation on the anus. He promptly went into laryngospasm, apnea, and deep cyanosis. Luckily, hypoxia-induced relaxation of the larynx, my instinctively tilting the head back, and the patient's terminal gasps turned the tide, saving the patient (and my reputation).

Before 1949 spinal anesthesia was used rarely, and primarily for urologic procedures. Hypotension was not recognized as a cause of sudden unconsciousness, which was assumed to result from the anesthetic rising to the level of the brain. Nupercaine and pontocaine already were available for longer-lasting spinal anesthesias than was possible with procaine (Novocain). Only later in the U.S. did I learn to make spinal anesthesia heavy and controllable with dextrose.

Cardiovascular emergency drugs also were used with misconceptions. Cardiazol, actually an analeptic, was assumed to be a cardiac stimulant and used for patients whose pulse and consciousness had faded away. Soon after the end of the war, I underwent an operation on my nasal turbinates. Much cocaine was applied topically. When I passed out (probably from a cocaine overdose, since I felt so happy going in and out of coma) I was given intramuscular Cardiazol. What brought me back was a correct instinct by my otolaryngologist to quickly put me horizontal. The surgeon, Alfred Poelz, said that he had to revive me; he had a superbly trained baritone I accompanied at usual "Schubertiades" at the Safar home.

As a medical student during and after the battle of Vienna, I also experienced nonsurgical catastrophes. Infectious diseases were rampant, and resistance was weakened by starvation, the civilian population of Vienna having lived for years on subviable rations. Only those who had contacts among farmers could obtain (semilegally) supplementation. As a clinical clerk on the medical ward of the Internal Medicine Klinik chaired by Jagic, I found several huge wards full of patients with severe acute infectious diseases. The diagnoses included abdominal typhoid, typhus, lethal dysentery, fulminating tuberculosis, and of course pneumonias of all kinds. For acutely dying young patients with what seemed to be abdominal typhoid, we were ordered to give intravenous hypertonic glucose injections (not infusions). The stated rationale was to spare the gut and reduce cerebral edema. There was no appreciation then of the need for massive intravenous fluid replacement, which could have saved many patients, even before the availability of penicillin in 1945.

A unique, somewhat positive feature of Viennese medicine was the waterbed. As student and intern, my responsibilities included helping to sustain patients in the dermatology wards who had severe extensive burns, open wounds, or body-covering skin diseases, by floating them in water to prevent decubital ulcers. The majority of these miserable patients were more comfortable in the water than they would have been in regular beds, but most of them died in the waterbed. In the regular or waterbed wards full of severely infected patients, we young doctors acted as if we were immune and indestructible. Handwashing was rare.

Soap was scarce. Gloves were used only for aseptic surgery. With one bare hand I would eat a sandwich, hastily prepared by the always-caring nurses, while with the other hand I was helping a dying, infectious patient.

In summary, the first 25 years of my life in and around Vienna were formative, confusing, threatening, and historic. They made me become a disappointed Central European who sought medical and general experiences in America. This opportunity presented itself through a chance to study surgery at Yale in 1949/50.

On the nonmedical scene, although representatives of the four occupying powers argued among themselves concerning Austria's future, a state treaty was signed in 1955. Austria was positioned again at the crossroads between East and West, officially neutral during the cold war and now, since the 1990s, a member of the European Union. This small central European country of now 8 million people survived two world wars and developed friendships with its neighbors, who for centuries before 1918 had belonged to the same empire of 50 million people in which 13 languages were spoken. Austria's history spans 1000 years, including the "discovery" and conquest of America by the Spaniards, who in the 15th and 16th centuries belonged to the Hapsburg empire.

NEW HAVEN
September 1949 – June 1950

Why did I become an American?

For at least six reasons: 1. I wanted to learn surgery and anesthesiology in the U.S., where around 1950 these disciplines were more advanced than in postwar Europe. 2. I was a disappointed European, sick of the still-present remnants of "-isms" (Nazism, communism, etc.), and sick of the still-prevailing dictatorial roles played by professors in Europe in general and in Austria in particular (the "Geheimrat System"). 3. Instinctively, I wanted to go to America because my father had missed the opportunity in 1939. 4. Although I deeply loved my parents and appreciated my European roots, I wanted to create a life and work of my own. 5. Around 1950, some Austrians feared that Stalin's Soviet Union might take over Austria. 6. I wanted to become a world citizen, felt restricted in small Austria, and was curious about America. After arrival in America, I became increasingly enthusiastic about Jeffersonian democracy enabling change. I soon felt, however, that when things are going wrong, it is up to each individual to attempt to change them.

I did not emigrate for economic reasons. I would have had an economically more comfortable life and easier career in postwar Austria. My wife Eva and I are proud of having built in America everything ourselves from scratch — our modest possessions, lives, careers, and family. After our start-up years some family heirlooms were welcome additions.

In Vienna, after a short year in pathology research in 1948, I started training in academic surgery as an intern in 1949, at the surgery Klinik (department) #1, chaired by Schoenbauer. There was an Austro-American Club near the hospital. The U.S. Ambassador to Austria, Dowling, and his family were kind hosts who dealt with young Austrians very informally, socializing and advising about scholarships. I applied for and received a fellowship in surgery at Yale for the academic year 1949-50. It counted as an internship in the U.S. I was accepted for this position because my professors Feyrter and Schoenbauer in Vienna wrote letters of recommendation, and my ex-Viennese friend Henry Hugerth, who then lived in Guilford, Connecticut (near New Haven), "dropped in" on the powerful chairman of surgery (emeritus) at Yale, Sam Harvey, and recommended me. After my arrival I could select a surgical field on which to focus my work; I selected oncology.

Henry Hugerth, considered Jewish by the Nazis, I had known of through friends, but did not meet until the summer of 1945, when he returned to Vienna as a US Army officer.[32] In the spring of 1938, he was a Viennese law student, and was sponsored (by chance) for immigration to the U.S. When the U.S. entered the war, he volunteered and became an American front-line officer in North Africa and Italy and a legal advisor for General Clark. When Henry entered Vienna in the summer of 1945 his suspicion was confirmed: His parents and his younger brother, Peter, who had escaped to Czechoslovakia in 1938, were murdered by the Nazis. In spite of this, Henry helped impoverished postwar Austrians by supplying still-scarce food and other amenities. He helped even some people who had been Nazi sympathizers. I met Henry through a mutual Viennese lady friend, Lilo Hunna, in the summer of 1945. In 1946, Henry finished law school in Vienna — as an American officer — but he never used this education professionally in the U.S. When demobilized in 1948, he returned to a modest existence in Guilford, Connecticut, pursuing various businesses for survival. He did more for others than for himself. In 1947, in Vienna, I detected in him a transient intercostal neuropathy, apparently triggered by trench warfare in Italy. In 1953 in New Haven, he was diagnosed with multiple sclerosis. In 1954, he married a lovely, smart American lady, Dorothy (who suffered from another chronic illness). They had 10 good years together in Boston. Then the disease crippled him severely and killed him in 1968. Henry Hugerth was a wise philosopher with a great, almost saint-like, loving personality and a brilliant mind — he was one of my best friends, the one who introduced me to America.

It seemed impossible for an Austrian to immigrate to America right after World War II, because a small fixed quota for Austrians seeking immigration was oversubscribed. I came to Yale on a temporary student's visa and had to return to Vienna for the summer of 1950. In October 1950, I returned to the U.S. (to the University of Pennsylvania), again on a student's visa. My intention was to study and later decide whether to return to Europe, but most likely to stay in America for good, if possible.

The currency of the new democracy of Austria (the schilling) was not internationally exchangeable right after the war. Outside of Austria I was therefore a pauper and needed a sponsor. I had applied successfully for support at the Vienna office of the Institute of International Education, a nongovernmental agency based in New York City. They provided free travel to New York and back to Vienna and a stipend of about $30/month (about $300/month in 1990s values).

On September 5, 1949, I said goodbye to family and friends in Vienna and kissed my girlfriend, Eva, goodbye. The cold war was already on. At the Enns River, which divided the Russian zone from the American zone of Austria, I and a few other (nonmedical) Viennese students with Austrian passports and American students' visas expected problems, but we passed the Russian checkpoint without difficulty (to our surprise).

We traveled by train through Paris to Le Havre. There we and a few German students joined American students returning from their first exposure to postwar Europe. Their attitudes were so free, their flirtations and game-playing so different from ours. We crossed the Atlantic Ocean on an American liberty ship, hundreds of which had ferried troops during the war. The trip to New York took ten days. I fought seasickness by playing piano duos (even a Bruckner symphony) with a chance acquaintance, an American music student Andy Heath, who was returning to

Harvard from a European visit. (He later studied music in Vienna and stayed with my wife's family. He became a well-known musician in the United States.)

The first major differences that we students noticed between the two continents was when we saw land (the Long Island coast) by night: car headlights in unending sequence. On the morning of September 16, 1949, we saw the Statue of Liberty. I arrived with no money. Henry Hugerth picked me up at the dock in Manhattan with a tiny Crosley car. My first impressions: skyscrapers; the Merrit Parkway with rest stops that served huge concoctions of ice cream, fruit, and whipped cream; and warm-air hand dryers in toilets. Henry's house in the woods near Guilford was very simple but practical. The small houses of stone and wood in New England, some over 200 years old, were so different from Austria's baroque architecture.

My monthly stipend of $30 was not even enough for basic food since, even in the New Haven Hospital, I had to pay about fifty cents for breakfast and two dollars for dinner. I survived by doing occasional work, installing (for Henry) television antennas on steep New England rooftops (they are slippery in the winter) and translating papers at Yale. With Henry's house in Guilford too far from the New Haven Hospital, I was allowed to sleep, gratis, in a cell-like room in Yale's Cushing Library behind the hospital, right under the dome. That was an incentive to study original medical literature, in contrast to my Vienna experience with only textbooks and scripta (reviews) written by our professors.

I was surprised when professors at the New Haven Hospital asked medical students their opinions and encouraged disagreement. I participated in journal clubs with chairman of surgery emeritus Harvey. The new chairman of surgery, Gustav Lindskog, a famous thoracic surgeon, had lunch at the same table with me and other students in a greasy-spoon diner across the street from New Haven Hospital, run by a kind Italian-Greek woman, "Mrs. K." She felt so sorry for me that she gave me free meals at first and later charged only one dollar per day for lunch plus dinner.

In America, to be free, you must have a car. I felt rich because, right after my arrival, I got a $75 check from a colleague of my father in Florida, who apparently was repaying a prewar loan. With this I bought a very-used 1936 Plymouth, a black sedan. I named it the Flying Dutchman because the hood was loose; at high speeds (my usual way of travel), the car looked like it had wings. It burned almost as much oil as gasoline. A gas-station attendant who pitied me gave me free used oil, which he drained from new cars at the first oil exchange. The Flying Dutchman had no emergency parking brake, so I used a brick instead. Some windows were stuck open, so I wore a dilapidated fur coat that my father had worn in World War I. The car registration fee was $5. I already had an international driver's license from Vienna. There was no mandatory inspection or insurance. The car enabled me to commute between Hugerth's home in Guilford and New Haven and to make frequent trips to Boston and New York. Professor Harvey, amused (impressed) by my use of the Flying Dutchman, gave me as Christmas gift a humorous book about the taming of an old Ford Model T.

Anesthesia at New Haven Hospital was administered by nurse anesthetists under the guidance of chief anesthesiologist Louis Hampton. I observed operations and anesthesias almost daily. General anesthesia was much smoother, more elegant, seemingly safer, and in every respect more advanced than in Vienna. At that time, general anesthesia in New Haven usually was induced with 0.5-2.0 grams of intravenous thiopental, without curare, for tracheal intubation with uncuffed rubber tubes. Maintenance was with nitrous oxide-oxygen, reinforced by ether, or intermittent thiopental with spontaneous breathing. When the bag moved too shallowly, it was squeezed gently. Surgical anesthesia seemed smooth, although patients slept long after operations.

This anesthesia experience was new for the Viennese, who had left an environment of local anesthesias, open-drop ether, and fast operations. The anesthesia machines used at Yale, and later at Penn, were mostly Ohio-Heidbrinck type (World War II army units) or Foregger type with water-depression gas-flow meters and closed or semiclosed circle systems with CO_2 absorption canisters. For ether vaporization, in-circuit draw-over vaporizers had wicks, and out-of-circuit vaporizers bubbled part of the O_2/N_2O flow through ether.

Hampton told me that anesthesiologist Ralph Tovell, at nearby Hartford Hospital, was promoting specialty recognition for physicians as anesthesiologists. (Tovell had played a leadership role in training military anesthesiologists in World War II.) Anesthesiology services at Yale were administratively under surgery. Not until many years later did I find out that even anesthesiology chairmen of greatest academic fame, such as Dripps in Philadelphia and Cullen in Iowa, were directors of divisions of surgery. Trained anesthesiologists seemed to be needed everywhere.

Harvey and Lindskog assigned me to Max Taffel, an assistant professor of general surgery. He did extremely careful cancer resections. He was kind, a great mentor and a fine scholar. Neurosurgeon German performed the most elegant brain surgery. Both operated with delicate tissue-saving techniques and hemostasis, taking many hours, since they were not restricted by limited anesthesia time. I was also assigned to see outpatients in the oncology clinic, which included doing minor surgery. That experience taught me that the more practice-oriented medical school training in the U.S. had advantages over the schooling I had received in Vienna, where, even as interns, we had little opportunity to acquire practical experience. There was no practical guidance in Vienna, although we had superb, scholarly lectures.

I was surprised at the way American patients were told about and accepted their cancer diagnoses and often poor prognoses. In Europe, it was then customary to be secretive about a dismal prognosis so as not to dash all hope. At Yale I also saw, for the first time, chemotherapy applied with nitrogen mustard. I learned about oncology animal models. Chairman of Pathology Harry Greene implanted and observed tumor tissue in an imaginative culture medium — the anterior chamber of the guinea pig eye. I learned about pulmonary collateral respiration in dogs, as investigated by scientist Liebow, who invited me to his home on my first Christmas in America. I was invited to the home of Redlich, professor and chairman of psychiatry, a former Viennese psychiatrist, a disciple of Sigmund Freud. Redlich copioneered electroencephalography. Former Viennese ophthalmologist Ilona DeSuto-Nagy and her husband, a Yale biochemist, invited me often for meals. In Vienna, she had learned psychiatry from Sigmund Freud and ophthalmology from Karl Safar.

At the New Haven Hospital I ran into a German-speaking Yale medical student, Karel Absolon, then a U.S. immigrant from Brno (Bruenn) Czechoslovakia. Karel became professor of surgery, researched early coronary artery surgery and liver transplantation, and became a medical historian. He wrote *the* biography of Theodor Billroth (1829-1894) of Vienna.

At Yale I did two research projects: During nights and weekends I conducted a laboratory rat project, initiated and guided by Taffel, to determine whether cortisone reduces wound healing. I measured stomach wound tensile strength (by rupture pressure). The results were suggestive but not conclusive and therefore remained unpublished. Taffel, a purist, did not want to clutter the literature: "Only important results should be published."

My second study concerned cancer of the stomach. It was a review of patient charts, suggested by Taffel. It was later written up under the guidance of Assistant Professor of Surgery Cliffton.[33] This project required many nights and weekends in the hospital's record room. My findings, which showed that the prevailing pessimism about stomach cancer was not totally justified, were well received when I presented them in broken English at Yale.

I took car trips through the northeastern United States, where I visited other hospitals. I thus learned that smooth, physiologic general anesthesia for long operations was not unique to Yale. In New York, my family's ex-Viennese friends Fred Blodi (an ophthalmologist) and Paul Freud (a pediatrician, no relation to Sigmund) and their wives hosted me frequently.

Fred Blodi was then a fellow at Columbia Presbyterian Hospital in New York City. In 1953, he became chairman of America's then-leading ophthalmology department at the University of Iowa.[34] Fred, who was a school friend of Hugerth in Vienna, was another chance survivor of the war in Vienna. His experiences included Nazi imprisonment for helping a deserter. He was motivated into ophthalmology by having assisted my father in operations. Upon liberation, Fred became a male "war bride." His childhood girlfriend, Otty, had immigrated to America long before the war with her mother (not for racial reasons). She returned to Vienna in 1945 as a U.S. soldier of the "WAC." She took Fred Blodi to the U.S. as an immigrant.

Paul Fuchsig, my surgeon role model in Vienna (assistant professor with Schoenbauer), briefly visited American hospitals, including Yale, on a Fulbright Scholarship. I drove him to Boston and we watched Francis Moore operate at the Peter Bent Brigham Hospital where he had just begun as a very young chairman of surgery. Fuchsig and I talked. He reinforced my impressions, saying that he would go into anesthesiology if he were my age. I had already decided that surgery could not advance without better life support, which I thought could and should be provided by anesthesiologists.

When my father, Karl Safar, came to the U.S. (by ship) in January 1950 as a Fulbright Visiting Professor, he gave invited lectures and some demonstrations of his eye operations for retinal detachment. I drove him with my unheated jalopy (it was cold without windows) to Boston, New York, Philadelphia, Pittsburgh, and Cincinnati. While he was with ophthalmologists, I met various surgeons and observed general surgery. On that trip

I decided for certain to become an anesthesiologist and was accepted at the Hospital of the University of Pennsylvania (HUP) (see Philadelphia).

Without the influences of Hampton and Fuchsig and without the obvious worldwide need for anesthesiologists, I might have become a surgeon. My friends believed that I would have been a skillful and compassionate surgeon. Retrospectively, however, surgery would not have given me the freedom, inspiration, time, and power base (i.e., a department chairmanship) needed to develop new programs. Such positions were then readily available to anesthesiologists, but rarely to surgeons. In 1950, I did not realize all these pluses (and frustrations to come) about anesthesiology.

During my bachelor year in Connecticut, I met several attractive young ladies, but they were too puritanical for a young Viennese man. Moreover, I missed Eva. She was young, beautiful, and mysterious. In 1949/50, our love was expressed in subtle ways by scribbled correspondence.

Honoring the promise to my sponsor, I returned to Vienna in June 1950. This was again a rough sea voyage of ten days, again with other international students, from New York's Fort Hamilton to Bremerhafen, Germany, and then by train to Vienna. My plan was to return to the United States in September 1950, with Eva, for a residency in anesthesiology with Dripps at Penn.

In summary, the year at Yale showed me the superiority at that time of the U.S. over Europe in the development of medical specialties; in the atmosphere of open dialogue in academic medical centers; in the tolerance of minorities (then still limited); in the appreciation of good hard work; and in the apparent ability of even common people to influence change.

On June 17, 1950, the day I arrived in Vienna, I immediately borrowed my father's little car and went to Leopoldsberg in the Vienna Woods for a romantic overlook of the city. Eva said she did not expect it when I asked her to join me in America. She later said she was ready to go to the moon with me. We gave our families little time for preparations; the wedding was on July 6, 1950. Just before the church wedding (with one of Eva's friends playing violin and the heavens unleashing thunder), Eva got a taste of her groom's workaholism; that morning I had agreed to do a radio interview about my experiences at Yale, which I gave after the church wedding and before the evening reception. In July 1950, Eva and I honeymooned briefly, driving with my father's little car through still war-damaged Italy—Lago di Garda, Florence, Rome, Monte Cassino, Amalfi, Capri, and back via Switzerland to Vienna. There, during August and September, I worked again in surgery, at Schoenbauer's department, as an unpaid volunteer intern. Professor Schoenbauer was pleased to learn that cancer treatment at Yale was not more effective than that in Vienna. He also was crushed, as most of us were then, about the news that war had again erupted, this time in Korea.

On September 26, 1950, with rumors (that proved false) that Stalin might annex Austria, Eva and I left Europe — for good. We boarded the train from Vienna to Luxenburg, where cheap flights for students were available to the United States. Eva found American students "so different." This was her first trip to America and our first of many transatlantic flights. Our plane was a modified U.S. bomber with four prop engines. We flew almost eight hours overnight to Iceland, then another nine hours to Newfoundland's Gander Airport, and then the next morning on to New York's Idlewild International Airport, which was then a collection of barracks. Flying low over colorful foliage in Canada and New England remains unforgettable.

PHILADELPHIA

October 1950-September 1952

In 1950, the leading academic department of anesthesiology in the U.S. was that chaired by Robert D. Dripps, at the Hospital of the University of Pennsylvania (HUP) in Philadelphia. How did I become one of about ten residents in that department? Dripps and his associates Eckenhoff, Vandam, Dumke, Lamont, Deming, and others had the most important, most formative influence on my professional life, along with (indirectly) some of their colleagues in other disciplines, such as surgeons Ravdin and Johnson and scientists Julius Comroe and Seymour Kety.

How I got to Penn is quite unconventional, different from the presently prevalent method of writing applications, submitting letters of recommendation, going for interviews, and waiting for a letter of appointment with details spelled out in a contract. There was no U.S.-wide matching plan. I grew up in an era when one trusted a man's word. It was not even essential to call for an appointment; one just dropped in.

During my fellowship in surgery at Yale, I came to the conclusion that surgery cannot advance without better anesthesia and perioperative life support. I decided to seek anesthesiology training in the U.S. from 1950 to 1952. The Board-required training then was two years, after one year of internship. (My short year of surgery at Yale counted as an internship in the U.S.) I have learned to favor rotating or "mixed" over straight internships. Louis Hampton, Chief Anesthesiologist at the New Haven Hospital, did not have a residency program. I did not know that published residency listings existed. I had heard only from casual conversations with surgeons at Yale about anesthesiology residency training programs under Dripps in Philadelphia, Papper in New York, and Adriani in New Orleans. Only much later did I hear about Beecher at Harvard and Rovenstine in New York's Bellevue Hospital. Waters in Madison, where Dripps trained, had retired. I was impressed by a rumor that Dripps's department was experimenting with intra-arterial transfusion.

While chauffeuring my father in my windowless 1936 Plymouth during his U.S. visit in January 1950, we were hosted in Philadelphia at the home of ophthalmologist George Spaeth, Sr. He did most of his surgery at the Graduate Hospital. On our last day in Philadelphia, I was ready to leave Spaeth's house by taxi for the Temple University Hospital to look at their anesthesiology program. This choice was because I had met some Temple University surgeons in Vienna the year before. The anesthesiology chairman at Temple then was Krumperman, whom I got to know only much later as a very fine colleague. I was already in the taxi when Spaeth ran out of his house, stopped me, and said, "Don't you know that *the* department of anesthesiology is the one under Dripps at Penn?" I, as an obedient ex-Viennese, told the taxi driver to go to the HUP on 34th Street. I dropped in unannounced, was interviewed by Dumke and briefly introduced to Dripps. They merely glanced at my two brief letters of recommendation from Vienna plus two new ones from Yale. They offered me, on the spot, a two-year residency, to start any time. We agreed that I would begin in October 1950. No letters of offer and acceptance were required. I did not ask about a stipend, night call, housing, or other amenities. Only after I started in the fall did I find out that the stipend would be $100 per month (purchasing power equivalent to about $1,000 in the 1990s).

When Eva and I arrived on October 1, 1950 at New York's Idewild (now J.F. Kennedy) Airport, we had five dollars and four suitcases. We were greeted by ex-Viennese pediatrician Paul Freud and his wife in New York and by Henry Hugerth in New Haven. We needed a "new" car. My Flying Dutchman of the previous year was a wreck, parked at Henry Hugerth's house in Guilford, Connecticut. I had to pay five dollars to have it hauled away. In Guilford, we picked up the $300 that my father and I had saved the year before and bought a 1937 Plymouth sedan (again black) for $250. We called it "Black Magic" because it was magic if the car would start. It had windows and even a radio. We kept that car until it was 24 years old and, even then, passed it on as a "gift" to a medical student at Johns Hopkins.

On October 9, 1950, we drove to Philadelphia, to the 34th Street entrance of the HUP. I left Eva waiting in the parking lot "merely to check in at the hospital." My adrenaline level was high, however, and I forgot all about her while enthusiastically attending a breaking-in session in the operating room.

For the first few days we lived in a cheap, rented attic room at 124 South 36th Street. I gave Eva coins for phone calls to find a job. As the only white girl in line at an employment office, she was mistakenly assumed to be looking for a maid. Instead, she applied for a housecleaning job, which she promptly got with a nice Jewish family in west Philadelphia, the Jacobsons. Her earnings of $5 per day, added to the $100 per month I earned, made us feel so rich! We moved to a $50-per-month room on Walton Street in a lower-middle-class district of West Philadelphia. We remained fit on bananas, bread, occasional cheese, raw bacon sandwiches (before saturated fat was declared dangerous), and five-cent ice cream cones. Eva, at age 20, did not express culture shock and shared my adventurism, despite homesickness. She was a great companion.

During our second year in Philadelphia, we lived with Hugh and Ruth O'Neill in their little house in suburban Gladwyn. In return, Eva took care of Ruth, who was confined to bed with a difficult first pregnancy. Hugh was then a medical student at Penn and later became a fine psychiatrist in Philadelphia. This housing was arranged by Walter Dandy, Jr. and his wife Anne-Allen. Walter was one of my co-residents with Dripps. He was a fine physician and skillful anesthesia resident. The Dandys wanted to help us by finding a job for Eva. Walter's father, the late Walter Dandy, Sr., was the world-famous pioneer of neurosurgery at Johns Hopkins. The Dandys and the O'Neills became our lifelong best friends in America (see Baltimore).

On my first day at the HUP, I met Joseph Marcy, who had just finished his residency with Dripps. Little did I know then that 11 years later he would become my department's first professor, as chief anesthesiologist at the Children's Hospital of Pittsburgh.

The Department of Anesthesiology at the HUP in 1950-52 was a legendary environment. None of us residents complained about hard work. There were no nurse anesthetists. Night call had to be shared by one older and one younger resident to provide in-house coverage at all times. Staff-faculty took call from home but came in if needed. I had to take in-house call about one weekend per month and two nights per week. The overall workload for the department to cover was smaller than now.

From the 1950s through the 1970s, the department at Penn produced the largest number of anesthesiology professors and chairpersons of any department in the U.S. Dripps and his full-time faculty members were the best role models for young physicians who wanted to acquire the techniques and capabilities of a clinician-scholar (teacher)-investigator-leader, all in one. The faculty-staff members personally demonstrated anesthesia procedures almost daily. All were good or outstanding clinicians, in addition to their other fortes and interests. Each was involved part-time in one or more clinical research projects. One of the great strengths of the HUP anesthesia department was its close relationship with the physiology department's Julius Comroe. The shadow of pharmacologist Carl Schmidt added to the department's unique position.

Robert D. Dripps (fig. 3) was the undisputed leader of our specialty at that time. I admired, adored, and loved him. Without him, we residents would not have developed beyond being technicians, into applied physiologists and pharmacologists, that is, skilled (academic) anesthesiologists. Because Dripps came from Ralph Waters's department in Madison, Wisconsin, which had the first anesthesiology residency program in the U.S. (probably in the world), I consider myself a professional son of Dripps

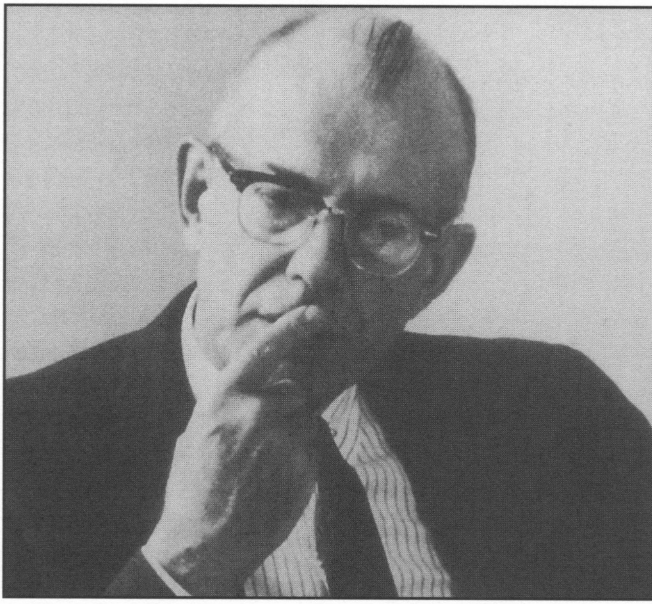


Figure 3. Robert D. Dripps, M.D., in the 1950s. My professional father, friend, and role model as clinician-scholar-investigator-leader. He nurtured many residents to become leaders of academic anesthesiology. His writings on cutting-edge topics are still classics.

and a grandson of Waters. My mentors at the HUP convinced me, through personal example, that clinicians as well as professors in our specialty must first be personally skilled and effective in the OR and postanesthesia recovery room. I tried to follow their examples. At first full-time and later part-time, I remained involved in the OR by laying hands on patients for 40 years — until age 65, when I "withdrew" into full-time research.

Dripps conducted or nurtured numerous clinical and laboratory research projects. He always looked for new ways of doing things. For example, I managed an extremely bloody laparotomy with massive transfusions, pumping many liters of banked blood in as fast as it came out, to maintain normotension (by cuff pressure). I used ether-O_2 and spontaneous (assisted) breathing to free my hands for blood pumping. At that time there were no OR ventilators. Dripps congratulated me for the patient's survival, but suggested that I might have used deliberate hypotension, a novel approach then explored in Europe. I researched this method during the following year in Peru.

Dripps or one of the other faculty members, one at a time, left briefly for Copenhagen to teach European anesthesiologists at the World Health Organization course. Dripps also went to Korea during the war there and reported on his observations: that in traumatic-hypovolemic shock, merely giving $N_2O:O_2$ 50:50% can be enough, by suppressing pain, to reduce the blood pressure to pulselessness.

I could go on for pages reminiscing about Bob Dripps's influence on us. To me he was the number-one role model of an academic anesthesiologist. His thinking, doing, and writing at that time, about open-chest CPR,[35] therapeutic hypothermia,[36] and other cutting-edge topics, are still classics. They "primed" me for resurrecting these topics for research 30 years later (see Pittsburgh, IRRC).

Dripps also helped residents solve personal problems. He and his associates invited us residents, one at a time, for dinner. Dripps tried matchmaking for science by inviting Eva and me with pulmonary physiologist Forster. Unfortunately, a research fellowship was not possible after my residency, since my visa forced me to leave the U.S. in 1952. Comroe's influence on me as resident brought me back to Penn in 1958 (when I was chief at Baltimore City Hospital) as one of his students in a course on medical education. That course was his pilot project for the Cardiovascular Research Institute (CVRI) that he founded in San Francisco later in 1958, and where I spent my sabbatical in 1969-70.

Dripps invited Eva and me to house-sit his home on the Philadelphia Main Line for one month in the summer of 1951, which we gratefully accepted so that we could save rent for that period. He paid my expenses for the College of Anesthesiology exam, which I took in 1952 in Chicago. Dripps was charming at Christmas parties. His humorous poems became legendary. He was liberal, tolerant, and cosmopolitan. (There were, however, no African-American doctors at the HUP at that time). His influence on my career continued beyond the residency. Bob and his wife Diane, when they visited Vienna in summer 1963, met my parents and reported that my father was in cardiopulmonary trouble. He died in his sleep in Vienna on the same day John F. Kennedy was murdered in Dallas. I grieved for both, in Vienna, during the last week of November 1963.

After my residency, Dripps and I stayed in touch, off and on, when he rose to vice-chancellor and then semiretired. I was with him last in 1970 in San Francisco when he and Comroe wrote a book, *The Retrospectoscope*. Dripps died, much too soon, while playing tennis in 1972, of sudden cardiac arrest, undoubtedly from ventricular fibrillation. No CPR attempts were made. There was no EMS system yet. Our specialty lost a guiding star. Medicine lost a giant physician, a great man, and a "Mensch." His department continued strong under the chairmanships of Harry Wollman and Dave Longnecker. In 1994 I was invited to give the annual R.D. Dripps Memorial Lecture at the HUP.

James Eckenhoff (fig. 4) also was a unique academic physician first, and anesthesiologist second. Jim was one of my cherished clinical teachers and role models. His approach to patients, particularly when giving regional anesthesia with a delicate touch, taught me about the art of medicine. He had a unique bedside manner with patients when giving anesthesia, using a superb mix of art and science, instilling confidence in patients and trainees, and guiding us residents with collegiality. In 1951, Eckenhoff created (at the HUP) one of the first block clinics for pain control in the U.S., using the postanesthesia recovery room. I joined him once, on a resuscitation attempt in a patient's room, in trying intra-arterial transfusions for cardiogenic shock. He explored narcotic antagonists. From Scandinavia to the U.S. he

Figure 4. James Eckenhoff, M.D., in the 1950s. My teacher, friend, and role model as clinician-scientist who taught us how to combine knowledge, skills, and judgment. He went on to become professor, department chairman, and dean at the Northwestern University's medical center in Chicago.

brought modern treatment for drug overdose, i.e., prolonged manual intermittent positive pressure ventilation (IPPV) instead of using analeptics. This was pioneered in the early 1950s in Denmark by Nielsen and Dam. In 1954/55, Jim was my sounding board during the Johns Hopkins anesthesia crisis. In 1960-61, he was offered the chair at Pittsburgh and declined (see Pittsburgh). I was offered it next — and accepted (because he said it could not be done).

Eva and I got to know Jim's family personally from the start. In 1950, he helped us supplement our income by asking Eva to babysit for his sons and to make dress alterations for his wife. He invited us to his home in Philadelphia in the 1960s, and on the shores of Lake Michigan in the 1970s. In the spring of 1984, at my 60th birthday celebration, Jim Eckenhoff, then age 70, gave the "Peter and Eva Safar Lecture in Medicine and the Humanities" at the University of Pittsburgh (introduced by my successor Peter Winter). In 1993, Eva and I visited Jim and his wife in Laporte, Indiana. Jim Eckenhoff died in 1996. In September 1997, Jim's son Roderick Eckenhoff, then a professor and an imaginative basic anesthesiology scientist at the HUP, and David Longnecker, the department's superb new academic leader, invited me to give the First James Eckenhoff Memorial Lecture at the HUP. Leroy Vandam came from Boston and spoke on that occasion. It was a sentimental reunion. The Dandys and Mary McNeal Wood (another HUP coresident of 1950) joined us. Jonathan Rhoads (over 90 years old), then chairman of surgery emeritus at the HUP, Ravdin's successor, attended and remembered the anesthesia resident of 1950. The topic of my Eckenhoff lecture at the HUP in 1997 was "Anesthesiologists as Intensivists." This revisited the topic Jim Eckenhoff had asked me to discuss and publish for the first time in the mid-1960s.[10]

Leroy (Roy) Vandam (fig. 5) was one of my role models as a scholar. When Eva and I arrived in Philadelphia in the fall of 1950, Roy and his lovely wife Jean were the first to invite us for dinner. Roy is well known as a pictorial artist. We are the owners of a genuine Vandam, a large, beautiful watercolor of a Nantucket scene. Vandam was (and still is) very eloquent in speaking and writing, unbiased in the evaluation of papers and facts, direct, and usually right. His comments at morbidity and mor-

Figure 5. Leroy Vandam, M.D., in the 1950s. My teacher, friend, and role model as clinician-scholar. Artist in anesthesiology, writing, editing, and painting. He went on to become professor and chairman of anesthesiology at the Peter Bent Brigham Hospital of Harvard University in Boston.

tality conferences at the HUP, particularly those presented jointly with surgeons, were refreshingly honest. He could render the bossiest surgeon speechless. These and other characteristics later made Roy the ideal editor of the journal *Anesthesiology*.

Vandam's spinal anesthesia study, to which all residents contributed cases, became a classic. Its results overcame the bias that spinal anesthesia causes neurologic damage. Roy was an academic role model, not only as a superb lecturer, discussion leader, and writer, but also by making every anesthesia seem like a pathophysiologic-pharmacologic experiment. Roy had me participate in a study of relaxants,[37] in which I was also a subject. Without anesthesia, I took subapneic doses of various relaxants. This gave me the thrill of overall weakness and double vision when driving home after the experiment.

In the mid-1950s, Roy accepted the chairmanship at the Peter Bent Brigham Hospital in Boston. In October 1996, at the commemorative 150-year anniversary celebration of the first ether anesthesia at the Massachusetts General Hospital, Eva and I, Andy Miller (a former student research fellow with me in Pittsburgh, then anesthesia resident at "the Brigham"), and Jan Smith hosted a party. Jan Smith was in the 1960s an anesthesia resident with Vandam and CCM fellow with me in Pittsburgh, and now is professor at Pittsburgh. At that party we thanked Roy Vandam for what he had done for our professional lives and for anesthesiology and medicine worldwide. The Pittsburgh-Brigham connection was represented by four professional generations: Vandam taught Safar. Both taught Jan Smith and (former Pittsburgh students) Gilbertson, Miller, and Kaplan. At the HUP, Vandam had taught my Pittsburgh coprofessor Joe Marcy, the late Leroy Harris (my coresident at the HUP and our Pitt department's resident leader of the 1960s), and the late Steve Galla (director of the anesthesiology laboratory in Pittsburgh). I then told Vandam how fortunate I had been as one of a dozen residents who were then taught by the pioneers at the HUP; and how happy we were to see him fit and bright.

Paul Dumke (fig. 6) had an impressive record of laboratory and clinical research before assuming his role in 1950-52 as manager of daily schedules in the OR of the HUP. His manner and appearance (he was a giant man) were awe-inspiring, even

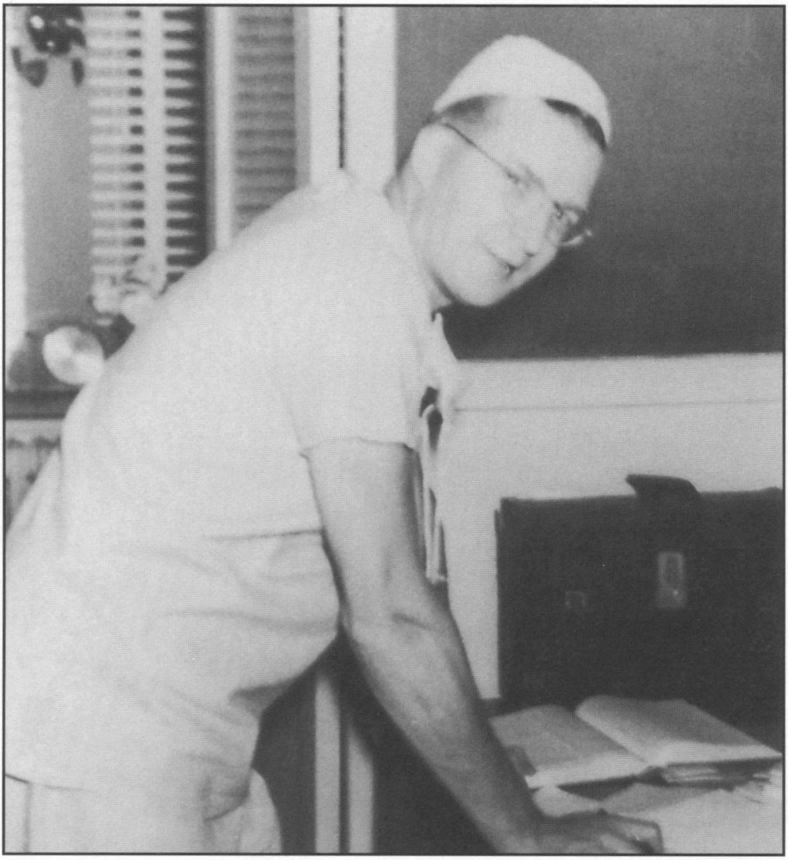


Figure 6. Paul Dumke, M.D., in the 1950s. My first clinical teacher at Penn. A role model as strong operating room leader. He went on to become chief anesthesiologist at the Henry Ford Hospital in Detroit.

for surgeons. Dumke usually got his way. He was a skillful, fast clinician, even with open-drop anesthesias (divinyl ether induction followed by diethyl ether maintenance), which we used for tonsillectomies in children. He was kind. He even made sure that we residents found decent housing.

My first case at the HUP, in the fall of 1950, was a spinal anesthesia for an elective cesarean section. Case assignments, then and now, have to be guided often by pragmatism. Before that case, Dumke had had me watch one spinal anesthesia, skillfully done by him. He then watched me do this one, and left the OR. After the painless incision and the baby's delivery, the conscious patient went "crazy," dislodging the intravenous line (the steel needle came out), trying to climb off the table. I had no instruction on how to handle such a situation. I had previously not received a formal instruction on how to use an anesthesia machine. Frightened, I called Dumke for help. He came fast and "tamed" the patient with a few breaths of cyclopropane-oxygen by mask. He then turned the rest of the case over to me and let me figure out how to use cyclopropane, the first time I used this (dangerous) champagne of general anesthesia. I liked this way of letting us learn from experience while keeping a distant eye on us. I acquired similar teaching habits. By the end of my residency, I believe I had become quite skilled with the use of this "champagne" — mastering laryngospasm, apnea, and bradycardia. The use of cyclopropane helped me acquire resuscitation skills. I thank Paul Dumke for having been an effective and kind clinical teacher, even when OR-coverage demands put pressures on him. When I later went to Peru, Dumke equipped me with special spinal needles. He went on to become chief anesthesiologist at the Henry Ford Hospital in Detroit. I recently learned that he died after a long incapacitating illness.

Austin Lamont (fig. 7) was a lord in character, wealthy but modest in lifestyle, a supporter and savior of those in need, an ethicist, a superb anesthesiologist, a very cultured man and art lover, and a promoter of research.[38,39] (He was the son of Thomas Lamont, partner of J.P. Morgan.) At the end of our residency, as a farewell gift, he gave me and other academically oriented residents Claude Bernard's book, *An Introduction to Experimental Medicine*. He became a good friend to Eva and me from the start when he gave us tickets to some of the Phila-

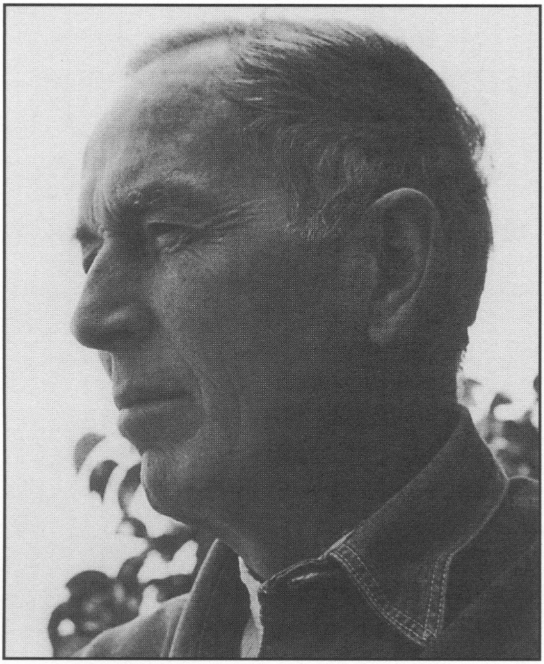


Figure 7. Austin Lamont, M.D., in the 1950s. My teacher at Penn, friend, and wise advisor. He was a disciple of Ralph Waters and performed the anesthesias for the first heart operations at the Johns Hopkins Hospital before joining Dripps.

delphia Symphony concerts. This friendship included his lovely wife Bo Lamont.

Lamont was also a disciple of Ralph Waters. In the late 1940s, Lamont and his then-resident Merel Harmel, performed the anesthesias for the first blue-baby heart operations and made heroic attempts to establish an academic department of anesthesiology at the Johns Hopkins Hospital. After they failed, because of chief surgeon Alfred Blalock, they joined Dripps. Harmel went on to become anesthesiology chairman at the State

University of New York in Brooklyn, the University of Chicago, and Duke University. Lamont advised me about Johns Hopkins when I became part of the second attempt to establish an academic department there under Don Proctor. At the HUP, Lamont was the medico-political troubleshooter for the department and an ever-present advisor. He helped residents find appropriate jobs at the end of their training. Lamont also guided medical students in OR anesthesia experiences. Once he showed me how to anesthetize a 400-pound patient with only cyclopropane by mask. He demonstrated skillful insertion of tracheal tubes, via nose or mouth, into awake patients under topical anesthesia.

Lamont and Dripps were instrumental in obtaining special-preference immigration visas to the U.S. for Eva and me in 1953 and in my ending up at Johns Hopkins in 1954. Lamont championed the use of full-time salaried anesthesiologists in teaching hospitals nationwide, at a time when leaders of the ASA wanted all anesthesiologists to be in fee-for-service private practice. He conceived the Association of University Anesthetists (AUA), as a counterforce to the more private practice (fee-for-service)-oriented ASA. He initiated the AUA in 1952, jointly with Dripps, Henry Beecher, and Emanuel Papper. We residents witnessed the first planning session at the HUP. In 1954, as a young staff anesthesiologist at Johns Hopkins, I had the privilege of being asked to join the AUA. The Lamonts and Safars remained in touch.[39] Austin Lamont remained at the HUP until he died in 1969, at age 64. In 1973, Bo reminisced with us in Pittsburgh when we hosted the AUA meeting. The department at Penn established a memorial: Jim Eckenhoff's son, Roderick Eckenhoff is the first Austin Lamont Professor.

Margot Deming was pediatric chief anesthesiologist. We residents rotated for one or two months through Children's Hospital of Philadelphia, which in 1951 was on nearby Bainbridge Street. Deming was dynamic, a clinical wizard, outspoken, somewhat rigid about the choice of anesthetic techniques, and tenacious. Joe Marcy of Pittsburgh and Eugene Conner, greatly influenced by her, were in touch with her until her recent death.

Deming's induction routine, with which we had to become skilled, was cyclopropane-O_2 by bag-mask, without CO_2 absorption, semiclosed to-and-fro. Intubation was without relaxant, under deep cyclopropane and controlled ventilation, "pumping" the patient into apnea and bradycardia (sometimes even cardiac arrest). These extremes, I was told later, were not always reversible. After intubation, we used either a Y-tube for open ether-O_2 or a to-fro canister for CO_2 absorption. With cyclopropane-O_2 we always aimed for a closed system. Later, I sometimes supplemented N_2O-O_2 (semi-open) with very low cyclopropane concentration, when light general anesthesia with spontaneous breathing was the goal. Quick recovery from cyclopropane anesthesia enabled us even to have patients who underwent herniorrhaphy go home the same day. Same-day-surgery was then unique, particularly when a tracheal tube was used, because this was always feared as a potential cause of laryngeal stridor and obstruction postoperatively.

Champagne can be risky; I once went too far with the deep cyclopropane induction-intubation technique of Deming. I stopped the heart of a child about to undergo heart surgery by Julian Johnson. He and I quickly reversed the cardiac arrest with open-chest CPR and the patient recovered fully. Johnson, delightful to work with, instead of scolding me, praised me and himself for the good result.

C. Everett Koop had just been appointed at Children's Hospital as the youngest chief of general surgery. I respected and liked him. Later I did not agree with him on futile surgery for severely deformed neonates. Leonard Bachman and I remembered Koop as a super-doctor when he was considered for Surgeon General of the U.S.. Koop and I later communicated when his son died from a climbing accident and Eva and I lost our daughter, Elizabeth, to status asthmaticus.

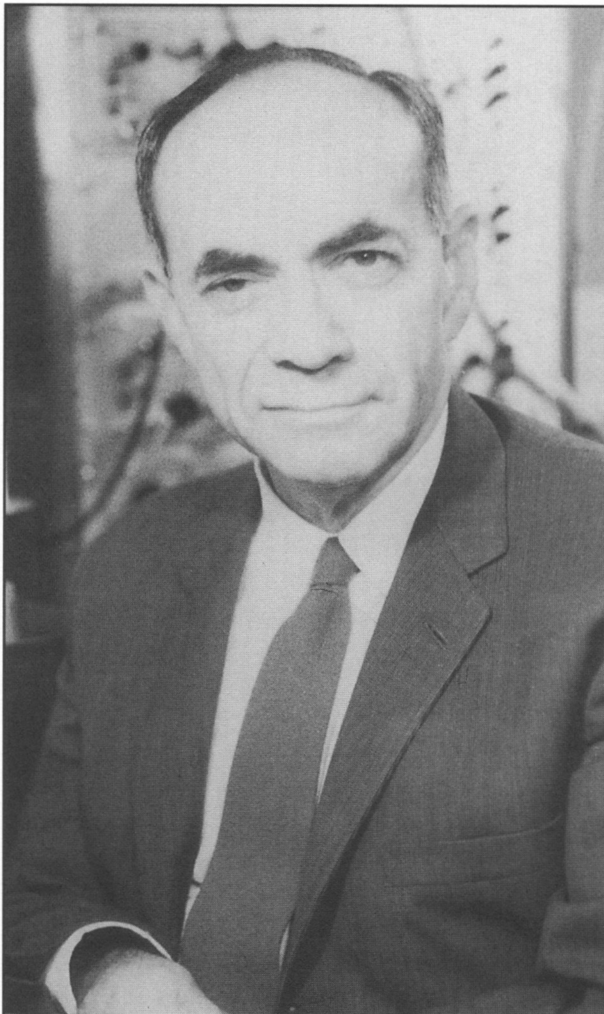


Figure 8. Julius Comroe, M.D., in the 1960s. My teacher at Penn and during my sabbatical in San Francisco. A giant as scientist, research mentor, and educator of many faculty members of the medical sciences.

Research flourished at the HUP. The co-investigators and advisors for Dripps and his research groups included cerebral blood flow (CBF) pioneer Seymour Kety and pulmonary physiology pioneer Julius Comroe (fig. 8) whose influence on me continued after the HUP (see Baltimore, Pittsburgh). We residents were drawn into helping with clinical studies coached by these two young giants.

At the HUP I learned clinical skills by watching staff and other residents and by doing about 1,000 cases during the first year and about 500 during the second year. I still have copies of some of the records. I acquired knowledge by reading and clinical judgment from doing and discussing cases. There were weekly anesthesia conferences and morbidity and mortality meetings, some with surgeons. Most instructive to me were the Sherlock Holmes-type complication conferences, with unbiased candid conclusions. Many were moderated by Dripps himself. I learned about broader, controversial issues during our lengthy debates in the residency "chart room" while waiting for cases or when on call, during interactions with staff-faculty and among residents. Controversial topics included the dangers of neuromuscular blocking agents vs. their safety in the hands of ventilating doctors (Dripps et al disagreed with Beecher and Todd); the safety of spinal anesthesia; and intravenous narcotics-supplemented N_2O (favored by Francis Foldes of Pittsburgh) vs. potent and more controllable inhalation anesthesia with cyclopropane or ether (favored by Dripps and us HUP residents). Breath-by-breath control of anesthetic depth, so important with spontaneous breathing, and feasible with cyclopropane and later the halogenated agents, was not possible with i.v. narcotics, not even with intermittent i.v. injections of thiopental for maintenance.

The OR suite at the HUP included 6 or 8 operating rooms (each with an induction room), three delivery rooms, and a recovery room in which patients on stretchers were supported by manual ventilation. Immediately adjacent were the anesthesiology staff offices, including that of Dripps. There was amiable collaboration among anesthesiologists, OR nurses, and surgeons. Immediately adjacent to the offices was the surgical dressing room, the site of collegial communication between surgeons and anesthesiologists. While some considered Chairman of Surgery Isidor Ravdin to be a despot, I very much liked to give anesthesia for him and respected his values, principles, and speed as an operator. To us residents he seemed fully supportive of Dripps's anesthesiology programs, something very important for the development of the specialty. He gave financial Christmas gifts to anesthesia residents.

The early 1950s at the HUP were the years when cyclopropane by closed-circle system was used predominantly; less often, induction was accomplished with cyclopropane followed by ether-O_2 by closed circle, or by N_2O-O_2 by semiclosed circle supplemented with ether or titrated intermittent i.v. doses of thiopental. We used Foregger and Heidbrinck machines, sometimes also with Waters's to-and-fro CO_2 absorption canister, which was clumsy. Most general anesthesias were given by mask, with an oropharyngeal or nasopharyngeal airway. Less often we used rubber tracheal tubes, with or without pharyngeal packing, with or without soft slip-on cuffs. The laryngoscopes were the same as in the 1990s. Assisted and controlled ventilation was by manual bag compression even without endotracheal tubes. For semiclosed systems we constantly had to juggle the pop-off valve to control leaks for appropriate bag size.

After general anesthesia with cyclopropane, the second most frequently used technique was spinal anesthesia with hyperbaric Pontocaine. When Ravdin demanded spinal anesthesia for his laparotomies, even in the upper abdomen, because it produced beautiful relaxation and a contracted gut, we would supplement it by using N_20-O_2-thiopental with spontaneous breathing via mask, to prevent retching and discomfort. Blood pressure control was challenging with these high spinals. Dripps and Vandam documented that postspinal anesthesia headache is rare when very thin needles are used, that the addition of epinephrine provided three hours of anesthesia, and that neurologic complications are near zero with aseptic, clean, atraumatic technique. Since Dripps was never fond of epidural anesthesia because it was unreliable, I never learned it.

As to equipment, there were no intravenous plastic tubes or needles, only rubber tubes, syringes of glass and metal, and steel needles. Banked blood was not yet stored in plastic bags, but in recycled (glass) milk bottles, and often contained large clots. This blood we poured via a gauze filter into an open hanging glass vessel. Its bottom outlet led via a rubber tube and in-line glass drip chamber (without filter) into a short steel needle in the vein. (When M.H. Weil introduced central venous catheters a decade later some conservative colleagues attacked him.) At the HUP in 1950, pressure-infusion was by pumping air into the bottle above the blood. Not yet available for use in the OR were electrocardioscope, oximeter, tri-electrode blood gas units, three-way stopcocks, mechanical ventilators, and external defibrillators. There was only an internal defibrillator for open-chest CPR available since the 1940s. Some of these now-essential devices, although invented, were then not commercially available. For IPPV, we used only manual compression of the breathing bag. In 1952, just before I left for Peru, the first ventilator appeared — a Jefferson bellows-in-box type oxygen-powered OR ventilator. I was told that there was one Mine Safety pressure-cycled ventilator available, somewhere outside the OR suite, for long-term ventilation. At the HUP, there were almost no helpers to care for equipment, no nurse anesthetists, no pagers to carry, and no telephones in the ORs. For emergencies, there was only paging via loudspeaker.

Rapid inhalation induction by mask was with cyclopropane: O_2, 50:50%, one to three breaths, and then the cyclopropane concentration was reduced and titrated, by closed system. This inhalation technique had a tendency to produce laryngospasm and apnea, making us artists in the control of the upper airway and ventilation with positive bag pressure and mask. We learned to get out of trouble. Relaxants were widely used in adult general anesthesia, at first decamethonium for induction and curare (as "introcostrin") or gallamine for maintenance. In 1951-52 we were among the first to use succinylcholine. We experimented with subapneic doses on ourselves. We also breathed various inhalation anesthetics to see how a patient feels "going under." To my knowledge, no one became addicted.

Here are some anecdotes:

During my first year, I worked on a case with a gynecologist named Payne. When, during a joint pre-anesthesia visit, we introduced ourselves as "Dr. Payne (pain) and Dr. Safar (suffer)," the patient became pale and frightened. When I pronounced my name differently as actually being "Safar (safer)," the patient relaxed.

Against their better judgment, but to keep peace, our staff supervisors had to tolerate and accept some surgeons' idiosyncrasies about anesthesia. Usually we residents could choose the anesthesia we wanted to use, but there were exceptions. For example, for an eye operation by an ophthalmologist who ordered me to do it "his way," I was made to inject 5% thiopental directly, without a diluting infusion, via a rubber tube and steel needle, into a vein of a totally draped patient. Because the surgeon insisted on "pentothal only," I failed to detect subcutaneous infiltration after the patient had moved and dislodged the metal needle under the drapes. This resulted in a skin necrosis and the need for plastic surgery. Nowadays, this would lead to a lawsuit, and the staff would be furious with the resident. None of this happened. I felt awful. Dripps barely mentioned the incident. In general, surgeons and anesthesiologists did not openly blame each other. That was collegiality!

The strangest idiosyncrasy was that of a leading neurosurgeon. He demanded that his intracranial operations be performed under local infiltration anesthesia with the patient often in the sitting position and the anesthesiologist at the patient's side, under the drapes, squirting ether on a gauze mask to keep the patient unconscious. Dandy remembers that, if conscious for most of the operation, the patient would scream when the trigeminal nerve was manipulated and would beg for the ether. This method, of course, often led to coughing, excitement, snoring, vomiting, secretions, and other ugly complications. I decided not to go along with this request. Realizing that asking him would have been useless, I inserted, under topical anesthesia, a naso- or oropharyngeal airway, insufflated N_2O-O_2, and titrated intermittent intravenous injections of thiopental. He would not allow a tracheal tube. After my month doing neurosurgical cases, He praised me for the smoothness of the anesthesias for his cranial operations. I confessed. From then on, I was told, he let the anesthesiologist decide what to use. Lumbar intervertebral disc procedures were performed in the prone, knee-chest position, using bag and mask without tracheal tube. We got away with it.

At the HUP, we used N_2O-ether anesthesia via semi-open circle in adults or open Y-tube in children, with tracheal intubation, even for head or neck operations. The ether fumes were often only one foot away from the cautery, also in some adult cases. I do not recall that the use of flammable agents in the presence of cautery ever led to an explosion in our or affiliated institutions. When George Thomas, anesthesiologist at Pittsburgh's St. Francis Hospital, visited the HUP and gave one of his lecture-demonstrations about the hazards of flammable agents and the need for absolute spark prevention, we felt that danger was exaggerated.

In obstetrics, deliveries were usually performed with intravenous meperidine-scopolamine for analgesia in stage I, followed by N_2O-O_2 ether analgesia or light anesthesia for stage II. Many newborn babies briefly needed assisted ventilation for a while, usually with O_2 by mask and Kreiselman valve. Cesarean sections were predominantly done under spinal anesthesia.

My coresidents were an interesting group. Brief comments on about half of them would be appropriate here.

Walter Dandy, Jr. was a very skilled resident. At the HUP, we "clicked," although Walter had come from a conservative and I from a liberal background. He jokingly considered me conservative because I drove a black sedan of the 1930s. Walter and I toured around the world in 1960. After the HUP, Dandy, originally from Baltimore, returned to the Johns Hopkins Hospital, but became frustrated (see Baltimore) and went into private practice. He said that the high point of that period was the opportunity to anesthetize Elizabeth Safar for her tonsillectomy (by Donald Proctor at the BCH, where Dandy was consultant for our department). At the Union Memorial Hospital of Baltimore in 1969 Walter Dandy, Jr. created what I believe was the first full-time physician-director position of a general ICU in a large private-practice hospital in the U.S. The acceptance of Dandy's plan was facilitated by the visiting professorship of one of my associates from Pittsburgh, anesthesiologist Clara-Jean Ersoz.

Leroy Harris was one year ahead of me at the HUP. He taught me anesthesia tricks during night calls. Eva and I introduced him and his wife, Laveta, to records of the music by Anton Bruckner and other classical composers. Harris was an excellent teacher in the O.R. and the laboratory. Harris later helped deliver our son Philip in Baltimore. In 1961 he joined me as associate professor and resident coordinator in Pittsburgh. After his sudden cardiac death in the 1970s, we created a resident award in his name at the University of Pittsburgh.

Ole Secher came from Denmark. During the German occupation, he, Henrik Bendixen (then of Harvard), and other Danish physicians were heroes of the anti-Nazi resistance. They helped many Jews survive, first as pseudo-patients and then by escape to Sweden hidden in boats. Secher was outspoken and imaginative in pharmacology. After his return to Copenhagen, he became professor and chairman there. He died in the 1980s.

Martin Zindler, the first HUP resident from Germany, was methodical; he was scholarly in his studies and teaching. He returned to Germany and, as professor and chairman of anesthesiology in Duesseldorf, became one of our specialty's leaders in Central Europe.

Tom Mackrell, after his residency at HUP, joined Grant Stone at the Graduate Hospital of Philadelphia. There they conducted daring but important prisoner-volunteer experiments. They documented — for the first time in humans — the reduction of cerebral blood flow (CBF) and the cerebral metabolic rate for oxygen ($CMRO_2$) during hypothermia under cyclopropane anesthesia, and the effect of hemorrhagic shock on CBF and $CMRO_2$ in awake humans. CBF experiments were also done in the recovery room of HUP on still-anesthetized patient "volunteers."

John Severinghaus (fig. 9) had his first year of anesthesia residency at the HUP in 1951-52. I was then in my second year and was chief resident. Bored, as we all were during long hours of anesthesia maintenance for surface operations, John made use of every anesthesia. Already as a resident he had documented for the first time N_2O uptake patterns. Before we gave patients the newest relaxant, succinylcholine, he asked me to give him a subapneic dose. It turned out to be apneic. I was then not aware of direct mouth-to-mouth ventilation, which was not then appreciated. We only occasionally inflated lungs by mouth-to-tube. John was conscious and not intubated. I had an anesthesia machine nearby, just in case. Unexpectedly, John first twitched and then turned very weak, gesticulating for the breathing bag. It all turned out well. Despite (or because of) this event, a lifelong friendship developed between the Severinghaus and Safar families. John later got even with me in self-experimentation. During a visit to Iowa in 1956, where John had his second year of residency (after a stint at the NIH, where he invented the PCO_2 electrode), he let me inhale the then newly introduced halothane. It smelled good and made me "pass out" as rapidly as with cyclopropane. In 1969, he stuck needles into me as an experimental subject on White Mountain in the Sierras, at an altitude of 14,000 feet. John became our specialty's basicscience guru. Even

Figure 9. John Severinghaus, M.D., in the 1960s. My coresident at Penn, friend, teacher, and role model as experimental investigator. An imaginative pathophysiologist. He went on to become a distinguished professor of anesthesiology research at the University of California in San Francisco.

as a resident, Severinghaus was one of my role models in research. He influenced me and others to pursue scientific questions while caring for patients. He is a superb gadgeteer. His knowledge of physics, chemistry, and physiology is amazing. We both developed the habit of freely giving away our ideas for research. During my sabbatical in San Francisco with John in 1969/70, we participated in anti-Vietnam War activism.

At the HUP, the collegial collaboration between clinicians and researchers of various disciplines not only led to many firsts in anesthesia-related publications, but also created research fellows who later became professors. The "breeding" of research fellows mushroomed in the late 1950s and 1960s. My main regret concerning my career is that I never had a full-time mentored research fellowship, which would have made me more knowledgeable in basic science and more competitive for National Institutes of Health (NIH) grants. This gap was unintentional, necessitated by visa requirements that Eva and I leave the United States in 1952 for at least two years.

In 1951, I was already contemplating what to do after residency. A research fellowship with Dripps et al would have been great, but we had to leave the USA. We considered going to Canada for two years, having learned that we could go there anytime, temporarily or as immigrants. Then in 1952, Dripps surprised me one day: "Peter, would you like to take the chief position at Yale New Haven Hospital next year? Louis Hampton quit there." I laughed, thinking how can a graduating resident (without a long list of publications) become the chief at an Ivy League university hospital. I told him about our visa restrictions. Soon thereafter he called me back: "Peter, I just got a letter from our past neurosurgery resident, Fernando Cabieses, of Lima, Peru. He cannot do what he learned here at the HUP, for lack of safe, modern anesthesia. They would sponsor a new chief anesthesiologist position." This was to be at the National Cancer Hospital, where Cabieses worked predominantly, the only hospital in Peru at that time with academic ambitions and some full-time staff-faculty. Everything else in Latin America seemed to be private practice plus very part-time work in charity hospitals. The chief anesthesia position in Lima would be filled by a foreign, U.S.-trained doctor (me) until a Peruvian doctor could be trained in the U.S. I grabbed the opportunity without even inquiring about salary or other details. Eva and I decided on a South American adventure rather than Canada.

Between Penn and Peru, after my residency with Dripps, in October 1952, we briefly returned to Vienna to fulfill our promise to the immigration service. There we took Spanish lessons. At that time, Mayrhofer's Division of Anesthesiology, in the Surgery Klinik #2 under Denk, was well underway. Kucher, who had been a surgery intern with me in 1949, had switched full time to self-learned anesthesia. Kucher had gained a good reputation and was soon joined by Steinbereithner. During my Vienna visit, my parents put subtle pressure on me to reconsider a professional opportunity to return to Austria. Professor of Surgery Spath at the University of Graz interviewed me for the position of chief anesthesiologist there. I gracefully declined. I was a still-disappointed ex-Viennese committed to remaining in America. I had become infected by the freedom bug of America. Eva and I had already agreed to go to Peru next.

In summary, the two years at Penn gave me the special medical skills, knowledge, judgment, motivation, confidence, and philosophies for all my professional efforts to follow. The people we worked and communicated with in Philadelphia influenced us greatly. During our years in Philadelphia and Peru, we were so busy trying to survive, adapt, and study, that geopolitical events passed us by. We had only marginal discussions on events that had enormous impact for others. These events included the war in Korea in 1950-54, the French war in Indochina, and the anticommunist witch-hunt by Senator Joe McCarthy in the U.S.

PERU

November 1952 – December 1953

During the first week of November 1952, Eva and I flew on propeller planes from Vienna to New York and on to Peru, via many stops. This was before the jet age. While passing through New York City, we were hosted by Eva's brother, Hans Kyzivat, and his wife Carry. We learned about Eisenhower's victory over Stevenson. Heroic allied soldiers stopped the aggression by North Korea. Thousands continued to die because of the arrogance of an American general who earlier had been celebrated for having transformed postwar Japan into a democracy. Eisenhower stopped the war in Korea, although slowly.

Tropical lands were new to us. We had stopovers in Miami and, at midnight, at a tin-shack airport of Guayaquil, Ecuador, with heat and insects. At daybreak we arrived in Lima. We had to overcome the shock of a strikingly different culture. There were (and still are) even greater contrasts between ultra-rich and ultra-poor people in Latin America than in the U.S. We became acquainted with some former Europeans. Those who helped us adapt to Latin America included Fernando Cabieses (neurosurgeon), Marino Molina (thoracic surgeon), Hans Lewitus (musician), and his wife Eva. My boss was Eduardo Caceres, U.S.-trained oncologist, general surgeon, and radiotherapist, who was the initiating, academically-oriented director of the new national cancer institute, the Instituto Nacional de Enfermedades Neoplasicas (INEN). Caceres and I "clicked" from the start. He was an honest, progressive, leading clinician-scholar. As time went on, Eva and I learned to love Peru and its people. A small evolving middle class in the cities and the Indians in the mountains instilled hope for the future. INEN pathologist Jorge Campos shared my concern for the poor.

I was appointed full-time chief anesthesiologist at the INEN on a salary of about $300 per month (perhaps $2,100 purchasing power in the 1990s). We had to pay $150 per month to rent a simple but friendly, modern apartment. The currency was 12-15 soles per U.S. dollar. As a full-timer I did not need a license. Food was inexpensive, but everything manufactured was imported and much more expensive than in the United States. This included film, which we purchased in great quantity to create slides and movies of our experience there. We could not afford a car. Eva worked as a technician with local nurses in the blood bank of the INEN for a nominal salary of about $30 per month. Blood donors were mostly soldiers, who were merely asked about diseases and then had their blood typed, with no serologic testing or crossmatching. Used equipment was cleaned, sterilized, and re-used. Fluid therapy with large volumes of salt solution became popular only years later, first in the U.S.

When I started at the INEN, anesthesia was administered there by nurse anesthetists, primarily two with considerable skills — Señorita Graciela Comacho (of Spanish ethnic background) and Señorita Zoraida Romero (of Peruvian Indian ethnic background). While awaiting my arrival, they had acquired a physician supervisor interested (not yet trained) in anesthesia — Mario Salem. He was a young, Peruvian-trained physician who had observed anesthesia across the street in the Hospital del Mayo. There, part-time anesthesiologists who had trained abroad visited while they worked mostly in the lucrative private hospitals (clinicas) for the wealthy. These private anesthesiologists including Cordero and Bouroncle, although experienced, came to some of my year-long lecture series at the INEN. The university-affiliated hospitals had no full-time faculty and no academic program in anesthesiology. Peru now has a new science-oriented medical university in Lima, the Cayetano Heredia, which did not exist at that time.

When we left the HUP for Peru, Dripps had asked me to train people there and to send one Peruvian doctor to him to take my place eventually. Dr. Mario Salem was a fine colleague who helped me get started by translating my first lectures. I recommended him for training with Dripps. He soon went off to the HUP with the understanding that he would return to the INEN two years later. Once in the United States, however, like so many other foreign medical graduates at that time (including myself), he decided to stay. During 1953, I trained two full-time residents in anesthesiology, Cesar Untiveros and Primo Negrete, and several part-time physicians.

Dripps wrote me that "the future of South America is 100 years from now." He wanted me to return to the HUP and join his staff. Dripps and Lamont would try to get Eva and me back soon, on a special-preference immigration visa through the McCarran Act.

My lifelong urge to share the useful things I had learned flourished in Peru, where everything I brought from the HUP seemed new to most people there. In Philadelphia, I was very shy and could not even present cases at meetings without hesitation. What I had learned at the HUP, however, gave me so much confidence that I came out of my shell in Peru. There I learned on the job how to teach, supplementing my rudimentary and fragmented Spanish with gestures and use of a blackboard, helped by translations from my English-speaking colleague Mario Salem, and by demonstrating in the OR. Immediately upon arrival, I started weekly seminars on anesthesia and resuscitation, which were attended also by anesthetizing physicians and nurses from other hospitals in Lima and surroundings. With daily teaching in the ORs and weekly seminars (almost all given by me), we were considered to be the first academic anesthesiology department in Peru.

The INEN had three operating rooms. On arrival I found most anesthesia to be induced with thiopental, followed by ether-O_2. N_2O was rarely used, owing to expensive transport of heavy cylinders. The anesthesia machines in Lima were mostly Foregger type. Supplies and minor equipment were not disposable. Blood was delivered in glass bottles. There were no ventilators and no hand-operated bellows. Ruben's self-refilling bag-valve-mask unit was invented three years later. We ran to emergencies and performed controlled ventilation outside the OR by bag squeezing, using a portable O_2 cylinder and a bag-mask to-and-fro (semi-open) system, without a CO_2 absorber. There were no monitors except for the blood pressure cuff, the finger on the pulse, and the esophageal stethoscope.

Richard Foregger, himself an ex-Austrian American, helped my new department with some equipment. In the U.S. I talked with Foregger several times at congresses before and after Peru. His company helped greatly in the 1950s and 1960s to improve modern anesthesia worldwide and to make rapidly available anesthesiologists' gadget innovations. This was before the era when patenting medical ideas by physicians (to me then a "no-no") became not only ethical, but even prestigious.

I had the opportunity of introducing to Peru cyclopropane, the esophageal stethoscope, more extensive use of regional anesthesia, lidocaine (introduced into anesthesia a few years earlier by Torsten Ghord of Stockholm), modern anesthesia for thoracic surgery, and succinylcholine. I imported succinylcholine from Austria, which was cheaper than the U.S. version. Succinylcholine was pioneered in Vienna — pharmacologically documented by Bruecke and Werner and introduced into anesthesia by Mayrhofer in 1951. In the 1970s Werner became Dean at the University of Pittsburgh School of Medicine. Within a year of Mayrhofer's demonstration, Foldes in Pittsburgh worked with it and other departments in the U.S. received it for trial. In Peru, I used Carlen's new double-lumen catheter for lung surgery. I introduced a relatively new method for major abdominal surgery — intercostal blocks, using lidocaine-pontocaine-epinephrine, injected just behind the midaxillary line, bilateral, at T5-11, after the induction of very light cyclopropane anesthesia with spontaneous breathing, which was continued for supplementation.[40] The large total dose of lidocaine (up to 1 gram or more per patient) that I used with epinephrine and cyclopropane shocked representatives of Astra, the manufacturer of lidocaine.

Epinephrine for prolonging nerve blocks was believed to be dangerous with cyclopropane. Light sleep under cyclopropane caused no palpable arrhythmias.

My skills were also sought outside the INEN, first as an unpaid visiting consultant at other public hospitals, later with a modest visiting physician fee. I was recruited to work at times as a teacher and demonstrator at the Hospital Obrero, the workers' hospital. There, Carlos Peschiera and Marino Molina performed modern lung surgery. Molina and I did the first, closed mitral valvulotomy at the Obrero in March 1953. These skillful thoracic surgeons also worked at the INEN. I taught and wrote instructional review chapters in Spanish about various topics, including resuscitation, emphasizing open-chest CPR for cardiac arrest in the OR,[41] and anesthesia for cardiac and great vessel surgery,[42] before the first use of the heart-lung machine. Molina helped with translations.

The thoracic cases in both hospitals included patients with hydatid cysts (echinococcus) of the lungs, a disease quite common in Peru, transmitted mostly by dogs. To help prevent cyst rupture and spillage, I used Carlen's endobronchial tube and gentle manual continuous positive-pressure ventilation (CPPV). All ventilation was by hand, as there were no ventilators. We also operated on large liver cysts.

The academic leadership role of the INEN was due to Caceres' vision and dedication. As director of the INEN and New York trained, he was an excellent surgeon and politician, committed to U.S.-style patient care. He provided his radical mastectomy patients for a study of blood loss during hypotensive anesthesia, probably the first such study.[43] I felt it was needed because Europeans embraced deliberate hypotension with enthusiasm, without data on reduction in blood loss. I lowered the systolic arterial pressure to 40-50 mm Hg, as determined by cuff and palpation of the radial or cubital pulse. Intra-arterial pressure monitoring had not yet been introduced into clinical use even in the U.S. I used the ganglionic blocker hexamethonium by intermittent i.v. injection or later trimethaphan (Arfonad) by titrated i.v. infusion, plus elevation of the operating site. Spontaneous breathing under light ether anesthesia suggested "enough" blood flow through the brain. Monitoring was by esophageal stethoscope. At that time electrocardiographic (ECG) monitoring in ORs was not in use in Peru or the U.S. In our standardized radical mastectomies, this experimental hypotensive technique reduced blood loss and operating time without killing any patients. However, the advantage of hypotension over randomized normotensive controls was offset by the need for more accurate, and therefore slower, hemostasis once normotension was restored before closure. In contrast to the normotensive group, we hesitated to make patients of the hypotensive group anemic, so most patients in both groups received one or two units of blood.

Some surgeons at the INEN were accustomed to coming and going as they pleased, since most of their work was private practice elsewhere. Some were angry when I demanded reliable scheduling and had to cancel some operations. One ear-nose-throat (ENT) surgeon got so angry that he was about to throw this Yankee down the stairs. Medical director Caceres supported me fully. An excellent abdominal surgeon, Sabogal, who also was mayor of Lima's harbor city, Callao, was startled and annoyed by my "arrogance." Over time we developed mutual respect. Now looking back, I was young and cocky.

I personally administered anesthesias, for service and demonstrations, almost daily, and lectured throughout the year in English and Spanish. The teamwork with the nurses was excellent. I realized then something about which I still feel strongly today, to be the basis for an academic department: The chief of a safe clinical service must first be accepted as an effective clinician himself. It is essential that he gain the trust of colleagues and trainees. We "chiefs" then could avoid much of the administrative harassments which chairmen must cope with now.

Anesthetizing for operations, performed by surgeons you respect, is like making chamber music: Both require no conductor, are performed best among friends, and focus on the "opus," the patient. I greatly enjoyed "making chamber music" in the OR with Caceres, Cabieses, Molina, Peschiera, and some of the other excellent surgeons of Peru.

Talking about music in Peru—I played piano duos with Hans Lewitus, an ex-Viennese teacher of clarinet, piano, and voice, recommended to us by one of his past colleagues, my and my sister's beloved piano teacher in Vienna, Leonie Urban. Hans Lewitus had both legs paralyzed from polio, which had hit him as a young musician in Vienna before his immigration via Palestine to Peru. He was a role model for musicians and for the handicapped. He and his much younger wife Eva became our best friends in Peru. Lewitus was a member of the Lima Symphony Orchestra.

Peruvian colleagues said that the changes in anesthesia practices under my activism saved many lives. I did, however, hurt one patient permanently. When I was at the HUP, Dripps made me participate in a spinal anesthesia trial with an experimental local anesthetic, Its use at the HUP provided longer anesthesia than we could achieve with Pontocaine. There was a slight suggestion that its use might have led to a higher incidence of temporary bladder problems after inguinal herniorrhaphies. In Peru, I had to provide very long anesthesia with profound relaxation for very radical abdominal cancer surgery. In March 1953, I tried this new local anesthetic for an ultra-radical hysterectomy, but, for the first time, with the addition of epinephrine 0.5 mg, as we did with Pontocaine-epinephrine-dextrose at the HUP and the INEN. The epinephrine apparently broke the camel's back. The patient, a middle-aged woman, "recovered" with severe pain and weakness of both legs, probably a permanent neurologic complication from what seemed to be adhesive arachnoiditis. I still feel guilty about this and another goof (in Baltimore). I was fortunate having never been sued for malpractice. In the 1950s, clinical exploratory trials were done after brief explanation to the patient. There was no requirement for written consent. There were no Institutional Review Boards (IRBs). It was customary to assume personal responsibility and to obtain the agreement of leading colleagues, usually the department chairman and/or medical director of the hospital, which I did.

We learned about the need for more thorough testing of new drugs in animals. I conducted a study in guinea pigs of the tissue-irritating histologic effects of various local anesthetics, including the "culprit," with the help of Campos, chief of pathology. All agents tested caused inflammatory reactions. Because of clinical responsibilities this study was not finished. Guinea pigs were abundant because Peruvians eat guinea pigs as we eat chicken.

My Peruvian anesthesia experience was mostly at sea level in Lima. High altitude offered different challenges. Eva and I visited in Huancayo, at about 13,000 feet, the government-run hospital Obrero, and the private miners' hospital. We also visited a high-altitude research station, Morococcha, and the highest mining town, Cerro del Pasco, both at above 18,000 feet, where I gave anesthesia to learn and to demonstrate.[44] High-altitude natives normally have hematocrits of 50-60% and high blood volume, lung volume, and cardiac output. Some developed "chronic mountain sickness" due to an exaggerated polycythemia with hematocrits of up to 80%. For these patients, one should avoid elective anesthesia and surgery until they have been moved to a lower altitude and the hematocrit has normalized. At high altitude, local surgeons preferentially used local anesthesia. For general anesthesia I found cyclopropane-oxygen to work best and to be the easiest to transport. I paid little attention to the explosive nature of the agent, despite dry, spark-prone air and the use of open electric heaters in the ORs. Nitrous oxide could not be used because it left little room for oxygen, and the cylinders were heavy, which made transport too expensive. Ether was too slow. Thiopental was well tolerated for induction. Spinal anesthesia often was followed by headache.

Some of the scientists with whom I interacted in Lima studied high altitude. Hurtado, a famous Peruvian physiologist, was United States government-supported. In his laboratory there was a pediatric burn injury researcher Kehl Markley (Cabieses' brother-in-law), also supported by the U.S. government, who studied simple oral fluid therapy strategies. Hurtado collaborated with Carlos Monge, Sr., the father of high-altitude physiology, whom I had the opportunity to meet. Monge, Sr. was the first to describe and explain acute vs chronic mountain sickness (Monge's disease). In the 1970s, when in Pittsburgh, I met his son, "Choclo" Monge, Jr., who was co-founder and dean of a new medical school in Lima, the Cayetano Heredia. This was long after our return to the United States. In 1953, Monge Sr. considered moderate polycythemia to be protective and supportive at high altitude. After his father's death, Monge Jr. found mild hemodilution (probably via resultant hypervolemia) to increase physical performance at high altitude. I did not pursue high-altitude research; my high-altitude experience in Peru was limited to clinical anesthesia.[44] Severinghaus, my former coresident at the HUP, came to Peru after us, to study high-altitude physiology with important results.

On the many Peruvian holidays, Eva and I usually traveled by "collectivo," a regular five-or six-seater sedan used as a long-distance taxi, with cost shared by several passengers. These "taxis" raced throughout the land, mostly on unpaved roads, up and down the coast and over passes 15,000 feet high. Even the Pan American Highway along the coast was partially unpaved. Sometimes we traveled by hitch-hiking rides on trucks, and sometimes on nonpressurized DC-3 planes, breathing oxygen from masks when the plane crossed the Andes above 18,000 feet. Several times we went by car or truck over the Andes and down the fertile Montagna (eastern slopes of the Andes) and on mud paths through the jungle to the tributaries of the Amazon

River. This was our first encounter with wild monkeys, huge insects, and people whose ancestors, 100 years earlier, may have introduced the German naturalist Humboldt to curare-containing arrow poison.

Using collectivos or trucks or the world's highest railroad (built by the British from Lima to Huancayo), Eva and I visited the altiplano from the Cordillera Blanca in the North, via Huancayo, Cuzco, and Machu Picchu in the center (before it was discovered by tourists), to Puno on Lago Titicaca in the south. Natives survived mostly on corn and potatoes, which were cultivated with difficulty at high altitude. Some natives were miners for mostly foreign companies. I went deep into a copper mine. Women were not admitted as this would bring bad luck. American companies seemed to treat their workers better than Peruvian or French companies.

In these mountains, offspring of the Incas habitually chewed coca leaf to stay fit and strong during hard work, running, and climbing. We of course joined the natives in drinking coca tea and corn beer (chicha). Cabieses was an expert on the beneficial effects of coca-leaf-chewing among high-altitude natives. When Eva and I, with our 11-year-old son Philip, revisited Peru in 1971, I brought home a bag of coca leaves, bought on the street for ten cents, which my son was going to use for show-and-tell in his health class in Pittsburgh. I declared this bag and its purpose upon entry in Miami. The customs officer was upset, "How dare you!!!" — and consulted his superiors. They confiscated the bag and let us go. My observations of cocaine- and opiate addicts abroad make me wonder whether our societies would not be better off by legalizing "drug abuse," under medical direction.

The efforts by Dripps and Lamont to obtain a special-preference immigration visa for me produced success on July 4, 1953! Eva and I celebrated with pisco sour, the favorite drink in Lima. With little money we managed moving our meager possessions, gratis, to Philadelphia with the help of a member of the U.S. Embassy in Lima, Bernard Brogley. Peruvian friends asked us to stay. They would get me a license for private practice, they said. Those who knew me (some Peruvians called me the "ex-Viennese Yankee social democrat"), however, predicted that if I stayed, I would become a revolutionary. I had become intolerant of so many fine but impoverished people being exploited by a few ultrarich families.

After more trips to high altitude late in 1953, we left Lima for good on December 23, 1953 — already with nostalgia for Peru — toward immigration to the U.S. We flew mostly on DC-3s, with stop-overs in Ecuador, Colombia, and Panama. We permanently immigrated to the U.S. on a night flight from Panama to Miami, in a thunderstorm with sparks on our plane's wings. We landed at dawn in a calm atmosphere, like that in Beethoven's Pastoral Symphony at the end of the thunderstorm movement. The Miami Customs and Immigration reception was so friendly that it brought tears to our eyes. We were so happy now to be full-fledged immigrants to the U.S.A. We flew on to New York, where friends and relatives received us on Christmas Eve. On Christmas Day, in the apartment of the Blodi family, we gave our first show-and-tell presentations of slides and movies of Peru. In the 1950s, such exotic travel adventures were rare.

We received our U.S. citizens papers in Baltimore on August 20, 1959. During a later visit to New York City, we received from the Ellis Island Foundation (after a donation) the "Official Certificate of Registration in the American Immigrant Wall of Honor," signed by Lee Iacocca. It says "Peter and Eva Safar came to the United States of America from Vienna, Austria." At Christmas 1953 we were elated about having become full-fledged Americans. Our daughter Elizabeth was born, prematurely, in Baltimore on August 18, 1954.

Why did we not stay in Peru? Peru is a beautiful country that needed us, and had so many challenges to offer. Although licensure would have been possible, I felt loyal to Dripps, who wanted me back. Finances were not a factor, but I believed that any professional accomplishments of mine would have a wider impact if originated in the U.S. My friend Hugerth suggested, "If you want to be effective in the good-works department, you have to be professionally and economically secure yourself." Peru prepared me emotionally for trying to help developing countries thereafter, from the U.S. After we became settled in the U.S., we did this primarily for other South American countries and Indochina. Europeans, who developed modern anesthesiology on their own, received my help later as needed, more for the development of intensive care and careers in resuscitation research.

In summary, our 14 months' experience in Peru was a type of venture comparable to the later-developed U.S. Peace Corps. It primed me for teaching, gave me the opportunity to introduce academic anesthesiology into Peru, and opened my eyes about the world's underprivileged. After my departure from the INEN, Negrete and Untiveros continued anesthesia services with nurse anesthetists Miss Comacho and Miss Romero. Since Salem did not return, my successor as chief anesthesiologist at INEN became Orlando Bernal. Later I met Guillermo Nieri, chairman of anesthesiology of the more-recently established second medical school, Cayetano Heredia. Eva and I have revisited Peru twice. A new generation has grown up. I do not know the current president of the Peruvian Society of Anesthesia, Analgesia and Reanimation, Eloy Nuñez and his co-officers, who invited me for a third revisit. Long-term results of our Peruvian adventure included three other positive spinoffs.

The first was Ruben Tenicela. He was born in the Peruvian mountains and trained in the medical school of San Marcos at the time I was chief across the street at the INEN. He joined me in 1959 as anesthesiology resident at the Baltimore City Hospital, and then, as an outstanding anesthesiologist, came with me to Pittsburgh in 1961. Here we wrote a review paper about anesthesia at high altitude.[44] Tenicela (praised and respected by John Bonica) founded, in Pittsburgh, one of the first and busiest multidisciplinary pain control clinics in the U.S.A. He became a tenured professor. Ruben, now retired, was a superb, skillful clinical teacher and anesthesiologist who was not only an artist with nerve blocks but also an experienced pain care consultant.

The second spinoff of our Peru experience was our son Philip's introduction to mountaineering when he accompanied us to Peru in 1971, at age 11. Stimulated by this fabulous country, and by having climbed Machu Picchu with his father as a boy in 1971, and the four highest Alpine peaks in Austria as a teenager in 1977, he returned twice to Peru — first as a member and then as a leader of climbing expeditions on the steep icy peaks of the Cordillera Blanca.

The third spinoff was disaster reanimatology. In June 1953, when Eva and I visited the altiplano in the Cordillera Blanca, a valley at 13,000 feet, called Callejon de Huayas, we came to the dream town of Yungay, with 25,000 inhabitants, mostly Indians. From there, in the 1930s, Austrians had made the first ascent of Peru's highest mountain, the Huascaran (22,000 feet). We thought that Yungay and its surroundings was what Shangri-la must look like. In May 1970, the western hemisphere's worst earthquake hit all of Peru and killed 80,000 people. Yungay was totally buried by an avalanche of rock, mud, and ice from the Huascaran. Except for a few who escaped to the cemetery hill, all inhabitants were buried alive.

During our return to Peru, in 1971, a trauma meeting at the Hospital Empleado in Lima included an earthquake disaster drill with use of new rescue helicopters fit for high altitude and dust. Such helicopters were not available in 1970. During our second return to Peru, in 1984, Eva and I visited the spot where Yungay used to be. We interviewed a geologist Ames, and two physicians: Hospital director Ramos and anesthesiologist Masquera of new nearby Huaras. The three had survived the 1970 earthquake in Huaras, when the altiplano was cut off for four days. The hospital of Huaras was severely damaged, had only two manual ventilators, and one mechanical. There was no power for over a week. Anything resembling a medical response was negligible, considering that one-half of the population of 30,000 in Huaras had perished. This, and similar interviews that I conducted after the 1980 earthquake in Italy (assisted by our Pittsburgh Resuscitation Research Center's Nancy Kirimli, R.N.) — suggested the following hypothesis: A considerable proportion of earthquake victims who are ultimately counted among the dead, who are not crushed to death immediately, die slowly, over minutes or hours after being injured, by crushing, compression, falling debris, hemorrhage, brain trauma with coma (airway obstruction), or dust inhalation. A significant number of those accessible might be saved through immediate application of modern resuscitation by uninjured lay persons, followed by advanced life support by rescuers and in hospitals. Thus, our experiences in Peru led to later research that pursued the above hypothesis and initiated "disaster reanimatology" as a new field of inquiry. I initiated this in the 1970s, and "nudged" my associates to carry out multiple field studies in the 1980s and 1990s.

BALTIMORE
February 1954–May 1961

Johns Hopkins Hospital, February 1954–June 1955

In January 1954, Eva and I had become immigrants, from Austria via Peru, to the U.S., with the plan that I join Dripps's staff at the HUP. I needed a permanent license to practice medicine in Pennsylvania. In spite of Dripps's influence, the Pennsylvania State Board insisted that I go back to medical school in the U.S. in order to be eligible for their licensure examinations. (Seven years later, when I was recruited to become chairman of the Department of Anesthesiology at the University of Pittsburgh, the same board gave me the Pennsylvania license with merely an oral exam. As department chairman in the 1960s and 1970s, I convinced them not to block the licensure of some of my best colleagues, whom I attempted to recruit to Pittsburgh.)

Frustrated about my licensure problem, my chief advisor at the HUP, Austin Lamont, suggested that I go elsewhere where I am needed and the board wants me. He phoned Donald Proctor, then chief anesthesiologist at the Johns Hopkins Hospital (JHH) in Baltimore. Proctor, who remembered me from 1952, when he learned anesthesiology in Dripps's department, needed and wanted me. The State Board of Maryland accepted me. Proctor offered me a job as instructor (later assistant professor) for about $7,000 per year. I accepted, based on Lamont's advice that it represented a great challenge. After one month in Philadelphia, Eva and I drove to Baltimore. When we parked our "Black Magic" (1937 Plymouth) briefly downtown, a thief relieved us of our suitcases. The most "valuable" possessions lost were Eva's grandmother's memoirs and my Alpine skiing-mountaineering outfit. This did not deter us from becoming loyal, happy Baltimoreans over the next seven years.

Don Proctor and his wife Janice treated us with extreme hospitality when Eva and I arrived in Baltimore in February

1954. We quickly became close friends, not only because of clinical and scientific interactions, but also because of music. Proctor had a professionally trained baritone voice and remains a singer in his eighties. I loved accompanying him on the piano, particularly for German Lieder. Proctor is a remarkable renaissance man — a multidisciplinary physician, scholar, scientist, musician, sculptor, philosopher, speaker, and writer; he is a man too great for medical politics.

Before becoming chief of anesthesiology at the JHH, Proctor had been a well-known otolaryngologist and bronchoesophagologist, with a reputation in pulmonary physiology research. ENT was a division of surgery at Hopkins. In 1952, with all anesthesias given by nurses since the departure of Lamont and Harmel in the late 1940s, the JHH was reportedly under pressure from various sources to have physicians direct the nurses. The chairman of surgery, Alfred Blalock, had asked Proctor to learn some anesthesia quickly at Penn and then become chief at the JHH. Unfortunately, because Proctor entered anesthesiology via an incomplete residency with Dripps, he himself was not eligible for the anesthesiology board examinations. The board, therefore, did not accredit his department's residency program. Proctor began looking for associates in 1953. Walter Dandy had come and gone. Leonard Bachman, after pediatric anesthesiology training in Boston with Robert Smith, became instructor in Proctor's department and then assistant professor. He and I became good friends. Bachman and I were board eligible and might have overcome the accreditation obstacle had the department survived. This depended on Blalock's wanting a strong anesthesiology department. The Johns Hopkins University and Hospital environment, in two different sections of town, were quite conservative and formal, although they sometimes recruited mavericks. For example, on my first day at the Hospital, I arrived at the doctors' breakfast room without a tie and was ordered to leave. I never returned to eat there. Later, I have in good memory evenings with guests at the main campus faculty club (not always wearing a tie), and being particularly impressed by the crab cakes and mint juleps.

The physician staff of the division of anesthesiology (of the department of surgery) at the JHH, in 1954, consisted of Proctor (in his second year as chief), Bachman, Safar, and Adriana Fenenga (a Dutch anesthesiologist). We had two residents, Steven Galla (who later joined Vandam as resident and then me in Pittsburgh as faculty member) and Jane Macquewen. Bachman's and my daily OR routine at the JHH consisted of helping nurse anesthetists under the overall direction of Miss Oliver Berger, understandably Blalock's favorite nurse anesthetist, since she began at the JHH before Lamont in the 1940s. Second in command of nurse anesthetists was Mrs. Mary Loucks, wife of an American physician at the Peking-Rockefeller Institute. Mrs. Loucks was a highly dedicated, delightful lady and a very competent nurse anesthetist. Most of the nurse anesthetists were adequately skilled to manage noncritical cases safely. Others, and those in training, caused quite a few life-threatening problems, particularly in critical cases. Physicians, however, can also make mistakes. We physicians were too few to be present for all anesthesia inductions and emergences. This was very frustrating, especially to Bachman and me. We administered anesthesia daily, mostly for various difficult cases.

For several months I gave many anesthesias for the blue-baby operations by Blalock — mostly with cyclopropane, closed to-and-fro system (Waters's CO_2 absorption canister), controlled ventilation, without relaxant. In the 1940s these were the first cardiac operations, with Lamont and Harmel as anesthesiologists. My OR work with Blalock I remember as smooth, even delightful. There was mutual respect. I admired him for his pioneering research on shock, and for his skills as a surgeon. Most of his associates were technically excellent, and, at least for me, fine to work with. This symbiosis in clinical work stood in contrast to our confrontations at the medico-political level, due to what we then perceived as an arrogance by Hopkins surgeons toward their nonsurgery colleagues. We anesthesiologists became increasingly frustrated when Blalock made his surgery residents "commandeer" our anesthesiology residents and staff. Proctor's department perceived that atmosphere as culture shock, compared with the collegial collaboration with surgeons we had encountered in Philadelphia at the HUP. Proctor taught us new features of pulmonary physiology. He obtained from Sweden a "Spiropulsator" OR ventilator.

Blalock's residents and associates included Henry Bahnson (who focused on the new aortic aneurysm resections), Frank Spencer, Jim Maloney, David Sabiston, Jerome Kay, and Thomas Starzl. Starzl, then an intern with Blalock, left Hopkins and rose to worldwide fame, via Chicago and Denver to Pittsburgh. I have very positive memories of OR work at the JHH with these skilled surgeons. Nonetheless, we anesthesiologists increasingly had clashes with Blalock and his residents when some gave our residents and staff a hard time. Looking back, we lacked the critical manpower to establish physician anesthesia.

Alfred Blalock was a giant as a leader in academic surgery and a successor of the legendary Halsted. I consider Blalock's most important scientific contribution to be his work in the 1930s, namely, the demonstration in animals that the cause of shock as a result of tissue trauma is hypovolemia, not nervous or other obscure mechanisms as previously believed. His second big "first" was the surgical mitigation of congenital cyanotic heart disease.

Helen Taussig (1898-1986) sometimes dropped in when Blalock and I worked together in the OR. She was the pediatric cardiologist who is credited with having "transformed the world of cyanotic congenital heart disease by correlating the anatomy of the defects with the clinical findings and by suggesting successful treatment, starting with the blue-baby operation then carried out by Blalock." She had suggested to Blalock in 1945 that tetralogy of Fallot be treated by creating a patent ductus. In 1982, Taussig's path crossed mine again when we both served on a Harvard-initiated committee that created and published "The Physician's Responsibilities Toward Hopelessly Ill Patients."[162,163] We subsequently corresponded. She sent me a letter concerning her relationship with Blalock, asking me to keep it confidential "until five years, perhaps ten years after we have died." In conversations we agreed, that at the JHH in the 1950s, surgeons ruled with an iron hand, and that the series of resignations of anesthesiology chairmen between the 1940s and 1970s (spontaneous or requested) was unfair and undiplomatic on the part of the JHH establishment.

The attempts to develop an academic anesthesiology department at the JHH lasted for 40 years. These attempts had begun with Austin Lamont and his only resident and assistant Merel Harmel in the late 1940s, and were continued by Proctor, Safar, and Bachman in the 1950s, by Donald Benson in the 1960s, and by Eugene Nagel in the 1970s. Finally, in the 1980s, almost two decades after Blalock's death, Mark Rogers and his associates succeeded in creating one of the strongest academic anesthesiology departments in the nation. His department's administrator Robert Johnson (who in the late 1990s became administrator of our department in Pittsburgh) remembers that by the mid-1980s, Rogers had replaced all nurse anesthetists at the JHH with physicians. This legacy continues under Roger Johns.

In 1954, Bachman and I cried on each others' shoulders almost daily. Blalock wanted nurse anesthetists to give the OR service and anesthesiologists to do research. While we did part-time laboratory research on dogs across the street, enabled by professor of pharmacology Kennerly Marshall, I always felt guilty about leaving the OR. Our lab research then primarily consisted of a dog study to explore the histaminic effects, hypotension and bronchospasm, of d-tubocurare.[45] We taught each other how to measure airway resistance and lung-thorax compliance.

The collegiality at Hopkins, outside of surgery, was great. Proctor, Bachman and I learned from and interacted very well with famous scientists in other departments, particularly pulmonary physiologists Otis, Riley, and Shepard. Philip Bard of physiology was another valuable colleague.

At the JHH I committed the second casualty of my personal clinical anesthesia practice, mostly because I tried to save face. "Fortunately" this was a hopeless cancer patient. The case has burdened my conscience lifelong. It was an ultraradical neck dissection and jaw resection for which I said I would minimize blood loss with hypotensive anesthesia, the technique I had used and studied in Peru.[43] When the ganglionic blocker failed to reduce the blood pressure (which sometimes happens), I pushed ether and caused cardiac arrest. Despite a prompt open-chest CPR attempt, the heart was not resuscitable. Given the hostile, confrontational environment between anesthesia and surgery at the JHH, I wanted to save face and could not admit that I had been unable to produce controlled hypotension as I had promised. The surgeon Kay, however, was collegial and consoled me. There were no accusations. We retained mutual respect.

In general, Blalock praised "improved clinical anesthesias" but also deplored arguments in the OR between anesthesiologists and surgeons. I remember smooth collaboration between us and nurse anesthetists, as I experienced earlier in Peru and later in Pittsburgh. In the spring of 1955, Bachman and I had reached a breaking point of frustration about Blalock's support of nurse anesthetists at the expense of physician programs. When surgeons were ready for the first open-heart operation with a pump-oxygenator, Blalock insisted that the anesthesia be managed by his chief nurse anesthetist, thereby overruling Proctor.

Seeing an article in the Cleveland Plain Dealer newspaper concerning nurse anesthetists, Bachman and I — young, cocky and frustrated — sent a letter to the editor expressing, among other things, the view that estimated anesthesia-related mortality in the United States resembled that of poliomyelitis (90,000 per year). Would it not be logical for physicians to concern themselves with this specialty? We sent a copy to Blalock. Furious, he ordered us to a confrontational session, with the hospital director, Proctor, and others present. Blalock requested that we withdraw the letter. We refused. We told Marshall about it. He advised us that, now that we had sent the letter, we should not withdraw it. I asked Blalock, "Do you or do you not want at the JHH a strong physician department of anesthesiology, with a strong residency training program?" He said "no." After that we all pondered whether we should stay at the JHH.

About two weeks later, we believe in April 1955, the case of an emergency laparotomy at night was the straw that broke the camel's back. Anesthesiology resident Galla called the staffman on-call (Proctor), who told him to do it with awake intubation, as he had learned, because of the well-known risk of regurgitation and aspiration. The surgery resident ordered our anesthesia resident to do it his way, with pentothal and succinylcholine and get going. Although the patient survived the risky method, Proctor wrote the surgical resident not to tell our residents how to anesthetize. Blalock got angry at Proctor and met with him and Bard (as a referee). Blalock told Proctor, "Why do you care? You are not even an anesthesiologist." (What an insult three years after Blalock was the one who had made Proctor take a shortened anesthesia training.) Proctor, principled and tough, resigned. When his anesthesiologists learned about these events, we all resigned, although we agreed to finish the academic year.

In hindsight, long after we had all decided to leave the department of surgery at the JHH and look for more appreciative environments elsewhere, we agreed that our decisions were principled but undiplomatic. In order to succeed with the establishment of physician anesthesia at Hopkins at that time we would have needed more money and spiritual support from the chairman of the department of surgery, in order to attract the number of top anesthesiologists required to create a viable residency program. Without that, academic anesthesiology had no future at the JHH. In contrast to some other chairmen of surgery, like

Ravdin and Koop at Penn and Mark Ravitch at the Baltimore City Hospital, Blalock did not seem to see the need for physicians guiding clinical anesthesia. Blalock seemed used to unsupervised nurse anesthetists who do not talk back. I believe that he agreed, probably under pressure from the outside, to recruit anesthesiologists. He wanted them to do research.

My one and one half years at the JHH as instructor and assistant professor was a frustrating time in my professional life. Yet in another way, it was exciting. I learned from Proctor and others. Except for that time at the JHH, I had been, since the end of my residency, always a department chief or chairman — until I gave it up at age 70. Anesthesiology in the 1950s and CCM in the 1970s were in dire need of leaders. Therefore, some of us could move from residencies straight into leadership roles, which were difficult but less frustrating than not being in charge while trying to make changes.

During that time at the JHH I studied vigorously and passed the Maryland State Board as well as the National Board exams, as one of the last foreign medical graduates allowed to take it. Both of these exams consisted of three parts, a written preclinical, a written clinical, and a practical bedside examination. The National Board Part I, which I had to take in Washington, D.C., reunited me with Severinghaus. John and Elinor hosted me in their little house in Bethesda, Maryland. At that time, John was with the Public Health Service (NIH) in lieu of military service, where he worked on the principal invention of his life, the PCO_2 and tri-electrode blood-gas analyzer.

I had stayed in touch, off and on, with my teachers at the HUP. When I visited Eckenhoff in the spring of 1955 to tell him about the revolution of our division at the JHH, he told me that at Penn, Deming had just resigned as chief anesthesiologist at the Children's Hospital of Philadelphia. I told him that Bachman, who had earlier pediatric anesthesia experience with Smith at Harvard, was available. Thus, I pride myself as a matchmaker who helped make Bachman the successor of Deming at

Children's Hospital of Philadelphia. Bachman started there in the summer of 1955. In 1968, one decade after our adult ICU at Baltimore City Hospital, Bachman initiated the first physician-staffed pediatric ICU in the U.S., at the Children's Hospital of Philadelphia. John Downes was co-initiator and assumed the ICU directorship.

In the summer of 1955, Proctor decided to go back to ENT. He was deeply frustrated and understandably hurt. He continued part-time research in pulmonary medicine with Solbert Permutt in the School of Public Health. He also extended his scholarly career into old age by writing impressive textbooks on the larynx, singing, and other pulmonary physiology topics. Even after Eva and I had moved to Pittsburgh in 1961, we stayed in touch with the Proctors in Baltimore, through musical connections. He contributed to one of my books[9] and lectured at our international CCM symposia in Pittsburgh.

When we decided to quit the JHH, I was offered several jobs elsewhere. A unique teaching opportunity arose when private-practice anesthesiologist Otto Phillips recommended the establishment of an academic full-time department of anesthesiology at the Baltimore City Hospital (BCH), across town from the JHH. (The BCH had its name changed several times. Before my time, and again now, it is the "Bay View Hospital," now part of Johns Hopkins Medicine). In 1955, through the recommendation of Proctor, I was offered a position as the first full-time chief anesthesiologist at the BCH. This appointment required and received reportedly enthusiastic approval from the BCH full-time department chiefs, particularly Mirick in medicine and Harrison in pediatrics — both affiliated with the Johns Hopkins Medical School. Blalock continued my assistant professor title in surgery, without salary, and without controlling me. BCH had academic affiliations with both the Johns Hopkins University and the University of Maryland, but was administratively independent of both, under the city government. BCH offered me a chief's salary of about $12,000 per year (purchase value of about

$60,000 in the 1990s), one-third of private-practice incomes in town at that time. It gradually increased over the six years at BCH to about $17,000. Private-practice moonlighting was not allowed, and I would not have wanted to do it anyway.

For three years, administrators at Hopkins tried to recruit a successor to Proctor but failed because Blalock would not give up total control. Candidates included Bill Hamilton of Iowa and Jim Eckenhoff, then still at the HUP. I did not discourage them but gave advice on what to demand, such as administrative (fiscal) and academic autonomy in the medical school and hospital. While division status for anesthesiology within the department of surgery seemed to be no obstacle to progress at Penn or Iowa, I considered it an obstacle at the JHH. The chairman of surgery made the main difference. In 1959, the JHH reached an agreement with Don Benson of Chicago to take on this troublesome job — still as the chief of a division in surgery. Benson introduced prolonged controlled ventilation for crushed chest, which he had initiated earlier in Chicago together with Trier Moerch. In the mid-1960s, the powers at the JHH finally decided to make the division a separate department, but conducted a search for a new chairman, instead of merely changing the name "division" to "department" and leaving Benson as chairman. This was an insult. Benson quit — a major loss for the JHH. He returned to his alma mater, the University of Chicago, as department chairman. There he soon developed cancer and died. Had he stayed at Hopkins, he might have succeeded.

After heroic efforts to establish an academic department of anesthesiology at the JHH by Lamont and Harmel in the 1940s, and by Proctor, Safar and Bachman in the 1950s, Eugene Nagel tried in the 1970s. (Nagel, one of America's EMS pioneers, had communicated with me regarding EMS developments at the national level; I turned over to him the chairmanship of the first ASA Committee on Acute Medicine, which I had started in 1965.) Why Nagel left the JHH I do not know. In the 1970s the spirit of Blalock was still there. In the 1980s, Mark Rogers succeeded. I summarized the history of four decades of anesthesiology at Hopkins in a document requested by Johns Hopkins historian, the late McGehee (Mac) Harvey, professor emeritus of medicine. I sent copies to Mark Rogers, Len Bachman, Merel Harmel, and Don Proctor. That history includes copies of Lamont's documents of the 1940s.

Baltimore City Hospital, July 1955–May 1961

After the unhappy year at the JHH, my six years at the Baltimore City Hospital (BCH) were the happiest in my professional life. They were exciting, academic, collegial, productive, and formative. They were packed with events and with national and international recognition and visitations. Modern resuscitation and intensive care were born then, and much of it there. Anesthesiology fathered a great deal of it.

Anesthesiology at BCH, 1955-61

In the 1950s, the BCH was a city-supported charity hospital, with all staff physicians having academic positions in one or both of the universities in town. The BCH department chiefs, however, were independent and had their own residency programs not controlled by chairmen at the JHH. Surgery was controlled mostly by the University of Maryland until July 1956. Anesthesia at BCH had never employed nurse anesthetists. Anesthesia was provided by surgical residents with off-and-on supervision by rotating part-time anesthesiologists who worked most of the time in private practice in other hospitals. Otto Phillips was their coordinator. Phillips's group, which included Al Nelson and Ted Stacy, was committed to the private practice of anesthesia in various hospitals throughout Baltimore, increasingly focusing on the Women's Hospital. Their reputation was superb. Several physicians, particularly Phillips, talked the BCH administration into initiating a full-time department of anesthesiology. This led to my appointment as the initiating chief.

I started the BCH position on July 1, 1955, with my assistant professor title at the Johns Hopkins University continued, and with a new associate professor title at the University of Maryland. After the experience at the JHH, I made sure that I had departmental status at the BCH. Some (not all) medical students from both universities rotated through various specialties at BCH, now also through anesthesiology. With all physician anesthesiologists having resigned from the JHH, I had no counterpart in anesthesia at Hopkins. The chairmen of Anesthesiology at the University of Maryland, however, Dodd in the 1950s, followed by Martin Helrich, were exemplary department chairmen and fine colleagues. Until 1955, surgery at BCH had been under Otto Brantigan, Sr., surgeon at the University of Maryland. (He was then the first to perform "bullectomy" reduction surgery on emphysematous lungs, a procedure that after him lay dormant for 40 years and is now being re-invented.) In 1956, owing to Baltimore academic politics, the University of Maryland and Brantigan gave up control over surgery at the BCH. Hopkins-trained Ravitch came to BCH from New York. All department chiefs at the BCH supported us, particularly Harrison in pediatrics, Mirick and Chinard in medicine, Pollack in pathology, Beacham in tuberculosis, McQueen in neurosurgery, and Ravitch.

In 1956, as a member of the BCH executive committee, I voted for Mark Ravitch (then in New York), Johns Hopkins's principal candidate, to become full-time chief of surgery at BCH. Before I voted, I had a phone conversation with him about the tragic impasse between surgery and anesthesiology at the JHH. I told him that I expected no replay of what happened at the JHH. He regretted very much what had happened there, and expressed the belief that it would not have happened had he been there at that time. (Ravitch had trained at JHH much earlier.) After his arrival at BCH, Ravitch and I quickly developed not only mutual respect, but a lasting friendship. Ravitch was a most scholarly professor of surgery, who became a giant in his discipline. After I left BCH for Pittsburgh in 1961, Ravitch stayed on for a while, then moved in the mid-1960s to the University of Chicago, from where he was recruited to Pittsburgh by Pittsburgh's chairman of surgery, Bahnson, with my strong encouragement.

The medical executive committee ran BCH within the fiscal restrictions of the city, through cooperative nonphysician administrators, first Mr. McMillan and later Mr. Hubbard. The latter was most progressive. We were outside the ivory towers' back-biting politics and free from the income-orientation of private practice. Although impoverished, BCH was the happiest and most productive academic environment. Soon after my departure in 1961, however, it changed and became fully controlled by the JHH, with integration at departmental levels. Chiefs assigned by JHH departments include now anesthesiologist Sieber.

In 1955 at BCH, I would have hired nurse anesthetists but did not have the budget for it. I had excellent experience with nurse anesthetists in Peru, and both good and bad experiences with nurse anesthetists at the JHH. In 1955, I managed to cover the not-overwhelming workload at BCH with our staff, surgery residents, block-assigned for several months to anesthesia, and my first anesthesiology resident, Lourdes Aguto. Rotating surgical residents, particularly Felicien (Felix) Steichen, administered general anesthesia under my supervision. There were four ORs, and, during my years at the BCH, there were only about 3000-4000 operations per year. During the first two years, I came to the hospital on nights and weekends for most emergency cases. Steichen remembered, "Peter was chief, resident, and intern all in one." I moved my family to our first purchased home, modest and closer to BCH, in northeastern Baltimore (4716 Meise Drive; price $12,000 for 2 x 1,200 square feet, paid through a minimal down-payment we had saved, and a 4% loan).

At that time, the anesthesiology board required a residency program of only two years. My department was immediately inspected and accredited for residency training. We began with two and ended with about six residents at a given time. In the

1950s, few U.S. medical school graduates went into anesthesiology. At the BCH we garnered an international mix of residents; between 1955 and 1961, my associates and I trained 17 residents (4 of them U.S. graduates). Twelve went on to practice in the United States.

Lourdes Aguto, a rotating intern at the BCH (from Manila) expressed interest in anesthesiology training when I arrived, and became my first resident in July 1955. Aguto (her later married names were Escarraga and Africa) turned out to be a superb physician. Despite her slight stature, she hand-ventilated even heavy patients "artistically" for hours; we had no OR ventilators. She helped with and became co-author of our landmark projects on CPR steps A and B. After four years with me at the BCH, Aguto became a fellow with William Howland at the Cancer Memorial Hospital in New York and returned to Manila in 1961. There she became chief anesthesiologist at the Philippine General Hospital and director of the World Health Organization anesthesiology training program. Her career peaked as professor and chairperson of anesthesiology at the University of the Philippines, succeeding Gomez.

We started at BCH with two staff anesthesiologists' salaries. As the only anesthesiologist at BCH in 1955, I searched for an associate. I was able in 1956 to attract Francis Chang, a recent graduate from Dripps's department, recommended by Lamont. This very skillful anesthesiologist and fine gentleman was an immigrant to the U.S. from China. After two years at the BCH, he went into private practice in Baltimore with Walter Dandy, Jr. In 1957, I recruited to BCH Thomas De Kornfeld, an anesthesiology graduate from Beecher's department in Boston. His family was originally from Hungary. The surgeons jokingly called our department "the Austrian-Hungarian empire at BCH." Influenced by the analgesia work of Beecher at Harvard and Louis Lasagna of Johns Hopkins, Tom De Kornfeld became primarily interested in and made significant contributions to the unbiased evaluation of postoperative analgesics. When I went to Pittsburgh in 1961, De Kornfeld briefly took over my position as chief at BCH, and then left for a better position in Michigan, where his primary interest became respiratory therapy.

A triumphant find for reanimatology was Joseph Redding, whom I recruited in 1958 when a third staff salary was approved. I am proud of having introduced Redding into resuscitation research. This was one and one half years after I had started human resuscitation experiments at BCH. Redding, a son of the South and a scholar of the Civil War, had been a general practitioner in Georgia. Although severely handicapped from early childhood polio, which had left him with both legs weakened and severe kyphoscoliosis, his upper extremities were strong. He completed an anesthesiology residency at Chapel Hill, North Carolina. He was a remarkable man. He and his family became our good friends. I met him first at the Southern Society of Anesthesiology meeting of 1957 in Gatlinburg, Tennessee. I believe I presented a paper there on our mouth-to-mouth volunteer experiments at BCH, and he impressed me with a resident paper on a laboratory study of cardiac contractility. Since he was at the end of his anesthesiology residency and had not yet decided where to go next, I found it rather easy to recruit him to the BCH. I immediately introduced him to resuscitation topics and research, encouraging him particularly to take on a laboratory program that we were ready to begin, with space provided by Nathan Shock, NIH gerontology researcher at BCH. Over the next few years, we developed a continuously active resuscitation animal laboratory, actually a Resuscitation Research Center. After I left for Pittsburgh in 1961, and De Kornfeld left for Michigan, Redding became chief anesthesiologist at the BCH. When the BCH lost its identity, Redding went via Omaha, Nebraska, to respiratory care in Charleston. For his research at BCH he received an AHA CPR award. He died in 1984.

The fourth staff anesthesiologist at the BCH was John Pearson. He had come from the United Kingdom. I appreciated his skills, collegiality, and humor. Although we had only three staff salaries, I could afford him because of my U.S. Army grant for resuscitation research and a slight boost for our meager salary budget for providing occasional anesthesia at the city jail. Pearson particularly "enjoyed" that. After I left for Pittsburgh, Pearson and Redding continued important laboratory research on resuscitation in dogs. When Redding left Baltimore, Pearson went into successful private practice in Hawaii. Thirty years later, in 1994, he graciously came to Pittsburgh in 1994 for my 70th birthday and resuscitation researchers' symposium. We clicked as if we had left BCH only yesterday.

Our BCH residents included Ruben Tenicela from Peru (who later became a professor in Pittsburgh); Sadao Morikawa from Japan (who contributed to our research); Robert Abraham from the U.S. (who became chief obstetric anesthesiologist at the JHH); Warren Holtey; Robert Ireland; L. Bondoc; Irma Gedang; Iraq Haghighat; Samuel Hernandez; Karol Hoffman; P. Layug; William Matusky; Park; Heldi Rink; and Adele S. We trained the chief nurse anesthetist of South Vietnam, Miss Li Thi Thuan, like a resident. Johns Hopkins medical students K. Cozine, R. Cozine, L. Drawdy, J. Bennett, and G. Voigt became research fellows. Laboratory assistants Diana Keyes and Gloria Carter and secretaries Lorraine Kowalewski and A. Logan were important team members for our simultaneously ongoing service and research programs. Morikawa gave me a souvenir plaque in Japanese which said "every day something new."

The work with surgeons at the BCH was uniquely pleasant. Elsewhere at that time, confrontation of some sort was common between anesthesiologists and surgeons. At the BCH we collaborated as friends. This collaboration extended beyond the OR. Having my office in a broom closet next to the ORs, needing additional office space for staff, and both Ravitch and I needing a seminar room, we repeatedly unsuccessfully appealed to the hospital administrator for more space. When that failed, without permission, chief surgeon Ravitch joined chief anesthesiologist Safar in taking up axes and shovels to tear down the big steps of an unused lecture room. We were not arrested, but could keep the new space we needed. The fact that Ravitch and I became friends and that we were department chiefs at an equal level also helped the house staff of the two departments to gain amiable collaboration in the OR, in the ED, and in America's first full-time physician-staffed ICU.

Felix Steichen, a native of Luxembourg, was a surgery resident with Brantigan and, later, a resident and then faculty associate with Ravitch. Subsequently, he joined Ravitch and me in Pittsburgh. Steichen became a well-known professor of surgery who copioneered stapling and minimally invasive surgery. Other surgery residents included Robert Wilder, an American (whom I motivated to help us promote resuscitation and EMS); Peter Weil (a native of Vienna); Benjamin Rush; and Steenburg.

For a few months in 1957, Niren Das from India was a visiting anesthesiologist to BCH. He was chief anesthesiologist in the Indian Army and had experience with field resuscitations of United Nations troops in the Korean War. Finding no physician anesthesiologist to talk to at the JHH, he came across town to watch anesthesia at BCH and talk with me. During that time I conducted mouth-to-mouth experiments on curarized human volunteers. Das helped. We became friends. In March 1957, having previously passed the written Anesthesiology Board examination, I was to take the oral examination in Asheville, North Carolina. I invited Das to come with me to see another part of the country. We were both naive about segregation south of Baltimore. Even in Baltimore, I was unaware that Black citizens still had to sit in the back of buses. On our way south, Das and I stopped at restaurants and motels, where I was surprised to see waitresses and others stare at him and me, first with threatening hostility — most likely because of his dark skin. That I had paid no attention to. They acted as if they were looking at a man from the moon — "you dare..." —. When they heard his British accent, however, these white southerners immediately changed their expressions and became friendly toward both of us. My board examinations went smoothly. Examiner Ron Stephen was particularly thoughtful and collegial — a real gentleman.

In those years, there was still a practical exam to follow, wherein a board-appointed anesthesiologist visited the candidate's environment. Among several cases in which I administered anesthesia at the BCH on the day of that board visit, one case did not go that smoothly. Although I felt confident about my skills with the use of cyclopropane, ether, and relaxants, I had little experience with N_2O-O_2-trichloroethylene, the only nonexplosive (but not relaxing) inhalation anesthetic then available. It was administered via a nonrebreathing system, with spontaneous breathing. (This was before the arrival of halothane.) I picked this rare technique to show that our residents are learning even "odd" ways of doing it. Trichloroethylene also tends to cause arrhythmias, which with no ECG monitors available then, were detected only by feeling the pulse. The patient moved. I saved face with a "dash" of thiopental. Ultimately, all went well, particularly the other anesthesias I demonstrated. I passed. A visitor was impressed with what I call "surgery-adapted manual-controlled ventilation," that is, adapting each lung inflation volume and timing to the surgical manipulations.

Research during my first year at BCH was minimal and concerned pulmonary physiology. Right after I arrived at the BCH, with Aguto's help, I extended our canine lung-thorax compliance studies of the previous year,[45] to compliance studies on patients.[46] Another anesthesia-related innovation was a modification of a regular rigid bronchoscope for ventilation.[47] At that time there were no flexible scopes. I added a large-bore side arm with removable window. This ventilation bronchoscope, I later learned, had been introduced earlier in Austria. I became interested in simplified anesthesia methods for primitive conditions, such as we had encountered in Peru. This was stimulated by the news from England about the Macintosh-Epstein air-ether inhaler with bellows. I replaced the bellows with the self-refilling bag-valve-mask unit, which Ruben of Denmark had just introduced. First, I assembled and tried out on patients a combination of the self-refilling bag-valve-mask unit with a modified Flagg can ether vaporizer.[48] Then Pearson and I used this with more sophisticated draw-over vaporizers.[49] This setup was a predecessor of the portable tri-service anesthesia apparatus of the British Armed Forces, finally acquired in modified form also by our Armed Forces.

For respiratory care, which then was in its infancy, I recruited at BCH in the late 1950s, the help of our first "respiratory therapist," Edward Hill, a skillful orderly whom we trained to assist with resuscitations hospital-wide and (later) in the ICU. The first respiratory therapist training programs elsewhere in the 1950s focused on chronic lung disease patients rather than on resuscitation and intensive care.

Our daughter Elizabeth made respiratory care a personal topic. She was born prematurely in August 1954. Soon thereafter, I diagnosed Elizabeth's asthma, as she vomited thick mucus (which came from the lungs and was swallowed) and wheezed. She had the worst case of asthma; almost every night during her difficult life of 11 years, she needed help from Eva or me to clear her lungs. While we were around, we kept seemingly life-threatening crises to a minimum. Austin Lamont suggested a respiratory specialist in Philadelphia, who taught Elizabeth and us how to use broncholytic aerosol inhalations, a new approach then. I provided Elizabeth with oxygen in her room at home. Her pediatric allergists suggested "deparentalizing" because of a vicious cycle of fear and tension between child and parents. We arranged for Elizabeth to spend the summer of 1957 in Austria, and the year 1958/59 in an asthma sanatorium in Davos, Switzerland. It did not work. We loved her deeply but we should have shown it more. She was difficult and we were immature. Oral prednisolone caused complications; inhalant steroid became available 10 years after her death. Her well-intentioned pediatricians and allergists, who just saw her briefly now and then, had no idea what her daily life was like. Eva and I going away together was associated with fear; it was made possible through a loving, reliable babysitter family we trusted — the Jakovics, immigrants to Baltimore from Latvia.

House music evenings in Baltimore, with various colleagues (amateurs and occasional professional musicians), are among our fondest memories. Initiated by Eva, they started at our little house in northeastern Baltimore. I still play on my family's Boesendorfer piano, a 1919 baby grand, which my parents, with considerable financial sacrifice, renovated and shipped from Vienna to Baltimore in 1954. These musical evenings, which we later also held at the Dandy's ultramodern new home and at the Bensons,.included as performers, Don Proctor, Paul Hacket, Don Benson, Margaret Benson, John Clements, Margot Clements, Eva Safar, Helen De Kornfeld, Adele Kruse, Virginia Apgar, and me (amateur pianist). The peak performance of our music group, before we left Baltimore, was Brahm's Liebeslieder Waltzes, by four voices and piano duo (Margaret Benson or John Clements and Peter Safar).

Resuscitation Research at BCH, 1956-61

Until 1956, my interests in resuscitation were clinical and educational, in-hospital, and involved use of the well-established open-chest CPR method.[5,35,41,50] The spark that started my life-long commitment to resuscitation research, including pre-hospital first aid, was a chance contact with James Elam (fig. 10). It happened at the ASA meeting of October 1956 in Kansas City.

Until then, I paid little attention to what pre-hospital first-aid personnel were taught about providing artificial respiration or to how they were taught. For the previous 100 years, various chest-pressure arm-lift methods had been used and, since the early 1950s, the back-pressure arm-lift method, because of data obtained by Archer Gordon[51] on curarized intubated volunteers. Until that ASA meeting of 1956, I had not been aware of a 1954 publication by Elam[52] that documented normal blood-gas values in apneic patients during IPPV with mouth-to-mask or mouth-to-tracheal tube ventilation.

Eva and I drove from Baltimore to that ASA meeting in Kansas City via Iowa City, where we visited the Blodis[29] and anesthesiologists Cullen, Hamilton, and Severinghaus. John Severinghaus was spending his second year of residency at Iowa. He gave me something new to inhale — halothane, from a noncalibratable draw-over bottle. Having previously tested on myself inductions with ether, cyclopropane, N_2O, etc., I thought this was a breakthrough — nonirritating, smooth, fast, and nonflammable. At the Kansas City meeting, halothane was the hit: it had just been introduced from Britain. Upon my return from Kansas, I immediately introduced it at the BCH.

At the ASA meeting in Kansas City on October 12, 1956, Eva and I had supper with James Elam. This was the first time I talked with him. Elam asked to hitch a ride back to Baltimore with us. That ride on October 13 and 14, 1956, from Kansas City via Chicago to Baltimore, became the birth of modern resuscitation. I thank Elam for having sparked my interest in researching resuscitation. It changed my professional life's orientation from focusing on OR anesthesia and teaching resuscitation in ORs, to extra-OR resuscitation and life support. Eva drove while patiently enduring our shop-talk for two days. I was inspired by Elam's first published proof that exhaled air is adequate resuscitative gas,[52] and by his imaginative personality. He was interested in ventilation with exhaled air because he (like myself and other anesthesiologists) had instinctively, for years, done brief mouth-to-tube inflations on anesthetized or even polio-paralyzed patients. Also, Elam worked at that time with the U.S. Army Chemical Warfare Center in Edgewood, Maryland, near Baltimore. The chest-pressure methods were too weak to ventilate nerve-gas-poisoned animals with bronchospasm and bronchorrhea.

Elam's story revealed to me that his results[52] were not then widely known. Although he later wrote that he had "besieged

Figure 10. James Elam, M.D., in the 1950s. He "sparked" me into research on "rescue breathing" and first aid. He was an imaginative researcher at the U.S. Army Chemical Warfare Center in Edgewood, Maryland, and chief anesthesiologist at the Roswell Park Cancer Hospital in Buffalo, New York.

the Surgeon General of the Army and the Red Cross to recognize the method of Elisha"[1] he gave me the impression during that trip that neither he nor others had challenged the then taught doctrine; he told me that first-aid agencies adhered to teaching the back-pressure arm-lift method.[51] It occurred to me that anesthesiologists' know-how in handling upper-airway obstruction had not penetrated beyond the OR. (Paluel Flagg's transient one-man campaign in New York to teach tracheal intubation to ambulance personnel was an exception.) My relationship with Elam moved unintentionally from friendship to competition to collegiality. Collaboration with him would have been difficult, because we both had domineering personalities. Moreover, he focused increasingly on one device at a time, while I became interested in systematically pursuing the pathophysiology of dying and resuscitation, and promoting life-saving efforts with a global perspective. The following is the way I remember it.

On that historic trip in October 1956, the (to me) needed studies and documentation had not been done. I proposed to act as rapidly as possible on the following: 1. A study of the mechanisms of upper-airway soft-tissue obstruction, which the manual methods cannot control, as they leave no hands free to support head-jaw position. Elam had used mask, pharyngeal tube, and tracheal tube, and no backward tilt of the head in his landmark study.[52] 2. A controlled study was needed in which ventilation volumes produced by the "push-pull methods" would be compared with those produced by direct mouth-to-mouth inflations, using both methods on the same adult curarized volunteers and on patients, without the use of a tracheal tube, with each subject serving as his own control. I considered the measurements made in the past via tracheal tubes[51] to be irrelevant. 3. A study of the feasibility of direct mouth-to-mouth ventilation (without devices) by lay persons, as tested on curarized adults was necessary. Elam had not reported on mouth-to-mouth without devices.[52] I had no intention to steal the initiative from Elam, but, in his position at the Army Chemical Center, he seemed neither interested nor able to pursue the above plan. I suggested that we do it together. I invited Elam to attend and advise on these experiments. He attended some of the initiating experiments and then moved to Buffalo and continued on his own, although we maintained communication.

Immediately upon my return to the BCH, in November 1956, I initiated the planning and conduct of studies on human volunteers, on what later became step A (airway control) of modern resuscitation, studies mostly on anesthetized patients,[53-55] and simultaneously, studies on what later became step B (breathing control by direct mouth-to-mouth or mouth-to-airway versus chest-pressure methods).[56-60] I conducted the first such volunteer experiment on Saturday, December 8, 1956. We worked, with the help of Lourdes Aguto-Escarraga and our other anesthesiology residents, "on victims" on the floors of the ORs, which on weekends were unoccupied.

John Clements, a brilliant civilian researcher at the Army Chemical Center, was a physiologist who then began to pioneer the effects of pulmonary surfactant. I asked him for advice on monitoring our subjects. He made good suggestions on our improvising a pneumograph, which I insisted we needed to record tidal volumes during IPPV with the face free for direct exhaled-air ventilation. We placed one pneumograph tube over the chest and another over the abdomen. I could show that this allowed calibration of tidal volumes during IPPV in the paralyzed subject. To monitor tidal volumes produced by chest-pressure arm-lift, I used a regular bell spirometer connected to a taped-on oronasal face mask, taking care not to interfere with the natural head and jaw position. I asked Arthur Norris, a BCH physiologist at the geriatric research center, to provide ear oximetry. We also had an infrared Liston-Becker end-tidal CO_2 monitor. Both had been available since the 1940s for research, but not for routine patient monitoring.

To strengthen my hypothesis while avoiding possible bias, I had the back-pressure arm-lift methods performed by[18] professional rescuers and the exhaled-air methods by untrained laypersons. For the former, I consulted the BCH administrator McMillan who suggested soliciting the collaboration of Captain Martin McMahon, chief of the Baltimore City Fire Department ambulance service. For performance of the exhaled-air methods, I recruited 167 laypersons including my wife Eva and Boy Scouts, all "trained" only by one live demonstration by myself on the subject.[59]

While I did most of the management of the volunteers myself, I was helped, particularly with monitoring, by Lourdes Aguto (our first anesthesia resident), Lois Drawdy (medical student), my associate Chang, and visiting anesthesiologist Niren Das, as well as other anesthesiology residents. Later, airway roentgenograms were done by radiologist De Carlo.[55] In 1959, my associate Redding also helped,[54] but more in the ICU than with the remaining airway-control experiments. There were outside visitors at these weekend experiments, off and on. They included Earl Breon of the American Red Cross, who helped with the subsequent rapid acceptance of mouth-to-mouth by the American and International Red Cross organizations.

In 1957, physiologist Bruce Dill, scientific director at the Army Chemical Center in Edgewood, Maryland, invited me to present our preliminary data there. Since Eva and I did not then have U.S. citizenship papers, which made it illegal (presumably for security reasons) for me to enter the premises of that center, he instead held that meeting in his home nearby.

Lois Drawdy, a brilliant Johns Hopkins medical student, was our part-time research fellow on these studies and for surgical research by Ravitch's associates. Drawdy taught me about writing scientific papers. She was "rescuer" in our Army-made documentary CPR movie.[61] She had the potential to become a clinician-scholar, had she not suffered from a psychiatric illness. In 1961 I offered her a laboratory assistant position in Pittsburgh, but she wanted to finish medical school at Johns Hopkins. In 1963 we received the shocking news that she had died in Baltimore. She is remembered by resuscitation researchers.[5,61]

Why did I use curarized unintubated human volunteers, a rather risky choice of subjects? The choice of human volunteers was crucial in obtaining convincing data. The 31 physicians, medical students, and one nurse who volunteered for 49 experiments[56-60] were extremely well informed after watching me manage the same experiments on other volunteers, and manage curarized unintubated patients. I had personally experienced the drugs I used but had not undergone the chest-pressure methods. Young and arrogant, I did not trust others with life support, although others trusted me. Patient volunteers would have been less informed. Moreover, using them in simulated field conditions for several hours, with each subject as his own control, paralyzed on the floor, would have been neither feasible nor justified. Conscious breath-holding volunteers would keep their airways open; they subconsciously breathe spontaneously with the maneuvers. Intubation, as used before by Gordon,[51] would bypass the crucial obstacle — upper-airway soft-tissue obstruction by the base of the tongue. Dogs have a different chest configuration from that of humans and a straight, open natural upper airway. Although the volunteers received about $150 per experiment (current equivalent about $1,000), most volunteered mainly for the cause rather than the money.

By 1958, when the main volunteer data had been obtained, Blalock and the dean of the Johns Hopkins Medical School forbade me to use medical students as volunteers. The reason quoted was the "unjustified" use of a prophylactic broad-spectrum antibiotic. This I used in case of unrecognized bacterial transmission from the rescuers' exhaled air. The prophylactic use of antibiotics was then, and is now, a debatable issue.

For the main study, each volunteer was sedated and paralyzed for several hours.[58-60] For sedation I used large intermittent intravenous doses of scopolamine (0.5 mg increments, up to 2 mg) and meperidine (50 mg increments to a 600 mg maximum). For full neuromuscular blockade I used a titrated intravenous infusion of succinylcholine, to a total of 1 to 3 grams, for one all-day experiment with 2-3 hours of paralysis each — without a tracheal tube! I personally managed each volunteer, using N_2O:O_2 50:50% for interval ventilation. I then had several lay operators ventilate the volunteer with various manual and positive-pressure methods.

Oxygenation was guaranteed by filling the lungs with 100% oxygen before 1-2 minute runs of (usually ineffective) ventilation with the back-pressure arm-lift (Holger Nielsen), chest-pressure arm-lift (Silvester), and chest-pressure (supine or prone) methods.[58-60] These manual methods were performed by experienced Red Cross instructors. Exhaled-air ventilation was performed by laypersons ranging in age from 10 years to elderly. Their only training for this consisted of one demonstration by me on the volunteer and/or illustrations.[59] Only at the end of the experiment did I intubate the trachea to obtain control measurements and to ensure that the lungs were clean.

The volunteers suffered minimal or no side effects. They ate supper the same evening, which rarely caused nausea. Most recovered rapidly, faster than one would expect for surgical patients with similar anesthesia drugs and doses. There was one exception. One special, courageous volunteer was Richard Fredericks, then a BCH pathology resident. Fredericks volunteered for the first time on April 19, 1957, again in 1958, and then again in 1960 for the shooting of a second documentary Army film,[61] which documented steps A and B, and also demonstrated Kouwenhoven's step C. In 1960, Fredericks had already moved to New York. He came to us by train and, not wanting to let me down on this important film-making, he did not mention that he had a gastrointestinal upset. He silently regurgitated at the end of the experiment. Although I did thorough tracheobronchial irrigation and clearing via tracheal tube, he suffered aspiration pneumonia, from which, fortunately, he fully recovered. This was the only mishap in these studies.

The other special, courageous volunteer was Felix Steichen, chief surgical resident at BCH at that time. He said he trusted me because of my personal management of his curarized patients. He volunteered for the first time on April 28, 1957, and later for a second and third time. He was the "star" of our first preliminary documentary film made on July 13, 1957. (Ravitch, who supported these experiments enthusiastically, had suggested making a documentary film.)

During the performance of the manual methods, we found various airway obstruction patterns.[59,60] Our results confirmed my suspicion that chest-pressure alone cannot reliably ventilate. The most dramatic demonstration of the efficacy of direct mouth-to-mouth ventilation were tests in which we permitted a short period of apnea to cause a moderate decrease in arterial oxygen saturation to about 80%, as monitored by an ear oximeter.[59,61] A few rapid and deep mouth-to-mouth inflations restored normal oxygen saturation within seconds. We proved that exhaled-air ventilation, even as performed by Drawdy for one half hour on a 200-pound volunteer, was strenuous but not impossible;[61] that lay people would not hesitate to perform direct mouth-to-mouth ventilation on adult strangers;[59] and that the prone position does not make the tongue fall forward.[53,55,58,61] In subsequent, more detailed studies of airway control without tracheal tube, we documented a valve-like behavior of the soft palate, which allowed lung inflation through the nose, but not exhalation unless the mouth is opened.[54] We also found the most effective natural airway control maneuver to be what I called the "triple airway maneuver," namely, a combination of backward tilt of the head, forward displacement of the mandible, and separation of the lips.[53-55]

The exact sequence of events during these eventful years, from 1956 to 1961, with research and innovations while clinical services had to continue, are difficult to reconstruct. We had started the resuscitation experiments without waiting for grant support. That came about in 1957, from the U.S. Army Medical Research and Development Command, then under Colonel Hamit, with spiritual support from the Walter Reed Army Institute of Research, then under surgeon Hardaway (of disseminated intravascular coagulation fame). I had applied to the Army following the suggestion of (and with supporting letters from) Elam and Dripps. Both were consultants for the U.S. Army. My application to the Army consisted merely of a long letter. I received quick approval for funding of $10,000, for what became the first and most productive year of my research career. That, in the late 1950s, equaled 1990s' purchasing power of about $50,000. In contrast to the inability of the NIH and other agencies to provide immediate pilot grants, the Army was flexible, quick, and visionary with its support. Army officials were not deterred by the fact that not even Lloyd's of London would insure us against mishaps. Our Army funding later also sponsored ICU and laboratory research at BCH and in Pittsburgh and ended in 1969 with $100,000 per year. That, in the 1960s, equaled 1990s purchasing power of about $300,000.

Our preliminary data in 1957,[56,57] and our definitive data in 1958 from the human volunteer experiments[3,53-61] which for the first time documented the failure of manual artificial ventilation and the efficacy and techniques of exhaled-air ventilation without adjuncts, were received as a bombshell in the U.S. and Europe. The National Research Council (NRC) held a meeting, chaired by Julius Comroe, in Washington, D.C., on March 8, 1957, concerning artificial respiration for children. I presented the data of our first experiments on adult volunteers. Mouth-to-mouth was recommended for children but not yet for adults. Archer Gordon learned about our preliminary results on adults at that meeting and quickly (I believe subsequently) gathered similar data on anesthetized children and adults.[62] The Army Chemical Corps' researchers, under Bruce Dill, met on May 10, 1957 in Denver; Elam's earlier[52] and my new research data[53-61] were hot topics.

The first public presentation and discussion of our definitive data on manual vs mouth-to-mouth ventilation at an open meeting occurred at the ASA meeting in October 1957 in Los Angeles.[2,57] That ASA meeting occurred during the week that the Russians launched Sputnik, thereby initiating the space age. Colleagues said later that my brief presentation on October 18, 1957 in Los Angeles launched what later became modern CPR.

This was followed by the meeting of the Federation of American Societies of Experimental Biology (FASEB) in February 1958. As a result of that presentation, pulmonary physiologist Richard Riley of Johns Hopkins recommended me for membership in the American Physiologic Society. Through the triumvirate of Elam (who after our experiments in 1956 independently resumed his own exhaled-air ventilation studies),[63] Gordon,[62] and Safar,[58,59] exhaled-air ventilation was accepted with unexpected speed. Our results achieved within one year a world-wide change from the teaching of the manual to the teaching of the mouth-to-mouth methods of emergency artificial ventilation.

In June 1958, we exposed colleagues at the American Medical Association (AMA) meeting in San Francisco to these data. At that meeting, I met for the first time the already-legendary surgeon DeBakey, who had an exhibit on news in vascular surgery next to our artificial respiration exhibit. Also at that meeting, I was invited by Robert Hingson to witness (as the only "outsider") the initiation of his "Brother's Brother Foundation." This organization set out to vaccinate the world against numerous infectious diseases, using jet injectors. I had no clue then that a decade later I would recruit Hingson to Pittsburgh.

The first international spreading of the gospel came at the Scandinavian Society of Anesthesiologists meeting in Gausdal, Norway, in September 1958. That meeting launched the change of resuscitation methods in Europe. Eva and I drove for the first time into beautiful Norway, starting in still-war-damaged Hamburg. In Gausdal, we experienced for the first time the warmth of hours of toasting and speechmaking, enhanced by aquavit — all Scandinavian pastimes. At the Gausdal meeting, anesthesiologist Bjorn Lind of Stavanger, Norway, heard my presentation on the artificial respiration data and my plea for training aids. I told him that our research sponsor, the U.S. Army, so far had failed to move on Elam's and my pleas to help us develop a realistic training manikin. Lind knew Asmund Laerdal of Stavanger, who owned and operated a company that made toys, dolls, greeting cards, and children's booklets. Lind thought Laerdal might be able to make a resuscitation training manikin and asked him to see me in Baltimore. Asmund Laerdal did so immediately, in the company of the Norwegian trade attaché in New York, Hans Dahll. (Dahll later became the chief of Laerdal USA).

At the end of that 1958 visit to Europe, my first since 1952, I was invited by the Austrian Society of Anesthesiologists and the Vienna Medical Society to present our data on manual vs mouth-to-mouth ventilation. Dignitaries, including my former chief Schoenbauer, congratulated me. Mayrhofer was gracious. I enjoyed the discussion of our preliminary documentary film of 1957 at Vienna's department of physiology, where I showed our rather dramatic film strip of deoxygenation in apnea and reoxygenation with mouth-to-mouth on curarized doctor Steichen. From that visit on, lifelong, my Viennese colleagues have asked me for advice on resuscitation, emergency care, and intensive care, and I learned from them.

Simultaneously with the artificial ventilation experiments, I tried to document the relative values of backward tilt of the head (that was a "first"), forward displacement of the mandible, and opening of the mouth (the "triple airway maneuver"), by carrying out studies of roentgenograms and tidal volumes on over 200 anesthetized patients.[53-55] For patient studies, only mutual trust and oral consent were required. After I had documented step A by head-tilt,[53,56] Elam confirmed it.[64,65] Illustrations by Elam before my airway studies[52] showed the head still in the mid-position. Usually he used an artificial airway under a mask, or a tracheal tube. Control of the natural airway under general anesthesia by forward displacement of the mandible had been mentioned or pictured in surgery textbooks since the late 1800s (the Esmarch-Heiberg maneuver). Simple backward tilt of the head was instinctively practiced by anesthesiologists when I was a resident. In addition to head position, I tested, introduced, and illustrated for teaching the "crossed-finger maneuver," the "finger-behind-teeth maneuver" (for trismus) to force the mouth open for clearing and for insertion of a pharyngeal tube or laryngoscope, and various ways of inserting pharyngeal tubes.[5]

There were, as to be expected, controversies, misunderstandings, and jealousies. The acceptance of direct mouth-to-mouth ventilation was resisted by authorities, rescuers, and physicians, some of whom objected on aesthetic grounds. Elam had recommended mouth-to-mask ventilation,[52] and produced a pocket mask. From the beginning of the human volunteer experiments in winter 1956/57, I introduced blowing through a Guedel-type oro-pharyngeal tube, to which I attached a mouthpiece, which later became the S-tube.[39,56] McMahon and I talked each other into using pharyngeal tubes already at the first volunteer experiment. Then Lamont suggested fusing adult and child Guedel airways.[39,56] The S-tube was followed by a multitude of inventors' and manufacturers' introductions of mouth-to-mouth adjuncts. The Johnson and Johnson company wanted to mass produce our S-tube, but I was opposed to patenting. At that time, most physicians refrained from controlling or limiting life-saving methods or devices with patents. Mr. Schackner of Johnson and Johnson and I had disagreements about the "Resuscitube." Elam warned against use of the S-tube, as being too dangerous for use by lay persons. The controversy was resolved by my insistence that lay persons should not be taught the use of gadgets, but that ambulance personnel should learn their use.[66] Another lengthy controversy was that between "mouth first" blowers (Safar, Gordon) and "nose first" blowers (Elam, Ruben). These petty controversies drained emotional energy. Mouth first, nose second became the standard because of data.[54,55] By the time the exhaled-air gadget controversies faded away, Henning Ruben of Denmark had introduced the self-refilling bag-valve-mask unit, a breakthrough in gadgeteering for resuscitation.

The second meeting on artificial respiration by the NRC was on November 3, 1958, after our definitive data had been published in *The New England Journal of Medicine*[58] and the *JAMA*.[59,62,63] At that meeting, chaired by visionary surgeon Sam Seeley, backward tilt of the head with direct mouth-to-mouth ventilation was decided upon as the preferred method of artificial respiration to be recommended, not only for children but also for adults.

Artificial circulation had to be produced by closed-chest cardiac massage (CCCM), also called external cardiac (chest) compression (ECC), to resuscitate victims of cardiac arrest outside of the hospital. ECC was first instinctively used by Koenig and Mass in Germany in the late 1800s, on laboratory animals and patients arrested by chloroform, presumably for producing ventilation and circulation.[1] It was not appreciated and then forgotten. In 1957, William Kouwenhoven, Johns Hopkins professor emeritus of electrical engineering, visited me to watch one of our artificial respiration experiments on human volunteers at BCH. He was then engaged in dog experiments on electric fibrillation and defibrillation of the heart, using external alternating current. It could restore heart beat without the need for CPR, if the shock was applied within two minutes of untreated ventricular fibrillation. Countershocks for longer ventricular fibrillation would not be followed by heartbeat because of myocardial hypoxia. Kouwenhoven conducted these experiments then in a laboratory loaned to him in the surgical department of Alfred Blalock. When Kouwenhoven visited me at BCH, he and I discussed, but could not decide, how one might produce artificial circulation without opening the chest. The need for such a procedure was obvious, considering the failure of artificial ventilation to restore a pulse once the pulse was not palpable, and the limitation of open-chest (direct) heart massage to being performed only by doctors in hospitals. When Redding joined me in 1958, I asked him to try to produce artificial circulation in dogs, using high-pressure IPPV. This had been suggested earlier by Ralph Waters of Wisconsin. Because that produced only trickle blood flow, we gave it up.

In 1958, external cardiac massage was rediscovered in Kouwenhoven's laboratory by chance. During ventricular fibrillation and immediate defibrillation experiments in dogs, Kouwenhoven's fellow, Knickerbocker, also an electrical engineer, made a crucial observation of an arterial pulse wave when he pressed the defibrillator paddles on the dog's chest. They then documented the efficacy of intermittent sternal compressions in dogs.[67] After these dog experiments, surgery resident James Jude at the JHH conducted the first clinical trials, aided by anesthesiologists under Benson.[68] The introduction at that time of halothane (without precision vaporizers) occasionally and accidentally provided transiently pulseless patients, as chloroform had for the "experiments" in Germany in the 1880s. The Kouwenhoven group was eager to publish their sternal compression data after a group of Russian visitors to the JHH vaguely alluded to the work of Negovsky's group in Moscow. Thus, this paper[67] became part of the cold war competition. Three years later, Knickerbocker and I visited Negovsky in a friendly, collegial environment.

The widely quoted landmark article on external heart massage by sternal compressions[67] took ventilation and oxygenation for granted. In 1960, at BCH, I therefore sought to examine that issue.[69] With or without cardiac arrest, sternal compressions alone could ventilate animals via tracheal tubes (dogs have straight upper airways), but not patients without tracheal tubes (humans have kinked upper airways that obstruct in coma) — and not even patients in cardiac arrest with tracheal tubes, probably because they often have stiff lungs.[69] I therefore combined — already in 1959/60 — steps A and B[58] with Kouwenhoven's step C,[67] into CPR phase I, "basic life support" (BLS).[66,69-72] Later in Pittsburgh, dog studies led by my associate Harris, documented the 1:5 and 2:15 ratios of ventilation:sternal compressions.[73] Although these ratios and many others gave similar results, they became the routine still taught worldwide.

Our findings that sternal compression does not produce ventilation were published in *JAMA*, but not until 1961.[69] Publication was delayed because the *JAMA* editor had sent the manuscript for review to Blalock (Kouwenhoven's sponsor). Blalock criticized (incorrectly) our spirometry method, as he probably perceived our data as critical of Kouwenhoven's paper, which indeed ignored (or took for granted) steps A and B. In Kouwenhoven's studies, ventilation was provided by tracheal tube and ventilator in dogs[67] and by Benson's anesthesiologists in patients.[68] We convinced the editor that our use of a Wright ventilation meter obtained valid measurements. It showed inadequate or no ventilation with sternal compressions alone.

Recently, our paper of 1961[69] gained renewed importance, when the AIDS paranoia made some colleagues question the necessity of performing mouth-to-mouth ventilation during BLS.[74] Would just pushing on the chest be enough? The American Heart Association (AHA) Guidelines Committee members of 1997 believed that chest pressure alone can move air, as measured in dogs, pigs, and rats with or without tracheal tubes,[74] while ignoring the only available human data to the contrary.[69] After all — they were published before Medline years. In the 1997 controversy,[75] we pointed out that we have documented repeatedly since the 1950s that patients have "kinked" airways, which obstruct in comatose patients and that the lungs are often abnormal to begin with, making chest pressure alone unreliable for ventilation, even with a tracheal tube in place.[60,69]

In the spring of 1959, Eva and I went around the world and worked in Indochina, under the auspices of "Medico" (the non-governmental "Medical International Corporation"); there, basic and preventive health care were more important than resuscitation. In the spring of 1960, I was invited for my second trip around the world (March 6 to April 17, 1960) to the World Congress on First Aid in Sydney, Australia, in the company of Walter Dandy.[70] I presented our new data on steps A and B in humans[53-61] and our new data on resuscitation from drowning in dogs.[76-79]

Dandy and I lectured and discussed. Kouwenhoven gave me permission to also present his external cardiac massage data on my world tour, just before its *JAMA* publication came out.[67] In Sydney, we learned about the military drills used by the Australian life-saving associations to teach and practice the manual methods of artificial ventilation. We were delightfully surprised at how they switched to the exhaled-air methods immediately. The Australians still had a frontier spirit that despised bureaucracies and organizational politics. We and their water-rescue experts tried to figure out ways to perform exhaled-air resuscitation in deep water. Rescue leader Alan Denny of Sydney kept visiting us for years to exchange ideas. These lectures in New Zealand, Australia, Indonesia, Ceylon, Egypt, Lebanon, and Vienna may have been the first on what I labeled the CPR-ABC (airway, breathing, and circulation) or BLS system. That trip launched a worldwide explosion of interest in external CPR. Walter Dandy described this world tour in a slide lecture.[70]

At the Maryland Medical Society meeting in Ocean City, Maryland on September 16, 1960, Kouwenhoven and I presented our data at sessions moderated by anesthesiologists Donald Benson of JHH and Paul Hackett of the University of Maryland.[71] I had already documented the need to teach steps A, B and C together.[66,69] Jude showed a film on defibrillation. After my remarks, Benson said, "Early on we thought that simple pressing on the chest would obviate...the need for artificial respiration. Gradually it has become apparent that this is not necessarily true and must not be counted on." I presented radiographs and our first (preliminary) documentary film (with Steichen as the volunteer) on deoxygenation in apnea and reoxygenation with mouth-to-mouth. This meeting did not "turn on" the audience. Practicing physicians seemed to have little interest in lifesaving measures outside the hospital. The details and sequences of events that led to what three years later was universally labeled "CPR" are somewhat different from the events described in the book *Life in the Balance*[80] by Mickey Eisenberg, the first

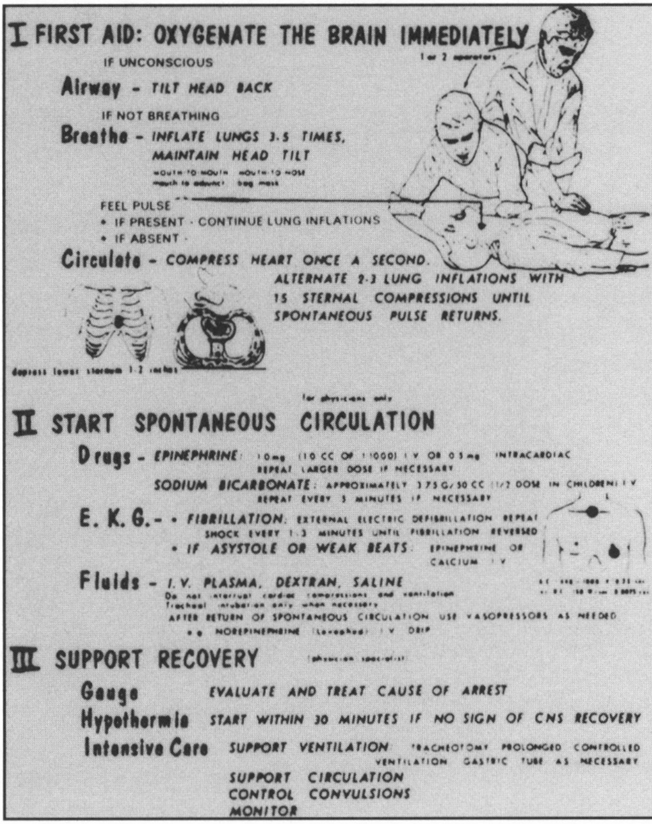


Figure 11. The A-B-Cs of heart-lung resuscitation, as illustrated by Peter Safar in 1961.[72] *This system was renamed cardiopulmonary-cerebral resuscitation (CPCR) around 1970.*[1-6] *phase I=basic life support (BLS); II=advanced life support (ALS); III=prolonged life support (PLS).*

EMS epidemiologist and an eloquent historian. Instead of worrying about who said what first, I learned from my late father to be grateful whenever progress is made. This is more important than "who gets the credit."

I was accompanied at that Ocean City meeting by visitors to our BCH department, anesthesiologists Airaksinen and Savolainen of Finland. They and other visitors to BCH at that time were on their way to the Congress of the World Federation of Societies of Anesthesiologists (WFSA) in Toronto that month.

There I presented for the first time our experiences with the ICU at BCH, initiated in 1958.[91]

By the fall of 1960, I already had conceptually extended the alphabet from steps A-B-C to steps D (drugs), E (electrocardiography), and F (fibrillation treatment) — transferred from open-chest CPR — which together I called advanced life support (ALS).[5,72] In 1961, I extended BLS and ALS further to prolonged life support (PLS), for which I extended the alphabet to step G (gauged), step H (humanized, brain oriented, and hypothermia [for cerebral resuscitation]), and step I (intensive care)[72] (fig. 11). Hypothermia had been introduced in the 1950s for brain protection (pretreatment) and preservation (intra-insult treatment) in patients,[36] but not for resuscitation (to reverse the insult and support recovery). We used moderate hypothermia, however, occasionally after CPR, already around 1960.[50] In the early 1970s I added "cerebral" and extended CPR to "cardiopulmonary cerebral resuscitation" (CPCR).[5,6,72]

Worldwide implementation of modern CPR depended on training not only physicians and nurses, but also the lay public. As mentioned above after the anesthesiology meeting in Gausdal, Norway, in September 1958, Asmund Laerdal visited me at BCH.[81] I gave his company our requirements for a life-size manikin to practice steps A and B, with airway obstruction on neck flexion and chest expansion on lung inflation. Within a few months, Laerdal showed me a prototype of the first (inflatable) Resusci-Anne manikin. Her face was Asmund's idea of the "girl of the Seine" (a suicidal drowning victim), a copy of a death mask, that Laerdal's parents had in their home. Having death masks on walls, like paintings, was a custom in Europe at that time.

Laerdal had started his toy and card company in 1940, at the beginning of the German occupation of Norway. This brought him once into a life-threatening confrontation with the Gestapo. Early after the war, the only medical product of the company was a kit with plastic wound molds for first-aid training. After Laerdal had created the (inflatable, life-size) Resusci-Anne manikin for steps A and B (first evaluated by Bjorn Lind in Stavanger), in 1960 he added a metal ring in the chest for also practicing chest compressions. His company was propelled into a leading position for the design, manufacture, and distribution of educational and resuscitation tools, particularly for pre-hospital use.[81] In the 1960s, through our education research in Pittsburgh, the manikin was modified to record ventilation and chest compressions — the birth of the recording Resusci-Anne.[82] The manikins and the pocket mask with oxygen nipple,[83] which I recommended and tested, and other devices, enabled Laerdal to become a worldwide distributor and a benefactor for resuscitation research.[81] The recent development by Laerdal and other companies of automatic external defibrillators (AED) was influenced by emergency cardiac care leaders in Seattle.

Most remarkable in my association with "Laerdal Medical" has been the close personal friendship that immediately developed between Asmund Laerdal, Hans Dahll, and me, which later extended to Asmund's son and successor, Tore. When Asmund died of cancer in 1981 I lost one of my closest friends. The collaborations between my group and Laerdal Medical never had any contractual agreement, patent, or other legal documents. Although the Laerdal Foundation provided major research support for our resuscitation research center programs, I personally was never paid by Laerdal. It was all based on friendship and trust, with complete openness. The Laerdal Company has been an ideal example of "free enterprise with social conscience."

Emergency medical services (EMS), from BLS by bystanders to ALS by ambulance personnel, were also co-initiated by our activism at BCH in the 1950s. There I had recruited Martin McMahon, chief of the Baltimore Fire Department ambulance service, into our volunteer experiments. I taught him basic respiratory physiology, while he taught me about some of the facts of life in the prehospital arena. When we obtained data clearly supporting the use of IPPV rather than the back-pressure arm-lift method, I urged him to redesign his ambulances. Hearses or station wagons were then used predominantly nationwide and were just for rapid transportation without life support. McMahon and I designed an enlarged patient compartment with the interior having a seat at the vertex of the stretcher, to permit resuscitation as performed by an anesthesiologist. We added a large oxygen source and powerful suction from the engine manifold. We provided intubation equipment and drugs for use by doctors in case they appeared at the scene. At first we provided an S-tube for mouth-to-tube ventilation. Later we also provided the self-refilling bag-valve-mask unit of Ruben (produced by Ambu) or a larger competing version (the "pulmonator"). The Laerdal folding bag with new valves followed later. McMahon's firemen learned in our OR how to insert pharyngeal tubes. Thus, ambulance design and emergency medical technician (EMT-I) BLS training began in the late 1950s in Baltimore. ALS training for ambulance paramedics (EMT-II) came later.

In 1960, two years after the switch to IPPV, the Baltimore Fire Department ambulance rescuers were also taught sternal compressions, mostly by Jude and Robert Wilder, my associate in surgery at the BCH. Consequently, for the first time, prehospital cardiac arrest victims could be rushed to the hospital for the restoration of spontaneous circulation, while external CPR steps ABC (BLS) hopefully kept their brains viable during transportation. I had motivated Wilder for resuscitation work through our volunteer experiments and our joint work in the ICU. Wilder reported on the first successful resuscitation of a sudden cardiac death patient outside the hospital. The patient had received steps A, B, and C en route to the hospital. At that time defibrillation could be done only in the ED, because only alternating-current wall-mounted units existed for use in hospitals. They were on big roll-around carts. Kouwenhoven of Hopkins and Zoll of Harvard had assembled such units. Portable defibrillators and ECG monitors were not available until later in the 1960s.

Zoll of Boston had performed the first successful closed-chest AC defibrillation in humans in 1956 and had also developed the first electrocardioscope (in the 1950s, still dependent on wall current). Gurvitch of Moscow (in the 1940s) and Peleska of Prague (around 1960) had produced the first portable direct-current battery-powered defibrillators for prehospital use. I visited them in 1963. The information barrier during the cold war delayed the introduction of prehospital defibrillation. Bernard Lown at Harvard took the initiative in the U.S. to introduce external direct-current defibrillation and cardioversion.

Resuscitation research on animals had been going on sporadically in physiology laboratories elsewhere since the late 1800s.[1] Jude at Hopkins studied in dog models the use of hypothermia, mostly for cerebral protection during heart surgery. I asked my associate Redding, right after he arrived at BCH in 1958, to develop and equip a dog laboratory program. We received space from Nathan Shock, chief of the NIH-supported gerontology patient research center at BCH. I asked Redding to compare blood flow produced by external vs internal cardiac massage in dogs,[84] and to delve into resuscitation experiments on "drowned" dogs.[76-79] Earlier, Swan in Texas had examined patterns of dying from submersion. Our work on drowning in the late 1950s did not focus on near-drowning (without cardiac arrest), where lungs, rather than heart and brain, are the most vulnerable organs. That was studied later by Modell in Florida. We focused on drowning to cardiac arrest, to explore resuscitability — we believe for the first time.[76-79] We used obstructive asphyxia plus water instillation via tracheal tube. For asphyxiation with sea water, which Redding had brought from Maryland's beach, we found more pulmonary damage than after freshwater inhalation. Both required positive-pressure ventilation with high oxygen. Plasma loss in sea water aspiration required plasma infusion. We documented the differences in dying patterns and acute resuscitability with fresh vs sea water in several papers.[76-79]

These were summarized in the *JAMA*.[79] Neither we nor others were then ready for cerebral resuscitation research.

After I left for Pittsburgh in 1961, Redding and Pearson continued with important resuscitation research studies on drugs, using modifications of our dog models of the 1950s. They showed that epinephrine in dogs helped restart the heartbeat through systemic vasoconstriction, not heart stimulation.[85,86] In the 1960s, Redding[87] and our group in Pittsburgh[88] initiated controversies on buffer therapy, which are still under debate and study.

The Medical/Surgical Intensive Care Unit, 1958-61 at BCH

Simultaneously with our resuscitation studies on volunteers, patients, and dogs, the need became apparent to provide life support for long periods for comatose or otherwise unstable patients from any service — medicine, surgery, gynecology-obstetrics, or pediatrics. Our postanesthesia recovery room (PAR) was not staffed at nights. A specially staffed area was needed, not only for patients still comatose after CPR attempts, but also for patients who had undergone critical operations and still needed controlled ventilation or fluid resuscitation from shock. I thought, why not have such a unit next to the BCH polio ward, using the same nurses. We started with the adult polio ward, which was underutilized in 1958. It had served the community since before World War II by accommodating many big clumsy "iron lung," i.e., tank (Drinker) respirators.

In Scandinavia in the early 1950s, the need for prolonged artificial ventilation of patients with barbiturate overdose[89] and patients paralyzed by poliomyelitis[90] had led to respiratory ICUs initiated and staffed by anesthesiologists. Ibsen of Copenhagen[90] had recognized the inability of the iron lung to adequately ventilate and oxygenate polio patients with bronchopneumonia; he switched to the use of manual bag compression by students taking shifts. He proved the superiority of manual IPPV via tracheostomy tubes over use of the iron lung. Ibsen and Stockholm's chest surgeons triggered the creation, in the 1950s, of many different mechanical ventilators for prolonged IPPV.

In 1958 at the BCH, we initiated what some observers have considered the first ICU in the U.S. I believe it was the first physician-staffed medical/surgical ICU in the U.S., and perhaps the first such ICU in the world that was meant not only for respiratory failure but also for cases with life-threatening failure of any vital organ system.[91] We reported the experience with our adult ICU at the World Federation of Societies of Anaesthesiologists (WFSA) Congress in Toronto in September 1960. For children we had, at that time jointly with the pediatrics department led by my dear colleague the late Harold Harrison, a small special care unit on a separate floor where we could treat children requiring prolonged artificial ventilation.

We initially treated several postoperative adult cases successfully with the Moerch piston ventilator, for what I called "progressive pulmonary consolidation" (PPC), what one would later call "shock lung" or "adult respiratory distress syndrome" (ARDS). When the lungs became stiff, I increased the piston rate, which caused the airway pressure curve to remain slightly above atmospheric pressure also during exhalation. We called it continuous positive pressure ventilation (CPPV). It is IPPV plus with what later was labeled "positive end-expiratory pressure" (PEEP). I believed that this "splinting alveoli open" was obvious and not worth publishing. Bendixen's concept of "sighing" and Hamilton's concept of positive alveolar pressure to counteract atelectasis made sense. Almost a decade later, when Moore of Harvard had already published a book on "shock lung," Ashbough and Petty reported on ARDS and CPPV.

Our ICU at the BCH[91] was covered during nights and weekends by anesthesiology residents in-house (and staff anesthesiologists on call from the outside). During days one of our staff (Safar, Redding, Pearson, and DeKornfeld) was available mostly for the ICU. Senior residents or staff members of Medicine and

Surgery were available in-house when needed. We learned much about respiratory intensive care in the summer of 1960, when we were confronted with the last severe poliomyelitis epidemic in the U.S., which hit Baltimore.[92] At BCH, we were overwhelmed with mostly adult patients who had not been vaccinated. Most children had received the Salk vaccine introduced in 1957. Over 100 adult polio patients arrived in a short time, of whom about 25 required artificial ventilation almost at once. Finding the iron lungs of the 1920s inadequate in number and ineffective when the lungs became stiff (and very clumsy for nursing), we apparently were the first in the U.S. to create a total switch from the iron lung to the seemingly more effective mechanical intratracheal IPPV or CPPV. We used mainly Moerch piston ventilators, some of which I had already bought earlier for our ICU from a manufacturer in Chicago. We used Moerch's metal tracheostomy tubes with swivel adapter, but (for the first time) with large, soft, atraumatic slip-on cuffs.[92, 93]

Trier Moerch of Chicago, originally of Denmark, had developed the piston ventilator as a modification of the Starling animal laboratory piston ventilator, for use in adult humans. The Moerch ventilator provided adjustable piston stroke (tidal) volumes of over 1.5 L. When Moerch was in Chicago, he had used this ventilator for "internal stabilization" of crushed chests by prolonged volume-controlled IPPV. Moerch's associate Benson introduced this method at the JHH for crushed chests. At the BCH I used non-rebreathing valves of the Moerch ball-type or the Beaver or Ambu type. Moerch and Benson used the ventilator with uncuffed metal tracheostomy tubes, compensating for a big leak with large stroke volumes. I used it for volume-controlled ventilation via the same tubes, but with soft, large (condom-like slip-on) cuffs. We performed tracheoscopies that showed no trauma caused by these soft, large cuffs even when used over many months. When needed, we slightly deflated the cuff to allow the patient to speak during positive-pressure gas leakage.[93] We learned details of respiratory care in part from patients. Some of this we captured in a documentary film.[93]

When the number of polio patients exceeded the number of ventilators, I played with connecting two patients to one piston via a Y-tube, adjusting relative volume distributions by partially narrowing one or the other delivery tube. When Martin Holmdahl from Uppsala, Sweden, then on sabbatical with Papper in New York, visited me at BCH in the summer of 1960, he made fun of the "impoverished American city hospital." Sweden then already had a universal health care system. They could afford the much more expensive Engström ventilators. The Polio Foundation in the United States initially was furious when I got rid of "their" iron lungs. I believed that ultimately they were impressed with our results.[92] Some of our polio patients remained on piston ventilators for several years, some at the BCH, into the era when Redding had taken over our department and ICU. Other patients were later transferred with mechanical ventilation to rehabilitation hospitals elsewhere.

Severinghaus's tri-electrode blood-gas unit was not yet available for monitoring ICU patients. Around 1960 it was pioneered and used only experimentally by Clark (PO_2) and Severinghaus (PCO_2, pH). To determine oxygenation at BCH, we merely monitored the color of lips, tongue, and blood. I even played with two arterial samples, one shaken with O_2, comparing the colors resulting from each. To determine the arterial partial pressure of CO_2, we used intermittently the end-tidal infrared CO_2 analyzer or the so-called "rebreathing technique."

An unpublished spin-off of our BCH experiences with prolonged ventilation in patients was my attempt to explore prolonged life support in experimental animals. At our BCH animal lab we kept dogs under light anesthesia over many days on IPPV by piston ventilator. Purulent nasal and tracheo-bronchial secretions appeared at 1-2 days. This led to pneumonia and the need to sacrifice the dogs at 3-4 days. I mention these experiments because they were my first attempt at bringing experiences from the patient ICU back to the laboratory. In the 1970s, in Pittsburgh, we created a research ICU for long-term life support of monkeys and dogs, a program still considered unique.[6]

Modern medicine now frowns upon single case reports. It relies on values of statistical comparisons and correlations of large series of patients. Not appreciated is the fact that the dynamics within each case often can teach pathophysiology and therapeutics better than statistical correlations of conditions that one cannot monitor or control. One such instructive anecdotal case report on emergency and long-term resuscitation is that of the "lightning boy."[50] In 1959, a 10-year-old boy was struck "dead" by lightning during a thunderstorm while bike riding on a street in eastern Baltimore. He received no effective resuscitation attempt, either in the field or during transportation. He arrived clinically dead in our BCH ED. Ravitch and I happened to be there when he arrived. With the help of our residents, we quickly initiated open-chest CPR. After restoration of spontaneous circulation, the boy remained unconscious as expected, with dilated, fixed pupils for several hours. We estimated an asphyxial cardiac arrest of close to 20 minutes. We managed him in our new ICU with (for that time) unusually aggressive techniques. We selected these because of their documented physiologic effects: intravenous urea to decrease assumed brain swelling, blood-pressure support with fluids and a vasopressor, prolonged controlled ventilation for several days with paralysis as needed, and prolonged moderate therapeutic hypothermia. Although moderate hypothermia (about 30°C) had been used for cerebral protection-preservation during open heart surgery since the early 1950s,[36] even by us it remained unused after cardiac arrest. In this case, the use of postarrest moderate hypothermia was instinctive. The boy regained consciousness and recovered completely, to lead a full normal life; he is now a healthy middle-aged man. We then believed that, without the new ICU and the for-then "radical" life support, this result would not have been possible. Cases like this made me add in my mind (perhaps in conversations) the term "cerebral" to CPR already around 1960[72] (see fig. 11). This led to the extension of CPR to CPCR, in our terminology, ten years before we started cerebral resuscitation research.[5,6]

Our 16-bed medical/surgical ICU at BCH, as far as we know, was unique around 1960 for several reasons:[91] 1. Its multidisciplinary nature, being staffed around the clock by residents and faculty members from anesthesiology, medicine, and surgery. 2. Its endowment with a special nurse-education program. 3. Its use in a BCH system of what we called "progressive patient care." This term referred to our hospital having had — a. an ICU in the acute building; b. general medical care on the acute hospital wards; and c. chronic care in the adjacent chronic hospital building.

Our general ICU development was paralleled or followed by several respiratory ICUs elsewhere. Pioneers included anesthesiologists Martin Holmdahl at the University of Uppsala;[94] Barry Fairley in Toronto;[95] Henning Pontoppidan and Henrik Bendixen in Boston;[96] and Matthew Spence in Auckland, New Zealand; and others. In the 1960s, Max Harry Weil in Los Angeles, William Shoemaker in New York, and the late Adams Cowley in Baltimore initiated small (one- or two-bed) research ICUs for studies of patients in shock. In the mid-1960s other multidisciplinary ICUs were reported, as the one by Rudolf Kucher in Vienna, admittedly influenced by our good and bad experiences in Baltimore and Pittsburgh. Pediatric ICUs came in the late 1960s.

Miscellaneous Memories of BCH

Baltimore City Hospital (BCH) — before my time and now again called "Bay View Hospital" — was a pioneering institution not only for resuscitation and intensive care, but also for research in pediatrics, medicine, gerontology, and other fields. During my six years at BCH, I learned from and influenced fine colleagues. With the help of some of them, we initiated the modernization of emergency resuscitation outside and inside hospitals, and long-term life support in hospitals. The results led to numerous invitations and experiences beyond BCH. Adequate staffing of my department, supportive leaders of other departments at BCH (particularly surgeon Ravitch and pediatrician Harrison), and a supportive administration gave me freedom and time for research and travel.

In 1957, I became a part-time student in Philadelphia with Comroe's course for medical educators. He was so impressed that a department chief would go back to school! For that course I drove from Baltimore to Philadelphia every week for two days of studying throughout the summer of 1957, when we did volunteer experiments at BCH on weekends. Comroe used that course as a pilot project for the Cardiovascular Research Institute (CVRI), which he started a few months later in San Francisco. In 1969/70 I joined him and Severinghaus there for a sabbatical. In 1957, when I showed Comroe our mouth-to-mouth data, he suggested that once I had documented this, I should go on to new topics of resuscitation. He mentioned that cardiologists had been nibbling away on the 10% of knowledge on electrocardiography not yet explored after 90% had been pioneered by Einthoven half a century ago. Comroe influenced me to pursue topics with the potential of achieving new, important results.

In Baltimore the chiefs of the three academic anesthesiology departments (Helrich, Benson, and Safar) organized the monthly meetings of the Maryland Society of Anesthesiologists. Out-of-town guest speakers that we attracted ranged from my former teacher Vandam, to my future associate Leonard Monheim of Pittsburgh, to Trier Moerch, to Nobel laureate Linus Pauling.

In addition, I helped Otto Phillips conduct monthly meetings of the "Anesthesia Mortality Study Committee," which he initiated and led until he joined me in Pittsburgh in 1961.[97] This imaginative, unique program was evaluation research at its best.

After reviewing all deaths that occurred during, or early after, anesthesia throughout Baltimore, this committee had blunt, anonymous discussions of selected cases once a month. Only physicians and nurse anesthetists were admitted, not lawyers. Those anesthesiology colleagues who were involved in the "casualties" recognized them and learned that — although erring is human (*errare humanum est*) — repeating the same error is unacceptable. The casualties related to anesthesia for patients in good physical status declined throughout the 1950s. I believe this was in great part because of Phillips. Community-wide evaluation of mishaps remains unique to this day. Later attempts in Pittsburgh by him and me to introduce something like it for anesthesia-related or EMS system-related mortalities failed. The fear of lawsuits suppressed the openness that is so important for learning from errors.

In Baltimore we pushed collegiality between gown and town. When I was elected president of the Maryland Society of Anesthesiologists, my inexperience in chairing business meetings according to Roberts Rules of Order was received with humor. Our innovations in resuscitation and intensive care prompted the Maryland Medical Society to appoint me as its first chairman of an EMS committee. However, the regional ALS ambulance service for Baltimore which we recommended in 1961, was initiated only many years after my departure for Pittsburgh.

The documentary films on emergency resuscitation[61] and intensive care[93] were created with the help of Harold Dixon of the Walter Reed Army Medical Center's medical illustrations branch of the department of pathology. In addition to the films, drawings of the exact maneuvers that evolved from these experiments were made first by me and then, more professionally and cartoon-like, by C. Thompson of the U.S. Army. Well into my time in Pittsburgh in 1961, I spent many nights and weekends in the film-making laboratory of the Walter Reed Hospital, puzzling together the scenes from several cameras which, by mistake, had not been synchronized. Mary-Jane Rhoads, chief ED nurse at BCH, who had performed as "rescuer" in the film shooting, helped edit in the film-making process. The final documentary film on CPR[61] was based on the last experience with Fredricks as volunteer. These two films[61,93] helped spread the word very rapidly around the world. Some years later, Archer Gordon popularized the BLS steps ABC sequence with his excellent training film, "The Pulse of Life," followed by ALS demonstrations in his film, "Prescription for Life." We used our Army films for documentation of physiologic facts, and Gordon's films for motivating and demonstrating.

In spite of my more-than-full-time involvement in professional work, and Eva's preoccupation with our ill daughter, world events did not go by unnoticed during the 1950s. In 1956/57, we responded to the revolution in Hungary, its occupation by the Soviets, and the resulting flood of refugees into Austria. We helped University of Maryland physiologist Amberson (of stroma-free hemoglobin fame), a friend of Austria, to give refuge to several Hungarian immigrants. Other events that stirred emotions and discussions included the building of the Berlin Wall. Global concerns, for medicine and beyond, were among the emotions that led me from beautiful but confined BCH to Pittsburgh.

In summary, the seven years in Baltimore gave me professional opportunities to become a reanimatologist and intensivist, to innovate, to lead, to learn how to teach, and to begin paying back to society a debt of gratitude for having survived World War II.

Part Two

PITTSBURGH
1961–2000

INTRODUCTION

The Pittsburgh section is long and detailed in order to give credit to as many team members as possible who helped initiate and develop our programs.

Now, in 2000 C.E., Pittsburgh has been our home for 39 years. I accepted the chairmanship in Pittsburgh with the goal to initiate and develop a "complete" academic anesthesiology department. This includes resuscitation research and ICU programs such as we had initiated in Baltimore. From 1961 to 1978, the almost two decades that I was department chairman, my associates and I have been credited with having built, from scratch, America's largest academic anesthesiology department (largest in terms of workload and manpower) and the world's first and still largest (visitors say still leading) multidisciplinary CCM physician fellowship training program. The department also "fathered" what visitors consider the following "firsts:" Guidelines for community-wide EMS; a research program on cerebral resuscitation from cardiac arrest; CPCR outcome models in an ICU for large animals; guidelines for brain-death documentation and certification and for "letting die" in persistent vegetative state; the International Resuscitation Research Center (IRRC); and "disaster reanimatology" as a new field of research. My associates and I succeeded despite horrendous political obstacles (turf battles) and inadequate budgets.[98]

Why did I leave Baltimore (BCH) for the University of Pittsburgh ("Pitt")? First, for the challenge to initiate academic *physician anesthesia* in a huge complex of university hospitals where nurses administered most anesthesias. *Second*, for a lifelong commitment to resuscitation research and intensive care, which I felt I could help develop better as university department chairman than as hospital department chief. *Third*, for the opportunity to influence all medical students and more members of the next generation of health professionals in a large main university hospital setting. BCH was at the fringes, where only elective students rotated through our department and we could influence only a few other trainees. *Fourth*, I anticipated that BCH would lose its autonomy, being swallowed up by the Johns Hopkins Medical Center; this happened soon after my departure.

By 1960-61 I had achieved a stabilized academic department of anesthesiology at BCH, with all-physician coverage of a manageable, medium-sized OR workload and creative programs in resuscitation, intensive care, and research. Collegial collaborations were beautiful. We had a strong anesthesiology residency program. I was professionally and personally happy in Baltimore. I did not leave because of academic rank or because of a low salary. Money never mattered to me much as long as the basics were covered. Eva and I were ready to move from our $12,000 mass-produced ranch-type home in East Baltimore to a $34,000 dream house (mortgaged of course), which we were building, according to my self-drawn blueprints, in North Baltimore. My priorities are revealed by the fact that our new house's kitchen was not much larger than a closet, but there were two large (40x30 ft) rooms, one for living and the other for dancing and music-making. The house was under construction when Pitt called.

In 1960, although I was only 36 years old, I became a candidate for some vacant chairmanships of university departments, probably because, in the young specialty of anesthesiology, academicians who had some experience as clinicians, teachers, researchers, and leaders were very scarce. The first invitation came from Buffalo, where I was interviewed without a subsequent job offer. I barely mentioned our ongoing research on artificial respiration, not appreciating then its importance. The second invitation came from the University of Alabama in Birmingham, which I visited in October 1960. The senior anesthesiologist in the city, a fine lady with considerable practical experience, clout, and Southern charm, wanted me to come, as did Lyons, the decision-making chairman of Surgery. His department's physiologist then was Leland Clark, who was about to become famous for having invented the oxygen electrode. Clark, being a Northerner, sensed my rather liberal background and warned me about Birmingham. The civil rights movement had not yet started. The offer looked attractive, but I gracefully declined.

The third invitation came from the University Pittsburgh, late in 1960. The challenge was great. The Pitt medical school had no academic department of anesthesiology. The previous February, the Western Pennsylvania Society of Anesthesiologists had invited me to give a talk on intensive care. When I returned to Baltimore, I told Eva that Pittsburgh was dirty and old-fashioned. The weather during that visit was awful. Half a year later, the chairman of Surgery at the University of Pittsburgh, Samuel Harbison, called me: he intended to get modern anesthesiology on board before modernizing academic surgery, as part of a renaissance movement for the whole university and medical school.

At my first visit, on November 28, 1960, they rolled out the red carpet. At that visit I ran into Jonas Salk of Pittsburgh, internationally famous for having in the 1950s developed the first polio vaccine. He was about to leave town for good. Salk told me then that he might have stayed at Pitt to develop the Salk Research Institute there (which was to be foundation-funded), had Pitt's administrators been less fearful about loss of control over an independent "institute." Salk's departure did not discourage me. I perceived Pitt as a unique challenge.

On this first visit, I learned that Pittsburgh had three anesthesiology departments with residencies, all outside the University. These groups had been initiated and were led by Francis Foldes and Ephraim (Rick) Siker at Mercy Hospital, Robert Patterson at Allegheny General Hospital, and George Thomas at St. Francis Hospital. Peere Lund, originally from Canada, well-known for epidural anesthesia, was chief at the Conemaugh Valley Hospital in nearby Johnstown. These fine anesthesiologists and their associates received me with great collegiality. The Foldes-Siker department was truly academic. That of Thomas, I believe, was not then board approved. Thomas was known for teaching anesthesia safety with explosion demonstrations. Residents of these outside programs rotated through anesthesiology at Children's Hospital of Pittsburgh, where Joseph Marcy was chief and the only board-certified anesthesiologist in what I inherited as ultimately five University hospitals. Francis Foldes and his lovely wife Edith were particularly hospitable to Eva and me during our visits and after we moved to Pittsburgh. When Foldes decided to move to New York in 1962, he called to tell me this (for us sad) news and asked: "Peter, do you want to buy my house?" The price he offered was fair. I expressed my regret that he had decided to leave. I answered "I know your house, you designed it, and I trust you — the answer is yes." We did not bargain. We exchanged signatures, a check, and a handshake, and the deal was made without middlemen, letters, or lawyers. This Foldes-Safar house (243 Hoodridge Drive, Mount Lebanon, Pittsburgh, PA, 15234), built in 1955, considerably enlarged and changed by us since 1962, hosted numerous colleagues from around the world. I reciprocated by giving Francis my plans for our large house in Baltimore; he used them to build his house in New York.

Until the 1950s, the University of Pittsburgh, although then almost 200 years old and the medical school nearly 100 years old, was considered a provincial college. This perception was unfair. The potential seemed great because of unique physical-numerical features: over 30,000 students University-wide and a campus adjacent to museums, libraries, concert halls, seven hospitals, and another great university, The Carnegie Institute of Technology (later renamed Carnegie Mellon University [CMU]) without a medical school. Moreover, in medicine, there were some giants at Pitt already in the 1950s and 1960s: In science, besides Jonas Salk, they included Ted Danowsky (endocrinologist), Richard Day (pediatrician, who promoted high-pressure intratracheal IPPV for newborn resuscitation; he was preceded by the legendary Benjamin Spock), Bernard Fisher (surgeon, pioneering oncologist, member of the U.S. National Academy of Sciences), Jack Myers (chairman of Medicine, the Osler of Pittsburgh), Campbell Moses (internist and atherosclerosis researcher, later director of the American Heart Association), Eugene Robin (imaginative pulmonary medicine researcher), Abraham Braude (infectious diseases), Gerald Rodnan (rheumatic diseases), Arthur Mirski (psychiatry-related research), and Klaus Hofman (biochemist who synthesized ACTH, member of the U.S. National Academy of Sciences). Hofman, of Swiss background, became our friend and my violin playing partner.

While clinical department chairmen were part-time until the 1950s, basic science chairmen were full-time. Between the World Wars, the "Department of Physiology and Pharmacology" had been chaired by Charles Guthrie (who should have shared the Nobel prize with Alexis Carrel for vessel anastomosis); Guthrie's successor, in charge in 1961, was Paul McLain. Microbiology was under Julius Youngner, whose collaboration with Salk was instrumental in the development of the polio vaccine.

In the mid-twentieth century, scientific discoveries and "firsts" in patient care improvements counted more than NIH grants and publications. After Thomas Detre came from Yale to Pittsburgh in 1973 as the new chairman of Psychiatry and medical director of the Western Psychiatric Institute and Clinic (WPIC), attracting NIH money became increasingly more important. For psychiatric research, Detre attracted many new talented investigators with NIH grants. After he became the powerful and highly effective senior vice-chancellor of Health Sciences in the 1980s, NIH support increased Health Center-wide. Detre's reign made Pitt move to the top 20% (and now the top 10% under his successor Arthur Levine) of U.S. medical schools in federally funded research dollars.

In the 1960s, for medical students and house-staff training, the University of Pittsburgh School of Medicine predominantly used the Presbyterian Hospital, which seemed to be its principal adult hospital. Its name was changed to Presbyterian-University Hospital (PUH) or "Presby." In 1961, it still had a wing called "Women's Hospital." Adjacent, but physically separate, was Children's Hospital of Pittsburgh (CHP). Attached to PUH on the opposite side, but administratively separate, was the Eye & Ear Hospital (EEH). Magee-Women's Hospital (MWH) was two blocks away, and the Veterans Administration Hospital (VAH) was two blocks up the hill in the opposite direction. Across the street from PUH-EEH, with a small block of houses in between, was Montefiore Hospital (MOH), which tried to maintain its independence. MOH was owned by the Jewish community of Pittsburgh, which was and is justifiably proud of its contributions, leadership, and catalyzing roles in Western Pennsylvania's medicine and cultural life. The psychiatry building (WPIC), with one of the largest and best programs in the U.S., seemed at that time not to be in need of anesthesia services. I was expected to develop the new Department of Anesthesiology to include PUH, EEH, CHP, MWH, and VAH — a complex of over 2,000 beds with 50,000-60,000 anesthesias per year, with the hope that MOH would, as the sixth hospital, become part of it later. This occurred in 1971.

The search committee for the Anesthesiology chairman was led by Samuel Harbison, chairman of Surgery — a gentleman, skier, and violinist. These attributes made us click from the start. He was the first full-time chairman of Surgery (1951-63). With wisdom and modesty, he was planning to step aside, hoping to recruit me to initiate an anesthesiology department with full-time staff, and then to recruit Henry Bahnson from Baltimore as chairman of Surgery. I instinctively trusted all the search committee members. They became our allies during the development of our programs, some of which were initially perceived by some privately practicing surgeons as threatening to their use of these hospitals.

The search committee wooed me. When I returned to Baltimore then, I told Eva that Pittsburgh looked great and I could not resist the challenge. On January 13, 1961, I visited HUP in Philadelphia to talk things over with Eckenhoff. He said that he had been offered this chairmanship in Pittsburgh but declined because "the Pittsburgh job cannot be done." He believed this job was impossible because of the conglomerate of free-standing hospitals and because the new chairman might have problems with Len Monheim, DDS. Surgeons liked Monheim as de facto chief at Presby in the absence of an academic department, and as a very skilled anesthesiologist, but without an M.D. Hearing this from my cherished teacher Eckenhoff, I considered it an even greater challenge and it stimulated my decision to accept. Ironically, soon thereafter Eckenhoff accepted the chairmanship at Northwestern University in Chicago with a similar conglomerate of hospitals and politics.

State licensure for me proved to be no problem. Dean Cheever and I obtained permission for me to take an oral examination in Harrisburg. Although I had been certified by the Maryland state board and the national board, graduates of foreign medical schools still were not admitted automatically to practice in Pennsylvania on the basis of reciprocity. I did not know what to study; there was no time. But the oral exam turned out to be very informal. My quick acceptance by the state board contrasted positively with the same board's insistence, in 1954, that I repeat medical school in Pennsylvania.

I remembered when I drove my visiting father from Yale through Pittsburgh in January 1950. His host, ophthalmologist Harvey Thorpe, of MOH, had put us up at the Schenley Hotel next to Pitt's main building, the Cathedral of Learning. That hotel has since become Pitt's commons and dormitories. In 1950 we also looked at the "Point" of the triangle formed by Pittsburgh's three rivers, which then was a polluted mess of warehouses. By 1961, it had become the Golden Triangle of a revitalized Downtown.

My second visit to Pitt was on February 8-9, 1961, together with Eva. Our son Philip had been born in Baltimore on December 25, 1960, and Eva felt uneasy leaving our two children with a babysitter for long. I was interviewed by the then most powerful (and by some admired) University Chancellor (president, Rector) Edward Litchfield. I have learned of no similar situation here or in other universities wherein the chancellor himself interviews a candidate for a department chairmanship. This is usually delegated to deans. Litchfield was an eloquent, visionary, persuasive, and dynamic leader of the University and for the tri-state area (Western Pennsylvania and nearby Ohio and West Virginia). He convinced me that this institution would become the Harvard west of the Alleghenies. Litchfield and his deans were in the process of recruiting young academicians from as near as Baltimore (e.g., Safar and Bahnson) and as far as Australia (e.g., ex-Viennese Kurt Baier, who became a distinguished professor of philosophy and our ally on biomedical ethics issues). Litchfield's building plans included a campus that would fill-in a huge empty canyon-like valley, "Panther Hollow." It never came about. During a witch hunt that slandered a history professor as communist, Litchfield defended him and preserved academic freedom. In 1966, the local wealthy patrons of Pitt, who had first supported Litchfield's ambitions for the University, stalled their support. For these and other reasons unknown to me, he was asked to resign. In 1968, he and his family perished in a private plane crash into Lake Michigan. I will never forget him. His leadership initiated our University's renaissance. I remember waltzing with his wife at our first Pittsburgh Symphony Orchestra ball in 1964, when Eva and I won the first of several symphony ball waltz contests. Litchfield said that we thereby honored the University. (Being Viennese with Strauss music in our systems, we felt later that it would be unfair to continue competing.) Litchfield's successor, Wesley Posvar, supported by the local patrons and his artistic, dynamic wife, Metropolitan opera star Mildred Miller, reigned over the further growth of our University for about 20 years of phenomenal expansion in programs and building plans. In 2000 I feel honored being a member of a small Posvar club of leading professors of his dynamic era.

Influenced by Litchfield, Dean Cheever, and the search committee, I accepted officially right after that second visit, in February 1961. The offer and my acceptance were oral. If formal letters were exchanged, I cannot find them. I did not inquire about my salary when I accepted. I trusted it would be fair, and enough to cover the basics for me and my family. I considered it poor taste to talk about money. It often spoiled collegiality and friendships. Salaries for medical school chairmen at Pitt in the early 1960s were about $30,000 per year plus fringe benefits, approximately twice what I earned at BCH in Baltimore. Salaries for most of the full-time anesthesiologists of our department in 1961-1971 came from the hospitals' Blue Cross and other insurance income, from "third parties," not from physicians' fee-for-service billings. Our checks were paid via the University. When, in subsequent budget negotiations years later, I found out that our anesthesiologists' salaries were only 30-60% of the incomes made in other general hospitals locally, and even some university hospitals elsewhere, I was compelled, as chairman of the Department, to become more concerned about salaries.

In March 1961, I still made repeated visits from Baltimore to Washington, D.C. to finish our documentary resuscitation films with the Army.[61,93] There were two more visits to Pittsburgh for further discussion. In April 1961 we said goodbye to Baltimore. The saddest farewells were from our loyal babysitter family, the Jakovics, and our music-making friends. On May 4, 1961, my family moved from Baltimore to Pittsburgh in two cars, Eva and I each driving behind the commercial mover. Our 6-year-old daughter, Elizabeth, rode with me in our VW Beetle. We talked happily and much. I even "interviewed" her on tape. This is the only recording we have left of her voice.

We moved to a Pittsburgh suburb, Rosslyn Farms, to a rented "villa," suggested by surgeon Jim Watson and his wife Marion, who lived there. They were very helpful and "took us under their wings" during our year in Rosslyn Farms. I have positive memories about my personal work in Presby's ORs from 1961 to 1989, with many superb surgeons, including Bahnson, Webster, Magovern, Rosomoff, Drapanas, Ferguson, Atwell, and the Watsons. I particularly enjoyed anesthetizing patients for Jim Watson, his brother Bill Watson, and years later for his son Charles (Chuck) Watson. Chuck came to Pitt in 1969 and soon became our medical school's most cherished professor and teacher of surgery, under the chairmanship of Henry Bahnson, who was followed by Richard Simmons. Doing operations with the Watsons was truly as enjoyable as playing Schubert trios. Jim died in 1999, in his 90s. Chuck died in January 2000, soon after his father — again as a role model — heroically, with full self-determination.

Department of Anesthesiology and Critical Care Medicine, 1961-70

Anesthesiology

The first decade of this new academic department in what later (through our initiative) became the University Health Center of Pittsburgh (UHCP), represents the history of the initiation and development of new service and teaching programs,[98] simultaneously in anesthesiology,[99] respiratory therapy,[9] critical (intensive) care medicine,[9-16,99] resuscitation research,[1-6] and community-wide EMS.[7, 8,99]

My first operational goal as chairman at Pitt was to gain physician control of all OR anesthesias in the five hospitals (PUH, EEH, CHP, MWH, and VAH). Our department developed the first unified full-time faculty to staff all these hospitals. Other clinical specialties had independent departments of practitioners in each hospital, and a small core of full-time faculty members reporting to the chairmen in the medical school. In 1961, I "inherited" only three anesthesiologists — Joseph Marcy at CHP (the only one who was board certified), Leonard Monheim at PUH, and Walter Bauer at EEH.

Until May 1961, almost all anesthesias in these five hospitals had been given by nurse anesthetists or dental anesthesia students of Monheim's program. At St. Francis Hospital, George Thomas, M.D. directed a school for nurse anesthetists, which provided not only St. Francis but also other hospitals of Western Pennsylvania with a pool of nurses trained in the basic tenets of anesthesia. Although primarily working at St. Francis Hospital, Thomas, through his nurses, was nominally in charge of anesthesia at PUH, EEH, and MWH. Monheim, under the aegis of Thomas was de facto chief of clinical anesthesia at PUH. At PUH, Monheim (see later) likewise had organized a school for nurse anesthetists. He also directed a training program for dentists interested in learning clinical anesthesia. He taught dentists to give anesthesia not only for dental cases, but also for other operations, under his direct and remote supervision. Before May 1961, the quality of OR anesthesia services reportedly varied among the hospitals.

At PUH, Monheim and his nurses and dental students provided efficient service. The OR setup which I personally took over as chief at PUH, was strange. I was the only physician anesthesiologist at that hospital in the summer of 1961, when Monheim went on vacation. I worked as intern, resident, and chief — all in one, just as I did at BCH in 1955. Monheim had established a few routine techniques, which he skillfully supervised by dashing from room to room to cover the split-level operating suite divided between the tenth and eleventh floors. He "supervised" all anesthesias at PUH, from dental cases to open heart surgery, which had just begun. Surprisingly few complications occurred among patients in good physical status. The safety of anesthesia routines for some patients in poor physical status I found questionable.

Monheim was a superb, highly dedicated clinician and colleague. He knew how to please surgeons. The other hospitals occasionally called him for troubleshooting. He was a recognized leader in dental anesthesia but was not eligible for the anesthesiology board examinations. With an M.D. degree, he would have become one of the nationally recognized clinical leaders of anesthesiology at that time. He had been enticed by his mentor Thomas and by his self-learning experiences during World War II in the Pacific (in part together with surgeon Jim Watson) to practice anesthesia far beyond dental cases. He became an anesthesiologist for any type of surgery. He had wisely standardized anesthetizing techniques. One was spinal anesthesia, administered by him personally and then monitored by nurse anesthetists. The other was general anesthesia with thiopental induction, intravenous infusion of succinylcholine for intubation, controlled ventilation by bag compression, with N_2O and O_2 by semiclosed circle, and repeat i.v. injections of thiopental plus continued titrated i.v. infusion of succinylcholine, whenever it seemed necessary. Reinforcement of N_2O with meperidine was rare. There were no ventilators. N_2O was the only inhalation anesthetic (analgesic) in use. There was no ECG monitoring in the OR, as the first ECG oscilloscopes were just becoming available. Oxygenation was monitored by the color of the blood and tongue. Hypercarbia was avoided by moderate hyperventilation.

Monheim became one of my loyal friends and advisors in local medical politics. There was mutual respect. In the early 1960s, he said:[98]

> Colleagues took bets as to how long the senior man (Monheim) would stay ... however, I stayed and considered the whole development as exciting, to say the least. Back when I started, doctor anesthesiologists were competing with nurses. Now, we are involved in the whole patient-care picture. We see the patient preoperatively, surgically, and postoperatively. We are consultants, not flunkies...I feel like I am playing for the Yankees (baseball champions) now.

At EEH, nurse anesthetists were supervised by Walter Bauer, M.D., who had been installed there by Thomas. Bauer was a good anesthesiologist but not board-eligible. The EEH administration and medical staff were staunchly independent. They created a Berlin Wall-type barrier against PUH, which they feared would take over EEH some day. This did not happen until after my chairmanship. Although EEH and PUH occupied the same building, I had to frequently jump over that wall to influence EEH services. Privately practicing ophthalmologist Joseph Novak and otolaryngologist Sidney Busis, both highly respected, helped us break down the barrier.

Thomas visited occasionally at the EEH to do "private" cases. Thomas's considerable anesthesia skills are illustrated by the following method I witnessed, which will seem strange to young anesthesiologists now: For that eye operation, he sat at the right side of the patient, facing cephalad. The patient's right arm was stretched out. There was no tracheal intubation. Coughing for eye operations had to be avoided. He had a large syringe with thiopental directly connected to an arm vein via one of the then still prevalent rubber tubes. With his left hand, he injected enough thiopental to depress (not stop) breathing. The surgeon gave local anesthesia and put a long suture through the tongue on which Thomas pulled, with his right hand under the drapes, also supporting the chin, to provide an open airway. When breathing stopped, he used the elbow of his right arm to push on the chest. This technique, I was told, "usually" worked quite smoothly.

At CHP, Joseph Marcy was a star. He started as chief at CHP in 1955 and retired in 1984. His temporary assistant anesthesiologist Mary Rose Truter, was about to leave when I came to Pittsburgh. Marcy's was the only hospital department of anesthesia at Pitt that received rotating residents from other programs. Among the Pitt hospitals, CHP had the most sophisticated anesthesia service, which Marcy had patterned after that of Margo Deming, chief anesthesiologist at Children's Hospital of Philadelphia, where Marcy and I had trained a decade earlier.

At MWH, in the spring of 1961, there was no board-eligible physician in anesthesia. A part-time anesthesiologist had left before I started. Twenty-four hour coverage of obstetric anesthesia was provided by one nurse anesthetist and two "moonlighting" medical or dental students (giving open drop ether), without physician anesthesiologists available even for consultation. Obstetricians gave spinal anesthesias.

At VAH, anesthesia by nurse anesthetists had been supervised by anesthesiologists John and Caroline Ziegler, who I believe, were board certified. Unfortunately, they were about to leave for private practice when I arrived.

MOH was not a member of the University programs in 1961. There were a few board-certified anesthesiologists there, led skillfully by Elliot Jacobson, who was succeeded in 1962 by Stephen Finestone as chief. His department joined ours at Pitt in 1971.

Thus in May 1961, five hospitals under my responsibility had a total of three staff anesthesiologists and about 70 registered nurse anesthetists for an annual 50,000-60,000 anesthesia cases. This total number of cases has remained similar until today, but with an increasing proportion of patients in poor physical status. Since the 1980s, transplant surgery has made some procedures much more complicated and longer. In May 1961, there was no extra-OR service or teaching by anesthesiologists. By the 1970s, in addition to OR work, after development of our extra-OR programs, we anesthesiologists and helpers also cared for 3,000-4,000 ICU patients each year, and up to 5,000 pain-control patients each year. I considered the enormous physician staffing effort to be necessary for patient safety and to enable the creation of previously nonexisting teaching and research programs. My leading associates felt that "this was the beginning of one of the strongest academic anesthesia departments in the world." In the department's first 15 years, we reduced the number of CRNAs only slightly, from about 70 to 60, while increasing the number of physicians from three to about 100 (i.e., to full-time staff from 3 to about 60 and residents plus fellows from 0 to about 40).

Starting immediately in the summer of 1961, my associates and I began to develop 10 new programs: 1. physician control of OR anesthesia with the goal of having anesthesiologists control all anesthesias in the five hospitals, although nurse anesthetists or residents would maintain anesthesia in most cases; 2. a new anesthesiology residency program; 3. the world's first CCM physician fellowship program; 4. Pennsylvania's first respiratory therapy technician-technologist training program; 5. a medical student program in anesthesiology and CCM in all four years; 6. CPR training for Health Center personnel and for the community and the first American Heart Association (AHA) CPR instructors courses; 7. laboratory research with fellowships; 8. special neuroanesthesia and cardiothoracic anesthesia with fellowships; 9. a pain-control clinic with fellowship; and 10. a community program in EMS organization, including America's first training programs for EMTs and paramedics according to national guidelines (which we co-initiated). Added to the three existing training programs (nurse anesthetists and dental anesthetists under Monheim, and pediatric anesthesia under Marcy), within about two years we had to conduct a total of 13 educational programs.

When I accepted the chairmanship of the Department of Anesthesiology in the medical school, which I was assured would be administratively independent of the chairman of Surgery, I assumed that this also meant departmental status automatically in all five affiliated hospitals. This turned out not to be the case. When I began introducing some orderly scheduling of operations at PUH, some old-time surgeons objected to our acquiring a separate independent department at PUH. They were used to servitude from nurse anesthetists. Some surgeons and colleagues in Internal Medicine backed us up, and the old-time surgeons came around during our first year, recognizing that we could provide better service than in the past. Departmental status was conferred at PUH in 1962 and followed rapidly at the other hospitals. At the same time, my associates and I acquired a generally good reputation throughout the community, because, compared with some full-timers in other departments, we were friendlier with our colleagues on the "town" side of Pittsburgh's physician groups.

Throughout my 40 years as a clinical anesthesiologist (1950-1989), I have considered it essential to demonstrate to surgeons that I personally could administer anesthesia. That meant my presence in the OR of PUH every day in the early 1960s (except when out of town). In the 1970s, I was in the OR about half-time, and in 1979-89, after I gave up the chairmanship, I decreased it to one day per week, because of research and commitments in teaching and internationalism. As chairman from 1961 through 1978, I was forced to spend too much time with administration and mediation. Against my better judgment I was caught in playing psychologist-psychiatrist without training, skills, or interest in these fields. I just did it to help colleagues with their problems and in their careers. Later on, some of my alumni considered this "one of your many strengths." I knew then already about my weaknesses. These included ignorance and indifference about financial matters.

Recruiting for our ambitious programs was my most important task in the 1960s, in order to achieve physician control of all anesthesias. My starting hard money University budget was about $300,000 per year. (Purchasing power then was about four times that of the 1990s.) Enlightened, usually collegial, hospital administrators, and moral support from our dean and vice chancellor for Health Sciences, enabled me to gain many new hospital-generated salaries. These salaries were not competitive. In the early 1960s we could pay faculty anesthesiologists $15,000-$25,000 per year plus fringe benefits.

Another priority was to introduce new OR anesthesia techniques, which were necessary for critical cases and for training anesthesiology residents. "New" anesthesia techniques that we brought to the Pitt hospitals included cyclopropane, different relaxants (d-tubocurarine and gallamine, soon replaced by pancuronium), halothane (with close shaves at first when we introduced it without precision vaporizers), nerve blocks, the esophageal stethoscope (some called it the "Safar telephone"), the ECG oscilloscope, OR ventilators, and others.

Recruiting was very difficult because of the low salaries and the unattractive appearance of Pittsburgh, which then still suffered from severe air pollution. In 1969-79 I became involved with "GASP" (Group Against Smog and Pollution). I founded its medical committee. My friend (surgeon) Charles Watson continued this to spearhead air-quality monitoring in the Greater Pittsburgh area. We pushed industries to honor state and federal laws. In addition, anesthesiology at the local and national levels still lacked recognition by many peers and remained an understaffed, relatively unattractive discipline. Our "revolution" stimulated a considerable increase in the interest in our specialty (J. Smith). Recruiting American residents was most successful at the beginning, when the first half-dozen residents came from Pitt's School of Medicine, either newly graduated or from general practice. Again, in the late 1970s, after we had initiated a private group practice within the medical school, many of our new residents were Pitt graduates. In between, our residents and fellows resembled the United Nations. Among them were many colleagues who gained future leadership positions in the U.S. and around the world.

My management style was merely instinctive, without any knowledge of business administration or psychology (table 2, see page 343). Dean Cheever said, "Peter rules with an iron hand in a velvet glove." My associates and I worked hard to promote anesthesiology as something important and worthwhile. Artistically talented medical students, who spoofed many departments' faculty members at their annual farewell musical, satirized our department with "today anesthesia, tomorrow the world."

I started with a benevolent pyramidal dictatorial governance. I still believe that during times of building something new (or times of crises) we need maximal democratic input with wise advice, but authoritative decision-making. As each hospital group grew to between six and 12 faculty/staff members, I switched to a decentralized governance, resembling a conglomerate of fiefdoms, like the Hapsburg empire. That loose pattern was fully developed by 1973, when we introduced a practice plan. This added quarrels about who controls the money. I requested throughout that each chief anesthesiologist, the director of CCM, and I receive the same salary. For our clinically busy faculty (staff), I pushed for salaries only moderately lower than the chiefs', and for an egalitarian administration.

Despite the handicaps under which we tried to recruit, we were rather successful, although many of our staff members left us in a few years for greener pastures elsewhere. Recruiting and re-recruiting over these 18 years necessitated my, and often Eva's, weekly visits to the few good restaurants then in Pittsburgh, usually the Tin Angel or LeMont on Mount Washington, overlooking the city. I always hoped for the wind to blow away the smog so that the visiting candidate could see our beautiful city. I acted like a member of the Chamber of Commerce. Recruiting became an enormous drain of energy also for Eva (fig. 12). She entertained visitors almost weekly. My time with the children was limited but concentrated. Without Eva, there would have been no family. Elizabeth's asthma needed nightly attention from one of us. There were days when I rushed home, with one of our respiratory therapists at my side, to help Elizabeth.

For physician-directed OR anesthesia, I followed in all hospitals the Scandinavian example of team coverage, namely: physicians plus nurse anesthetists. Simultaneously we introduced CCM and other "para-anesthesia" programs. In deference to Monheim, I endorsed his continuance of the dental anesthesia program but I encouraged him and his trainees to limit their OR assignments as much as possible to dental and head-neck cases. I also discouraged his trainees from doing anesthesia for non-dentistry work after leaving Monheim's course.

Staff recruitment began with Otto Phillips and Leroy Harris in May 1961. The chief of Obstetrics at Baltimore's Women's Hospital, who had delivered our son Philip just before we left for Pittsburgh, was unhappy with me for luring them away; he called me "the Pied Piper." Phillips and I had become friends in Baltimore. Because of his extensive experience in obstetric anesthesia in Baltimore, I appointed Phillips as the first chief anesthesiologist at MWH. He was a senior statesman in medical politics. Phillips arrived at MWH with fanfare and became medicopolitically strong very quickly, teaming up with part-time private-practice obstetricians and gynecologists. When soon after his arrival word got around that a laparotomy would be done under endotracheal anesthesia, nurses and administrators gathered in the OR. When Phillips successfully inserted the tracheal

Figure 12. Eva Safar in the 1980s.

tube (a unique occurrence, apparently, around 1961 at MWH), the audience applauded.

Leroy Harris, senior to me when we were residents together at Dripps's department at the HUP in Philadelphia in 1951, had gone into private practice in Baltimore with Phillips. He wanted to return to academic medicine. He became a superb teacher for our residents at Pitt, particularly as a knowledgeable physiologist. He introduced some residents into laboratory research. In the social setting we would spend many hours listening to Leroy's hilarious stories (J. Smith). In the late 1960s, Harris, Phillips, and our MWH anesthesiologist Nancy Swenson took over the department of the nearby independent Western Pennsylvania Hospital. Harris died suddenly in 1974. I initiated a Leroy Harris Award and Fellowship in our department.

I recruited Stephen Galla, also starting in 1961-62, to coordinate laboratory research. He had been resident with Proctor and me at the JHH in Baltimore in 1954-55. When the JHH department resigned, Galla went to Boston to complete his anesthesia residency with Vandam, and began being productive in laboratory research.

I also brought from BCH, as residents, Ruben Tenicela, originally from Peru (he later initiated our pain clinic at PUH), and Warren Holtey (he later practiced in Wisconsin). This "gang of six Baltimoreans" (Safar, Galla, Harris, Phillips, Tenicela, and Holtey), plus Marcy, Monheim, and Bauer, our first residents, Herbert Kunkel and Samuel Milai, and administrative secretary Joanne Rosendahl of Pittsburgh, were the "founders group" of our department in 1961. Dr. Lowrey, whom Thomas had sent to PUH as one of his St. Francis residents, on a rotation with Monheim, was also briefly a resident with us. In 1962, I recruited Bulent Kirimli (trained in Johnstown by Peere Lund) and Hugh Franklin (trained in the U.K.) as staff members for the VAH.

My first office at the PUH was a broom closet on the OR floor, right next to the OR desk and next to Monheim's broom closet. There was enough room in another small area nearby for two secretaries. Ms. Rosendahl kept us "organized." She was skillful in "public relations" with Presby surgeons and administrators. Her charm, dedication, and spirit helped calm surgeons when they were shaken by our new philosophies and more orderly scheduling requirements. Patricia Sands joined us as a young secretary to help Rosendahl in the early 1960s. Pat was with us from 1964 through the beginning of Winter's chairmanship in the early 1980s. Because of her intelligence, compassion, organizational talent, ability to listen, and motivation, she became the department's monitor, reporting to me about events in this widespread operation. She became the coordinator of our department's medical student program. Simultaneously, she pursued part-time academic studies, which led to her earning a Master of Public Health degree and to an assistant professorship in our department. She helped not only as an editor, but also became co-author of various papers concerning our research projects in EMS and of our department's report on the first 15 years.[98] In the early 1980s, she remarried and moved to the Northwest (where she still resides) to nurture her family life and pursue a different career. Starting in the mid-1960s, Diane Maling was our administrative secretary, followed in the 1970s by Ruth Smith. Sands and Maling helped me part-time in San Francisco during my sabbatical in 1969-70.

Ernest Shortliffe, M.D., was the first administrator of PUH, from 1961 to 1963. I wish he had stayed. He was most progressive. He and I had a true academic multihospital medical center in mind. He was opposed by turf-obsessed administrators and boards of the other hospitals. He was followed by his associate, Edward Noroian, whose title changed to "President of PUH." Both were in tune with our department's progressive ideas. This could not be said about all the other hospital administrators, who resisted some of our different approaches. Noroian's wisdom and support were crucial for the changes and new funds we needed to develop CCM and community EMS.

In 1963, the medical school executive committee (which included me) attracted Henry Bahnson as full-time chairman of Surgery. I remembered Bahnson from Hopkins as a technically superb young surgeon. Although Bahnson's mentor, Alfred Blalock, had made the surgeon to always be the "captain of the ship," Bahnson began at Pitt with a similar posture, but also with openness and collegiality. My department's team members were able to develop multidisciplinary CCM. I believe this was in part because Bahnson and I developed mutual respect, and even surgeons were impressed with some of the intensivists I recruited — particularly Ake Grenvik and Jan Smith, who are still with us three decades later. I learned to appreciate Bahnson's integrity, sound principles in medical politics, and stoicism. He (as I) liked fast downhill skiing and the high mountains (he climbed in the Himalayas, which I did not). Just before coming to PUH, he had helped my past colleagues in Vienna to introduce heart surgery there.

In 1968, at Pitt, Bahnson performed one of the first successful human heart transplants, the first in Pennsylvania. At Pitt, Bahnson recruited stars, such as, in 1969, Mark Ravitch (who died in 1986) and Chuck Watson (who died in 2000). In 1981, Bahnson recruited the transplant pioneer Thomas Starzl from Denver. Starzl, helped by the CCM program of Grenvik and the anesthesiology program of Winter, made Pitt in the 1980s the world's leading transplant surgery center. Bahnson's trainees Bartley Griffith and Robert Hardesty excelled in developing cardiac and pulmonary surgery, transplantation, and biomedical engineering, and are among nationally leading surgeons now. Bahnson also recruited my BCH friend Felix Steichen and trauma surgeons Ted Drapanas and Larry Carey who became our allies in developing trauma-related EMS. (Drapanas and I, with our spouses and my son Philip, toured Peru in the early 1970s.) In 1974, while going to a Board of Surgery meeting, Drapanas, then chairman of Surgery in New Orleans, died when his plane crashed in Boston while trying to land in bad weather.) During my 18 years as department chairman, despite off-and-on turf problems, the majority of anesthesiologists and surgeons at Pitt enjoyed an amiable relationship, as far as I perceived it.

In the early 1960s, before Bahnson established himself as the leading thoracic surgeon, George Magovern was one of two or three privately practicing chest surgeons at PUH. Magovern did the first lung transplant; unfortunately it was followed by subacute rejection (Milai, et al, Anesth Analg 1964). Magovern left PUH and became chief surgeon and medical director of Allegheny General Hospital across town, where we staffed his new anesthesiology group after Patterson's retirement.

Neurosurgeon Hugh Rosomoff and I were comrades in arms. Rosomoff, whose research on therapeutic hypothermia in the 1950s I knew from the literature, was, in the early 1960s, chief of Neurosurgery at our VAH. He quickly became one of my special colleagues and friends. Neurosurgery was a division in the Department of Surgery. In 1963, I invited Rosomoff on a tour to Prague and Moscow. Rosomoff left us in the mid-1960s to become chairman of Neurosurgery at Albert Einstein in New York, and then chairman at the University of Miami. When he left us, he jokingly bragged that he had been "Safarized" in the 1960s in Pittsburgh.

Those of us anesthesiologists who were interested in neurosurgery were frustrated after the loss of Rosomoff. In the 1970s, I urged the dean to strengthen academic and clinical neurosurgery at Pitt and recruit a leading chairman for an independent department. One or two previous attempts had failed. With the promise of departmental status, which materialized within two years, we attracted Peter Jannetta as chairman in 1971. He became a star. Jannetta became the Walter Dandy (Sr.) Professor at Pitt, which his department initiated and named after the father of Eva's and my first American friend, anesthesiologist Walter Dandy, Jr. Before World War II, at Hopkins, Harvey Cushing and Walter Dandy, Sr. were the world initiators of modern neurosurgery. I helped Jannetta by attracting Maurice Albin for neuroanesthesia. Albin had been a pioneering researcher with Robert White, chairman of Neurosurgery in Cleveland. They had pioneered therapeutic hypothermia for spinal cord injury in laboratory models. Robert White and I shared collegiality with Negovsky's group in Moscow starting in the 1960s.

Our department was the first to unify the five university-affiliated hospitals under a full-time faculty and with joint training programs. We thereby inaugurated the University Health

Center of Pittsburgh (UHCP) — later called University of Pittsburgh Medical Center (UPMC). It began as a loose partnership of the medical school and the five hospitals (six hospitals from 1971, when MOH became part of us — again first in Anesthesiology). In the 1980s and 1990s, under Thomas Detre's strong leadership, UPMC became rigidly controlled, more so under "managed care." PUH administrators became a single UPMC administration including MOH, EEH, MWH, and numerous community hospitals. CHP has a separate administration, but is functionally part of UPMC. Anesthesiology/CCM remained unified in all these hospitals throughout — chaired by Peter Winter in 1979-1996, and then by Leonard Firestone.

Our anesthesiology residency became accredited by the ACGME during our first year (1961-62). Residents' OR anesthesia assignments at PUH could be made according to their training needs, because many nurse anesthetists, under the skillful guidance of Monheim full-time and our other staff anesthesiologists part-time, were available to free residents, as necessary, for extra-OR work. This I considered desirable, for the purpose of education, service, or both.

My first new physician trainees in anesthesiology, Herbert Kunkel and Samuel Milai of Pittsburgh, had had brief experience as general practitioners. They joined residents Holtey and Tenicela who came with me from Baltimore. Milai and Kunkel had three years of training with us — a mixed experience of anesthesiology, resuscitation, and intensive care. They became the first critical care medicine (CCM) fellows in the world. (Max Harry Weil and we in Pittsburgh substituted the term CCM for "intensive care" around 1970.) Kunkel and Milai, together with our subsequent anesthesiology residents Jack Scott, William Stept, and Steven Karpinsky, went on to initiate a new physician Department of Anesthesiology at the nearby Shadyside Hospital. Other fine early anesthesiology residents, who co-pioneered the department in the 1960s and 1970s, included Richard Costanza (who became chief in Aliquippa; he suffered sudden cardiac death in 1970), Mickey Black, Dave Solosko, Tom Glushien, Miro Klain, Paul Berkebile, and others. The esprit de corps of the first group of faculty and residents was tremendous.

Our residency program immediately included an optional third year. All anesthesiology residents received some CCM experience in the then-required two years of training. Several third-year residents became full-time CCM fellows. After successful resident recruiting in the early 1960s, the recruitment of residents became increasingly difficult, in part because of anesthesiology's low image nationwide, and perhaps also because of the great service demands of our multifaceted department. Nevertheless, we were able to stabilize the numbers to be on board at any time, in the early 1970s, at about 20 anesthesiology residents plus 20 CCM fellows.

Between 1961 and 1978, a total of over 350 physician trainees and full-time faculty in anesthesiology and/or CCM went through our department. Between 1962 and 1977, we had trained a total of 105 CCM fellows — mostly for one year, six of them for two years. After my chairmanship, which ended in 1978, the CCM program continued to flourish and expand under Ake Grenvik. Between 1963 and 2000, our CCM program trained more than 500 fellows in adult and/or pediatric CCM.

In 1961, in spite of overwhelming work at Pitt, I had to honor previous commitments. They included a longstanding invitation by John Bonica to be a visiting professor in Seattle. That visit began a friendship with the Bonicas. In the mid-1960s, when Bonica was president of the ASA, he and I agreed that the ASA should have a "Committee on Acute Medicine" to promote interest in extra-OR activities. He appointed me as its first chairman.

Our group's OR experiences throughout the 1960s showed us that probably the safest and most cost-effective staffing ratio for anesthesia was one full-time staff anesthesiologist (FTE equivalent) plus two "helpers" for every 1,000 cases (or 12,000

ASA service units) per year.[99] When we achieved this goal, experience in the 1970s confirmed this. "Helpers" would be trained nurse anesthetists or residents who had had approximately at least six months of training. This ratio made more sense than staffing merely on the basis of the number of ORs to be covered.

Personally, from 1950 until 1989 (age 65), I never left the OR entirely. Throughout the 1960s, I spent several days a week in the OR, doing demonstration teaching, using tricks I had acquired since my residency. I taught by first demonstrating how I would do a case personally, with the trainee helping me. Then the trainee would do it with a similar technique with me coaching. Finally, the trainee would give the anesthesia on his (her) own while I kept a distant eye on the case. Some of these introductory OR teaching approaches "of old days" were captured on a 12-hour videotape made in the 1980s, which we hope to transform sometime into a teaching tape on "the art of anesthesia." It includes endotracheal "surgery-adapted" controlled ventilation by hand; unusual uses of the nasopharyngeal tube; and "jungle anesthesia." That began in my Baltimore time,[48,49] with no anesthesia machine, no compressed gases, only a self-refilling bag, air, a draw-over halothane vaporizer and non-rebreathing valve, with oxygen optional. It could be used even for laparotomies or thoracotomies. This simple, portable anesthesia "machine" principle was later adopted by the U.S. and British military. I emphasized how to administer general anesthesia without the use of a tracheal tube. Another legendary experience that my OR trainees recall is my discretely turning off gases on the ventilator, or injecting a relaxant, to test the trainee's vigilance — of course, with me ready to take over if he (she) failed to straighten out the "complication."

The leading anesthesiologists of our department, during my 18 years as chairman (the majority of whom continued into the 1980s and 1990s under Peter Winter's chairmanship), can be summarized as follows:

At PUH, I was chief at the start, but in the late 1960s freed myself more for academic programs. I turned the chief position over to David Torpey, who after service with the U.S. Navy in Vietnam, joined us in 1966. He later became chief anesthesiologist at Allegheny General Hospital. Torpey was followed by Maurice Albin and then Brian Smith as chief anesthesiologists of PUH in the 1970s. Brian Smith rose from one of my best residents to professor in one decade. After I resigned as chairman in 1978, Albin, Smith, our cardiovascular anesthesiologist Malcolm Orr and Babinsky went to the University Hospital in San Antonio, Texas, to re-develop the anesthesiology department there.

At EEH, I retained Walter Bauer as chief. After his unexpected sudden death in 1966, there was transient leadership by Oscar Farmati, followed by strong clinical leadership under Jack Quinn (from Johnstown) from 1967 to 1971. Quinn was followed by Brian Smith and, in 1976, by Bulent Kirimli.

At CHP, Joseph Marcy had run a nationally recognized program of pediatric anesthesiology since 1955 and continued as chief until 1977. He also trained residents from other programs. In 1977 he turned the leadership over to my former medical student and resident, Ryan Cook. Marcy retired in 1984. Cook, after his residency with us, had a fellowship at Yale, returned as a pediatric anesthesia faculty member with us, became chief at CHP, and was acting chairman of the overall department during the transitional year 1978/79.

At MWH, when Phillips decided to become chief anesthesiologist at Western Pennsylvania Hospital in 1968, the legendary Robert Hingson (known for pioneering regional anesthesia in obstetrics, for mass vaccination by jet gun, and other "firsts") resigned as chairman of Anesthesiology at Case Western Reserve University in Cleveland. I recruited him as our professor and chief at MWH. Hingson recruited Ray McKenzie in 1969 for OR anesthesia, and Ezzat Abouleish in 1970 for OB anesthesia. In 1973, Hingson decided to go full-time with his

Brother's Brother Foundation. That world-famous nongovernmental organization, now led by Bob's son Luke Hingson, provides world-wide jet-gun vaccinations and many other relief actions. In November 1994, with the help of Hingson's present successor, Ramanathan, recruited by Winters, I arranged a program to honor the late Otto Phillips, who had died in 1983, and also to honor Bob Hingson, who was then in retirement. In 1973, McKenzie succeeded Hingson as overall chief anesthesiologist at MWH, while Abouleish remained in charge of OB anesthesia. In 1970 they introduced a switch from predominantly spinal to predominantly lumbar epidural anesthesia. That important change came under Abouleish, with Hingson first and McKenzie later as overall chief at MWH.

At MOH, Stephen Finestone provided excellent stable leadership from 1961 until his retirement as chief in 1991. He was helped by one of our more progressive hospital administrators, Irwin Goldberg. In the 1970s, MOH chief of surgery Ravitch and I recruited anesthesiologist Arnold Sladen, a superbly conscientious clinical intensivist, to initiate and run the surgical ICU at MOH. It became part of our health-center-wide CCM program. In 1971, I asked Finestone to take on the management of our practice plan, the first at UPMC. He was aided by Brian Smith. I had no experience in billing and managing the flow of money. Winter, who became chairman in 1979, relied heavily on Finestone to keep an eye on our department's finances. When Monheim died in 1971, we transferred the leadership of the student nurse anesthetist training program to Finestone, who combined it with his MOH nurse anesthetists program and made it a model for physician-nurse team function. This program trained approximately 350 nurse anesthetists between 1971 and 1991, and continues to graduate approximately 15 students per year.

At VAH in 1961, I first asked Derek Duncalf of Mercy Hospital to be acting part-time chief, until we got the U.K.-trained Hugh Franklin to be the chief. Bulent Kirimli joined him in 1962. Franklin soon left for private practice, and Kirimli became a

VAH powerhouse as long-term chief and ICU initiator. He was joined by Tappan Datta, a favorite teacher of residents. Kirimli arranged a contract with the Veteran's Administration, which I believe was the first such arrangement in the U.S., whereby the university department administered a full-time anesthesiology department at a VA hospital, with VA reimbursement to the university.

Jan Smith (originally an internist from South Africa), after residency with Vandam and CCM fellowship with us in the 1960s, took over the ICU at the VAH. He then left Pitt, went to Iowa and San Antonio (as anesthesiologist and pulmonologist), became chairman in Nebraska's and later Northern Ohio's medical schools, and came back to Pitt under Winter to be professor and chief anesthesiologist at PUH. During my time as chairman Smith acquired board status in internal medicine, anesthesiology, pulmonary medicine, and critical care medicine. He thereby became at that time the most multidisciplinary certified anesthesiologist-intensivist. Jan, after a fine career as a superb clinician-scholar (i.e., role model, teacher), became recently the medical director of a UPMC affiliated hospital.

The Pain Control Center at Pitt actually began in 1964, after our graduating anesthesia resident William Stept visited Bonica in Seattle. After Stept went to Shadyside Hospital, the pain program was quite dormant until, in 1969, Ruben Tenicela single-handedly initiated a pain clinic. It started mainly with nerve blocks, but soon he extended it to a multidisciplinary pain-control center. It became, in terms of workload, probably the largest in the country in the 1970s. Other anesthesiologists emulated Tenicela at other hospitals. Publications and grants for pain-control research, however, did not come about until Peter Winter, in the 1980s, recruited pain scientist Rudy, which led to the exemplary leadership of our pain control center now, in the 1990s, by Doris Cope.

Medical Students

The medical student program of our department in the 1960s and 1970s was (we were told by Winter) the largest such program in anesthesiology and CCM in the country.[98] In 1961, Surgery chairman Harbison dedicated two weeks of the third-year block of surgery to anesthesiology. We expanded that later to three weeks. In that course, we emphasized life-support skills and the concept and practice of "titrated patient management" (for anesthetizing, resuscitating, and intensive care life supporting). We rotated students not only through our UHCP hospitals, but also to Allegheny General, Western Pennsylvania, Shadyside, and Mercy hospitals. This program maintained good relationships with Siker's group at Mercy Hospital and Patterson's and later Torpey's group at Allegheny General Hospital. I personally spent some time with medical students every week throughout these 18 years, both in small-group seminars using the Socratic approach and with coached hands-on instructions in the OR. I illustrated to students the difference between titration and the general medical practice of "rounding, prescribing, and leaving."

Since 1961 we have taught CPR-BLS to medical students with practice on Laerdal's manikins and patients, beginning with the first year. It began with the so-called "Medical Education for National Defense" course. In the second-year Introduction to Medicine course, we added CPR-ALS. We also gave lectures in pharmacology and physiology. For third-year students, in addition to the obligatory 2-3 week rotation through anesthesiology, we ran the anesthesia component of the course on surgical techniques practice on laboratory animals. In the fourth year, when electives became available in the late 1960s, we offered two months' experience in anesthesia or intensive care. Almost one-third of the class took such electives.

Respiratory Therapy

We needed the help of technicians from the start — primarily to look after equipment and to help with hospital-wide resuscitation attempts and prolonged ventilation outside the OR. Therefore, as soon as I arrived at PUH in 1961, I began a respiratory therapy school. My first anesthesiology resident, Kunkel, learned side-by-side with the technician trainees. He simultaneously helped me teach them. In 1961, PUH had no ventilators for prolonged artificial ventilation, inside or outside the OR and PAR. All artificial ventilation was done by bag-squeezing. There was only one Bird ventilator in the whole hospital, used occasionally on the wards to give "puffing" treatments for emphysema. I had brought several Moerch piston ventilators along from Baltimore. There were no resuscitation team and no crash cart for hospital-wide resuscitation.

First, as paramedic leader, I hired Gilbert Davis from a traditional respiratory therapy program in Chicago that focused on chronic lung disease patients. Gil was a smart, tall, African-American man on a then-exclusively white medical team. He quickly acquired our resuscitation-oriented approach and became an excellent teacher. Then I ran into PUH orderly Bela Eross, a bright young man who delivered oxygen cylinders throughout the hospital for nurses to give oxygen by open mask or nasal cannula. Eross, as a teenager, had escaped from his native Hungary during the failed anti-Communist revolution of 1956. He came via Austria to the United States. He became our first respiratory therapy student. A few years later, Davis moved to more lucrative fields elsewhere. Eross became the director of our respiratory therapy service and teaching programs, and one of the paramedic leaders of respiratory therapy developments at the national level. He was an innovator of life-saving devices, such as slip-proof, kink-proof connections between tubes and ventilators.

Under the technical leadership of Eross, we quickly expanded the respiratory therapist training program to the entire Health Center. Ours was the first school for respiratory therapists in Pennsylvania and one of the first six nationally.[9,98] In 1961-62 I gave daily lectures-seminars for respiratory therapy trainees, house staff, and other interested individuals. In 1963, I delegated the medical directorship of the respiratory therapy program to anesthesiologist Sidney Winchell and, after he left, to Clara Jean Ersoz, and then Ray Whitney. Thereafter, until now, the chief intensivist traditionally was also the medical director of respiratory therapy. In the 1970s, an affiliation with the Community College of Allegheny County expanded the program to 25 students per year. Between 1967 and 1975, the respiratory therapists also became valuable teachers of our EMT and paramedic training program, which was part of the development of modern ambulance services.[8]

Respiratory therapy teaching nationwide was helped by the simultaneous and independent publication of two textbooks — one by the Boston group,[100] which focused on physiology, and one by our group,[9] which focused on techniques and practices. The latter included chapters on respiratory therapists' services and schools and on ICU design and organization. At that time, I was frustrated daily by the gap between the existing knowledge in cardiorespiratory life support (which only a few physicians had) and the lack of implementation of this knowledge. I therefore personally focused more on controlling airway, ventilation, and oxygenation than on sophisticated pulmonary research, which became increasingly a status symbol among academic anesthesiologists.

We did, however, conduct some respiratory care research from the start. That included some medical students. For example, during his fourth-year research elective with us in 1967-68, I asked medical student Robert Carroll to acquire new knowledge on damage to the larynx and trachea caused by tubes and cuffs. He made the first measurements (in dogs) on lateral tracheal wall pressure produced by cuffs.[101] This led to the introduction of boggy large cuffs, which allowed intracuff pressure (which can be monitored) to equal lateral tracheal wall pressure. This was not possible with the narrow high-pressure cuffs that the industry then forced us to use. This study initiated a series of tube and cuff papers by us and others, particularly contributions by the Swedish visiting professor Lindholm and Grenvik, who began our CCM program's leadership in 1970. All this airway research prompted industry to change tube designs. More respiratory device developments followed.[102-104] The large, soft, atraumatic cuff goes back to Sanders's slip-on cuffs, which I had used already as a resident in 1950 for anesthesia, and at BCH for polio cases.[92,93]

Critical (Intensive) Care Medicine (CCM)

This field of practice, research, and education actually began at the BCH, but not under the name CCM.[91-93] In the 1950s, I had already come to the conclusion that every anesthesiologist who is skilled in managing patients in poor physical status, is (should be) also a reanimatologist and intensivist. Colleagues in other disciplines do not have such first-hand experience with titrated life support, as they practice medicine by rounding (or operating), prescribing, and then only intermittently seeing the patient. Therefore, I decided that the anesthesiologists' skills should be applied not only in the OR-PAR, but also beyond surgical anesthesia, inside and outside the ICU, for both surgical and medical patients in need of life support. This concept should be taught throughout, starting at the beginning of an anesthesia residency. Colleagues abroad followed this concept. Anesthesiologists in the U.S. gave it up.

We initiated in 1961-62 (at PUH) what became the world's first CCM physician fellowship training program (intensivist program).[10] This would have been impossible without the vision of our PUH administrator Ed Noroian. He honored my request for full-time CCM salaries, to start with two ICU faculty and three to five ICU fellowship stipends. This was a "first" in the 1960s. Noroian later became Chief Executive Officer at Columbia Medical Center in New York City.

I believe that we used the term "intensivist" already in 1960 at BCH. I expanded the intensivist (CCM) program from PUH[9,10,13-15] to VAH[105] in the mid-1960s, to CHP in the late 1960s,[105-107] and to MOH in the early 1970s. It all began with our first two anesthesiology residents, Kunkel and Milai. They worked part-time in the PUH ICU, with me, in their first year of residency, and as "most-time" CCM (part-time in the OR) fellows in their second and third years.[108] Thus, a more formalized fellowship training began during 1962-1964. Among the first foreign graduates as CCM fellows were Jean Penninckx and Arsene Mullie from Belgium, who returned to Bruegge to found what probably is the world's first EMS system from scene via ED through ICU under anesthesiologists' leadership. At the PUH, fellows learned daily on the job in the OR as well as in an improvised ICU and in hospital-wide resuscitation efforts. The three of us supervised nurses and respiratory therapists administering life support to patients requiring prolonged artificial ventilation and patients in shock or with multiple organ failure. During the mid-1960s, staff anesthesiologists, particularly Harris, Kirimli, and Winchell, helped me supervise the PUH ICU.

When I arrived in 1961, there was no ICU at PUH. There was an improvised "intensive" nursing care unit of a few beds on the fifth floor (the ORs were on the eleventh floor). That unit was meant primarily for suctioning tracheotomized patients. Only occasionally was a newly purchased Bennett or Bird pressure-set ventilator brought into action. In 1961, we expanded the use of that unit to include patients on prolonged artificial ventilation. Anesthesiology resident Kunkel and I began to cover the unit part-time. Nurses had been trained before our arrival with only one routine for these patients, as illustrated by the following example. I made the mistake of introducing cuffed tracheostomy tubes and volume-set piston ventilators before the nurses' re-training.[92] The plastic surgeons who did head-and-neck operations had ordered these nurses to plug the uncuffed metal tracheostomy tube before moving the patient to the floor. When one such patient had to be moved from that unit to the OR, a nurse plugged the tube of the spontaneously "breathing" patient without releasing the cuff. Cyanosis brought us to the rescue before onset of pulselessness. This near-casualty prompted me to initiate an ICU nurses training program at PUH, as we had at BCH.

In 1961-62, hospital director Shortliffe, chairman of Surgery Bahnson, trauma surgeon Drapanas, and I helped to design a new front wing for PUH. It was an aesthetic beauty, symmetrical like a Mayan pyramid. It included our new 16-bed medical/surgical ICU, which opened in 1964. The floor plan and staffing were presented at the history-making European Anesthesiology Congress in Vienna in 1962. Many forthcoming European intensivists later said they were influenced by our ICU experiences. With 14 ORs, the PAR, the 16-bed medical/surgical ICU, and new Anesthesiology offices all adjacent to each other on the same floor, and a new emergency department suite one floor below, our design became a model for the region and the country.

The first head nurse of our PAR at PUH was Rosalind Burrelli. She alternated as ICU nurse educator and, in the 1970s, co-initiated the American Association of Critical Care Nurses. The first head nurse of our new ICU was Mary Ann Scott. I asked her and our first female CCM fellow, Clara Jean Ersoz (nee Babb), to restructure our CCM course for nurses, which was the first in Pennsylvania. Much later, Scott became an initiator and leader of advanced ambulance services in Pittsburgh's southern suburbs. These services recently co-pioneered automatic external defibrillation in the hands of policemen, who can reach the scene faster than paramedics.

Clara Jean Ersoz was a unique, compassionate intensivist. She had anesthesiology training in Toronto and with Siker at Mercy Hospital. She was a caring, skillful, dynamic ICU physician with us in the 1960s. She had married a Turkish physician, Namik Ersoz, who also trained with Siker and later joined our anesthesiology team at MWH and MOH. Clara Jean was with me and Eva at CHP when our daughter died. After her CCM fellowship at PUH in 1966-67, I turned over to her the role of medical director of our ICU at PUH (1967-1969). She left us to become intensivist, and EMS and hospital medical director, based at the suburban St. Clair Hospital. Recently something terrible happened: Clara Jean and Namik, together with a young nephew, died tragically in the 1996 crash of TWA flight 800.

The early CCM fellows were anesthesiologists, but in the late 1960s, non-anesthesiologists also approached us about CCM training. The first, in 1968, were internists Claude Bernheim and Mark Eisenstein and surgeon Eldon Seibel. Jan Smith, our "super physician," was our CCM fellow from 1966 to 1969. During that time, he also became board-eligible in pulmonary medicine, working with Eugene Robin. He did research with Robin and me.

In 1968 I appointed Ake Grenvik as fellow, and in 1970 as faculty member. Grenvik, who was trained in both general and cardiothoracic surgery in Sweden, did research with anesthesiologist Holmdahl there. Later with us he did anesthesiology training and became a world leader in multidisciplinary CCM. In the early 1970s, other leaders emerged from our fellowship program, such as Jim Snyder and Don Benson. Then internists began to surpass the number of anesthesiologists in our and others' CCM fellowship programs. Our multidisciplinary fellowship program remained based in the Department of Anesthesiology. In the early 1970s, internist fellows included champions such as David Powner, who left us to be an intensivist in Phoenix and Indianapolis, but returned to Pittsburgh in 1990 and succeeded Grenvik as director of the CCM fellowship in 1996. Our fellow Dennis Greenbaum, internist of St. Vincent Hospital in New York City, and John Hoyt, anesthesiologist and one of our CCM fellows, later became presidents of the SCCM. I believe that those without an anesthesiology background who want to become intensivists should get first-hand experience under anesthesiologists' guidance in titrated life support of anesthetized patients in ORs, with experiences far beyond learning how to intubate. This principle was not followed through as I would have liked.

During 1961-62 we also initiated hospital-wide CPR team function, as part of the CCM program.[5,9,98,99] It also looked after life-threatening cases in the ED. Because of training needs, for several years, we did not hesitate to attempt CPR even on patients who were clearly hopeless. Then and now one often does not know if a case was futile until after the start of CPR. A short resuscitation attempt, which was inexpensive, helped residents and staff from various departments to become skilled in CPR-BLS and ALS. If the patient did not regain consciousness during the first three days, life-support efforts were discontinued. Essential in the hospital-wide CPR plan was that staff/faculty anesthesiologists participate in resuscitation attempts. At first, CPR was performed by our team of anesthesiology personnel. Soon, however, we assembled a hospital-wide multidisciplinary resuscitation team. This consisted of a staff anesthesiologist as team leader (helped as soon as feasible by an anesthesiology resident or fellow), and members of the departments of Surgery and Cardiology at the fellow level. Respiratory therapists rushed the crash cart to the scene. A somewhat superficial training program for nurses enabled some to come from the floor to help. Our CPR-BLS and ALS courses were for ICU and ED nurses plus residents together. Team members were notified by loudspeaker paging; radio beepers did not come about until the 1980s. In the early 1960s, we conducted the first education research on CPR-BLS for lay persons.[109] Our results gradually led to CPR-BLS teaching of the lay public by AHA and ARC. In the early

1960s, we also then started the first national CPR instructors courses for the AHA, through its Pennsylvania affiliate, spearheaded by Kirimli.[5,99]

At CHP, there was no ICU until 1968. It was delayed because of territorialism. Our pediatric ICU and CCM fellows program at CHP were initiated in 1968 as the second in the U.S. (after Penn) — initiated by a fine German anesthesia fellow (later associate), Stefan Kampschulte.

The CHP-ICU was "sparked" by a very personal loss. Our daughter Elizabeth, who was 11-12 in 1966, had asthma since infancy. On June 26, 1966 she developed lethal status asmaticus when Eva and I were at the AMA meeting in Chicago. When Eva and I reached CHP at midnight, I could resuscitate her heart and lungs, but not her brain. Elizabeth's death triggered four major developments: 1. organ donation by the next of kin of a heart-beating, brain-dead donor;[110] 2. initiation of the ICU at CHP in 1968;[106,107] 3. the first annual International Emergency and Critical Care Medicine teaching symposia of physicians and nurses which we conducted at Pitt for 10 years (1967-76); and 4. focusing of my own research on cerebral resuscitation. Elizabeth's image, suffering, spirit, courage, and kindness have been with me throughout since that fateful Independence Day of 1966. In the 1990s the Safar family endowed an award for Pitt medical students in CCM, in Elizabeth's memory.

Stephan Kampschulte had arrived from his native Germany as one of my new anesthesiology/CCM fellows on July 1, 1966. He helped me and Marcy to overcome resistance to an ICU at CHP. Chairman of Pediatrics Donald Medearis was compassionate and also crushed by Elizabeth's death. Our adult ICU fellows already rotated to CHP, beginning with anesthesiologist Pauline Lieberman, a CCM fellow from Israel (who later became the first ICU director in Israel at the Tel Hashomer Hospital). Unsupervised, she covered CHP part-time in an expanded PAR. In 1968, after his adult CCM fellowship, Kampschulte accepted my proposal to take over leadership at CHP. I sent him for two months to Jack Downes at my alma mater, the Children's Hospital of Philadelphia. The first pediatric ICU in the U.S. was initiated at Penn by my former Baltimore colleague and friend, Leonard Bachman, who was chief anesthesiologist there.[111] He had turned the ICU leadership over to John Downes around 1967. Kampschulte witnessed early attempts at computerized monitoring of physiologic variables. Under Downes, the program produced many pediatric intensivists, including (much later) the present chief of our CHP ICU in Pittsburgh, Ann Thompson.

Kampschulte assumed the leadership of our CHP ICU in 1968. He physically modernized its design and equipment. He initiated and developed our pediatric CCM fellowship program between 1968 and 1974. Among our pediatric CCM fellows in the early 1970s was Peter Holbrook, a pediatrician, who in turn became chief intensivist at the National Children's Hospital of Washington, D.C., and a president of the Society of Critical Care Medicine (SCCM). Other pediatrician CCM fellows under Kampschulte were Alan Fields, who joined Holbrook, and John Mickel, who became a leading pediatric intensivist at the Medical College of Virginia in Richmond. Mickel, Cook, and I worked on intracranial pressure-reducing measures in Reye's syndrome patients, but missed the cause of this terrible disease, namely aspirin plus virus. That was discovered soon thereafter by Australian colleagues.

Kampschulte's style was that of a caring, strong, and benevolent leader. He quickly gained the loyalty of nurses and colleagues, because of his competence, charming personality, and workaholism. He required that the department chairmen from Anesthesiology (Safar), Pediatric Surgery (Kiesewetter), Pediatric Psychiatry (Reinhard), and Pediatrics (Medearis, Oliver) round in his ICU, each once a week.

During Kampschulte's time, there was another resident applicant from Germany, Marie Louise Lembcke, the daughter of a renowned German surgeon. She was trained in anesthesiology in Munich but wanted a residency with us. Although

Kampschulte was not sure whether we should take her on, I appointed her. Soon she and Kampschulte fell in love, and they married in 1971. She became one of our best anesthesiology residents and a pain-controlling staff anesthesiologist. They started their family in Pittsburgh but returned to their native Munich, for personal reasons, in the mid-1970s. Before their return to Germany, the CHP staff and administration thanked and honored Kampschulte for his extremely dedicated ICU work. He remained in touch by visiting Pitt almost every year. We and other Pitt colleagues visited the Kampschultes. Kampschulte's career in Germany led to his position as chief anesthesiologist at Munich's largest city hospital, in Schwabing. He visited us in Pittsburgh in May 1994 for our third international resuscitation researchers' conference and my 70th birthday. He chaired a pediatric resuscitation research discussion group. His words there were the last ones recorded. Ten days after his return to Munich, in seemingly perfect health and fitness, while with his wife at a concert, he was struck by a massive embolic stroke, followed by brain swelling and death. Downes and I, as well as Germany's CCM leader Peter Lawin, one of Kampschulte's earlier mentors, went to the memorial service. The grieving lasted a long time. Stephan did so much for Pitt, for Munich, and for the many physicians that he trained and influenced. He is survived by his wife and three wonderful children.

After Kampschulte's departure from CHP, interim chief intensivists there included anesthesiologist Robert Binda and pediatric anesthesiologist Anthony Galvis (who came to us from Johns Hopkins). In 1981, Peter Winter recruited Ann Thompson. She is board certified in both anesthesiology and pediatrics. In the 1990s, she expanded our CHP-ICU into a CCM center of over 100 beds. She and Kochanek developed our CCM fellowship program into the largest and most research-oriented pediatric CCM program in the U.S. In 2000, Ann became SCCM president.

At VAH, in the 1960s, I enticed Kirimli not only to succeed Franklin as chief anesthesiologist, but also to design a new ICU.[105] That ICU became a modern consolidation of the medical ICU, surgical ICU, and coronary care unit adjacent to each other, with a total of 26 beds. It created an amiable collaboration with the hospital's chief surgeon, Francis Jackson (who also collaborated with me in the EMS arena in the 1960s) and his successor John Stremple (who collaborated with me on military and disaster medicine in the 1970s and 1980s). The director of our ICU at the VAH was Jan Smith in the 1960s, and a variety of rotating faculty members later. During Winter's time, that VAH ICU became the basis for the careers of well-known CCM researcher Michael Pinsky and award-winning CCM teacher Paul Rogers, both internists in our department.

At MOH, Arnold Sladen become chief intensivist of a new surgical ICU. Thus, between 1962 and 1978, our growing department fathered ICUs in four of our six hospitals. All four served our CCM physician fellowship training program. Physicians at MWH used their PAR for occasional patients in need of intensive care, and EEH used the ICU at adjacent PUH. By the 1990s, under Grenvik, Snyder, and Powner, our CCM program had expanded to 10 ICUs, about 30 full-time faculty, and 30 CCM fellows at a time.

In May 1967, we held the world's first international symposium on "Acute Medicine," later named the "International Symposium on Emergency and Critical Care Medicine." I called it "ECCM." We continued these teaching programs every May for 10 years (1967-1976). They came about after Elizabeth's death, from conversations I had with Kirimli, who had an asthmatic son. He and I agreed that the general attitudes in the medical profession about respiratory emergencies must be changed. The symposia quickly expanded from respiratory emergencies to include various acute vital organ failures. In the early 1970s, the specialty practiced by intensivists increasingly adopted the term "CCM," mostly under the influence of Max Harry Weil. Annual attendance at our Pitt symposia, which meant to teach clinicians "how to do it," ranged between 600 and 1,600 physicians and nurses per meeting. Grenvik's co-leadership was crucial. Guest faculty represented the who's who around the world in acute medicine. Weil had started shock researchers' symposia in Los Angeles in the early 1960s. After our symposia began, he expanded his into CCM symposia, which he moved from Los Angeles to Las Vegas in the 1980s. The Pittsburgh and Los Angeles/Las Vegas symposia represented the annual SCCM congresses between the founding of the SCCM in 1971 and 1975. In 1976, we stopped our symposia because the SCCM was able to carry on with its own annual teaching and scientific symposia.

Many new programs were initiated or catalyzed out of the ECCM May symposia in Pittsburgh. For example, Maurice Albin, whom I had recruited from Cleveland to Pittsburgh as a highly productive professor of neuroanesthesiology, and neurosurgeon Thomas Langfitt of Penn, who was a speaker at our May 1972 symposium, plotted the Society of Neurological Anesthesia and Neurologic Supportive Care (SNANSC) (see Bull. Anesthesia History, April 1998). Albin, when after his time in Pittsburgh, asked how it was working in Safar's "hurricane," replied that in the center of the hurricane it is (was) quite peaceful. Albin reported that a UPMC hospital administrator after "negotiations" with me present, felt "stripped." I did not realize then that administrators often felt forced by me to give in on our demands. I felt they did it because they agreed with my goals (table 2, see page 343). I also tried to tie our hospital-based activities to the "lower campus," i.e., the non-health professions. Edison Montgomery, a frequent troubleshooting academic administrator, contributed much to such bridge building and was a strong supporter of our and others' innovative programs.

Among colleagues in the other disciplines, our CCM program at Pitt had the greatest collaboration and support in the 1960s from the research-oriented, imaginative chief of Pulmonary Medicine, Eugene Robin. Robin was enthusiastic about my idea of studying the pathophysiology of so-called "natural dying" in very old persons. I asked Robin to teach our fellow Kunkel about body-fluid compartment measurements and a new method for measuring intracellular pH. Kunkel, under our joint guidance, applied this to a half-dozen naturally dying patients at a local "old-age" hospital. These data, which remained unpublished, suggested that slow dying from cancer or multiple organ failure in very old age involved the closing off of peripheral blood flow, resulting in centralization of the remaining cardiac output (like the diving seal), intracellular acidosis, and dehydration, while gas and acid-base values in arterial blood remained surprisingly normal until the end.

CPR training programs, which we started immediately in Pittsburgh in the early 1960s, used the most modern Laerdal training materials.[81] We initiated the first American Heart Association (AHA) CPR instructors' courses (under Kirimli) while I was, in 1963, a founding member of the AHA's Guidelines-setting Committee for CPR and emergency cardiac care, together with Jim Elam, Jim Jude, Archer Gordon, and Baltimore cardiologist Leonard Scherlis. This committee was most productive from 1963 to 1969; then it was replaced by a new committee of mostly nonresearchers. CPR training programs at the University of Pittsburgh hospitals and in the surrounding communities were directed by our anesthesiologists Kirimli, Winchell, Benson, Sladen, and Paul Berkebile. These CPR training activities in Pittsburgh influenced those at the state, national, and international levels.[5,99] At Pitt, as in Baltimore, our resuscitation instruction for medical students included dog laboratory demonstrations with practice sessions every 2-4 weeks throughout the 1960s. These courses, conducted by Harris, Kirimli, Kampschulte, Hugo Pfaeffle, myself and others, continued to include open-chest CPR, as we did at BCH in the 1950s.

I had started educational research in CPR with our volunteer experiments in Baltimore, which documented the ability of laypersons to acquire the skills to perform steps A and B after seeing a live demonstration on curarized volunteers.[59] In 1964, I initiated Winchell to conduct a study that documented the acquisition of skills to perform steps A and B plus C on a Laerdal manikin that we had modified for this purpose.[109] This modification led to the development of Laerdal's recording Resusci-Ann Manikin.[81] All of this opened the door for the training of nonphysicians in CPR-BLS at the national level, unfortunately with almost a decade's delay. At the AHA-NRC guidelines conference for CPR of 1966, training of laypersons in CPR steps ABC was not (yet) recommended. The AHA objected to a Cleveland-based movement for laypersons' training in CPR-BLS, led by Claude Beck and Mrs. Horvitz ("Resuscitators of America"). I should have defended them more. After data were available, I did strongly support a similar movement in the 1980s, out of Chicago, the "Save-a-Life Foundation" (SALF), led by Mrs. Carol Spizzirri. In the early 1970s, we showed in the study led by Paul Berkebile, one of our best residents and later staff anesthesiologists, that external CPR self-training with audiotape-coached practice on Laerdal manikins, aided by illustrations on flip charts, is more effective than instructor courses.[112] The same results supported self-training in life-supporting first aid (LSFA) for adults[113] and children.[114] Berkebile continued helping the AHA.

Asmund Laerdal and I introduced the term LSFA in the 1960s, defined as CPR-BLS plus external hemorrhage control, positioning for shock or coma, and "rescue pull." We also used illustrations to encourage people to practice certain steps on one another without a manikin. I knew then that the courses sponsored by the AHA and ARC would not reach the majority of the population. They have until now promoted too-detailed, instructor-led courses of fixed hours, which can reach only a few. What we tried and failed to get widely accepted is now being revisited by a similarly positive study of self practice on inexpensive take-home manikins coached by a special (tailor-made) videotape designed and researched by Allan Braslow, our long-standing consultant on education research.[115] A great disappointment in my implementation efforts over two decades has been the reluctance of the responsible agencies to use and apply such mass-training methods. We recently reviewed this topic and again recommended action.[75,116]

At the international level, I was asked already in 1958 to report the "new First Aid" at the International Red Cross in Geneva. Asmund Laerdal sponsored two history-making researchers' and guidelines' conferences during the 1960s.[117,118] In 1967, the World Federation of Societies of Anesthesiologists (WFSA) asked me to write the CPR instructors' manual for anesthesiologists and others.[5] In its second edition (1982), this manual became a booklet and, in its third edition (1988), a book on Cardiopulmonary Cerebral Resuscitation (CPCR). It was translated into 12 languages, and over 200,000 copies were distributed or sold with the help of Laerdal Medical, who illustrated, printed and financed the venture. W.B. Saunders published the English version of 1988.

Emergency Medical Services

The development of EMS we began in the late 1950s in Baltimore. The following were obvious to me then: 1. Because of the time constraints dictated by the vulnerability of the brain and heart, we should take to the scene, inside and outside hospitals, the resuscitation and lifesupport measures that we have learned as anesthesiologists. 2. EMS systems must be more than ambulances; they must include resuscitation and life support at the scene and during transport to (and in) the ED, OR, and ICU of the most appropriate hospital.[119,120] These links of the EMS chain should be tied together by communication and education, and upgraded periodically, based on ongoing evaluation. 3. The EMS system is only as effective as its weakest link and the weakest step of CPCR within each link (fig. 11). 4. EMS development should be multidisciplinary and adapted to the needs and potentials of each region.

There were no EMS systems in the 1950s. Most patients with acutely life-threatening conditions were picked up and rushed to the nearest hospital by police or firemen in station wagons or hearses, usually without an attendant at the patient's side and without life support. Some "ambulance services" were staffed by firefighters or policemen who had received first-aid training, which at that time consisted of splinting, bandaging, and giving back-pressure arm-lift artificial ventilation; this was difficult to do in the ambulance. Some ambulances were equipped with suck-and-blow pressure-cycled oxygen resuscitators or inhalators to be used via a face mask, which in our patient trials did not produce adequate ventilation. Paluel Flagg's attempt in New York to take tracheal intubation to the streets was a laudable, unique, and transient phenomenon. In New York and a few other cities where doctors rode in ambulances, they were interns without experience in life support.

In Europe, anesthesiologists developed and had been staffing general mobile ICUs since the early 1960s, starting in Magdeburg (by surgeon Werner Lembcke and anesthesiologist Wolfgang Röse), in Prague (by Bohumil Sefrna and anesthesiologists Hugo Keszler and Jiri Pokorny), in Moscow (by Negovsky's associates) and in Mainz and Ulm (by anesthesiologists Rudolf Frey, Fritz Ahnefeld and Wolfgang Dick). They all implemented Pittsburgh's guidelines[119] before we could. Subsequently, in the mid-1960s, an impressive mobile coronary care unit (CCU) program initiated in Belfast is often credited for the first mobile ICU to deliver ALS; that mobile CCU was meant mostly for preventing cardiac arrest in patients with suspected myocardial infarction. To me only general mobile ICUs have made sense, since lay persons cannot diagnose the course of sudden coma.

When I moved from Baltimore to Pittsburgh in 1961, I had a plan for EMS already sketched out.[120] Then, "ambulance services" in the city of Pittsburgh were provided by policemen with only limited first-aid training, using station wagon-type vehicles. A community surgeon who had vested interests in his title as "medical advisor for the Pittsburgh police ambulances" refused to train people in modern CPR according to the guidelines that were evolving through our efforts in Baltimore and Washington. Surgery Department Chairman Harbison told that surgeon to comply with my request. This was a rather unusual example of "gown" telling "town" what to do. We were backed up by the county medical society. It took many years of frustrating efforts before EMS implementation was accomplished in the city of Pittsburgh.

Since 1957, I had been an invited member of Sam Seeley's National Research Council (NRC)-National Academy of Sciences (NAS) ad hoc committees on artificial ventilation. Seeley was a former distinguished military surgeon with great spirit. He became interested in EMS improvements because of "trauma as a neglected disease."[121] In the early 1960s, he invited me to co-initiate a new NRC-NAS committee to develop national guidelines for community-wide EMS. I was the first nonsurgeon on the committee. The majority were orthopedic surgeons focusing on fractures. Another open-minded trauma surgeon on the committee was John Howard. I tried to represent resuscitation potentials not only of anesthesiology but also cardiology and other nonsurgical fields. CPR became the territory of cardiologists much later. Seeley asked me to chair the subcommittees on ambulances.[122] My work with Seeley's NRC committee in Washington enabled me to clarify at the national level the difference among mere first aiders, EMT-I level BLS ambulance attendants, and EMT-II level (paramedics) ALS ambulance attendants.

In 1964, I drafted for the Pittsburgh metropolitan area what apparently were the first guidelines for community-wide organization of EMS. This I did with the blessing of the Allegheny County Medical Society and the Health and Welfare Association and Hospital Council of Western Pennsylvania. When I presented these guidelines in 1965 at a meeting of the International Association for Traffic Medicine in Stockholm,[120] they were well received. Some Europeans implemented them right away. Authoritarian health ministries in "socialized countries" enabled implementation of such guidelines much faster than the democratic approach in the West would permit.

Between 1961 and 1966, I felt that efforts to improve EMS locally were hopeless without guidelines at the national level. Everybody with vested interests started to meddle. I wanted to publish our EMS guidelines with the blessing of American anesthesiologists, so that anesthesiology would get credit. Some academic leaders of anesthesiology preferred to seek recognition by publishing laboratory findings; they hesitated to step into what they perceived as the territories of trauma surgeons and orthopedic surgeons. Some political leaders of organized anesthesiology seemed more interested in OR work, the source of personal income. Both gave me the impression that they were not interested in getting the specialty involved in the prehospital arena, as European anesthesiologists had done following our urging in the 1960s. In Europe, EMS and CCM involvement brought much social recognition to anesthesiology.

In the U.S., John Bonica was an exception. His attitude toward all of this was global, like mine. When he was ASA president in 1965, he agreed with me that the ASA needed a Committee on Acute Medicine. He asked me to chair it.[1,9,119] I sought input from the committee members on my draft of community-wide EMS organization guidelines. After a long delay, ASA powers eventually agreed to have our EMS guidelines published — in the name of the ASA Committee on Acute Medicine — in *JAMA* in 1968.[119] These guidelines, accepted as a goal for Pittsburgh four years earlier, became the spark for national guidelines of the EMS Systems Act for the U.S. in the 1970s. We then also stressed and recommended "regional centralization of critical trauma care." Twenty years later it is finally being promoted by the International Trauma Anesthesia and Critical Care Society (ITACCS). In the 1960s, my only outspoken supporter of EMS among anesthesiologists (besides Bonica) was Eugene Nagel, then in Miami. Nagel became a kindred spirit on EMS activism as a leader at the national level, and as my successor as chairman of the ASA Committee on Acute Medicine. Nagel pioneered the guidance of ambulance paramedics by physicians via radio. Later, Nagel and I helped Israel's EMS through assisting Nancy Caroline.

Ambulance services are only one link of the EMS chain. I learned from non-physicians in Pittsburgh, as I did in Baltimore, where I had learned from ambulance leader Captain McMahon. In Pittsburgh, I sought out Mr. Richard Brose, a past ambulance leader with first-hand experience, who was in charge of EMS programs at the Pennsylvania Department of Health in Harrisburg. He and I developed the first ambulance design modification plan for station wagons and hearses.[123] In 1963, I discovered that Mr. Gerard Esposito was president of the Pennsylvania Ambulance Association. He was also in charge of an ambulance service in nearby Indiana, PA, which was more advanced. We improved the ambulance design guidelines for ALS.[124] The input from Brose and Esposito was valuable in drafting national guidelines for ambulance design and equipment and training of attendants at basic (EMT-I) and advanced (EMT-II, paramedic) levels. I drafted and pushed these guidelines through in the 1960s as chairman of the NAS-NRC EMS subcommittee.[8,122] Esposito and I were mission oriented.

In Pittsburgh, I had strong support throughout from my anesthesiology resident Don Benson and surgeons of town and gown, via the Allegheny County Medical Society. Nevertheless, EMS implementation efforts in Pittsburgh also ran into obstacles. Hospital administrators opposed our recommendation for regionalized centralization of special critical care (which also means expensive care), particularly for severe trauma cases. Leaders of the volunteer fire department ambulance services of Western

Pennsylvania, who controlled suburban ambulances, did not want to learn life-saving methods, nor to lose to trained, salaried attendants their country club status in the communities. It took me quite a while to learn about the vested interests and politics of these organizations. Fighting EMS politics taught me to first try to charm them (cocktail party technique); then to bypass underlings and go to the top man (helicopter technique); and if this also failed, to threaten briefly and then move in with force (bulldozer technique).

Our and others' efforts of the 1960s led to the EMS Systems Act of the federal government, which began in the early 1970s. That act, however, influenced change only through grants, not through law. In Western Pennsylvania, Esposito and I decided to bring the vested-interest groups together for a democratic approach. In 1968, I asked county medical society president Fred Brady, a fine neurosurgeon at Mercy Hospital, to meet with me in the plush basement restaurant of the Pittsburgh Playhouse. There, under the influence of martinis, we plotted the nation's first Community Council on EMS. In 1969, with the help of my associate Steve Galla and VAH surgeon Francis Jackson, we created a "white paper" for EMS community councils throughout Pennsylvania, sponsored by the state medical society. Ours was the first state to set up such councils state-wide.[125] This move was made possible by the fact that an anesthesiologist, my old friend Leonard Bachman, was Secretary of Health of the Commonwealth of Pennsylvania from 1972 to 1979. He had run for U.S. Congress in 1964 but lost the election. He gave up his position as chief anesthesiologist at Children's Hospital of Philadelphia in 1972. He credited me for having led the effort to professionalize ambulance services. I credit Bachman also for his handling of the legionnaires disease outbreak in Philadelphia, for wise handling of federal EMS grants, and for having hired emergency physician Arnold Muller to run state EMS. Muller became Bachman's successor with the next (Republican) administration.

In Pittsburgh, I tried to make peace between feuding groups by arranging cocktail parties. When we came to Pittsburgh, most private clubs discriminated against Blacks and Jews. I refused to join them. I used only public restaurants for recruitment dinners for our department. I was able also to accomplish medicopolitical networking for EMS and other missions, without being a member of any social club.

David Lawrence, the former mayor of Pittsburgh and former governor of Pennsylvania, was one of the finest leaders of the Democratic Party in the U.S. He was the pioneer who brought about collaboration between the labor unions and local billionaires to achieve the first renaissance of Pittsburgh, one of America's most industrialized cities. I have his family's permission to talk about his death: In October 1966, while in retirement, Lawrence gave a political speech in our city's symphony hall, a few blocks from PUH. He collapsed with what seemed to be sudden cardiac death. A nurse and a physician in the audience started CPR steps A, B, and C. The police ambulance service, then still staffed with policemen untrained in life support, came late and did "scoop and run" to PUH without life support. I happened to be in the ED when the governor arrived. I immediately started steps A, B, and C, found him in ventricular fibrillation, countershocked him successfully, intubated his trachea, and, using my ventilation bronchoscope, cleaned out his lungs, which were full of his last meal. There was excellent return of his heartbeat. He did not seem to have a myocardial infarction. We continued all-out life support in the ICU. Jan Smith was CCM fellow on call. The patient unfortunately, in spite of what Smith remembers as excellent life support, showed no recovery of brain function except spontaneous breathing. He remained unconscious and unresponsive. It soon became evident that although he did not develop the clinical picture of brain death (which then was only vaguely understood), he would remain comatose or severely brain damaged forever. Experience at that time indicated that several days' unconsciousness after a cardiac arrest and restoration of spontaneous circulation meant permanent severe brain damage. Therefore, his primary physician (Campbell Moses), his family, and I jointly decided to withdraw life support. He died soon from the usual complications of coma. Governor Lawrence's tragic death was followed by positive developments: 1. To my knowledge, this was the first case of open discontinuance of all futile "life-support" efforts in the absence of "whole brain death." A decade later, letting patients in persistent vegetative state die with dignity was declared ethical and legal by a Presidential Commission. 2. This event drew attention to the need for an improved local ambulance service. This led to the Freedom House project and then the present EMS system, both leaders for U.S.-wide EMS developments. Our improvement efforts were slowed by Pittsburgh's mayor in the early 1970s, who backed the police department's jealous grip on "ambulance service."

The Freedom House Enterprise (FHE) ambulance project of 1967-1975 was something unique. In 1967, leaders of the predominantly African-American community near PUH asked the hospital's administrator, Edward Noroian, to advise them on the type of ambulance to buy to take sick citizens to PUH for checkups. Noroian referred them to me. Although I was not interested in elective transport for nonemergent conditions, I saw an opportunity for Pittsburgh and beyond and offered them a deal: My department would help them (gratis) not only to get ambulances, but also to transport critically ill or injured patients with life support, if they would let us train attendants as a pilot project. For taking grandmother to the hospital, they could use a taxi. Another challenge occurred to me: we would try to train "unemployable Blacks" as EMTs and paramedics.[126,127] They would staff an experimental ambulance service to test the national ambulance design, equipment, and training guidelines we had just developed at the NRC committee. They agreed. The project eventually included 44 African-Americans who had been labeled "unemployable." This program, started in 1967, occurred during times which included summer revolts in U.S. cities (outcries for civil rights, re-distribution of wealth and peace abroad), and the murders of Martin Luther King (in April 1986) and Robert Kennedy (in June 1968).

I delegated the medical direction of the FHE program to Benson and Esposito from 1967 to 1969 and to Nancy Caroline from 1973 to 1975 (see the 1970s). From 1969 to 1973, the medical direction was erratic because I left for a one-year sabbatical and, when I returned, had to struggle with financing the anesthesiology department. Also, Benson left for military duty, and none of our department's other trainees or staff physicians showed commitment to prehospital EMS.

Benson, who in 1967 began work with the FHE during the third year of his anesthesiology/CCM training, received his stipend in part from an NIH anesthesiology fellowship. Such grants had been promoted at the national level by Manny Papper. After fulfilling his military duties, Benson returned to us in 1970 as assistant professor and continued working with ambulances. Frustrated with the FHE program, he focused on the suburbs. There he became the first to demonstrate that volunteer fire department ambulance attendants could be trained as ALS paramedics. Benson later became chairman of Anesthesiology at the Northeastern Ohio Medical School in Akron. He also achieved a doctorate degree in education. He was and still is wise and education-oriented about EMS.

Research

In 1961, immediately upon my arrival in Pittsburgh, with input from my associates and residents, I "packaged" the known to be effective resuscitation steps into the CPCR system of 3x3 steps (fig. 11). Phase III, PLS, paid already attention to the brain, including step H for hypothermia.

In the 1960s, despite the overwhelming clinical service tasks described above, we delved into research activities from the beginning. Experimental research needs teamwork under leadership. Much of my research in Pittsburgh would not have been possible without my associates and fellows and co-investigators from other specialties. Owing to my lack of a formal education in research, having learned mostly on the job from experience and from co-investigators, I relied on my colleagues for data management, statistics, and later for computerism.

My forte in research, according to collaborators, has been a general appreciation of basic pathophysiologic processes (before the molecular revolution), a clinically induced instinct of what is important, and raising new ideas, questions, and initiatives, often many years before others did. I considered most of the research projects that I initiated over the past 45 years potentially to be of immediate clinical importance. I then gathered and coached into action the teams necessary to do the documenting research. In laboratory research I tried to be systematic, pursuing unpursued questions (hypotheses) with series of (hopefully publishable) studies on the same topic. The results of each study raised questions for the next.

Throughout, I have had a disdain for two now-so-popular priorities: 1. A new device or method is looking for a project. 2. A donor's funding priority is dictating the project's topic, goal, hypothesis, and methods. We somehow remained productive with my perhaps naïve attitude of "get started and later look for money to finish it." I usually led the protocol-writing and participated in pilot experiments, modifications, troubleshooting, and the first definitive experiments. I usually sought input from several consultants while developing the protocol. I then delegated team leadership for series of experiments to colleagues, mostly research fellows, keeping in touch almost daily. I also was deeply involved in interpreting data and writing abstracts and papers for which I assumed ultimate responsibility, whether I was author or co-author. Fellow team leaders, who pursued my ideas and protocols experimentally, usually became first authors. This is fair and important for their careers. I did not like the European custom of having the name of the chairman attached to work by others without personal involvement.

Laboratory research at our Pitt department began immediately after I arrived in Pittsburgh in 1961. Stephen Galla and I applied successfully to the NIH for a large laboratory-construction grant. We used it to develop half of the tenth floor of the connecting wing between PUH and the medical school building (Scaife Hall) into our department's core research laboratory space. This is still one of the present department's three research lab spaces.

I appointed Galla, a fine, religious, loyal colleague and friend, as coordinator of the laboratories throughout the 1960s and early 1970s. He obtained an NIH grant to study the effects of anesthetics, particularly halothane, on cardiovascular variables. He sought advice from Pitt professor Robert Olson, a nationally recognized biochemist who worked on myocardial metabolism. In the mid-1960s, Galla also attempted to evaluate Pauling's theory of narcosis, with the help of a chemist from the Koppers Company. Galla helped our fellows and me to write papers. He was acting chairman of the department during my sabbatical in 1969-70. In the mid-1970s, Galla became severely ill from juvenile diabetes. He "retired" into helping others finance their estates. After his much-too-early death in 1981 (which he controlled), I initiated the Galla memorial lectureship (which needs an endowment).

Steven Galla, in 1969, recruited Mr. Henry Alexander as laboratory technician. Alexander was then a very young African-American citizen of Pittsburgh, who became a role model for his community. His skillful devotion to the best animal laboratory research has continued, so far, for over 31 years. He continued with me in our resuscitation research center, since 1979.

Galla discovered Mr. William Stezoski, a laboratory technician with Olson in 1953-66 and with cardiologist Scheuer in 1966-71. When Scheuer left Pittsburgh in 1971, we attracted Stezoski as our chief lab technician. Stezoski developed an enormous publication list as co-author. He has been a skillful, dedicated, loyal laboratory coordinator. He coached many young research fellows (medical students and physicians) in lab methods and skills. In the 1980s, I decided to promote him to a full-time faculty position. To his credit, he is the only research assistant professor at our university without an academic degree. In 2003, Stezoski will have been at Pitt for 50 years (over 30 years with me, since 1979 at the IRRC). Stezoski and Alexander contributed much to the development of our many new animal models. I thus have been blessed with two superb laboratory technologists (and others) who trained numerous technicians, conveyed "tricks" in lab methods to fellows, and maintained continuity.

Our laboratory resuscitation research in the 1960s was supported by my U.S. Army resuscitation research grant, which I brought from Baltimore. It had begun in 1957 with about $10,000 per year (in 1990s purchasing power it would be about tenfold that amount) increasing to about $100,000 per year in 1969 (in 1990s purchasing power it would be about fivefold that amount). My Army grant had to be inactivated in 1969-70, when I went on a sabbatical leave. When I returned, I did not reapply to the Army because of my opposition to the Vietnam War and because Army-sponsored medical research was then done mostly intramurally. In the early 1970s, some money from our new anesthesiology practice plan and a small academic budget from the dean helped to support our core lab. Some research fellows were supported by our NIH training grant. Starting in the mid-1970s, I applied for NIH grants and succeeded with some.

For CPR, there were still many unanswered questions in the early 1960s. I first motivated Leroy Harris to spend time in the lab to evaluate, in dogs, various details regarding blood flow produced by CPR steps A-B-C. Much had to be worked out before we could consider specific guidelines for human resuscitation. Some of that work by Harris was the foundation for the still-recommended CPR-BLS ventilation:chest compression ratios of 2:15 (for one operator) and 1:5 (for two operators).[73] I always considered it a compromise. Attempts to improve the unpredictably borderline blood flow produced by standard external CPR-ABC with simultaneous ventilation/compression or abdominal compression were unimpressive; we gave it up.[73,128] Around 1980, these attempts to improve blood flow by pneumatic modifications of external CPR were resumed by Myron Weisfeldt and other Johns Hopkins investigators who used more sophisticated methods and clarified the mechanisms by which chest compressions move blood. Now I see ready for clinical guidelines only one method that improves CPR-ABC flow without need for devices, namely, intermittent abdominal compression (studied by Babbs).

My general interest in the pathophysiology and reversibility of dying processes moved "naturally" from airway obstruction[53] and apnea[58] to cardiac arrest,[69-73] to drowning,[76-79] to acute blood loss,[129-131] to exsanguination cardiac arrest[132,133] (applied by my later associate Torpey in Vietnam),[134] and to shock lung.[135-139]

From 1962 to 1966, we were blessed with a star research fellow from Japan, Masuhiko (Hiko) Takaori. I considered resuscitation from severe hemorrhage to be the next priority. I coached a systematic series of eight published studies by Takaori on the limits of acute normovolemic hemodilution, using various colloid and crystalloid solutions for the treatment of acute hemorrhage.[129-131] We documented that physiologic decompensation begins with a hematocrit of 10%, and cardiac arrests occur with a hematocrit under 5%. We invited the collaboration of renowned hematologist Jessica Lewis (the wife of our then chairman of Medicine, Jack Myers), who evaluated clotting disturbances with plasma substitutes.[131] Our work included some of the first papers on hydroxyethyl starch. Colloids proved superior to crystalloids. Takaori's research helped him to advance to chairman of Anesthesiology at Okoyama.

After my first contact with Negovsky (fig. 13), in 1962, at the first European Congress of Anesthesiology in Vienna,[140] and my visit with Negovsky in Moscow in 1963 (see The World),[141] we reproduced Negovsky's exsanguination-induced cardiac arrest experiments in Pittsburgh. Kirimli was team leader.[132,133] In five published studies with a modified dog model, we found Negovsky's arterial reinfusion with epinephrine to be effective in restarting the arrested heart without heart massage, but only with use of warm, heparinized, oxygenated blood — not the more clinically realistic cold bank blood or plasma substitute. Massive venous infusion of bank blood or plasma substitute, with epinephrine, pumped through the lungs with external CPR, could reverse exsanguination-induced cardiac arrest almost as rapidly.

In 1964, we were happy to have anesthesiologist Robert Loehning from Salt Lake City with us for his sabbatical year. He and I had known each other from my previous visits there. We did preliminary studies in dogs on intracranial pressure monitoring. Loehning had done good lab studies on brain swelling before. Loehning and I were also concerned about a still-lacking resuscitation approach to acute stroke, which we thought deserved the same immediate action as cardiac arrest. This concern is only now being appreciated by neurologists. We also tried to develop a dog model of epidural hemorrhage and hypothesized a toxic effect of extravascular blood on brain tissue. After Loehning's departure, my preoccupation with other topics and the need to focus kept these projects from getting off the ground, and our ideas were not published. The only publication from Loehning's good year with us concerned non-rebreathing valves. Our friendship continued on the ski slopes.

On brain trauma, I suggested to neurosurgeons, then and now, to do research on attempts at mitigating traumatic brain damage by wide decompression (let it swell), debridement (remove necrotic tissue, which poisons the adjacent brain tissue), and flushing blood and other toxic humors out of the CSF space, while directly cooling and medicating the brain. A wild idea? No – just simple and logical, but not focusing on a single mo-

Figure 13. Vladmir Negovsky, the "father of reanimatology," and Peter Safar, in Moscow in the 1970s.

lecular mechanism, the goal of popular and fundable research today.

Our CCM clinical research in the 1960s included the introduction of several techniques and apparatuses for resuscitation and long-term respiratory care. Clara Jean Ersoz improved long-term arterial catheterization.[142] Michael Hedden studied ways to prevent laryngotracheal damage by tubes and cuffs.[143] He also did some of the first computerized monitoring and measurements of blood O_2 content. He was a fine addition to our team, but later left us for private practice in Florida. Our new ideas on airway and ventilation devices[47-49,92,101,102] were transformed by manufacturers into profit. None except Laerdal[81,103,109] offered us research support, but I refused to go begging.

My research interests in the 1960s also included "progressive pulmonary consolidation" (PPC),[139] motivated by clinical observations I had made in Baltimore.[91,92,108] At the same time, Francis Moore of Harvard was calling it "shock lung." A few years later, Petty and Ashbough of Denver called it "adult respiratory distress syndrome" (ARDS). In patients we explored the hypothesis that PPC has a multifactorial pathogenesis that includes at least two of the following: trauma, shock, atelectasis, and sepsis.[139] The cytokine research explosion began a decade later. In the lab in the early 1960s, we explored oxygen toxicity as a possible factor in PPC; for this I initiated Stan Pautler, then a CCM fellow from Prague, to conduct dog studies with the help of pathologist Robert Totten.[135,136] We used a double-lumen endobronchial catheter to ventilate one lung with air, the other with oxygen. We found greater morphologic damage in the lung ventilated with 100% oxygen for longer than six hours.[136] This was mitigated but not prevented by artificial ventilation. Pautler returned to Prague, and after 1968 he returned as a Czech refugee staff anesthesiologist with us and became the faculty director of our PUH operating room anesthesia service. After my resignation as chairman, he became chief anesthesiologist of Pittsburgh's St. Francis Hospital.

In summary, in the 1960s my associates and I met the challenge to create — from scratch — a large department of academic physician anesthesia, which began "fathering" resuscitation, EMS, and ICU programs with impact beyond the local level.[144]

On the global scene, in the 1950s, my struggle for adaptation to and survival in America left little time and energy to think about the cold war. In the 1960s, however, in spite of department-initiating struggles, one month after my history-making first friendship meeting with Negovsky (in Vienna), we in Pittsburgh felt deeply how civilization was at the brink of self-destruction during the Cuban missile crisis in October 1962, the madness of superpower confrontation with nuclear weapons.

Department of Anesthesiology and Critical Care Medicine, 1970-1979

Sabbatical

The second decade of our department began with my first sabbatical, one year in San Francisco (7/1969-6/1970). This sabbatical year was a break that my family and I needed badly, I to retain my sanity. One-half of my salary was paid by Pitt, the other half by an NIH grant. I decided to learn from my friend John Severinghaus, who by then had become a professor and renowned researcher in the Department of Anesthesiology at the University of California, San Francisco (UCSF), in affiliation with the Cardiovascular Research Institute (CVRI). John has been a most impressive physiologist and researcher. He is not only a highly informed and genial thinker, but also a terrific team leader, gadgeteer, and scientist who communicates superbly with his colleagues. He and I have always given away our new ideas. The initiator and director of the CVRI was Julius Comroe, world-renowned pulmonary physiologist and general scientist. I had known both since the 1950s.

My scientific goals for the sabbatical year were three: 1. To study the literature on the pathophysiology of traumatic and ischemic brain damage; 2. to participate in mentored research if feasible; 3. to observe and learn from the CVRI for the later creation of a resuscitation research center or institute at Pitt. My commitment to explore cerebral resuscitation potentials after cardiac arrest was motivated in part by the fact that our daughter, Elizabeth, had died in 1966 with the clinical picture of brain death several days after Eva and I had rushed back from Chicago and I restarted her heart. I thought that methods to resuscitate lungs and heart had become quite effective and that post-ischemic-anoxic brain damage was the next challenge to focus on in resuscitation research.

Our second son Paul was born on June 17, 1969. I flew to San Francisco in July, and went into the mountains with Severinghaus for a high-altitude study. My year in San Francisco began with man's first landing on the moon. That night, I was stuck in traffic while looking at the moon and hearing on the radio "one small step for man, one giant leap for mankind." It truly was a year for contemplation and reaching into the sky. I had time to climb mountains, including Mount Rainier with Ted Eger under the guidance of Tom Hornbein. I skied on almost all the slopes around Lake Tahoe, and with my son Philip on Mount Hood in June.

That year, Nixon had just extended the war into Cambodia. The news and the peace movement about the war in Vietnam stirred memories of the killings in World War II. Our rented house was next to the Haight-Ashbury intersection and Golden Gate Park, where some hippies taught me the value of peacemaking and tolerance. Although I had been focused before on profession and family, the sins against humanity by the generation in power now became more apparent to me. Angry marches by the UCSF students and faculty included me, John Severinghaus, and my son Philip. That sensitive young boy, whom I had never allowed to play with guns, became a lawyer 15 years later in the U.S. Army. He remembers the peace marches in San Francisco positively. When he served with the U.S. Army in Germany, it was during the time when the Berlin Wall came down and the Gulf War exploded. He remained at heart a qualified pacifist, like his father. Philip's law and army career, I believe, had been motivated by a feeling of duty to the country and to help others.

In July 1969, Severinghaus took six fellows and me to White Mountain in the Sierra Nevada. Severinghaus wanted to pursue his idea that pulmonary artery hypertension could be the mechanism of high-altitude pulmonary edema. We "schlepped" lab equipment in regular automobiles up to the lab station. Within one day, Severinghaus had set up a physiology lab for human experiments. We stuck needles and catheters into each other, even on the nearby summit at 14,000 ft. The results suggested a causative relationship between high pulmonary artery pressure and shunting from subtle subclinical high-altitude pulmonary edema.[145]

Under Severinghaus's guidance, I conducted a dog study on brain pathophysiology. The question was one that Eugene Robin (pulmonary physiologist at Pitt) and I had raised a few years earlier. In patients with emphysema who have severe chronic hypercarbia and high bicarbonate levels, we had observed that coma can result during artificial hyperventilation with rapid normalization of arterial PCO_2.[146] In San Francisco, John and I reproduced this condition in bicarbonate-loaded dogs, helped by John's postdoctoral fellow, Edwin Nemoto. During blow-off of CO_2 to normocarbia, we found a drastic reduction in CBF.[147] Although the paper reporting these results would have been suitable for the *Journal of Applied Physiology*, I instead "donated" it as the first scientific paper of our newly established journal of the SCCM, *Critical Care Medicine*.[147] In addition, Severinghaus wanted me to document this CO_2 blow-off phenomenon in patients at San Francisco General Hospital. My attempts at CBF measurements in such patients failed. I learned not to attempt doing patient research (nor giving anesthesia) in a clinical environment where one is not at home.

Considering as outdated the then-current practices regarding the treatment of patients who were comatose after brain trauma or cardiac arrest, I spent much time that year studying, presenting, writing, and publishing literature reviews on this subject. These activities led to my conclusions and recommendations on "brain-oriented life support" for these patients.[5] During our year in San Francisco, I was blessed by secretarial help from Pittsburgh's team members Diana Mailing and Patricia Sands, and by Steve Galla's loyal role as an effective acting chairman in Pittsburgh.

Anesthesiology

In the summer of 1970, my family and I returned to Pittsburgh. I appointed Dave Torpey as chief anesthesiologist at Presby. This superb clinical teacher left us in 1973 to assume the chief position at Allegheny General Hospital, taking several of our faculty along, with my blessing. The chief anesthesiologist position at PUH was then assumed by Maurice Albin in 1973-76, and by Brian Smith in 1976-79. In the mid-1970s, we reached stable staffing. Residency and subspecialty programs were of high caliber as judged by my co-leaders.

In 1970-78, our OR anesthesia services were running smoothly. So were the residency and medical student programs, and special anesthesia services at PUH and in the specialty hospitals. Cardiothoracic anesthesia was under Malcolm Orr, neurosurgical anesthesia under Maurice Albin. Neuroanesthesia under Albin was most productive, with one or two fellows and an active laboratory research program. Albin chaired the Pennsylvania task force on spinal cord injuries and a spinal cord injury center at Pitt, newly created by him and Jannetta.

In 1975 I appointed Cook as residency program director. We succeeded in attracting 13 new U.S. graduates as anesthesia residents, which was rare for those days when anesthesiology was still not a very desired specialty in the U.S.

I myself directed the medical student program in the early years and then turned the coordination over to Winchell, Kirimli, Brian Smith, Berkebile, and Pat Sands. The nurse anesthetists' training program and services were crucial to keep this large department's OR coverage-by-teams going. In general, the collaboration between the nurse anesthetists and the physicians (who were responsible for every anesthesia administered) was good, thanks in part to the collegial and competent work of the nurses' leader Mr. Kaleita (who in 1974 moved on to the national office), Mr. Stanich, and Ms. DePaolis. I personally had the best collaboration in the OR with nurse anesthetists throughout, based on mutual respect and assistance. I insisted that physicians teach nurses (and learn from them).

A sore point in the 1990s has become the attendance at departmental meetings. Our department's conferences began in 1961 with weekly "grand rounds," where one session per month was devoted to the Western Pennsylvania Society of Anesthesiologists' guest speaker. In the early years it was preceded by a mortality and morbidity conference by Dr. Phillips. These weekly meetings were organized by a member of my department in conjunction with the president of the society, helped by the fact that many former members of our department had become anesthesiologists in several major community hospitals. Journal club sessions at faculty homes waxed and waned over the years. The Thursday 5 p.m. grand rounds lecture every week was expected to be attended by every resident, fellow, and faculty member who was not occupied with patients or out of town. The attendance in general was good, I believe because chairman and chiefs were there. In addition, of course, there were almost daily seminars, lectures, and conferences for the CCM program and for medical students, and the especially structured resident seminars. I personally could "drop in" on hospitals other than PUH and on the subspecialty programs only about once per month. Pat Sands's more frequent visits and phone conversations with the chiefs kept me in touch with the goings-on in the five hospitals outside PUH, where I was based.

In the 1970s, the University Health Center of Pittsburgh (UHCP) became a reality, spearheaded by our department. The

MOH Department of Anesthesiology was led since the early 1960s by Stephen Finestone. He joined our UHCP department in 1971 and became one of my effective allies in the further development of the department. Once we had successfully placed our associates in the six hospitals, the departments of other disciplines, still fragmented among hospitals, gradually followed our lead. At MOH, Internal Medicine particularly resisted integration. Much time was wasted on MOH turf issues at monthly medical school executive committee meetings. Some called Montefiore Hospital "Mount Fury." In order to remain functional, I brought my briefcase full of homework along to these boring, repetitive, unproductive fighting sessions that broke out among decent colleagues who were excellent clinicians and/or scientists. Discussions which seemed petty at the time can be explained by an urge for power (control), in order to gain money, trainees, patients, space, and co-workers — all considered essential to be "successful" as an academic department chairman. In 1973 the newly appointed chairman of Psychiatry, Tom Detre, just watched; he probably, quietly, considered remedies for later when he became Senior Vice Chancellor of Health Sciences in the 1980s.

Financial independence of our department became a major issue.[98] In the 1960s, the hospital administrators had allowed me to offer only roughly one-third to one-half of the salaries my colleagues could get elsewhere in the tri-state area. By 1970, it became clear to me that socialism surrounded by capitalism cannot survive. We had to gain control of the finances. In order to be competitive, I felt compelled to confront the administrators and force them to let us change from hospital-paid salaries to a practice plan for physicians. This allowed us to have at least some control over our department's budget. Faculty salary checks continued to come from the University. This was the first practice plan within our medical school. It helped us to achieve a stable full-time staff of about 60 faculty-staff members by the mid-1970s. The hospitals continued to provide the salaries for nurse anesthestists and 40 physician trainees. Our first practice-plan administrator was Gerald Cochran. He went on later to law school and became an assistant to our present chancellor of the University. Cochran's career is only one of several examples of our department having initiated careers of administrators. Being ignorant in financial matters, I then asked Rick Siker for advice on billing. His daughter Katherine became our second manager of the practice plan.

Neither our highly labor-intensive initiation work in the 1960s, nor the service and teaching engagements in the 1970s are reflected in the now-so-popular measures: the number of publications and the research dollars attracted from the outside.

Critical Care Medicine

The CCM program flourished in the 1970s under the wise and strong leadership of Ake Grenvik, M.D., Ph.D. He was a graduate of the Karolinska Institute of Stockholm and trained in anesthesiology, general surgery, cardiothoracic surgery, and research at the universities of Lund and Uppsala in Sweden. He had visited Pitt almost by accident in 1967-68. I had invited Martin Holmdahl, chief anesthesiologist and intensivist at Uppsala, to speak at our first International Emergency and Critical Care Medicine symposium in May 1967. Holmdahl could not come but sent his fellow, Grenvik, who gave an impressive presentation. We talked, and he accepted my offer for a fellowship. Returning in May 1968 for the second symposium, he brought his family to Pittsburgh. The Grenviks stayed for life. After he had served two years as a CCM fellow and simultaneously an anesthesiology resident (to satisfy the American Board of Anesthesiologists), I appointed Grenvik director of the ICU at PUH and, starting in 1971, of the CCM program. He became a star and the international leader of developments in multidisciplinary CCM from the 1970s to the 1990s. In 1996 our University named him distinguished professor.

The Grenviks and Safars have been close friends from the start. Ake and I skied together (snow and water) and traveled with our wives to Japan and elsewhere. We had similar work habits, except that he usually made ICU rounds at 4 a.m., while I tended to use the fresh morning hours of 4-6 a.m. for reading and writing. We shared a commitment to multidisciplinary CCM. Ake became a renowned worldwide lecturer. He co-pioneered brain-death certification, management of organ donors, and critical care triage. The Grenviks suffered a terrible loss, their son, Christer, from a glioblastoma multiforme, during his anesthesiology residency at Pittsburgh's Mercy Hospital. The family established a Christer Grenvik Memorial Award in the Society of Critical Care Medicine (SCCM) for work on biomedical ethics, topics Christer was interested in.

My philosophy regarding CCM in the 1960s and 1970s was the same as it is now: 1. Every well-trained anesthesiologist is (should be) a reanimatologist and intensivist. 2. Anesthesiologists-intensivists benefit the patient more if they work with those internists, pediatricians, and surgeons (and colleagues in other relevant disciplines, in recent years also emergency medicine physicians) who are acute medicine-oriented. 3. All physicians with special interest in emergency and long-term resuscitation should have "super-specialty" training in life support, so that they can replace each other to provide round-the-clock coverage for emergency life support, and help each other in subacute care. 4. Life support is titrated care by the physician present (not rounding and prescribing) that is best learned in extensive, guided OR anesthesia experience.

At the national level, I co-initiated the plan for a multidisciplinary SCCM when I met early in 1968 with internist Max Harry Weil and surgeon William Shoemaker at the Federation Meeting in Atlantic City, and then at Weil's shock conference in Los Angeles. In the winter of 1969-70, during my sabbatical's weekend ski excursions from San Francisco to Lake Tahoe, Ake Grenvik and I drafted the SCCM guidelines for ICU organization[11] and CCM physician education,[12] which were later finalized by SCCM committees chaired by Downes [11] and Winter.[12] In February 1970, Weil hosted a gathering in Los Angeles of about 20 intensive care-committed physicians. We founded the SCCM. Weil,[148] and Safar[149] each hosted one SCCM symposium each year between 1972 and 1975. As the second president of the SCCM in 1972-73, I gave the green light for the world's first CCM journal. It seemed crazy to start a new journal with only 100 society members, when publishers considered 1,000 a minimal requirement. I took a chance. I asked Shoemaker to be first editor, which he did superbly for 15 years. To start, Shoemaker recruited an interim "publisher" as entrepreneur until two years later when Williams and Wilkins took it on.

In 1973, our department at Pitt hosted the Association of University Anesthesiologists. During that event, Grenvik and I helped Bill Hamilton of San Francisco, then chairman of the American Board of Anesthesiology, to invite to Pittsburgh the board chairmen of Internal Medicine, Surgery, and Pediatrics, to discuss and hopefully agree on a unified mechanism for multidisciplinary CCM certification for trained or experienced intensivists, regardless of base discipline. Although all board leaders present on that occasion seemed to agree, subsequent negotiations, which Grenvik spearheaded into the future, ran into one obstacle after another — all due to the territorialism of the specialties' leaders. In the 1980s I was in full-time research, withdrawn from politics. The end result was that each base-specialty board developed its own certification process for a CCM subspecialty fellowship program and for its candidates. I still consider this a mistake.

The tenth (and last) international May symposium that we conducted on "emergency and critical care medicine," which represented the fifth annual meeting of SCCM, in 1976, had about 1,200 registrants from 29 countries, 100 faculty, 100 scientific papers, 6 panels, and 11 special lectures. We concluded these annual meetings, which were a considerable drain on time and manpower, because the SCCM began its own annual meetings.

At UHCP, struggles continued for many years (sometimes subtle and sometimes not so subtle) over who is in charge of the ICU patient. When life support was the primary concern, the ever-present intensivist finally "won," not as primary physician, but as obligatory team member in charge of life support. I promoted the appreciation that there is a difference between full-time ICU team members (or team leaders) and ICU consultants who come and go. A so-called "inverse hierarchy principle" prevailed in major teaching hospitals nationwide, particularly with departments of internal medicine, whereby control of the most-critically ill patients was delegated to the most junior house officers. This was a source of major frustrations that I endured for over two decades. Medicine and surgery began rotating their own house staff through our ICUs at PUH and VAH, which helped the full-time ICU staff under Grenvik to manage greater patient responsibility.

In 1971, I successfully applied to the NIH for an anesthesiology fellowship training grant, which I used to give selected anesthesia residents a third year as CCM fellows. Columbia University anesthesiology chairman Emanuel Papper had influenced the NIH to make such third-year fellowships available. Our CCM fellows who benefited most from this included Don Benson, who worked with me on EMS programs, and James Snyder, who became a CCM leader. Both rose to tenured professorships.

James Snyder wanted to become an anesthesiologist after a time in the Navy. My recruitment interview of him (in Florida) was memorable for its heavy doses of scotch, which made us deeply philosophical about the world and the medical profession. Snyder's experience at Pitt changed his life. He became one of our best anesthesiology residents and CCM fellows before joining our faculty. He was a part-time laboratory research fellow already in his first year of residency. In 1996, Snyder became Grenvik's successor as the director of our CCM division, and David Powner, another of our special fellows of the early 1970s, an internist, succeeded Grenvik as leader of our CCM fellowship program in the 1990s.

In 1972, I asked our Dean Donald Medearis to consider creating a multidisciplinary CCM program advisory committee, which was appointed the next year. Grenvik and I raised the possibility of making CCM a separate department, but the dean and other department chairmen insisted that the CCM program remain administratively within the Department of Anesthesiology. Financial security in Anesthesiology was the main reason quoted. I believe that the chairmen of Surgery and Medicine feared loss of control over CCM, which other specialties increasingly considered to be "icing on the cake." The result was the same when I re-opened the question in 1976 and when Grenvik again did so in the 1990s. In the latter case, chairman Winter wanted to hold on to the CCM program. After all, Thomas Detre, senior vice chancellor of Health Sciences, called our CCM program "a jewel in the crown of the University and Medical Center." Indeed, during worldwide travels in the 1960s and 1970s, I found our University's medical school most often known for resuscitation research, EMS, and CCM — our department's programs. In the 1980s, Starzl's transplant surgery made Pitt famous worldwide.

Ethics

Modern resuscitation and intensive care presented a challenge to biomedical ethics. Surgeon Bernard Fisher (who had been at Penn when I was an anesthesiology resident there) began transplanting cadaver kidneys into patients at Pitt in the 1950s. Letting these organs deteriorate after clinical death made no sense. Certification of brain death had no guidelines in the mid-1960s. When our daughter Elizabeth developed brain death on July 1, 1966, Eva and I gave permission to remove her organs. In 1967, Rosomoff and I raised the brain-death topic at the

Second International CPR Researchers' Conference held in Oslo under the chairmanship of Ivar Lund, Bjorn Lind, and Asmund Laerdal.[118] Immediately thereafter, I co-initiated a committee in Pittsburgh, chaired by Cyril Wecht, a lawyer and forensic medicine specialist.[110] We came up with local guidelines for brain-death determination and certification the year before the Harvard group published their recommendations. The Harvard group focused on organ donation, whereas we considered the picture of brain death to be equated with death, irrespective of organ donation. I asked Grenvik, also a member of the local task force, to take the lead, which he did in an exemplary way. Grenvik, Snyder, Powner, and Safar contributed to the U.S. Presidential Commission's statements on the re-definition of death.[150]

Next came allowing patients in a persistent vegetative state to die. For Pitt, this began with the tragic case of Governor Lawrence in 1966. Since the 1960s, there have been ongoing dialogues at the local, national, and international levels concerning the ethical dilemmas produced by resuscitation and intensive care. In the 1970s, our group provided input for the U.S. Presidential Commission on relinquishing life support in futile cases without brain death.[151] We initiated early discussions with philosophers on this subject.[17,152] Grenvik's recommendations of "CCM triage" and "letting die" in futile situations were crucial,[153] In the 1980s, I was invited to contribute to two Harvard University-initiated guidelines about "The Physician's Responsibility in Hopeless Cases".[154,155] My main contributions were the concepts that physicians must decide on the appropriate level of care,[154] and that "titrating terminal care" can be learned from CCM.[155,156] The informed-consent dilemma for clinical CPCR research I first confronted in 1978. We thereby triggered the development of the multidisciplinary biomedical ethics program at Pitt, now under attorney Alan Meisel.[157]

Emergency Medical Services

In the 1970s, our and others' EMS community programs which had become increasingly frustrating, aroused national attention. National guidelines were based on our guidelines.[7] Development in Washington of the National EMS Systems Act was led by Chicago surgeon David Boyd, and helped by progressive administrators like Mr. Reardon and his assistant Mary Kent. I could help a little bit in that legislative move because politicking by my former anesthesiology resident Sol Edelstein had resulted in President Ford inviting me to serve on the White House Interagency Advisory Committee on EMS in the early 1970s. I learned through the grapevine that a similar attempt earlier, in President Nixon's time, did not lead to a similar invitation because Nixon saw on my CV that I was a member of Common Cause, a liberal watchdog of governments.

Between 1961 and 1979, I was frustratingly obsessed with the implementation gap in acute medicine (EMS and CCM). Inside and outside of hospitals, resuscitation potentials were not being applied. I therefore encouraged numerous colleagues to help advance EMS. We created an increasingly positive collaboration between town and gown. My involvements "in town" are illustrated by one of many anecdotes, this one, probably in summer 1974, about a Serkin concert among the famous "Pittsburgh Jewish Y concerts." In the audience were Dr. Caroline, Eva, and I. They remember that I jumped over rows of people (silently) to resuscitate (successfully) a suddenly comatose listener, who had collapsed. Serkin did not interrupt the Diabelli variations he played. The patient, president of a corporation, I saw 25 years later in good health.

Nancy Caroline, educated at Harvard, had completed an internal medicine residency at Case Western Reserve University Medical School in Cleveland. That included some anesthesiology experience under Gravenstein. She had been active with the civil rights movement. In 1973 she began a CCM fellowship with us. Her takeover from Benson of the medical leadership of our Freedom House Enterprise (FHE) ambulance program[126,127] upon my (she says irresistible) offer, led her into a new career pattern and helped establish modern ALS ambulance services first at Pitt[158-160] and then in Israel. One reason for her great impact was the fact that she is a caring, dynamic, compassionate "super doctor," a Renaissance woman, and an eloquent writer. The FHE program gave Caroline the opportunity to demonstrate her exceptional skills in laying hands on victims in emergencies outside the hospital.[160] Her writing style, which gained international fame, resembles that of *New England Journal of Medicine* essayist Lewis Thomas. I asked Caroline to write the national curriculum for training ambulance attendants, which the Department of Transportation had asked me to prepare. Little Brown published a slightly modified form of her document as a series of books that became the most widely read EMS and paramedics' texts in the world during the first decades of the EMS movement.[159] The royalties from her writings helped Caroline support her later missions.

In 1976, Caroline decided to leave Pitt and become an Israeli citizen. For five years, she was the medical director of Magen David Adom, the organization responsible for ambulance services throughout Israel. She advanced that service from BLS to ALS capability. Nagel and I visited her programs in 1978. She has saved lives in terrorist events. She spent the next five years in East Africa, where she applied EMS principles to overall health care, i.e., extending the physician's impact through the hands of nonphysicians. She initiated medical and agricultural programs. Recently she returned to Israel, again changing her career; she became an innovator in palliative hospice care of terminally ill cancer patients and founded a hospice program in Galilee. It all ties together, because principles learned in CCM are again applied, in this case to titrated terminal care.

Our FHE ambulance program had suffered from lack of finances throughout its existence, i.e., from 1967 to 1975. We had to meet the dovetailing needs of providing training and employment for the black community and providing a testing ground for our national standards of prehospital EMS. Before I got into the act, the primary goal of a "Freedom House Enterprise" was to develop business ventures in the ghetto areas of Pittsburgh near PUH. The Falk Foundation (under the leadership of Philip Hallen), the Ford Foundation, and other local philanthropic groups, provided some financial support. My department provided EMT training and medical leadership, gratis. Initially, I personally taught some of the student EMTs in ORs and ICUs.

Besides uncertainties over funding, the FHE project was beset with other problems during its eight years of existence: 1. Reluctance of the city's mayor to let FHE provide city-wide services; we were limited to the university district and other eastern sections of the city. 2. Suspected racial prejudices with white police officers eager to maintain control of ambulances city-wide. (Our FHE ambulance program began in 1968 for African-American trainees only, but integrated in reverse in the 1970s.) 3. The previously noted variable, intermittent medical direction during 1969-1973 — in part my fault[4]. Differences between medical and FHE board priorities.

Nonetheless, the service continued to render care in the eastern half of Pittsburgh and demonstrated the validity of the national guidelines for ambulance design and equipment and EMT training.[122-127] By 1974-75, under Caroline, FHE reached my original goal for the program, namely ALS capability for paramedics (EMT II). In April 1975, the city elected to implement its own mobile ICU services. The FHE service ceased to exist in October 1975, and most FHE employees were transferred to the city service. That was directed by Glenn Cannon, a FHE trainee, in the early 1970s. Cannon rose to safety director of Pittsburgh. Other FHE trainees also made unexpected careers. For example, Mitchell Brown, FHE paramedics' instructor, rose to safety director of Cleveland and Ohio. The majority of the more than 40 trainees initially declared "unemployable," rose in educational credentials, documented the national mobile ICU guidelines as practical, and found worthwhile jobs. "The birth, crucifixion, death, and resurrection of FHE" is a complex story.[158] The nationwide impact of the FHE program was commemorated in Pittsburgh's History Museum in November 1997.

My attempts to establish university leadership of regional EMS were frustrated not only by the controversy between the mayor and the FHE ambulance service, but also by the lack of a UHCP director of EDs; disinterest in the prehospital arena among most UHCP anesthesiologists, surgeons, and internists; and the need for a medicopolitically skillful person who could dedicate full time to EMS negotiations. Sol Edelstein made a partially successful attempt. After his anesthesiology residency with us, he joined our staff in 1975 to devote himself to EMS negotiations. He formed the Emergency Medicine Operations Center (EMOC), based at PUH. The EMOC provided full-time medical radio command of city ambulances via radio, optional radio guidance of other ambulances in Western Pennsylvania, consultation from CCM and other specialties by radio and telephone (to influence regionalized centralization of special care), and help in EMS evaluations. This was to be done as an arm of the Western Pennsylvania EMS Community Council. This dream, based on our plans of 1964, was almost accomplished by Edelstein in the early 1970s. He attracted some national EMS grants to our community.

My initial goal, still elusive in the U.S. but successful abroad, has been to have physicians of multiple specialty backgrounds, with special interest and skills in life support, jointly lead and cover the emergency and critical care medicine (ECCM) continuum.[15] That would include critical cases out-of-hospital, in the ED, and in the ICU. Multidisciplinary ECCM was defeated when the traditional base disciplines failed to cover community hospitals' emergency departments and the new base specialty emergency medicine was initiated by some general practitioners to fill the gap. Academicians in emergency medicine have taken on EMS leadership. Current politics preventing emergency medicine from giving additional CCM subspecialty certification is wrong.

In 1979, the EMOC became the Center for Emergency Medicine (CEM) of Western Pennsylvania, led by emergency physicians Ronald Stewart and Paul Paris. Originally from Canada, Stewart was an emergency physician leader in Los Angeles in the late 1970s. Our then-new dean, Donald Leon (cardiologist) and I recruited Stewart as Caroline's successor. Stewart recruited Paris. They developed the pre-eminent emergency medicine residency programs in the U.S. It became so attractive in part because Pittsburgh was the only American city where physicians go into the field together with paramedics — a tradition established in the 1960s and 1970s by Benson and Caroline of our department. Stewart and Paris did it through initiation of an academic emergency medicine residency, which made resuscitating on the streets appealing. It attracts applicants for emergency medicine residencies. In contrast, most of our anesthesiologists and intensivists have since the 1970s refused extra-hospital work. Other incentives for the emergency medicine specialty included high salaries, shift work (comfortable lifestyle), and "romantic" episodic work, avoiding the long-term care of critical cases. Although physician staffing of ALS ambulances (mobile ICUs) still has not been proven to add significantly to lifesaving, it proved important for prehospital research and for creating EMS-oriented emergency physician leaders, who must have hands-on experience to guide paramedics appropriately and to understand the prehospital arena.

We finally made peace with City Hall when the totally cooperative Mayor Richard Caliguiri was elected. Cannon's reluctance to let physicians control paramedics eventually abated when, in 1975-76, a regionalized EMS system was put into place, at least as far as it concerned ambulances.

When I decided in 1978 to switch full-time into basic research and turn the medico-political issues over to others, I made sure that the continued leadership of intrahospital ICU programs by Grenvik and of EMS programs by Stewart were secured.

All of these EMS developments locally were catalyzed also by efforts at the national and international levels. In the U.S., I was joined only by Eugene Nagel. Around 1970, he was an anesthesiologist in Miami, where he initiated the first medical control of fire department ambulance attendants in giving ALS by standing orders and radio guidance from himself. He and I also joined forces through the Emergency and Disaster Medicine Congresses with Rudolph Frey of Mainz, Germany and the second World Congress of Emergency and Disaster Medicine in Pittsburgh. Nagel later became chairman of Anesthesiology at Johns Hopkins. The only other American anesthesiologist who has remained active in EMS until now is Roger White of the Mayo Clinic.

Much credit for cardiac EMS should go to the cardiologists of Seattle, starting with Leonard Cobb, whom I first met in 1969-70. Based at Seattle's King's County Hospital, he not only initiated paramedic training in the 1970s, but also introduced CPR courses given by the ambulance attendants, for as many laypersons as possible in Seattle. This made Seattle the first community with a relatively high proportion of CPR performed by bystanders in cases of out-of-hospital sudden cardiac death. A decade later, Mickey Eisenberg and his associate Richard Cummins expanded Cobb's work, by what was probably the first epidemiologic community-wide study approach to emergency cardiac care.

My concern for the potentially weakest link in the life-support chain, namely the bystander, first arose in Baltimore. Although we had documented the ability of untrained laypersons, from Boy Scout age up, to perform effective steps A-B on curarized human volunteers,[59] we did not test the retention of their skills. In 1964, after my friend Lind in Norway had shown that steps A-B can be taught to laypersons, using Laerdal manikins, we conducted the first research of external CPR-ABC, i.e., the ability of laypersons to acquire these skills;[109] we found self-training more effective than courses.[112-116] We inspired similar studies in developing countries by my friend John Lane of Brazil[161,162] and others. They again confirmed the desirability of self-training by mass media.[116] Recently I have been re-engaged in ending this frustrating delay in globally implementing the first step of the life-support chain.[116] I believe that every fit human being above a certain age (e.g., 10-12 years) should learn in school, re-enforced by the media thereafter, the simple few steps of LSFA, including CPR steps A-B-C. Bandaging and splinting are unimportant. The LSFA concept was initiated in the 1960s jointly by Safar, Asmund Laerdal, and the Laerdal Company's Dahll, Eikeland, and Egeland.

Research

In the 1970s I succeeded in getting funded two NIH research grants — one for laboratory studies of cerebral resuscitation, and the other for education research on acute respiratory insufficiency, actually CPR-BLS-ALS-PLS training. During my 18 years as department chairman, we attracted grants of about $4 million. That would now have a purchasing power of about $8 million. Moreover, in those days full-time faculty's research time did not have to be paid out of research grants. Researchers' salaries in general were covered by the institution.

The value of research efforts should be measured by the importance of results. My associates, who in the 1970s produced important results outside resuscitation research, included Albin, Klain, Bleyaert, Cook, McKenzie, Abouleish, and Major Cohn. Cohn had been recruited by Hingson as a Ph.D. researcher at the MWH.

For resuscitation research, my focus on "brains too good to die" after cardiac arrest began with Bjorn Lind, anesthesiologist of Stavanger, Norway, who joined us for his sabbatical year in 1970-71. Lind and I had been friends since 1958 when he brought Laerdal and me together. I was ready to initiate studies into the mechanisms and mitigation of the post-cardiac arrest encephalopathy, encouraged by EEG recovery experiments of Kampschulte on dogs in 1969. I hypothesized that after prolonged cardiac arrest, even with normotension, there is cerebral hypoperfusion. In the summer of 1970, I asked Lind,[163] and first-year resident James Snyder[163,164] to conduct what seemed to have been the first measurements of CBF and $CMRO_2$ after prolonged cardiac arrest. Arrest caused intracellular edema, monitored with an improvised brain-impedance method. These dog studies documented, after prolonged cardiac arrest (no-flow), transient cerebral hyperemia followed by protracted cerebral hypoperfusion. This was accompanied by suggestions of inadequate O_2 delivery. Strangely, colleagues do not refer to this paper in the then-not-widely-read journal *Resuscitation*,[163] but instead cite our second study, which confirmed our results of the previous year; that second study was with Nemoto as team leader.[164] In the 1980s, at the IRRC, I resumed such studies of postarrest CBF and $CMRO_2$. These later studies documented hypertensive reperfusion to prevent no-reflow and improve cerebral outcome; and CBF heterogeneity and inadequate cerebral O_2 delivery were also demonstrated. We could improve cerebral outcome using protracted hypertensive hemodilution.

Edwin Nemoto, Ph.D., continued with Severinghaus for a year after I left San Francisco in the summer of 1970, and then inquired about working with us at Pitt. In San Francisco, Nemoto had worked on hyperthermia of the brain. He joined our group at Pitt in 1972 as a laboratory physiologist. I encouraged him to work on temporary complete global brain ischemia, as it occurs in cardiac arrest. After Galla's death, I appointed Nemoto to be his successor as coordinator of our research lab. I do not remember anymore how I managed to finance all these academically active people.

Based on our CBF results of 1971, with the help of Nemoto and Stezoski, we could, in 1974, demonstrate apparently for the first time, the ability to improve outcome after cardiac arrest with a therapy employed after reperfusion.[165] This therapy attempted to promote CBF with a combination of hypertension, hemodilution, and heparinization. I recommended this treatment for clinical trials, but let myself become distracted by barbiturate loading, and returned to CBF promotion later. In a letter to me regarding our 1976 paper on this work,[165] Fred Plum (the leading neurology scientist on coma at that time), called it a very important work and encouraged my commitment to cerebral outcome studies. These results led to my first NIH grant for laboratory research on cerebral resuscitation. Plum's 1971 conference on global brain ischemia in New York gave me a chance to meet Alexander (Pete) Hossmann of Cologne, Germany, who had shown in cats that the majority of cerebral neurons (but not all) can achieve electric activity and protein synthesis after one hour of global brain arterial occlusion. His reports provided optimism for our cerebral resuscitation research.

Next I followed my instinct, in agreement with our renowned neuropathologist John Moossy, to avoid what he called "the laborious climb up the phylogenetic ladder" (i.e., from rodents to humans). I therefore asked Nemoto to develop a model of global brain ischemia in monkeys, using neck-tourniquet occlusion.[166] This taught us the need for postinsult intensive care life support for the control of extracerebral variables that might influence cerebral outcome. Thus, in the 1970s, in our Scaife Hall labs, we created an animal research ICU and launched the first systematic research into cerebral resuscitation from cardiac arrest.

On July 7, 1973, at Galla's suggestion, we held a departmental research planning session on Cheat Lake, near Pittsburgh. We agreed that the time had come to boost research by our groups. In the early 1970s, leaving promotion of anesthesia-related research to Galla, I repeatedly called together about 20 colleagues of various disciplines to explore their interests in a resuscitation research center. I drafted plans for research interactions among laboratory, patients, and community. I identified broad areas in need of investigation in my first "doctor honoris causa, Gutenberg University of Mainz" presentation in 1973;[144] before the Senate Health Subcommittee in January 1976; and during the Baltimore EMS conference in May 1976. I proposed about 10 globally-oriented resuscitation research centers in the U.S. For Pitt, I prepared a plan to develop and raise funds for a major multidisciplinary international resuscitation research center (IRRC). These plans had input from Asmund Laerdal. The IRRC became a reality in 1979.

In 1974, my former fellow and then associate Achiel Bleyaert (originally from Belgium) raised the idea of trying barbiturate loading after cardiac arrest. Nemoto and I agreed with his initiative because anesthesiologists Al Smith and Phil Larsen of San Francisco and John Michenfelder of the Mayo Clinic had shown in dogs and monkeys that postinsult barbiturate loading diminishes the size of experimental focal ischemic infarcts. Our histologic findings in monkey brains suggested that prolonged cardiac arrest can be followed not only by the death of selectively vulnerable scattered neurons, but also by some scattered microinfarcts. I figured that scattered low-flow areas after cardiac arrest should benefit from barbiturates, which in large doses silence the EEG. Using our monkey model of global brain ischemia by neck tourniquet and long-term life support,[166] Bleyaert, et al,[167] found a significant reduction in neurologic deficit and histologic damage by post-arrest thiopental loading. These findings stirred much debate at the national level, starting with a group session at the ASA meeting in New Orleans in 1978, where we agreed on pushing controlled patient studies while pursuing the topic further in the lab. Our neuropathologist Moossy participated. We saw good physiologic reasons why barbiturates might help resuscitate the brain.[168] Some colleagues, however, did not appreciate the differences between protection (treatment induced before the insult), preservation (treatment during the insult), and resuscitation (treatment to reverse the insult and support recovery); nor did they appreciate the differences between the focal (incomplete) brain ischemia of stroke, the global (complete) brain ischemia of cardiac arrest, and brain trauma. The possible benefit of barbiturates reducing $CMRO_2$ during post-arrest incomplete ischemia seemed rational.

Miroslav Klain and I first met in 1963 in Prague, where he was a research surgeon. He then worked with Kolff on artificial organs in Cleveland, returned to Prague, and was among several Czech colleagues who left that city when the Soviet Army took over their country in August 1968. He first returned to Cleveland but joined our anesthesiology residency program in the early 1970s. He was bioengineering-oriented and committed to resuscitation research, which he felt is best pursued via the specialty of anesthesiology. He became our most famous anesthesia resident. During his residency he was invited as a visiting professor to various places around the world. During and after his rapid rise to tenured professor with us, he made several contributions. The most important was his initiative to develop high-frequency jet ventilation. He credits me for having triggered this interest. I had merely invited Jacobs, a young surgeon then in Washington, D.C. for a visit, because he had revived the idea of transtracheal jet ventilation (by normal frequency) previously reported by anesthesiologists Jacoby and Singh. Klain combined high frequency and jet. He did this in his lab at MOH, which had a dog lab separate from the one run by Galla and Nemoto. In the 1980s, Klain became a co-investigator with us on bioengineering-oriented projects in the IRRC, including portable emergency cardiopulmonary bypass. He also served as interpreter for our work with the Soviets, because he is fluent in the Russian language.

My research in the mid-1970s included a large three-year NIH-supported community education research project to study the teaching and delivery of resuscitation (the "Acute Respiratory Insufficiency" [ARI] project). I gathered a team to include anesthesiologists Berkebile, Ersoz, and Sladen; public health evaluation specialist Edmund Ricci; and a majority of hospitals in greater Pittsburgh (Allegheny County). The results included systematic improvement of our CPR self-training systems,[112-114,116] teaching texts of CPCR,[5] and several key chapters of the first textbook on emergency medicine,[169] based on incremental improvements of measured acquisition of knowledge. This program also improved the measured quality of resuscitative care delivered in the community.[170]

Conclusions

The highlights of the 1970s[98] included the marriage of MOH anesthesiology with our department; improved departmental finances through the creation of a private-practice billing system allowing us to stabilize staffing and academic programs; a new leading subspecialty program in neuroanesthesia and research by Albin; the CCM program's multidisciplinary expansion; the biomedical ethics programs; the initiation of cerebral resuscitation research; the Emergency Medical Operations Center (EMOC) of Western Pennsylvania by Edelstein; the Club of Mainz for Emergency and Disaster Medicine (by Frey); and "peace medicine" through the continued Pittsburgh-Moscow connection in reanimatology and my activism with the World Federalist Association.

Our large department would not have been created from scratch in the 1960s and 1970s without many fine, dedicated, and loyal associates and trainees. Some rank-and-file leaders of our specialty apparently did not like my push into new extra-OR fronts. We felt that we received little appreciation for our most important mission: safe physician-conducted or physician-directed OR anesthesia in a previously nonphysician environment of six hospitals. Between 1961 and 1978 we grew from three to about 60 full-time faculty-staff and from zero to about 40 physician trainees (20 primarily for anesthesia and 20 for CCM). I am deeply grateful to the more than 200 physicians and nonphysicians of our programs who provided increasingly safe anesthesia services to over 1 million patients, and who in more than 10 education and research programs helped to develop service and leadership careers worldwide.

Transition

In 1978, it was time for me to return from politics to science. I went on a second sabbatical leave (six months of 1978), this time in Pittsburgh, devoted to writing and tying up loose ends of resuscitation research. I had decided to quit the department chairmanship in order to dedicate myself fully to resuscitation research. Its importance had been steadily amplified for me personally by Elizabeth's death in 1966 and my sister Hanni's sudden cardiac death (in Vienna) in 1978. Our department and its CCM program were strong, and I believed that almost two decades was enough, even for a department-initiating chairman. Moreover, I am a better initiator than maintainer. I am also a poor psychologist; counseling and mediating as a psychologist (sometimes even as a psychiatrist) are roles a department chairman must play, increasingly so when responsible for over 200 team members. This role I could not and did not want to play anymore. Finally, my principal scientific interest, resuscitation-related research on a broad scale, I felt, deserved most of my attention. That led to the IRRC.

During my chairmanship of the Department of Anesthesiology and CCM at Pitt (1961-78), we attracted about $4 million in research grants and published more than 1,000 titles, over 200 of which were peer-reviewed papers. The majority of our research publications were not on anesthesia per se. By the time I terminated the chairmanship in the summer of 1978, our department was strong not only in resident recruitment and faculty staffing, but also in research productivity. We had laboratories at PUH/Scaife Hall, MOH, MWH, and the VAH. Patient research of one kind or another was conducted in all six hospitals. The CCM program was under Ake Grenvik's able leadership. Anesthesia services were funded by our group practice plan. That was still small in income, not well run, and too much controlled by administrators. The residents I appointed for 1978-79 were mostly graduates from medical schools in the U.S.

In 1978-79, I negotiated my resignation as chairman. That was complicated by a change of deans (from Werner to Leon), and budget problems in the medical school. Ryan Cook became interim chairman of the department. He was associate professor of pediatric anesthesiology and Marcy's successor as chief anesthesiologist at CHP. When Cook was a Pitt medical student in the 1960s, I had recruited him into anesthesiology.

The year 1978 was personally traumatic, with deaths and illness in my family. My beloved sister, a physician in Vienna, suffered sudden cardiac death at age 50. Her 87-year-old mother and teenage son tried to resuscitate her. My brother-in-law remained permanently physically handicapped from Guilland-Barre Syndrome. My professional activism in the U.S. and abroad faced challenges and problems. My change from the department chairmanship to initiation of the IRRC was difficult. I learned too late that a chairperson should remain in charge until the successor takes over, avoiding acting interim leadership.

During and soon after the transition year 1978-79, into the start of Peter Winter's chairmanship in July 1979, staff anesthesiologists at the specialty hospitals remained stable under the leadership of Cook (CHP), Finestone (MOH), Kirimli (VAH), Bleyaert (EEH) and McKenzie-Abouleish (MWH). They all continued under Winter. PUH anesthesia coverage, however, was threatened. After I declared my resignation in spring 1978, PUH lost 3-4 faculty members each on three occasions, during and after the transitional academic year 1978-79, a total of about 12 from among 60 at UHCP. It did not hurt clinical coverage significantly, because graduating residents were appointed as junior faculty members and stepped effectively into the vacuum. The CCM program remained stable and fully staffed under Grenvik's leadership. Departures from PUH started in summer 1978 with Brian Smith (PUH chief), Maurice Albin, Malcolm Orr, and Maciej Babinski. They left us to "resuscitate" the anesthesiology department in San Antonio, Texas. They later remembered the 1970s in Pittsburgh as highlights in their professional lives. Pautler, other remaining staff, and graduating residents, however, continued safe coverage in this difficult hospital environment of PUH with demanding surgeons and many PS 3-5 patients. Pautler was in charge of day-by-day OR operations at PUH. After Winter's start in summer 1979, a second group under Pautler left for St. Francis Hospital. The third departing group, Alfred Tung, Leonard Rosenfeld, and others left for a community hospital in East Pittsburgh. In the summer of 1979, Peter Winter took over the department and I started the IRRC.

The search committee for my successor, chaired by Ferguson (chairman of Orthopedics), recommended that Peter Winter be my successor. Winter was then on the senior faculty in Bonica's department in Seattle, after anesthesiology training and research at Harvard, and imaginative hyperbaric oxygen research in Buffalo. He and I had known each other since my attempt to recruit him to Pittsburgh in the late 1960s. Our paths crossed again in the early 1970s, when I recommended him as one of the founding members of the SCCM. Moreover, Winter's parents and mine had similar roots in Vienna. Ferguson consulted me; I did not select my successor, but Ferguson thought it to be beneficial that the successor have the respect and support of his predecessor. Of course, I strongly favored Winter. He accepted and started in Pittsburgh in July 1979. Winter inherited a department which was academically weakened in anesthesiology at PUH, underfunded in spite of its practice plan (compared with some other academic departments in the U.S.), but still strong in CCM and clinical anesthesia services in all the hospitals of the UHCP.

International Resuscitation Research Center (IRRC), 1979-94

In these 15 years, I gave priority considerations to efforts toward more effective resuscitation from prolonged cardiac arrest with focus on cerebral resuscitation.

Since the 1950s the goal of resuscitation-related research, by me and my associates, has been quite broad — to help maximize the reversibility of acute terminal states (e.g., asphyxiation, shock) and clinical death (cardiac arrest) by the study of the pathophysiology of acute dying processes and their reversibility, including what Negovsky calls the postresuscitation disease. I felt obligated to pursue resuscitation research to maximal potentials of lifesaving in animal models and patients. This, I felt, required a long-term, most-time, goal-oriented, multidisciplinary commitment. The research projects under my guidance when I gave up the chairmanship of the department of Anesthesiology and CCM, looked promising for cerebral resuscitation from cardiac arrest (temporary complete global brain ischemia) longer than 5 minutes, because of promising outcome data from our CBF promotion study in dogs[165] and barbiturate study in monkeys.[167]

I also believed that resuscitation medicine in general (beyond CPCR) requires a few centers in the world where thinking, debating, and exploring the pathophysiology and reversibility of terminal states and clinical death can be done with a global approach. Others who have joined the ranks of resuscitation researchers since 1960 have made laudable contributions, but from the basis of one specialty, one organ system, or one model. I was already engaged in multiple levels "from cell to community" (from C to C). Before the end of this millennium our Center contributed to topics from DNA to disasters (from D to D), with the molecular level added by Pat Kochanek, my successor as director of the IRRC. The only other resuscitation research center that historically had a global approach was that of Negovsky in Moscow (fig. 13). Our group has been, apparently so far, the only one in the Western World that was trying to tie it all together.

In 1978-79, I initiated the IRRC via negotiations with the dean of the medical school, Gerhard Werner. I am grateful to him for having understood my intentions. He surprised me by recommending to the powers my promotion to "distinguished professor of resuscitation medicine," saying this was to acknowledge my labors as department chairman and to give me full freedom for research. During 1978-79, when I took a half-year in-house sabbatical, pharmacologist Werner turned the deanship over to cardiologist Donald Leon. It is Leon to whom we are grateful for having discovered and assigned to us the generous laboratory and office space for the headquarters of the IRRC, which we still occupy. This space is over 12,000 square feet of a small brick building at 3434 Fifth Avenue, on the UPMC campus. In the distant past, the building housed a casket factory; we joked that it went from resurrection to resuscitation. I directed the IRRC from July 1979 through June 1994. Then, at age 70, I remained one of its full-time principal investigators, but turned the center's directorship over to the young generation — to Kochanek.

Peter Winter took over a Department of Anesthesiology that still had strong physician leadership of clinical services, although it was academically weakened by the loss of several professors. The CCM program remained continuously strong. Winter wisely held on to the hospital anesthesia chiefs from my time, and built on our foundations. The department was a leader in CCM, and under Winter also became a leader in anesthesiology research. Under his reign, between July 1979 and June 1996 (when he resigned into emeritus status), he and his associates doubled the number of faculty and house staff (from about 100 to 200, combined), increased the practice plan income fivefold, successfully promoted anesthesia research, and created previously nonexisting infrastructures for administration, budgeting, computerism, media production, and others. He brought the department's administration into the twenty-first century. He was a compassionate listener and advisor for many faculty members and students. He gained control over practice plan income, which I did not have. He continued supporting the growth of the multidisciplinary CCM program under Grenvik, Powner, and Snyder. Winter's talents, kindness, and style, and the size and complexity of our department, I believe made him become during the 17 years of his chairmanship, the CEO of academic anesthesiology. I am grateful to him and our faculty not only for having brought our department to new heights, but also for continued financial and spiritual support of resuscitation research, my full-time or most-time baby since 1979.

Eva and I are grateful to Winter for his continued loyalty toward his predecessor, the Peter and Eva Safar endowed chair (created mostly from practice-plan money under Winter's initiative); for my sixtieth and seventieth birthday celebrations and conferences (table 2; fig. 14); for his interviewing me for the ASA living history videotape;[21] and for the Peter and Eva Safar annual University and community-wide lectureship in "Medical Sciences and Humanities," sponsored by the Department of Anesthesiology and CCM. Through Winter's initiatives, the lecturers, since 1980, included anesthesiology leaders, medical scientists, physicists and Nobel laureates. In 1966, Winter's successor, Leonard Firestone, turned the leadership of these special lectures over to Pat Kochanek.

My founding the IRRC was influenced by my wish, since I was in Baltimore, to create such a center; on my watching Comroe in San Francisco manage his CVRI; on a previous abortive attempt to create an IRRC in Pittsburgh under the Themis Program of the U.S. government; and by suggestions on organization, management, and funding by my good friend Asmund Laerdal — two years before he died. When I prepared the founding documents for the IRRC for Dean Werner, some members of the executive committee of the medical school objected to the term "Institute." They feared it would undermine departmental power. Such fear of losing control was reminiscent of why Salk left Pittsburgh in 1961. I did not care about the name. We have had full freedom under the name "Center." The IRRC concept, as a combination of resuscitation research programs, began in Baltimore in the 1950s, and the Pitt Department of Anesthesiology in the 1960s and 1970s.

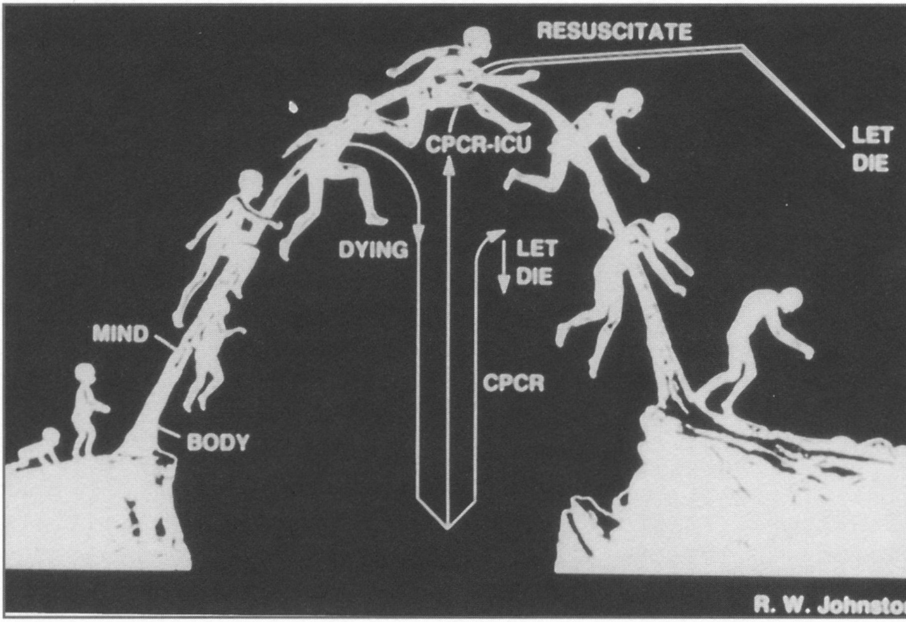


Figure 14. Sculpture "Nine Ages of Man," by Canadian artist R.W. Johnston, a gift from the Department of Anesthesiology (Peter Winter) to the School of Medicine, University of Pittsburgh, on Peter Safar's 60th birthday. Modified by P. Safar to describe the philosophy of resuscitation medicine. It focuses on the individual, whose arc of life is acutely interrupted — by a natural or man-made insult — in the absence of a lethal incurable disease, and before severe senile dementia. In old physical age, the mind can continue to advance. When an individual's life is cut short unexpectedly (downward moving arrow), before his/her mind has declined (right upper corner of picture), all-out emergency resuscitation should be attempted. When, during subsequent prolonged life support, efforts clearly will not achieve a chance for a good cerebral outcome, death should be permitted. When the mind has a chance to recover, long-term expensive intensive care is justified. When an old person has decided that the time has come, or is severely demented or unresponsive, and develops a sudden terminal state or cardiac arrest, resuscitative efforts are not justified. CPCR, cardiopulmonary cerebral resuscitation; ICU, intensive care unit.

During 1978-79, the transition year before we moved into the IRRC building, Albert Ferguson, then chairman of Orthopedics, and clinical pharmacologist Robert McDonald of the Department of Medicine, generously allowed me to use their laboratories. I am grateful for their collegiality. In 1978-1980, three fellows joined me for research experiences: second-year Pitt medical student Nicholas Bircher, who pursued several attempts in dogs to improve blood flow produced by external CPR;[171-181] graduating anesthesiology resident Joseph Sassano from Pittsburgh; and pediatric intensive care fellow Gideon Eshel from Israel.[182-185]

Bircher's studies suggested minimal blood flow benefit from pneumatic modifications of external CPR. We conveyed this later to Johns Hopkins cardiologist Weisfeldt, who in the 1980s spent a decade exploring this subject with mechanistic studies. Our prediction proved true, namely that something more potent than pneumatic modification is needed to optimize reperfusion pressure and flow. This we later found to be open-chest CPR[173-181] and emergency cardiopulmonary bypass (CPB).[186-189] Bircher became very knowledgeable about the resuscitation literature. After anesthesia residency under Winter and payback years in the U.S. Navy, he was CCM fellow and IRRC research fellow with us. His scholarship made him a good candidate for rapid promotion to associate professor. He could increasingly take my place on national and international CPR guidelines committees. In the early 1990s, he became the outcome-oriented CPR laboratory researcher. He showed that treating acidemia during and after cardiac arrest is good for heart and brain. Bircher has been one of my professional sons since his medical school days. I am proud that he surpassed me in CPCR-related literature knowledge, teaching, computerism, statistics, and activism. He became one of few clinician scholar-investigators among anesthesiologists-intensivists. He also helped me with some review papers. In the late 1990s, he became a valiant defender of academic freedom and shared governance, vis-à-vis hospital administrators, who threatened these values in the managed-care era.

Sassano and Eshel, under my initiation and guidance in 1978-79, conducted novel studies of hyperthermia:[182-185] having learned about dying from ventricular fibrillation, asphyxia, and hypothermia, I was curious as to how the organism dies during hyperthermia. We produced hyperthermia in monkeys by hot-water immersion. These experiments on monkeys and dogs revealed 42°C to be the critical temperature for survival. In hyperthermia-induced cardiac arrest, immediate resuscitation was possible with standard external CPR (provided the hypoglycemia and fluid loss caused by heat were corrected), but delayed irreversible shock with hemorrhagic diathesis followed. The latter also occurred in a delayed fashion, after transient recovery from initially nonlethal hyperthermia.[184] Petechial hemorrhages occurred everywhere except in the brain. Sidetracked by new ideas, Eshel's data have been published late and we are still preparing some now for publication.

Our chief laboratory technicians, William Stezoski and Henry Alexander, came with me to the IRRC. They and Bircher, who continued with me part-time throughout medical school and residency in anesthesiology, provided continuity in the laboratory.

I transferred to the new IRRC facility the 10 ongoing resuscitation-related projects and consolidated them into four programs.[190] My co-leaders in the 1980s were Bircher for program I on cardiac arrest laboratory studies; Abramson for program II on cardiac arrest patient studies; Tisherman for program III on trauma; and Pretto for program IV on disaster reanimatology. Each program had multidisciplinary, high-powered co-investigators. Program II on clinical CPCR research, the so-called Brain Resuscitation Clinical Trial (BRCT) involved 20 groups in seven countries, a steering committee, and the UPMC data-management center under Katherine Detre. Program IV on disaster reanimatology also involved investigators outside of medicine.

Additionally, I continued miscellaneous other activities, including writing of books and chapters, and contributing thoughts and dialogues to the bioethical dilemmas of CCM.

During the first 15 years of the IRRC, the leaders of our four programs were most-time researchers; there were about 10 part-time faculty investigators from various departments, about 30 part-time co-investigators or consultants, 15 full-time laboratory technicians, and 8 full-time office staff. We trained 60 full-time physician research fellows (26 of the 60 were anesthesiologists) and 20 medical student research fellows. The majority of the fellows continued later as senior faculty members in acute medicine-related academic work. Physician specialists obtaining a Ph.D. was too time consuming in the U.S. where Ph.D. programs are based in basic science departments, which require course attendance, research accomplishments. Ten M.D.s from abroad achieved a Ph.D. all or in part through their IRRC research (Bar-Joseph, Capone, Cerchiari, Edgren, Gisvold, Hendrickx, Kuboyama, Oku, Sterz, Vaagenes).

How did we fund the IRRC? We started with small but crucial priming grants from the Pennsylvania Department of Health, then under secretary Arnold Muller, M.D., emergency medicine leader of Hershey. The Rippel Foundation helped with an equipment grant. The total average IRRC budget, needed and obtained for 1979-1994, was about $1.0 million per year (about equally divided between laboratory work and clinical cardiac arrest program II) from mainly three donors: the Department of Anesthesiology and CCM, the NIH, and the Laerdal Foundation. When Asmund Laerdal died in 1981, he left a large special grant for my research, which partially supported the IRRC between 1982 and 1994. Faculty investigators' salaries came from their respective departments. That was the time when clinical practice-plan incomes supported up to 50% of faculty time for research. For anesthesiologists and intensivists, Peter Winter strongly supported and defended these academic expenditures of clinical incomes. I was 100% university-salaried. Rent of our facility came from "indirect costs" of grants. My clinical work (one day per week in the OR) was voluntary. In addition to the $0.5 million per year BRCT clinical research grant from the NIH (which received the best score and percentile), our NIH applications succeeded in attracting two laboratory research grants. Several applications to the NIH for animal outcome studies received good but not quite fundable scores — at a time when the NIH review committees began to be dominated by molecular, predominantly gene focused, biologists with no reanimatology peers among them. NIH review committees are still dominated by specialists in epilepsy or stroke who do not seem to appreciate the pathophysiology of the postcardiac arrest resuscitation disease of the whole organism. For studies with our new animal outcome models, which reanimatologists consider important and unique, over several years we drained about 20% of our groups' time, energy, and money into often futile reapplications to NIH.

In these 15 years, the IRRC facility, located across the street from the UPMC hospitals, had become the hub of our activities. IRRC investigators generated about 220 peer-reviewed publications, 450 abstracts, and 460 other publications including five books and three conference proceedings. A community advisory board for the IRRC was created, led by Mr. William Boyd. This fine group of leading citizens created a brochure and taught us how to communicate better.

We developed a research ICU program for large animals. Although more expensive and emotionally more difficult to manage than rat experiments, my focus on outcome with clinical relevance was shared by one of our most supportive and crucial co-investigators, John Moossy, internationally renowned professor of neuropathology. Long-term life support was essential if one was to go beyond emergency resuscitation studies and evaluate the maturing of secondary brain damage after a temporary insult. That required control of extracerebral variables, which, of course, influence the brain. This is why I had already decided in the 1970s to apply our human ICU life-support experiences to animals high on the phylogenetic scale — monkeys and dogs. These allowed for better insult control and life support than is possible in patients and small animals.

Weekly conferences and laboratory meetings catalyzed research priorities and cross-fertilization with other investigators in our university community and beyond. Our IRRC programs had to be oriented toward a common overall goal. I considered it my role to initiate, stimulate, and guide, and to be responsible for the whole show — creating an environment of productive on-going systematic research programs (and projects within each program), in which serendipitous discoveries and changes in direction are possible. From 1979 to 1994, we pursued laboratory and clinical studies on cardiac arrest, laboratory studies on shock and suspended animation, a brain trauma project on dogs, and extramural disaster, first aid, and related projects.

Program I, Cardiac Arrest Laboratory Research

The legendary Claude Beck, thoracic surgeon of Cleveland, who in the 1940s initiated clinical open-chest defibrillation, exchanged visits with me in the 1960s. Program I grew from my conviction that there are not only many "hearts too good to die", but also many "brains too good to die." In the 1980s, over half of our center's effort was devoted to searching for a breakthrough in cerebral resuscitation after prolonged cardiac arrest. We aimed to achieve normal brains after normothermic cardiac arrest of 10-20 minutes, so that advanced ambulance crews could arrive before the brain is doomed. The then-accepted limit of tolerance was still four minutes of normothermic no flow, the time it takes for normothermic brain to run out of glucose and ATP. We began with our outcome model of global brain ischemia in monkeys,[166,167] and continued with newly developed models of prolonged cardiac arrest in dogs.[191,192]

In addition, in the early 1980s under Hendrickx a Dutch research fellow we began to develop the first asphyxial cardiac arrest outcome model in rats.[193,194] Only in the 1990s, under fellow Laurence Katz, did we achieve reproducible three-day outcomes.[195-197] Katz had joined me as a premed student laboratory research fellow in the early 1980s. After training in internal medicine and emergency medicine, he demonstrated the importance of attention to minutia in modeling and of tenacity to reach an apparently evanescent goal in research. Katz left us for a residency in medicine in Boston, returned to Pitt for additional residency in emergency medicine, and continued part-time research with me. He is now a resuscitation researcher at Chapel Hill.

Using our previously established monkey model of global brain ischemia by neck tourniquet,[166] fellow Sven-Erik Gisvold from Norway partially supported by his government, spearheaded something unusual. I decided (and asked him) to re-evaluate the Bleyaert data in monkeys of thiopental effects on brain recovery after prolonged temporary global brain ischemia.[167] I suspected that details in modeling and life support can influence outcome. We decided to confirm the Bleyaert data in our new laboratory at the IRRC, since no one else had reported on an intensive care life-support model of such severe insults in large animals. The Gisvold study, with more accurately titrated blood pressure and temperature controls, ended with no statistically significant difference in functional and morphologic outcomes of the brains after thiopental loading following reperfusion from prolonged global brain ischemia.[198] Simultaneously and later, others pursued the thiopental brain resuscitation potentials, also with mixed results. I still believe that barbiturates have something positive to offer, not a breakthrough effect, provided their hypotensive effect is avoided. Gisvold found in monkeys improved cerebral outcome with a multifaceted protocol including the first re-exploration of moderate hypothermia after cardiac arrest since the 1950s; this study showed moderate benefit.[199]

In the 1980s, Michenfelder of the Mayo Clinic said that barbiturates can be helpful only in incomplete, and not complete, ischemia. I countered that we were not treating in but after cardiac arrest, and that we had evidence that after cardiac arrest, there are foci of incomplete ischemia. The barbiturate controversy was followed by a calcium-entry blocker controversy. The first positive calcium-entry blocker outcome study on cardiac arrest was our study in dogs by Vaagenes.[200]Michenfelder hired Gisvold to introduce and use our monkey model at the Mayo Clinic for a study of nimodipine.[201] It showed the same outcome benefit as did lidoflazine in our dog model.[200] Gisvold was one of our most productive fellows who also showed leadership talent. After his time with us at the IRRC, Gisvold became professor and chairman of Anesthesiology at the University of Trondheim and editor of the *Scandinavian Journal of Anesthesiology.*

After Gisvold I switched from monkeys to dogs, for scientific and emotional reasons. In our animal studies of clinical death and resuscitation we certainly tried everything possible to prevent not only pain but also fear and suffering. We used large rooms, not cages, for holding the animals. Shortening the lives of custom-bred dogs (of identical subspecies, age, sex, and weight) seemed easier to justify on the basis of the food chain, and the reasoning that without research, these dogs would not exist. In contrast, affordable monkeys were then caught in the wild and thus were not uniform. Also, in behavior studies we observed that they behaved (sexually) seemingly with human-like emotions. I therefore felt that even the food chain cannot justify shortening the lives of animals so close to us.

Vaagenes pioneered the idea that leakage of intracellular enzymes into the CSF on days[2-3] predicts permanent (irreversible) brain damage after cardiac arrest.[202] He and I also documented for the first time, in dogs, the greater brain damage after asphyxial cardiac arrest, as compared with ventricular fibrillation cardiac arrest of the same duration, and different response to drugs.[203,204]

We learned that the postischemic encephalopathy has a multifactorial pathogenesis that would be very difficult to untangle.[6,205,206] Several drug treatments in our large-animal outcome models failed to mitigate complex deleterious chemical cascades and improve outcome. Each dog or monkey outcome experiment of 3-4 days duration costs $3,000-5,000, because it requires technicians and CCM physician fellows in-house around the clock. Thus, one outcome answer costs 20 dog experiments, i.e., $100,000.

In the 1980s, we completed 10 large outcome studies in dogs that showed that portable emergency cardiopulmonary bypass (CPB) enhanced recovery of heart and brain better than with standard external CPR-ALS.[186-189] This opened the door for our design of devices — a portable CPB unit for closed-chest arteriovenous pumping via an oxygenator and heat exchanger, for emergency resuscitation in the prehospital arena and the ED. We found in a clinical CPB study for CPR-resistant cases in the ED that CPB is powerful but must be initiated earlier.[207] More-rapid vessel access is needed. I also suggested a double balloon aortic catheter for differential perfusion, cooling, and medication of the brain, heart, and rest of the body. Upon the University's request, we pursued and received two patents.[208,209] I still feel uncomfortable about physicians patenting treatments, because the related profit and delay in publication hamper application. On the other hand, the Cardeon company of California got these patents licensed from Pitt and will hopefully bring these devices to clinical use soon.

Building on our earlier studies of cerebral blood flow (CBF) after cardiac arrest,[163,164] in the 1980s we pursued a sequence of studies examining CBF and cerebral metabolism[211-217] using the new stable xenon computed tomography method of multifocal CBF measurements, pioneered by Pitt radiologists and surgeons. We found that between 2 and 12 hours after normothermic cardiac arrest of 10-12 minutes of no-flow in dogs, cerebral oxygen delivery was 50% normal when cerebral oxygen consumption had increased to or above baseline. We documented the ability to normalize oxygen delivery to the brain after cardiac arrest[213] and showed that such CBF enhancement improves cerebral outcome.[165,218] The team leader of these CBF-enhancement projects was Fritz Sterz from Austria, who later became professor of emergency medicine at the University of Vienna. We recently found support also in patients for a brain-resuscitative effect of a postarrest hypertensive bout. CBF studies were quality controlled by Walter Obrist, Ph.D.; this pioneer of CBF methods and TBI, also a historian, taught Eva and me about the Hapsburg empire of 600 years.

Protective-preservative moderate hypothermia at 30°C during circulatory arrest had been in clinical use since the 1950s. I then tried resuscitative hypothermia after normothermic cardiac arrest on a few patients,[50] but, because of management problems and side effects, that work lay dormant for 20 years. Rosomoff and I used prolonged moderate hypothermia in comatose craniotomy and brain-trauma patients during the early 1960s;[219] it lowered ICP, but the studies were uncontrolled. In the early 1980s I discussed with my research fellows Brader and Gisvold, and later also with Leonov, the disappointment about the failure of drugs for cerebral resuscitation from cardiac arrest, and the history of therapeutic hypothermia. We agreed in the mid-1980s to embark on a revival of research into resuscitative (post-arrest) moderate hypothermia. Brader developed an external head cooler.[220] Leonov's moderate resuscitative (post-arrest) hypothermia study,[221] reported at our 1987 researchers' conference, showed only a borderline benefit.

Resuscitative (postarrest) mild hypothermia was a discovery: Over three decades I invited to Pittsburgh leaders working on cerebral resuscitation — in 1976,[222] in 1987,[223] and in 1994. During the 1987 conference,[223] I rediscussed the variable cerebral outcome data from our canine studies with my fellow Harvey Reich and asked him to look into correlations between outcome and multiple factors that can influence cerebral outcome. The only one we found to correlate with normal outcome after ventricular fibrillation no-flow of 10-15 minutes in dogs was accidental mild preservative hypothermia (34-36°C) at the beginning of arrest.[189,224] Mild hypothermia is easier to induce and safer than moderate hypothermia. Therefore, I immediately initiated a series of five major outcome studies.

In the first mild resuscitative hypothermia dog study,[225] under the team leadership of Leonov, outcome after ventricular fibrillation-induced cardiac arrest with 12 minutes no-flow was improved with the use of brief postarrest mild cooling using CPB as an experimental tool. In the second study, under fellow Sterz,[226] clinically relevant external CPR and mild cooling improved cerebral outcome after cardiac arrest of 10 minutes. In the third study, under fellow Weinrauch,[227] lower temperatures after normothermic cardiac arrest made outcome worse. Although if induced before arrest, the lower the temperature the more protection — after normothermic arrest, mild resuscitative cooling seemed more effective than deep cooling. In the fourth study, under fellow Kuboyama,[228] we found that delay of cooling after a normothermic arrest reduces the beneficial effect of mild cooling. Our fifth study[229] proved to be a breakthrough. After 11 minutes of normothermic no-flow, produced by ventricular fibrillation, mild hypothermia of 12 hours not only improved, but actually normalized, functional and morphologic outcome, when this therapy was combined with CBF augmentation by moderate hypertension, mild hemodilution, and normocapnia (in contrast to the previously standardized hypocapnia). These findings led to the first controlled clinical trials of mild postarrest hypothermia by our alumnus Sterz in Vienna. Mild hypothermia following normothermic cardiac arrest did not reduce cerebral oxygen uptake, thus it mitigates brain damage by other mechanisms.[6]

In 1988-94, simultaneously and independently, unknown to me at the time, two other investigative groups (Siesjo of Lund, Sweden; and Ginsburg of Miami) discovered that mild protective or resuscitative hypothermia reduces postischemic hippocampal damage in rats after incomplete forebrain ischemia. Their models are clinically unrealistic but served well in documenting numerous mechanistic details regarding therapeutic hypothermia. In the early 1990s, we also used our rat cardiac arrest model[195] to reconfirm the benefits of mild preservative and resuscitative hypothermia.[197]

Program I included several other "firsts" during the 1980s and early 1990s. Nicholas Bircher documented with me in dog studies and through reviews the advisability of reviving open-chest CPR, because it is physiologically superior to closed-chest CPR.[171-181] In the 1990s, upon my urging, our colleagues and alumni in Belgium could restart hearts with open-chest CPR outside the hospital, after external CPR attempts of 30 minutes or longer had failed. Open-chest CPR, however, was initiated too late to save cerebral neurons; it should be revived, inside and outside hospitals, with less delay.

In the 1970s, the Heimlich maneuver (abdominal thrusts for laryngeal foreign body obstruction) had to be put into its proper place, as its support were anecdotes, not data. Redding and I had served the NRC at a conference appraising this issue in the 1970s. In the 1980s, Bircher documented in dogs that the Heimlich maneuver should be the first step in resuscitation of drowning victims.[230] It rather should be mouth-to-mouth ventilation.

Bircher, with his own knowledge and initiative, challenged other colleagues on the bicarbonate controversy.[231] He demonstrated that after prolonged cardiac arrest, normalization of arterial pH with buffer therapy improves cardiac resuscitability and short-term cerebral recovery. We had explored buffer therapy 20 years earlier, but did not pursue it.

Negovsky and I debated the postresuscitation disease[232] (Moossy preferred the term "syndrome"). I persuaded fellows to use our cardiac arrest-outcome dog models for pursuing not only the cerebral but also the extracerebral postresuscitation syndrome.[233-235] We found the limit of reversibility for the cardiovascular system to be 20 minutes of normothermic no-flow.[188] After 30 minutes of normothermic no-flow, in dogs, we could get the heart restarted with jump-start CPB, but the heart soon thereafter fell apart with hemorrhagic necrosis (suspected reoxygenation injury).[186-189] The viscera seemed to be partially offended but recovered relatively quickly, even after 20 minutes of normothermic no-flow. With good respiratory care, there was no evidence of pulmonary failure. Fellow Neumar found in rats potentially deleterious effects on the heart by excessive doses of epinephrine.[196]

Fellow Xiao was team leader for a very labor-intensive study of a neuron specific calcium-entry blocker, SNX-111 (Neurex). This produced no significant benefit to the brain after cardiac arrest in dogs, using various doses and timing strategies. That agent had shown histologic benefit in the incomplete forebrain ischemia rat model. Fellow Harvey Zar, stimulated by our famous biochemistry consultants Lars Ernster (from Stockholm) and Robert Basford (from Pittsburgh), embarked on attempts to monitor free-radical reactions in resuscitation models using chemilluminescence. Technical obstacles were insurmountable. Pretto spent several years perfecting and using the Langendorff rat heart preparation, for the study of global heart ischemia and reperfusion. He combined this preparation with an isolated liver ischemia model to study the effect of ischemia-reperfusion on the heart.

Program II, the Brain Resuscitation Clinical Trial (BRCT)

I initiated the BRCT believing that convincing laboratory results would still require subsequent controlled clinical trials to become part of clinical guidelines. In 1979 I did not realize how difficult it would be to truly "control" such trials in resuscitation medicine. I recruited Norman Abramson as coordinator, at the end of his clinical training in emergency medicine (in Cincinnati) and CCM (in Pittsburgh with Grenvik). I initiated this study in 1979 because of the success with thiopental loading in monkeys by Bleyaert, et al,[167] encouraging results from clinical feasibility studies,[236] and an explosion of uncontrolled clinical trials by others. I was principal investigator for this first multicenter, prospective controlled randomized study of CPCR from 1979 to 1984 (BRCT I) and 1984 to 1989 (BRCT II). I turned this role over to Norman Abramson from 1989 to 1994 (BRCT III). The BRCT was supported by the NIH. Most of the grant money went to outside clinical investigators for their data reports, and to the data-management center of Katherine Detre's group of the Department of Epidemiology in our Graduate School of Public Health. Robert McDonald of Medicine, then chairman of the first Institutional Review Board (IRB) for clinical studies at our university, recommended this collaboration. Katherine Detre, M.D., D.P.H., a brilliant epidemiologist, wise physician leader and fine colleague, her associate Sheryl Kelsey, Ph.D., and others on that team, contributed greatly to this unique study of 15 years and beyond.

I first invited into the BRCT plan some of our best anesthesiology alumni then working in Europe, since studies on patients were easier to do abroad. Lind had enticed Breivik in Norway, and I enticed Mullie of Brugge, Belgium, to lead pilot experiments,[236] we started with seven centers. PUH was not one of them because our CCM (ICU) physicians had little control in the ED, where department of medicine's interns were given control (the inverse hierarchy principle). Abramson and I added mostly ECCM physicians from other centers in the U.S. Ten years later we had 20 clinical groups in seven countries in the BRCT. After 15 years, our three studies had acquired an enormous database including over 4,000 patients. We collected up to 200 pages of data per six-month survivor. The study was unique because it estimated the cardiac arrest time and CPR time, controlled the resuscitation and life-support protocols, and quantified outcome to six or 12 months. We modified the Glasgow Outcome Score from 1 (normal) through 5 (death or brain death), by separating overall performance categories (OPC 1-5) from cerebral performance categories (CPC 1-5).[5] We did this because one can be incapacitated and still have a good brain.

BRCT I (1979-1984) concerned thiopental loading.[237] There was no overall significant difference between the proportion of patients who achieved good cerebral outcome with thiopental compared with the control group. However, a subgroup of patients — those with long arrests — showed a suggestion of benefit from thiopental. BRCT II (1984-89) evaluated calcium-entry blocker therapy with lidoflazine immediately after arrest.[238] Again, there was no overall statistically significant difference between treatment and control group. When, however, cases with inadequate blood pressure control (hypotension or re-arrest) were eliminated from both groups, we found a significant difference in favor of lidoflazine.[239] A catch 22: some statisticians do not consider retrospective substratification acceptable. In BRCT III (1989-94), titrated high doses of epinephrine to restore spontaneous circulation in prehospital cardiac arrests resulted in enhanced restoration of spontaneous circulation (ROSC) but no significant difference in cerebral outcome.[240] We learned that randomized clinical outcome studies, considered the "gold standard" for evaluation of new treatments in chronic diseases, is not a gold standard for resuscitation research.

An important spin-off was the milking of BRCT data beyond the primary treatment evaluation goals. This yielded more than 10 side-studies with important results. They included: some patients with estimated arrest times much longer than five minutes who achieved good cerebral outcome (I now suspect that they probably were mildly cold); clinical confirmation to support hypertensive and avoid hypotensive reperfusion;[241,242] elderly survivors of CPR can have the same chance for good neurologic recovery as younger patients;[243] and — as obvious — outcome depends mainly on arrest time.[244] The clinical prediction of a persistent vegetative state was possible three days,[245] even 12-24 hours, after arrest.[246] Another spin-off was international networking for the implementation of CPCR. Components of the BRCT study mechanism have been adopted by ongoing CPR case registries, as in Belgium, by other investigators' outcome studies, and by the international guidelines for cardiac arrest and CPR evaluation on a community-wide basis.[247]

After we concluded BRCT III in 1994, when I terminated my role as director of the IRRC, we were ready for a clinical study of mild resuscitative hypothermia. We did not reapply for NIH funding because the federal government then did not permit the waiver of prospective informed consent. At the start of BRCT in 1979, McDonald, chairman of our IRB, and I convinced the FDA and NIH that informed consent cannot be obtained prospectively in resuscitation research. We laid the groundwork for overcoming this obstacle to clinical resuscitation research. In 1979-94 there was no objection. Nobody else was willing to tackle the problem. We informed relatives as soon as possible about a patient drawn into the study, and followed other, universally accepted safeguards. We used the term "deferred consent," which of course is a misnomer.[248,250] We meant waiving the present consent request. The NIH and FDA prohibited waiver of consent in randomized clinical trials from 1994 to 1996. Therefore, only our alumni and colleagues in Europe and Japan became engaged in clinical resuscitation research concerning the most promising treatment, mild hypothermia.

The main lessons I learned about randomized clinical trials (RCTs) from the BRCT experience of 15 years, are: 1. The enormous number of unknown or uncontrollable clinical variables makes it impossible to control RCTs, and to satisfactorily prove "no effect." 2. The need to randomize patients into groups within seconds renders impossible any prospective stratification into subgroups that might be within the therapeutic window. 3. Convincing positive results from outcome studies in reproducible large-animal outcome models should replace clinical randomized outcome studies in CPCR research, while clinical feasibility and side-effect studies should precede any treatment becoming part of guidelines for routine use. 4. After clinical feasibility trials, simple, safe, and inexpensive treatments with breakthrough effect in animal outcome models, like mild hypothermia, should not be randomly withheld because this could be considered unethical.

Program III, Trauma and Shock in Animals

I pursued these projects because hemorrhage is the second most common noninfectious acute dying process. We used novel outcome models in dogs, rats, and monkeys. We first extended Takaori's studies of the 1960s[129-131] with a dog trial comparing lethal normovolemic hemodilution with albumin vs salt solution and found the former, as expected, to be more effective.[251] Then, fellow Gad Bar-Joseph (pediatric intensivist from Israel) developed and used a volume-controlled, pressure-adjusted new outcome model of hemorrhagic shock in monkeys.[252,253] We found that severe hemorrhagic shock with mean arterial pressure of 30-40 mm Hg for two hours, in the absence of tissue trauma or sepsis, can be reversed to complete recovery, including histologically clean brains, regardless of the type of fluid used. The fluids evaluated included stroma-free hemoglobin.

Samuel Tisherman started as a medical student research fellow with us in 1982 and is now, 18 years later, a faculty trauma surgeon and intensivist (Pitt-trained), with national reputation. He has remained throughout a productive investigator of our Center. When he was a medical student, Tisherman, with other Pitt medical students Alfonsi and Gilbertson, tried to clarify in dog models some questions I posed on ice-water drowning. During asphyxiation, the brain cooled to protective levels before the heart stopped.[254] This can explain "miraculous" recoveries after prolonged ice-water submersion without having to postulate a diving reflex. The heart could be restarted with CPB after profound hypothermic cardiac arrest of two hours.[255] In the 1980s, Tisherman pursued exsanguination cardiac arrest of 12 minutes in dogs; it resulted in the same cerebral outcome as 12 minutes of ventricular fibrillation with no blood flow.[256] This disproved our hypothesis that blood in the microcirculation during stasis (as in normothermic arrest) might be deleterious. Tisherman became associate professor and a role model as a clinician-scholar-investigator.

Hemorrhagic Shock. When David Crippen was a fellow with us, I asked him to develop a new outcome model of volume-controlled hemorrhagic shock in conscious rats[257-260] to explore possibilities for preventing acute death from hypovolemia before the rescue helicopter arrives. Without fluid resuscitation, survival times and rates were greatly increased by the use of oxygen or moderate hypothermia during shock;[259] the best results were from a combination of both therapies.[260] Peitzman, our trauma surgeon, promoted studies of withholding fluid resuscitation during uncontrolled bleeding. I felt it obvious that aiming for normotension is silly while arteries have holes. He felt that practice will not change without proving the obvious. I also felt that withholding all fluid during hemorrhage, as suggested by colleagues in Houston, is going too far when cardiac arrest is imminent. We therefore studied hypotensive (limited) fluid resuscitation, maintaining mean arterial pressure at around 40 mm Hg until hemostasis. Brazilian trauma surgeon research fellow Antonio Capone developed a rat model of volume-initiated uncontrolled hemorrhagic shock (by tail amputation), with three phases — a shock phase, a resuscitation phase, and an observation phase to three days.[261] After severe shock longer than 1 hour, delayed deaths were due to necrosis of the gut. In low-flow, one must focus on saving the abdominal viscera. As expected, during uncontrolled hemorrhagic shock, using plasma substitutes instead of blood to achieve normotension, mortality rates were increased as a result of washout anemia; however, limited fluid resuscitation to a mean arterial pressure of only 40 mm Hg improved survival time and rate. We went on to pursue systematically a search for optimal fluid resuscitation in the prehospital phase to extend the "golden hour."

Suspended Animation. In 1984, when I visited San Francisco as an invited reviewer of Army grant applications, I discussed resuscitation potentials for military combat casualties with Army surgeon Ronald Bellamy, a distinguished epidemiologist of the Vietnam War, then at the Letterman Hospital. Penetrating brain trauma is usually lethal or permanently crippling. Most soldiers "killed in action," who were shot through the abdomen or chest, without brain trauma, exsanguinated internally to cardiac arrest within 5-10 minutes. Autopsies often revealed repairable internal injuries. Bellamy and I talked each other into the need to develop a totally new approach to resuscitating such casualties: we called it "suspended animation" for transport and repair without pulse followed by delayed resuscitation to survival without brain damage.[262] I believe that we need to develop a way to initiate in the field, before loss of viability, before five minutes after heart stoppage at normothermia, a state of preservation, for one, or more hours, with prolonged no-flow or trickle-flow.

In 1988 I initiated fellow Tisherman into team leadership of six studies in newly developed dog models, using hypothermic strategies.[263-268] Severe hemorrhagic shock is followed by deep (10-20°C) or profound (5-10°C) hypothermic circulatory arrest, induced and reversed by CPB.[264] In suspended animation study #6,[230] Capone and Tisherman achieved survival without functional or histologic brain damage in dogs after one hour of normothermic severe hemorrhagic shock followed by one hour of profound hypothermic circulatory arrest with a brain temperature of 10°C. Arrest was preceded by blood washout. In the next section I will summarize further development of the suspended animation project. In 1994, we organized a provocative panel discussion during our resuscitation researchers' conference in Pittsburgh.[262] In spite of the science-fiction flavor of the term "suspended animation," all participants considered research in this area to be realistic and promising.

Traumatic Brain Injury (TBI). Another feature of our trauma-oriented laboratory studies was the creation of a new outcome model in dogs. I remembered our uncontrolled patient trials of hypothermia after TBI by Rosomoff in the 1960s. In dogs we created a simulated epidural hematoma, which, after drainage, led to brain swelling and herniation.[269] Using this model, in 1992, we documented the ability of moderate hypothermia to prevent intracranial hypertension. For ICP control mild hypothermia was insufficient. Rewarming two days later, unfortunately, led to herniation, as it occurred earlier in the normothermic dogs. Ebmeyer extended moderate hypothermia to 48 hours.[270] He found coagulopathy and pulmonary problems. Our dog study was partially funded by a preliminary NIH grant for a UPMC-based program project (center) on TBI, which began in the mid-1990s under the fine leadership of neurosurgeon Donald Marion. I remained an advisor for Marion's program.

Program IV, Disaster Reanimatology

This program is based on my recommendations during the inception of the Club of Mainz in 1976. Until then, research about mass disasters such as major earthquakes was the domain of public health, sociology, and engineering. Interviews of survivors that I conducted after earthquakes in Peru in 1970 [271] and in Italy in 1980 [272] made me suspect that a major proportion of persons counted among the dead were not instantaneously crushed to death, but died within minutes or hours of being crushed or otherwise injured. This prompted me to invite Edmund Ricci, my friend and colleague from the Graduate School of Public Health, to help us design a structured-interview study mechanism to begin evaluating such patterns of dying during major earthquakes. We used postevent interviews of survivors and rescuers.

The first such study concerned the Armenian earthquake of 1987.[273-276] For this study, considered a "first" in disaster research, I assembled a multidisciplinary team from Pitt, and invited Negovsky's group in Moscow, who added some colleagues in Armenia. Because I could not go myself, I asked anesthesiologist Miroslav Klain, who speaks Russian, to be team leader. Anesthesiologist Pretto was a team member. The results of semistructured interviews revealed a "golden 24 hours" of disaster reanimatology after which outside help for the severely injured is largely useless. Life-supporting first aid (LSFA) by uninjured co-victims could have a strong lifesaving potential, whereas advanced trauma life support (ATLS), being difficult to bring to the scene early, seemed to have only moderate lifesaving potentials. From then on, Pretto decided to give up lab research and devote himself fully to disaster reanimatology. The Armenia study was followed by similar studies, under the leadership of Pretto, after earthquakes in Costa Rica in 1991, Turkey in 1992, Japan in 1993, Los Angeles, and Kobe. They all confirmed the results of the Armenia study with increasingly structured interviews. The data supported my longstanding recommendation that the U.S. National Disaster Medical System change its civil defense (nuclear war-focused) model to a regional EMS-based model.[277]

After the first international EMS congress in Mainz,[278] the initiation of the Club of Mainz for Emergency and Disaster Medicine in 1976,[279] its first congress in 1977 in Mainz,[280] and its second congress in 1981 in Pittsburgh,[281-285] I decided to initiate a new journal, first called *Disaster Medicine*.[281-286] It was started with the excellent editorial assistance and mission-oriented dedication of my good friend Nancy Kirimli-Linden, R.N. Nancy has contributed to our department since 1962. The disaster medicine journal, the first of its kind in the world, had growing pains because the Club of Mainz had only 100 members and there were difficulties with the first publisher. In 1985, we changed its name to *Journal of the World Association for Disaster and Emergency Medicine* (WADEM), the new name for the Club of Mainz. That revived journal was produced and distributed by us at the IRRC, under difficult conditions and with a rescue grant from Laerdal. I turned the editorship over to R.A. Cowley of Baltimore, who two years later became terminally ill. In the late 1980s the journal finally ended up with a third and permanent name, *Prehospital and Disaster Medicine* (PDM). This was under the auspices of the WADEM. In the 1990s, under the excellent editorship of Marvin Birnbaum of Madison, Wisconsin, an emergency physician and intensivist, after priority struggles with the National (U.S.) Association of EMS Physicians (NAEMSP), the PDM became stabilized and entered into the Index Medicus.

Our disaster medicine interests automatically led me to "peace medicine." In 1981, as the International Physicians for the Prevention of Nuclear War (IPPNW) organization was founded in Washington, D.C., we hosted the Second World Congress for Disaster and Emergency Medicine (Club of Mainz) in Pittsburgh.[281] I brought together for the first time military leaders of several countries with peace medicine representatives of the American branch of the IPPNW, the Pittsburgh chapter of the Physicians for Social Responsibility, and anesthesiologists interested in disaster medicine. We believe that this helped to change the attitude of U.S. military leaders toward a more active role in responding to non-military mass disasters, using primarily the military's airlift, supply and command capabilities, as is the case in other countries.[282]

Man-made disasters, such as war, came into the limelight of our disaster program when, in frustration over the wars in the Balkans (1990-1996), I corresponded with our government, the United Nations, and colleagues in former Yugoslavia (I am an honorary member of the Slovenian Academy of Sciences). I encouraged Ernesto Pretto to get involved.[286] He organized a coalition of Pittsburgh organizations to centralize humanitarian aid for Bosnia. Pretto, out of his own initiative, in spite of obvious danger, flew into Sarajevo during the murderous siege by the Serbs, at risk to his own life. Pittsburgh oncologist Mirsada

Begovic, originally from Sarajevo, and a satellite phone link provided by the Soros Foundation, gave us a communication link with physicians in Sarajevo during the siege. The brief use of NATO air power and diplomacy with Croatia (also no saints) enabled President Clinton to end the killings in Yugoslavia, bringing about the Dayton Agreement and deployment of peacemaking troops. The later prolonged use of air power in the Kosovo peacemaking mission is a separate topic. At least the killing was stopped.

In summary

The activities of the IRRC during its first 15 years, from 1979 to 1994, went far beyond what I have described here. I felt best about the following six results: 1. We learned that the pathophysiology of dying and resuscitation is much more complex than initially thought, at least as far as the encephalopathy after prolonged cardiac arrest is concerned. 2. We established the first (perhaps still only) animal research ICU program and outcome models in large animals. 3. We had made clinically important strides toward our goals by documenting in dogs ways to significantly increase the resuscitability from prolonged normothermic cardiac arrest (with mild resuscitative hypothermia), and from profound hypothermic suspended animation (with emergency CPB). 4. We initiated and developed the first multidisciplinary clinical trial mechanism for CPCR. 5. We conducted the first field studies in "disaster reanimatology." 6. We provided research training and support for academic careers for the majority of our 60 physician research fellows and 20 medical student research fellows during these 15 years.

At the personal scene, the 1980s included highlights, turmoil, peace activism and much professional travel. At the medical ethics scene, my activism for good dying when "one's time has come" (including support of the Right to Die organization), was enhanced by the sudden death, resuscitation, and dignified departure of my mother in Vienna, at age 92, in 1983. I had the opportunity to be present. Also, I was invited to contribute to national guidelines on the management of hopelessly ill patients.[154,155]

In 1989, at age 65, I decided to stop laying hands on patients, and in 1994, at age 70, it seemed good to give up the directorship of the IRRC and turn it over to the younger generation. I spent my seventieth birthday, April 12, 1994, with Eva and our children and grandchildren, in the Dominican Republic, near the place where Columbus first landed.

In May 1994, IRRC fellows Ebmeyer and Katz organized a unique researchers' conference in Pittsburgh, to celebrate the IRRC's first 15 years, and my seventieth birthday. I led this conference to initiate the change in IRRC leadership, and to catalyze resuscitation research at the international level.[287-293] Over 100 investigators, including past fellows from many countries, participated in this resuscitation researchers' conference of May 1994. The knowledge and research plans that evolved from that conference have been documented only partially.[287-293] Young researchers considered these discussions to be stimulating background for moving resuscitation medicine into the future.

Uwe Ebmeyer, chief IRRC fellow from 1992 to 1994, summarized the uniqueness of the IRRC as follows:[287]

First, the IRRC has always tried to focus on the patient, not on molecules. The questions selected for research have concerned patient problems and challenges. As the NIH focused on molecular genetics, the IRRC's priorities remained loyal to clinically relevant questions.

Second, the IRRC created a strong link between old and young colleagues, who have been cross-influencing each other. Often the older ones know history better and help avoid re-inventing the wheel. Often the younger ones come up with fresh ideas. Older scholars-investigators should listen to and accept younger colleagues' ideas. Fellows with a European background have been impressed by this, as it is not a trend in Europe. This shows that Safar, an ex-European, had become Americanized.

Third, the four programs of the past 15 years, namely cardiac arrest-related laboratory research, cardiac arrest-related clinical research, trauma and suspended animation-related laboratory research, and disaster reanimatology have cross-fertilized each other continuously. This has been through the presence of investigators from the four programs at the same weekly lab meetings, and through the proximity of labs and offices concerning all four programs.

Fourth, worldwide communication by IRRC faculty investigators has been expanded to include fellows. Most alumni of the IRRC have become world citizens. All enjoy the worldwide IRRC network. During the cold war, that worldwide outreach included difficult East-West communications between Pittsburgh (Safar, et al), Moscow (Negovsky, et al), and Prague (Keszler, Pokorny, et al).

Fifth, the most valuable spin-off of the IRRC, perhaps even more important than the specific research results, has been the creation of some young doctors who became, after leaving the IRRC, leading clinicians-scholars-investigators, all in one. These doctors have not been satisfied with merely publishing papers. The breeding of such Renaissance physicians in the IRRC has led to the development of new resuscitation research programs elsewhere in the world.

Safar Center for Resuscitation Research (SCRR) 1994-2000

In these six years, with others continuing research on sudden cardiac death, I gave priority considerations to presently unresuscitable conditions; starting with trauma, i.e., severe, prolonged, traumatic hemorrhagic shock (HS) and exsanguination.

In 1994, at age 70, I decided to give up the leadership of the IRRC. I continued as a principal investigator of one or two programs, responding to needs and research opportunities, and to be advisor as called upon.[294-296] On normovolemic cardiac arrest in dogs, we had reached my goal of the 1970s, to extend the longest period of normothermic cardiac arrest which is reversible to survival without functional and histologic brain damage from 5 minutes to 10 minutes.[229] With mobile ICU ambulances' response times being about 8 minutes, this is a potential breakthrough. We also learned from our BRCT (1979-94) about the limits of randomized clinical outcome studies of CPCR, which clinicians have to consider as they now move resuscitative CBF promotion and mild hypothermia to clinical use. The other at present greatest potential breakthrough for resuscitation from sudden cardiac death is automatic external defibrillation by police and the lay public. This was pioneered by several communities. Positive clinical trials include the one in a Pittsburgh suburb by Mossesso of the University of Pittsburgh Center for Emergency Medicine and Mary Ann Scott, R.N., now EMS leader in that community (former head nurse of our PUH ICU).

In 1994, an intradepartmental committee headed by Peter Winter appointed Patrick Kochanek as the new director of the IRRC. My former fellows and then co-leaders of the IRRC, anesthesiologists Nicholas Bircher (focusing on cardiac arrest research) and Ernesto Pretto (focusing on disaster reanimatology), continued as associate directors with Pat Kochanek. He insisted on changing the name of the IRRC to "Safar Center for Resuscitation Research" (SCRR). I said that such an honor might be considered after one's death, but not now. Kochanek, administrators, and my associates insisted.

Pat Kochanek is a tenured professor of anesthesiology and pediatrics. He is a pediatric intensivist with deep knowledge of cellular-molecular mechanisms and of clinical CCM. Although not trained as an anesthesiologist, his primary appointment is in our department, as are the appointments of all intensivists at our UPMC, regardless of their specialty. Exceptions, since 1979, after my time as chairman, have been a few internists of a small parallel adult pulmonary medicine ICU program. Pat Kochanek, a most delightful colleague and friend, is now leading our research center into the future, with novel ideas, workaholism, multidisciplinary orientation, role-modeling, and great collegiality.

Kochanek had clinical CCM training at the Children's Hospital in Washington D.C., under my and Kampschulte's alumni Peter Holbrook and Alan Fields; he also had laboratory research experience at the Bethesda Naval Medical Research Institute. Pat has high scientific standards, is a superb mentor of research fellows, has learned grantsmanship, and has become an ideal role model as a clinician-scholar-investigator-leader, all in one person. I respect him greatly. This kind of faculty member is an endangered species when clinical demands by health care business administrators force clinician-scientists to "buy" research time with grant money. This Kochanek has been able to achieve so far with NIH grants. He also became the first SCCM-salaried investigator. The scientific future of the SCRR looks great, its financial future, however, is uncertain.

Kochanek has been particularly interested in the brain's inflammatory reactions to mechanical traumatic brain injury (TBI). He is investigating this in rats and in patients. Kochanek's molecular know-how became available to all SCRR programs, including mine on cardiac arrest. The merging since 1994 of Kochanek's knowledge of molecular mechanisms, and my and my previous associates' experience with treatment and outcome-oriented research (my projects are now again funded by the Department of Defense) has been good for our center, our medical school, and our university. Pat wants me to be advisor and principal investigator of whatever I want to do.

As an SCRR advisor, I have conveyed all along that in my opinion future breakthroughs in resuscitation medicine will come from coordinated studies at multiple levels — from molecules and cells to organisms and community.[294-296] Goal-oriented planned research by teams should be in an environment that fosters serendipitous discoveries (as we did with mild resuscitative hypothermia). I consider it unlikely that breakthroughs will come from measuring concentration changes of one molecular species at a time, in such highly multifactorial processes as the encephalopathy after ischemia or TBI, or as polytrauma and septic shock. I have little hope that designing single-target-oriented pharmacologic strategies will bring breakthroughs. Searching for combination treatments to mitigate multifactorial processes make more sense but are more difficult and tedious.

Scientific knowledge and medical technology have exploded in recent years. Therefore, much teaching and research have become ultra-focused and specialized. In the future, however, academic medicine will also need some inspiring colleagues like Pat Kochanek, who are globally-oriented, and who can assemble and guide teams. I have tried to do this since the 1970s, but without knowledge of the molecular level. The NIH is preferentially funding studies of molecular mechanisms; results so far have yielded no therapeutic breakthrough for resuscitation. That may still come, if mechanism and outcome-focused researchers openly communicate. Let us not forget that the mechanisms are still not known for many breakthrough treatments that have saved millions of lives, discovered partially by serendipity, such as anesthetics, antibiotics, insulin, and corticosteroids.

After the conference in May 1994, which was well organized by our last IRRC fellows under the leadership of Ebmeyer and Katz, Kochanek remodeled our laboratories and slightly modified the organization of our four IRRC programs. He is conducting excellent weekly Journal Club sessions for topics of all programs. Team leaders for laboratory work have mostly been M.D. research fellows. In the 1990s, Kochanek attracted research fellows mostly from our clinical pediatric CCM fellowship at CHP. The pediatric CCM Board requires research experience. I am now attracting mostly advanced colleagues from abroad, because adult-CCM fellows in the U.S. do not have to do research, are heavily in debt, and are eager to go into lucrative practices.

This section of my memoirs will be brief, since the last six years of the SCRR have been summarized in annual reports (which are included in the SCRR website: www.safar.pitt.edu), in my videotaped slide lectures of 2000 (now in preparation), and in some of our review papers of the 1990s.

SCRR program I, traumatic brain injury (TBI)

This is under the direction of Kochanek. He had developed several cortical contusion models in rats, starting at the anesthesiology laboratory in the early 1990s. His studies on inflammatory reactions to TBI were so successful that I recommended to neurosurgeon Don Marion, principal investigator of a new NIH-funded TBI research center at our UPMC, to make Kochanek his co-principal investigator. Neurosurgeon Marion is focusing on clinical aspects of this TBI research center program.[297] Essentially all laboratory research of this TBI program is conducted in the SCRR under the leadership of Kochanek.[298,299] The clinical TBI study has led to the first controlled documentation of improved outcome with mild to moderate hypothermia after TBI.[297] The laboratory TBI projects have focused on various molecular and cellular mechanisms.[298,299]

My own TBI research involvement, although meant to be a transient contribution, preceded 1994. In 1991, our fellow Shlomo Pomeranz and I developed a TBI outcome model in dogs[269] and we documented control of ICP using prolonged moderate hypothermia (31°C). We then used this model for a study by Ebmeyer,[270] which showed complications with coagulopathy and pulmonary infection if moderate hypothermia is extended to 48 hours. Both studies showed ICP increase to herniation during rewarming, a problem in need of solving. That dog model, however, was found to be too expensive to be covered by the NIH TBI grant.

Marion and Kochanek are superb leaders. In 2000 they received renewal funding from NIH for another 5 years. They attracted several new, NIH-funded investigators to the SCRR. C. Edward Dixon, Ph.D. is a physiologist with special expertise in testing cognitive function in animals. Larry Jenkins, Ph.D. is a neuroscientist with broad knowledge also about post-ischemic encephalopathy. His special expertise includes morphology (including electron microscopy) and tissue chemistry. Edwin Jackson, Ph.D., is our guru in pharmacology. Robert Clark, M.D., a pediatric intensivist and one of Kochanek's past star-fellows, is an expert on DNA damage and apoptotic mechanisms. Valerian Kagan, Ph.D. is our expert in free radical measurements. In 1996 I helped Pat get additional funding for TBI rat studies from the U.S. Army, focusing more on treatments than on mechanisms. He asked me to be Co-PI. In 2000, Kochanek, with me as Co-P.I., applied and got funded a NIH research training grant for 5 years, for pediatric neurologic CCM and resuscitation.

SCRR program II, cardiac arrest

Swedish anesthesiology research fellow Rubertsson, guided by Bircher, began to explore in dogs intra-aortic balloon catheter strategies for restoration of spontaneous circulation from prolonged normovolemic cardiac arrest.[300] Our asphyxial cardiac arrest outcome model in rats[195] is still being used by other young investigators, mostly from the Center for Emergency Medicine, collaborating with our SCRR group.

Bircher contributed to national guidelines modifications of the AHA. I spent a considerable amount of time and energy on promoting at community levels LSFA training of the public.[116] To the flawed questioning by some recent AHA CPR committee members of the need for mouth-to-mouth ventilation in CPR,[74] we were forced to reply with lengthy rebuttals in writing and in conferences.[75] Some young AHA leaders forgot to look at the pre-Medline literature, which has the only data on ventilation produced by sternal compressions in humans, i.e., none.[69] Although I am not ambitious to resume education research, I helped review research on first-aid training so far.[116] I am available to help with suggestions and contacts, also for other members of the Department of Anesthesiology who pioneered use of simulator training.

On clinical trials, the BRCT has been dormant from 1994 to 1996 because of the prohibition by NIH and FDA to waive informed consent. In 1989 I had introduced into our first application for BRCT III the idea of a cardiac arrest clinical study of mild hypothermia. The reviewers thought that the animal data in support of hypothermia were strong, but we had not shown how to quickly induce mild cooling in cardiac arrest patients in a clinically feasible way. They approved the epinephrine arm of the proposal, but not the hypothermia arm. The waiving of prospective informed consent was disapproved by the NIH and FDA in the early 1990s, but this has recently given way to modified approval.

In 1996, our alumnus Sterz, who has become the leading professor of emergency medicine in Vienna, initiated a multicenter randomized clinical-outcome study of mild postcardiac arrest hypothermia, supported by the European Union. The data will be out by the end of 2000. The BRCT study mechanism is still available, although I personally have neither the ambition nor the current clinical expertise to conduct a major clinical trial now. In the late 1990s, I began leading others to resume milking our large BRCT I, II, and III database. I have mentored so far two studies by a Ph.D. student, Howell Sasser, of our School of Public Health. One study supported our and others' animal data showing that brief arterial hypertension after prolonged cardiac arrest saves neurons.[242] The other study, on clinical outcome prediction, found 100% correlation between absence of certain cranial nerve reflexes at 12 or 24 hours after restoration of spontaneous circulation, with poor cerebral outcome.[246] This will be helpful for deciding on "letting die" before expensive futile intensive care after cardiac arrest and CPR is initiated.

SCRR Program III, Disaster Reanimatology

This has been from 1994 to 2000 under the direction of Pretto. He had earlier received a grant from the late Mrs. Grete Singer of Florida (formerly Vienna), who was a close friend of the Safar family; and recently a grant from the Commonwealth of Pennsylvania to help transfer the results of his retrospective interview studies after earthquakes for better preparedness for disaster-threatened communities in our state. For a few years, Pretto's office had been the main office for the WADEM. In 1998, Pretto helped organize in Costa Rica a very successful regional WADEM congress for Latin America. The future of the disaster reanimatology program of the SCRR beyond 2000 is uncertain. Whenever I decided to turn leadership of an organization or program over to a successor, I did so with trust in him/her and with delegation of authority and responsibility, trying not to meddle. The future of disaster research lies now with young colleagues who are continuing the leadership of WADEM. In 2000, the young generation is planning the future of the WADEM with new objectives. They are asking the surviving co-founder for suggestions.

SCRR Program IV, Hemorrhagic Shock (HS) and Suspended Animation (SA)

For this program I retained leadership as PI, with Tisherman as Co-PI. In 1994 I decided to turn my attention to attempts at maximally increasing the tolerance of severe hemorrhage — hemorrhagic shock (HS) (low-flow) beyond the "golden hour" of HS tolerance, and exsanguination cardiac arrest (no-flow) beyond the current 10-minutes tolerance of normothermia. The latter requires suspended animation research. We had already earlier developed and used outcome models of HS in rats and suspended animation in dogs. Our goal now, for both HS and suspended animation, is to achieve promising results and move them rapidly from rats to patients, perhaps via dogs or pigs.

I first sought funding from the U.S. Army. We needed a grant large enough to support continuance of our unique research ICU program in dogs. That alone would require a minimum of $0.5 million per year, mostly for personnel covering 24-hour experiments. While waiting, we succeeded in retaining key team members. Such a program cannot be turned on or off, depending on erratic funding of projects, as it requires an experienced, stable team. The Army had funded my resuscitation research in the 1950s and 1960s and had given support for our 1994 conference publication.[287] In 1994, however, we were told that the Army is committed predominantly to telemedicine and development of devices. Army surgeon Ronald Bellamy and I initiated three conferences during the mid-1990s to explore novel research topics for resuscitation of combat casualties. Others called these discussions "Lazarus meetings." I have recommended since 1994 secure long-term funding of a multicenter "Mini-Manhattan Research Project" on suspended animation and related topics for presently unresuscitable rapidly dying combat casualties and patients.

Navy Commander Lyn Yaffe, M.D., pathologist and scientist, showed wisdom and imagination: he decided to give priority to the topics of severe hemorrhage and suspended animation that I had proposed. His U.S. Naval Medical Research and Development Command (USNMRDC) began funding our studies of HS in rats in 1996, and of suspended animation in 1998. Safar and Tisherman, with over 10 part-time co-investigators, 3 M.D. research fellows, 5 technicians, 2 secretaries, and 5 outside "subcontractors" received a Navy grant of $2.4 million for 1998, $1.9 million for 1999, and $1.2 million for 2000. This was mainly for the simultaneous pursuit of HS in rats and suspended animation in dogs. Although a five-year program was scientifically approved, funding is on a year-by-year basis, depending on how Congress supports medical research by the Department of Defense. When Commander Yaffe retired, Commander Douglas Forcino followed, who was followed by Jeannine Majde-Cottrell, Ph.D., our present project manager (and scientific advisor) for the Office of Naval Research (ONR), which since 1999, has assumed the role of the USNMRDC. Dr. Majde-Cottrell is particularly interested in novel fluid resuscitation for HS.

Hemorrhagic shock (HS). Throughout the 1990s, our HS studies in rats continued from where we had started in 1990 with volume-controlled HS in conscious rats[257-260] and uncontrolled HS in anesthetized rats.[261] Fellows Capone and Kim documented a survival advantage of hypotensive (limited) i.v. fluid resuscitation before hemostasis — maintaining MAP around 40 mm Hg, to prevent cardiac arrest. Using our new three-phase outcome model in rats of uncontrolled HS (by tail amputation),[261] fellows Kim, Takasu, and Prueckner documented a doubling of survival time during HS without resuscitation, and of survival rate after resuscitation, using moderate hypothermia (34°)[301,302] or mild hypothermia.[303-305] The latter, of course, is simpler and safer. This dramatic benefit from mild hypothermia in HS in rats we found to be the result of hypothermia itself, rather than its ability to raise blood pressure.[305] We also conducted studies on molecular mechanisms. Our documentation of benefit from controlled "keep him cool" in rats is in contrast to the clinical reports on traumatic shock showing patient mortality rates correlating with spontaneous uncontrolled hypothermia.[306] Tisherman is now taking mild hypothermia for traumatic HS to pigs, and has plans to conduct a multicenter clinical study. It needs further confirmation and exploration of risk mechanisms, particularly of shivering and coagulopathy. If safe, first-aid guidelines of "keep him warm" should be changed to "monitor temperature and keep him mildly cool."

Fellow Carrillo and co-investigator Dixon also documented that pressure-controlled HS in rats (MAP 30-40 mm Hg for 45-60 minutes) is not followed by brain damage, not even in terms of cognitive function and histology.[307] The healthy brain protects itself with maximal vasodilation down to a MAP level just above that which causes cardiac arrest. Delayed death after hemostasis and restoration of normotension, in our models, seems to be from gut necrosis, liver failure, and MOF. This we are trying to clarify.

In 1999 and 2000, we decided to search for the fluid composition which would sustain viability during maximal duration of HS with the smallest volume. Anesthesiologist Rainer Kentner from Mainz joined us for two years.[308-310] He is exploring the effects of various resuscitation fluids on outcome and on mechanisms.[309] Even in uncontrolled HS, titrated hypertonic/hyperoncotic solution for hypotensive life support before hemostasis, is as effective as 10 times the volume of lactated Ringer's. How about adding an oxygen carrier and drugs? Anesthesiologists Xianren Wu, who joined us from Switzerland, and Kentner are pursuing mechanisms and drug treatments for HS.[308,310] The antioxidant Tempol i.v. is effective but only if started early during HS.[310]

When Joseph Barr and our past fellow Gideon Eshel of Israel published that peritoneal ventilation with O_2 in rabbits could increase arterial PO_2 in experimental pulmonary consolidation, I suggested to try in our rat HS model mitigating the ischemic hypoxia of gut and liver with peritoneal O_2. I invited Barr and we found that this is indeed the case.[311] We since then have explored peritoneal and enteral routes for pharmacologic treatment of the viscera. Adenosine seems effective.[312] I would like to see these topical treatment strategies tried for the delivery of oxygen, drugs, and cooling, starting in the field. In dogs and human cadavers we found blood cooling more rapidly effective than surface cooling, with peritoneal cooling in-between.[313-315]

In June 2000, Dr. Majde-Cottrell conducted an excellent meeting on animal models of severe hemorrhage. Upon her request we submitted for discussion at that meeting, reports on 15 such models that we had developed and used since the 1960s — in rats, dogs, and monkeys. For starting in 2001, I turned over to Sam Tisherman, the principal investigator role of the HS rat studies. His expertise and unique talents make continued funding by the DOD fairly certain. He is planning to take therapeutic hypothermia for HS to patient trials.

Suspended animation (SA). This requires dog models, CPB, and our animal ICU program, for which the necessary big grant was not available from 1994 through 1997. Having explained above (see IRRC) what we mean by suspended animation,[262] and having initiated hypothermic strategies with CPB in dogs by Tisherman in the 1980s,[263-268] we embarked in 1998 on a systematic search for a method for the rapid induction of SA at the start of exsanguination cardiac arrest in dogs. I would like to see this topic brought to clinical trials, at least in trauma centers' EDs, before I give up the principal investigatorship. In 1998, under Navy funding through Commander Yaffe's vision, we could resume dog outcome experiments, about two long-term 72 hour dog experiments per week. If successful in the exsanguination cardiac arrest dog model, suspended animation might also save some currently unresuscitable patients of normovolemic sudden cardiac death. It may also enable, under elective conditions, presently impossible operations, particularly on brain, heart, and large vessels.

While the HS studies in rats must focus on the viscera as the most vulnerable organs, because they go into complete vasoconstriction while brain and heart protect themselves with vasodilation, research on cardiac arrest must focus on the brain. In the spring of 1998, we completed a modification of our exsanguination cardiac arrest dog outcome model. By flushing brain and heart with the aid of an aortic balloon catheter, at the beginning of exsanguination cardiac arrest, with saline at ambient temperature (24°C) vs chilled (4°C), we have been able to induce varying degrees of preservative hypothermia within two minutes, without the need for CPB.[316-318] I asked my associate Klain to search for a feasible vessel-access method for use by emergency department physicians, and by military medics in the field (a rapid method for inserting an aortic balloon catheter by Seldinger technique via the femoral artery or parasternally). In a systematic series of dog studies, we achieved complete long-term functional recovery of dogs after 30 minutes of exsanguination cardiac arrest no-flow, by flushing a very large volume of 4°C saline via balloon catheter into the abdominal aorta.[318] Flush with very large volumes can push tympanic membrane temperature to 10°C for preservation during cardiac arrest of 1 hour, without the need for CPB. We learned that aortic arch flush is not enough for cardiac arrest >20 min, as the viscera and spinal cord cannot fully tolerate normothermic no-flow that long.

Combat medics cannot carry many liters of fluid and a fridgidaire. We are searching for a preservative portable cool solution. In 1999, we embarked on the systematic exploration of many pharmacologic preservation potentials.[319] There was only minimal benefit from thiopental, but a significant benefit from a preservative flush with the antioxidant Tempol.[320] All drug effects were less than those of hypothermia.

Our Navy project's four outside collaborating distinguished neuroscientists did not come up with a pharmacologic breakthrough suggestion. Bo Siesjo, who had moved from Sweden to Honolulu, focused on mitochondrial damage. Ronald Hayes from Houston, now in Gainesville, Florida, focused on protease inhibitors. Thomas Sick of Miami and Philip Bickler of San Francisco explored membrane-active and excitotoxicity-suppressing strategies in brain slice models. My skepticism that single-molecular strategies must fail in mitigating the multifactorial complex postischemic-anoxic encephalopthy prevailed, recognizing that it was not possible to address the issues of pharmacokinetics and brain penetration in our outcome experiments. Hypothermia suppresses all chemical reactions (good and bad).

Advised by Howard Champion, trauma surgeon of the Cowley Shock-Trauma Center in Baltimore, and Ronald Bellamy, the ideologic co-initiator of our SA program, Tisherman is plotting clinical trials also of suspended animation by cold flush, in pulseless patients with exsanguination cardiac arrest, in ED patients who, in spite of emergency thoracotomy, have a near 100% mortality. Thoracotomy should make aortic catheter insertion easy.

The above-mentioned possibility, to explore whether hypothermic suspended animation may also be useful for normovolemic sudden cardiac deaths, is intriguing. Over 100,000 out-of-hospital sudden cardiac death cases per year in the U.S. are not resuscitable by standard CPR-BLS-ALS and are given up. Many might be resuscitable, provided one could buy time with hypothermia, until initiation of CPB, which could support oxygen delivery for long periods. This could lead to heart and brain being evaluated, and the heart fixed or replaced.[321] This scenario we intend to explore first in our dog models. Another five year plan for our suspended animation project is in preparation, encouraged by Lyn Yaffe.

Miscellaneous – SCRR and our Department

The IRRC-SCRR celebrated in November 1999 the first 20 years of the present facility – with Siesjo as the visiting guest speaker. My personal goals at the turn of the millennium, as an active laboratory researcher, are to help extend maximally the golden hour of uncontrolled HS tolerance in rats, to achieve suspended animation goals (above) in dogs, and to see both taken to patients in the hands of young colleagues.

Funding of IRRC research in my last year as director (1993-94) was adequate for laboratory studies (about $0.5 million from the Department of Anesthesiology and the Laerdal Foundation combined) and the last year of the BRCT (an additional $0.5 million from the NIH). I regret that, for his first year as director (1994-95), I had only a minuscule budget of $0.3 million to turn over to Kochanek, although I was able to turn over to him talents and facilities. Since 1994-95, SCRR funding has increased steadily, due primarily to the grantsmanship of Kochanek and Safar. In the year 1997/98 the SCRR has produced research with a budget of about $3 million (the NIH TBI program project grant led by Marion and Kochanek, an Army TBI grant led by Kochanek and Safar, the NIH grants of Ed Dixon and Larry Jenkins, and the Navy grant led by Safar and Tisherman). Since 1998, my research program on severe hemorrhage (HS and SA) has worked with a DOD grant of $1-2 million per year. For the distant future, the SCRR needs an endowed professorship (a gift of $1.5 million total, to secure the academic time of the SCRR director), plus an endowment of at least $3 million to provide a core budget in times of lacking grant support and enhancing grant support for the initiation of new programs, new ideas, and bright young investigators.

Personal Plans

I have continued my 80-hour work weeks. Devotion to the mission is required as I am a principal investigator of program IV and have to coach a backlog of more than 20 original papers by fellows, and have to respond to various demands for advice and consultation. I volunteered also to help with the recruitment of young physicians into the anesthesiology residency program. Additionally, I am available to spend contact hours with medical students. When my successors use me as a sounding board, it flatters the old man. I enjoy being surrounded daily by bright young people from whom I learn. In the year 2000, I am devoting some time to semiprofessional travel and giving over one special invited speech per month. These talks include the 40th anniversary celebration of CPR at Johns Hopkins in September 2000; and the Wright memorial lecture at the ASA meeting in October. The history and future of resuscitation occupy my brain as I start working on the single-author book *Resuscitation Medicine in the 20th Century*, under contract with Springer. On the personal side, I take some time for our music evenings and with my grandchildren. In July 2000, much of 2 weeks was devoted celebrating in Pennsylvania our 50th wedding anniversary, family birthdays, and other events by a transatlantic family of 26 people.

Compared to European colleagues who must retire at age 65, I am fortunate as a nonclinical full-time professor. I have a larger office than I had as IRRC director, two laboratories, and two secretaries — Ms. Fran Mistrick has been with me since 1980, and Ms. Valerie Sabo since 1997. Without them I would be lost, since I have not yet found the time to rescue myself from a state of shameful computer illiteracy, although I took two computer courses. "If you don't use it, you lose it." Fortunate are old professors who have assistants like Fran Mistrick. She is not only loyal and efficient, but also organized, diplomatic, and computer-wise.

In the late 1990s, I have been responsible simultaneously for 4-6 laboratory technicians and the mentoring of 2-3 full-time and several part-time fellows. During the 20 years of work in our IRRC-SCRR facility, most of our 100 research fellows have been helped in their careers through resuscitation research. I consider this an even more important contribution by our center than the acquisition of new published knowledge.

State of the Department in 2000

Peter Winter decided in 1996 to give up the chairmanship of our Department of Anesthesiology and CCM. I suspect that the principal reason for his decision was that he did not want to reign over the dismantling of a special academic department of anesthesiology, developed over 35 years of hard labor by Peter Safar, Peter Winter, and their associates — because of what I call "mismanaged care for managed profit by nonprofessional middlemen." Liver transplant anesthesiologist Kang, educators Willenkin and Wilks, neuro-anesthesiologist Kofke (one of my former students), and others were major losses. Boucek continued arranging excellent grand rounds. I consider Winter's departure very unfortunate because his leadership skills would be needed now more than ever. At this time, Peter is professor emeritus.

Leonard Firestone succeeded Winter as Peter and Eva Safar Chairman of the Department of Anesthesiology and CCM at our medical school and medical center, starting in 1996. He has been facing enormous challenges through the often unreasonable demands from managed care, an off-and-on frustrated faculty resenting clinical overwork, requirements for billing, and an expanding UPMC. The essential openness and democratically shared governance of academic medical centers have become difficult.

The clinical workload of about 60,000 operations per year in hospitals in the 1970s has increased to about 120,000 operations per year in 16 locations now in 2000 C.E. This was due to the hospital administration's response to managed care with the acquisition of 10 "colonies." In response to this, Firestone succeeded in stabilizing the coverage, after initial exodus of a large number of anesthesiologists from the main hospital (PUH), to now a total of about 170 full-time faculty members, plus 75 physician trainees in anesthesiology plus adult and pediatric CCM. He had to increase the number of nurse anesthetists (which was about 75 in the 1970s) to about 200. The hospital administration for this enormous conglomerate has so far prevented "UPMC plus" from a financial collapse, as has happened in many other academic medical centers. We hope this is not entirely because of gains from investments. Finances for clinical departments are now entirely in the hands of hospital administrations. This makes upholding the three-legged stool of academe an enormous challenge.

I am delighted to learn that our faculty leaders, as coordinated by Len Firestone, have succeeded in not only maintaining almost all programs of our department developed since 1961, but having added new programs. The anesthesiology residency is filled. Research funding is greater than ever. Our department has become number one in the U.S. among departments of anesthesiology in the amount of federal grant money received; and number one within our medical center in the amount of funding per square foot of lab space. The research support, in addition to the $4-5 million per year for SCRR research (from NIH and DOD) includes eight NIH grants for the study of molecular pharmacology of anesthetic action; two large federal DARPA grants concerning the study of sepsis received by Mitchell Fink, our new Director of CCM (successor of Ake Grenvik); an NIH program project grant chaired by Thomas Rudy on pain rehabilitation (within the Pain Control Center chaired by Doris Cope); an adult CCM fellowship NIH research training grant under Pinsky; and a pediatric neuro-CCM fellowship NIH research training grant (for 2000-2004) under Kochanek. The department houses the North American malignant hyperthermia registry under anesthesiology Professor Barbara Brandom; and the clinical trials programs for the entire UPMC under the direction of anesthesiologist Professor David Watkins. Our department has a simulator training center initiated by Peter Winter, which is about to be expanded into an education research program in collaboration with Laerdal Medical. Our Pain Control Center chairperson Doris Cope is a trustee of the Wood Library-Museum of the ASA. Other faculty members are active in our state medical society. In summary, the department now seems to enjoy a collegial atmosphere, has gained leadership in many fields, and is looking forward toward a bright future.

The future also looks bright under the wise leadership of our new Dean and Vice Chancellor, Arthur Levine, for our medical schools of health sciences; and of our new Chancellor Mark Nordenberg, (former Dean of the Law School), for our great University at large. Although for clinical services, what I said in the 1970s "bigness is not necessarily goodness" is now more true than ever, our University-wide academic programs' increasing quality and depth are good indeed. Now, four decades since my start at Pitt when Chancellor Litchfield led our University's first renaissance, his vision for Pitt and UPMC to become the Harvard west of the Alleghenies seems to have come true.

THE WORLD

Introduction

My leading the life of a world citizen, our programs' worldwide associates and alumni, and my involvements in disaster medicine and "peace medicine" are inseparable. Contacts, demands, and perceived responsibilities worldwide, evolved spontaneously from my life as an academician. It would not have been possible via private clinical practice. A chairmanship of a clinical department gives a power base and freedom over one's time. Both are necessary for service beyond the local level. I have been away from my departments, on the average, for a total of about two months per year. That has included vacations, which were always mixed with worldwide teaching and networking. My professional travels would not have been possible without clinical services and teaching continued by associates, and without my wife looking after the children at home. With the help of trusted babysitters, she accompanied me on some professional trips. In the 1950s to 1970s we traveled to unusual places, including twice around the world, when such travels were rare.

Honors came as surprises. They have included many "corresponding (honorary) memberships" abroad in societies of anesthesiology, resuscitation, CCM, and emergency and disaster medicine; elected membership in the academies of sciences of Germany (the prestigious Leopoldina), Austria, Slovenia, Costa Rica, and Russia, and three honorary doctor degrees — at the Gutenberg University of Mainz, Germany (in 1972), the University of Campinas, Sao Paulo, Brazil (in 1996), and the Otto von Guericke University of Magdeburg, Germany (in 1997). The highest honor from Austria, the "Honor Cross First Class for Science and Art," also came as a surprise (in 1999). The Society of Critical Care Medicine (SCCM), U.S.-based but international in membership, awarded me the Distinguished Investigator Award, its highest honor. The American Heart Association awarded me the CPR Pioneer Award. In 1994, Peter Winter, chairman of the Department of Anesthesiology and Critical Care Medicine at Pittsburgh, announced at a faculty meeting that "Peter Safar of Pittsburgh and Vladimir Negovsky of Moscow were officially nominated three times for the Nobel Prize in medicine — in 1990 by the USSR Academy of Medical Sciences, in 1992 by Scandinavian professors of anesthesiology, and in 1994 by the University of Pittsburgh."

My interest in the world began in my boyhood when my family traveled and had international visitors, and I read books in German by Karl May about America, and in English by Kipling and J.F. Copperfield. Parents and friends told us about their travels in Egypt and Spain. Gymnasium teachers taught geography, history, and about ancient Greece and Rome. I loved to study maps. After World War II, as a young physician, first in Vienna and since 1949 in America, developing worldwide collegiality came naturally.

Already in 1951, during my residency with Dripps, Eva and I began traveling beyond the borders of the U.S. We drove on a vacation through eastern Canada, where I visited Griffith, the initiator of the use of curare in anesthesia (in Montreal), and watched Hudon at the Hotel Dieu (in Quebec) skillfully intubating children with blind naso-tracheal technique. In Ottawa, we confirmed the possibility of immigrating to Canada should the oversubscribed small Austrian immigration quota of the U.S. prevent our becoming U.S. citizens. At the end of my residency in 1952, when few people went to exotic lands, we ended up spending 14 months in Peru. Eva was initially homesick for Austria but rapidly became Americanized. I was not homesick and quickly became a world citizen.

Concerning medical meetings, I went to the majority of ASA and AUA meetings while I was a department chairman. Since 1979, as most-time researcher, I preferred small researchers' conferences, but remained loyal to the majority of SCCM symposia in the U.S. and the congresses of the World Association for Disaster and Emergency Medicine (WADEM, Club of Mainz), and World Federation of Societies of Anaesthesiologists (WFSA). I helped as advisor in the start of the International Trauma Anesthesia and Critical Care Society (ITACCS) and urged them to become multidisciplinary.

Starting in 1958, I made one or more visits each year to Europe. A lifelong trans-Atlantic and later also trans-Pacific commitment developed. Exciting nonmedical experiences often were mixed in with professional work. These often involved skiing, including the Mont Blanc glacier in the West Alps (several times, also with my son Philip), the Tasman glacier and Hochstaedter Dome in New Zealand, the Olympic slope in Sapporo, Japan (with my son Paul), most of the ski resorts of North America, part of the Haute Route in the Alps, and virgin slopes in the Canadian Rockies (helicopter skiing). I also rafted and hiked-climbed (scrambled) on several continents. I never had the courage like my son Philip to climb on vertical rock and ice. Eva and I flew low in small propeller planes over mountains and jungles in South America and Asia. I scuba-dived in warm and icy waters. The risk of asthma made me give up scuba diving. I climbed Austria's glacier peaks with Philip when he was age 17, and the Dolomites with Paul when he was 11. I was proud of my sons when they surpassed me. Philip became a climbing and skiing instructor in his early twenties and led a group on ice peaks of the Cordillera Blanca of Peru. Paul became a composer and a musical and poetic defender of nature. Now, in old age, Eva and I have begun giving travel experiences to our grandchildren, passing on the appreciation that we homines sapientes are part of nature.

Latin America

Our year in Peru in 1953 gave Eva and me some ability to communicate in Spanish and some feeling for Latin America's people, from the ultra-rich to the ultra-poor. They also needed anesthesia care with simplicity and safety, and effective resuscitation and life support. An average of about one visit to a Latin American country every two years during the past 40 years included the Caribbean Islands, Mexico (three times), Costa Rica, Panama, Columbia, Ecuador, Peru (twice), Bolivia, Brazil (six times), Argentina, and Chile.

When Eva and I first returned to Peru in 1971, we found my former department at the National Cancer Institute under the leadership of Orlando Bernal, with only slightly changed techniques over the past. In the aftermath of the 1970 earthquake, which killed 80,000 Peruvians, we participated in a disaster medicine meeting and drill to help prepare them with better traumatology. However, the authorities seemed more interested in new high-altitude-fit helicopters than in providing even basic life-support capability by ambulance services nationwide. Highlights were climbing Macchu Piccu for the second time (this time with my son Philip), drinking coca tea, and driving over the Altiplano to Puno and LaPaz, Bolivia (where anesthesiologist Castanos gave a superb impression). In Lima, the abrazzos with my former associates, colleagues, and friends were emotional. In the mountains, the natives' lives seemed less depressing than what we saw in the 1950s.

In 1983, at our second revisit, I lectured at the Latin American Anesthesiology Congress in Lima. A small earthquake interrupted a cocktail party, and a terrorist bombing of a bank nextdoor indicated "social changes" since 1953. Prehospital EMS still seemed elusive, while the technology in ORs seemed to approach that in the U.S., at least in the private hospitals. I was impressed by new academic interests at the second medical school, the Cayetano Heredia, which had recently been founded by internist and high-altitude physiologist Carlos Monge, Jr., with Guillermo Nieri as chairman of Anesthesiology. After that congress, Eva and I drove over the familiar 15,000 foot pass to Huaras, in the high valley of the Cordillera Blanca, to pay our respects to the 1970 earthquake victims of Yungay. We went to the cemetery hill, the only remnant of that town. We walked over the nearby field of what was an ice-rock-mud avalanche from the Huascaran, with haunting thoughts that under our feet lay the remains of over 20,000 people, most of them descendants of the Incas. We taped interviews with Dr. Victor Ramos, who was the former medical director of the hospital in nearby Huaras, anesthesiologist Masquera, and a government official, Mr. Ames, concerning their experiences during that earthquake 13 years ago. These interviews,[271] in addition to those I conducted regarding the 1980 earthquake in Italy,[272] led to the first quantifying studies of dying and resuscitation potentials in major earthquakes that our team made in Armenia (see Pittsburgh, IRRC).

Despite my six visiting professorships there, Brazil failed to teach me Portuguese, because my hosts spoke English and visits were brief. I was hosted by former Brazilian Leonardo Rosenfeld (one of our Pittsburgh anesthesia residents in 1975), Brenildo Tavares (internist-intensivist, in 1978 and 1987), and Carlos Parsloe (former president of the WFSA, at the anesthesiology congress in Porto Allegre in 1991). Throughout, our main Brazilian host and friend (with generosity and imagination) has been John Lane, renowned professor of vascular surgery at Campinas and resuscitation education pioneer of Brazil. Lane is a remarkable man. In the 1970s, Lane wrote the first Portuguese-language manuals and textbooks on first aid and resuscitation. He and his wife, both with U.S. heritage, have dedicated themselves lifelong to helping the people of Brazil with a missionary spirit.

In 1978, Eva and I and 9-year-old Paul toured the entire country of Brazil, including the coast, the huge flood plains of the Rio Grande, the beautiful capital, Brazilia, and the Amazon region including Manaos with its 19th-century opera house. I was surprised by the prevalence of asthma in the Amazon region. Visits to hospitals, seminars, and conversations suggested that the modernization of OR technology in the rich patients' private hospitals was only a few years behind that in the U.S., while charity hospitals lagged far behind, and EMS systems still do not exist. Both rich and poor have little chance, even in big cities, of surviving a sudden cardiac death or severe polytrauma with head injury. Antonio Capone from Brazil, a young trauma surgeon and intensivist (and John Lane's son-in-law) was one of the most productive research fellows with us in Pittsburgh in the early 1990s.[261,268] His research with us led to a Ph.D. Out of Chile, Eva and I and the Lanes went to the Magellan straight (so far the closest we have been to the South Pole).

Indochina and Around the World

Early in 1959, my department at BCH was well staffed; steps A and B of what later became CPR were documented; and I had turned over to Redding resuscitation laboratory research. I was ready for an outside challenge. Baltimore internist Berman was a leader with MEDICO (Medical International Cooperation), a nongovernmental organization that selected physicians and nurses to help developing countries with demonstration teaching there. Berman asked me to go on a medical mission to South Vietnam. The communist armies had defeated French colonial troops at Dien Bien Phu, and Vietnam was split. The North became a client of the Soviet Union, and the South a client of the U.S. At that time, the Soviets brought an open heart surgery team to neutral Cambodia next door. With the one-upmanship typical of the cold war, our government responded by sending a thoracic surgery team (us) to South Vietnam, where tuberculosis was rampant. Our government's International Cooperation Administration (ICA) asked MEDICO to provide the team. Having first sent chest surgeon Overholt of Boston and anesthesiologist Sadove of Chicago to Saigon, they now mobilized chest surgeon Jim Littlefield of Charlottesville and anesthesiologist Peter Safar of Baltimore to succeed them, with some overlap in time. Two nurses from the U.S. were already there. I volunteered for more than two months, leaving on April 28, 1959, and returning on July 5. I asked Eva to come along. She had already planned to take our asthmatic daughter, Elizabeth, to a sanatorium in Davos, Switzerland, for a year. Flying eastward, Eva met me in Saigon, as I went around the world westward. Since we were to receive our American citizenship papers in August 1959, we ended up conducting this adventure as American medical missionaries with Austrian passports. Our teams were chaperoned in Asia by what turned out to be members of the Central Intelligence Agency (CIA). It was easy for them to recognize that we were not spies. They were polite and helpful in connecting us with the right natives in order for us to learn and do good. Another agency in the act of chaperoning us was the U.S. Operation Mission (USOM).

Littlefield and I flew across the Pacific to Tokyo on propeller planes of the newly established Japanese Airlines (supported by the U.S. government), pampered by geisha-style stewardesses. We found Tokyo still considerably damaged by World War II. Our local host was anesthesiologist Fugiko Mohri. From there, we flew via Hong Kong into Saigon, where we were received by our ICA (not CIA) contact, public health specialist and internist Plagge. Among local physicians, surgeon De was most helpful and influential. An outdoor reception dinner given by the hospital was a culture shock. The food included the spiced blood, eyes, and hairy skin strips of animals — all considered delicacies. Not accustomed to this, I could not eat it; saving face was tricky.

I quickly learned that this country with a population of about 13 million had only 110 trained physicians and not a single anesthesiologist. Local anesthesia was given by surgeons; general anesthesia, by nurses trained in France. Our team was put up in the very center of Saigon, in the Hotel Continental, a remnant of French colonial times. Meals, served on the veranda, were spiced with the same Tschaikowsky piano concerto #1 everyday.

We went out to multiple hospitals but finally settled on doing most of our work at the Cho Ray (city, charity) University

Hospital in the suburb of Cholon. Equipment and supplies were a disaster, namely, nonexistent. Even getting a needle larger than gauge 20 for intravenous infusions was impossible. There were basic anesthesia machines, uncuffed tracheal tubes, old laryngoscopes, no ventilators, and only breathing bags for manual ventilation. The main mission of this trip became dubious when doing resective lung surgery on patients with active tuberculosis who had medicated themselves into resistance to antibiotics obtained on the black market. Overholt and Sadove had begun with lung surgery, but Littlefield and I, after a few thoracotomies, decided instead to demonstrate modern perioperative care for truly urgent life-threatening conditions such as incarcerated hernias, large goiters, and trauma.

The local doctors, nurses, and common people were delicate and beautiful. I watched the habits of the anesthesia nurses, then tried to modify their methods slightly, for safety. Ms. Li Thi Thuan was the chief nurse anesthetist at Cho Ray—I was told the leading one in South Vietnam. After our return to the U.S., Plagge and U.S. government agencies helped me get Ms. Thuan to Baltimore. She worked there with us for one year like our anesthesia residents. We also taught her regional anesthesia techniques. She returned to Saigon and trained over 200 nurse anesthetists there throughout the Vietnam War. The first Vietnamese physician anesthesiologists who emerged there later came about through contact with the U.S. Armed Forces. In 1975, when the North Vietnamese Army approached Saigon, I sent Ms. Thuan a letter advising her not to run away. I knew from World War II that a civilian's chance of survival during battle is greater when one stays where one is known.

In 1959, we visited and lectured in other places throughout South Vietnam, including the Bien Hoa refugee camp. There, via interpreter, we talked with Catholic refugees from communist North Vietnam. We visited Banmethuat (in Montagnard country), Natrang, and the imperial city of Hue. Driving through the jungle on dirt roads was threatened by sporadic communist guerilla attacks, which caused civilian casualties. We had no idea that a few years later the beautiful city of Hue would be in rubble. Except for medical efforts, we and our hosts were replaying the story of Lederer's book *The Ugly American*. The title is a misnomer, since the hero was good and not ugly.

While working with patients, medical personnel, and politicians in Saigon, we got to know Madam Ngai. She ran a famous orphanage with the help of young American ex-Navy medical officer Tom Dooley. Before leaving Southeast Asia during the second half of June 1959, I arranged for Eva and me to fly first to Phnom Penh, the capital of Cambodia (which 15 years later became the killing fields), and via Laos, Thailand, India, and Europe westward, to return to Baltimore. French colonialism had a mixed impact on Phnom Penh. There was a nice but poorly supplied hospital. The new Soviet heart surgery team had arrived in an environment where basic medicine and surgery had not yet caught on. We then flew in a small propeller plane (piloted by a French beer-drinking bush pilot) via Vientiane to the jungle village of Muong Sing on the border between Laos and Red China, to visit Dooley's jungle hospital. The village Muong Sing, like others, could be reached only by air (landing on grass) or by riverboat. We flew in, low enough to count elephants and trees. For longer distances, the most reliable plane in that area then was the old DC 3 of World War II.

Tom Dooley, just after his service as a young medical officer in the U.S. Navy, was a committed Catholic medical missionary. Out of his "hospital" in Muong Sing he distributed antibiotics in the countryside, like manna raining from Heaven. He performed minor surgery under local anesthesia, and whatever else was feasible in that setting, all mixed with propaganda to win the souls of the natives for Christianity and the U.S. He was aided by two excellent American ex-military medics. He was an effective ambassador of good will, recognized by the media. When Dooley had to fly to Bangkok for a personal medical check-up, I was "in charge" of his hospital for a few days. I tried to introduce a combination of local-regional anesthesia reinforced with morphine, titrated intravenously. I still recommend this approach as very safe in primitive conditions, even for major surgery. The village was in a part of the world with a centuries-old trade of opium on the open market. When Dooley returned, he gave me the bad news: a melanoblastoma had been found in the skin of his chest. Two months later, back in Baltimore, I got a call from him in New York: Dooley announced that he was dying from metastases. His death followed soon, despite heroic operations at the Cancer Memorial Hospital in New York. He was a remarkable medical missionary who was prevented from becoming a Schweitzer of Southeast Asia by the cold war rivalries, and his early, tragic end.

On June 21, 1959, Eva and I began our return journey from Muong Sing to Bangkok, and then, for the first time on a jet plane, to New Delhi (and Agra), and on to Kashmir for two days, and a brief trip to see (not climb) the Himalayas. Then, by propeller plane on to Hyderabad, India, where we were hosted by our former Baltimore visitor, anesthesiologist Niren Das. They put us up in an Indian guest house. In Hyderabad, at the Ismania University Hospital, anesthesiologist Venkata Rao was chief, and internist K.N. Rao was dean. (His wife was the daughter of India's past president Rhadakrishna.) K.N. Rao, a very cultured man, later became health minister of India, in Delhi. He soon visited us in our simple little house in Baltimore, to continue our philosophic discussions of 1959. After Hyderabad, Eva and I went on to Bombay, Karachi (Pakistan), Istanbul (Turkey), Vienna (meeting with Mayrhofer), Paris, and home. In the 1980s, Das sent his daughter to be chaperoned by us in the U.S., and sent me his original thoughts on death and afterlife, just before he passed away in 1996.

The political spin-off of our trip turned out to be disappointing. Upon debriefing by the ICA (not CIA) in Washington later in 1959, I expressed the following opinion: The political situation in Southeast Asia cannot and should not be solved militarily. Our government should treat Ho Chi Minh as a friendly, nationalistic, socialist leader, as we were treating Tito of Yugoslavia. Let him take over Indochina. Third-world countries need some form of humanitarian socialist democracy, with a changeable but benevolent dictatorship, before a U.S.-style democracy is feasible. I regret that I did not record these suggestions. In the 1960s, under President Johnson, and for five more years under President Nixon, our country's policy in Southeast Asia was a murderous arrogance of power. Two administrations tried to save face while killing more than two million human beings, including 50,000 Americans, and mutilating many more. Our "defense" of South Vietnam was unwise and immoral. Throughout the cold war I recommended, in correspondence with the U.S. government and others, the use of education, humanitarian aid, and open nonmilitary competition, to stem the spread of communist dictatorships.

The Tet Offensive in South Vietnam in January 1968 was followed in March by President Johnson ending his candidacy for a second term, which was followed by the murders of Martin Luther King in April and Robert Kennedy in June. These assassinations, and the murder of JFK in 1963, cloud the democratic principles that Thomas Jefferson gave to the world. During the Democratic National Convention in August 1968, with its antiwar demonstrations, we were in London at the WFSA Congress. Eva and I helped Czech colleagues, who had escaped the Soviet takeover of Prague that occurred the same week, to get settled in the West. Also in 1968, in the U.S., during the "long hot summer" of riots, our Freedom House ambulance program of Pittsburgh's black community helped to prevent casualties. Peace demonstrations all over the U.S. expressed frustration and anger over the continued killing in Vietnam until 1975. Visiting the Vietnam Memorial in Washington is a stirring experience, and not only for those who were there during the war.

My Japan connection, mostly with anesthesiologists, began with Morikawa's superb work at the BCH, my 1959 trip via

Japan, and Takaori's fine research in the 1960s at Pitt. There followed residents and research fellows in the 1980s and 1990s, such as Kuboyama, Tanigawa, Oku, Kawabata, Kai, Watoh, and recent shock hypothermia researcher Takasu. He was preceded by Sim from Singapore and surgeon Kim from Korea. We had six research fellows from China, culminating in Xiao and Wu.

Down Under and Around the World

In 1960 I received an invitation from surgeon Coppleson of Sydney, Australia to participate as keynote speaker in the World Congress on First Aid, to be held in Sydney on March 14-19, 1960. I accepted and left on March 6 for another around-the-world tour of six weeks, accompanied by my good friend Walter Dandy, Jr., anesthesiologist of Baltimore's Union Memorial Hospital.

We plotted an unforgettable journey, which Walter Dandy later documented with a slide talk. Although Walter Dandy did not present formal lectures on that trip, he was an excellent discussant and sounding board. Whereas my previous year's trip around the world was mostly by propeller planes, much of this 1960 trip was by jet. We started on a flight from San Francisco via Hawaii and Fiji to Auckland, New Zealand, on the new Australian airline Quantas. Walter and I crossed the Pacific as the only passengers (with a full crew of pretty stewardesses) on a four-engine B-707. In Auckland we were hosted by anesthesiologist Matthew Spence, who had recently initiated there a medical/surgical ICU and the beginning of mobile ICU ambulances.

In Sydney, internist Blackburn shared the Congress chairmanship with Coppleson, while anesthesiologists Shea and Bedford of Sydney and Crankshaw of Melbourne were also prominent. The highly regimented lifesaving associations of New Zealand and Australia, particularly one of their leaders, Mr. A. Denny (who paid us many visits later), were interested predominantly in resuscitation of drowning victims. The First Aid Congress was dominated by the Baltimore data on steps A, B, and C and our BCH data on drowning experiments in dogs. I had received the permission of Kouwenhoven to present, in his name, his data on step C, external cardiac massage, which were about to be published in JAMA a few months later. This may have been the first presentation of steps A, B, and C together, of modern CPR basic life support (BLS).

Local first-aid instructors gave us an opportunity to figure out (while swimming in the surf and deep water) how we might initiate rescue breathing before reaching shore, even while treading water; blowing into the snorkel-like nose proved to be easier, while blowing into the mouth was easier on land. We were surprised and impressed that the water-rescue teachers and leaders quickly gave up their lifelong rituals of back-pressure arm-lift resuscitation attempts and adopted mouth-to-mouth without delay. The Australians' bluntness, openness, and frontier spirit were refreshing. Lasting collegiality was initiated.

Dandy and I then flew on to give lectures or seminars in other places. First was our maiden flight on the new British jet plane Comet III (which took off like a fighter), via Darwin to Djakarta, Indonesia (anesthesiology chairman Untung), Singapore (with its British-style anesthesia in the world's busiest maternity hospital), Ceylon (now called Sri Lanka), to Cairo. At the airport of Darwin, Australia, I forgot, but recovered, my diary, which I have kept since 1949, as an important reminder of my professional appointments. Unfortunately, it contains very few impressions. In Cairo we met anesthesiologists Aboul-Eish and Boutros; both later joined us in Pittsburgh. We were hosted by the widow of Dr. Hess, Swiss-Egyptian friends of my parents. We went on to Beirut, Lebanon, which was then the Switzerland of the Middle East, hosted by anesthesiologist Brandstatter (who later became an American). In Athens, Dandy and I interacted with Dripps-trained anesthesiologist Couremenos, and met the Safars of Vienna (on vacation there).

On both trips around the world, we were impressed by the relatively advanced practices of physician anesthesia in areas influenced by British medical education, contrasted by horrible backwardness and lack of safety in many other places.

Western Europe

Although the anesthesia knowledge, skills, and methods I used for 40 years were influenced more by American than by British schools, I did learn from visits to the U.K. and its former colonies. Several partly professional and partly tourism-oriented visits to the U.K. had begun in the 1960s. They led to good collegial relationships with anesthesiologists, particularly Wollmer of the Royal College of Surgeons, Macintosh at Oxford, Sykes at Hammerschmidt, Nunn in London, and others in the key hospitals of London where physician anesthesia was born. The countries with the greatest number of Pittsburgh alumni have been Scandinavian countries, Belgium, Israel, and Japan — each having benefited from between six and 12 colleagues who trained as anesthesiologists or intensivists in Pittsburgh and who then became leaders in their countries. Israeli, Belgian, Norwegian, and Swedish colleagues were among the first to participate in our NIH-supported BRCT.

Our Scandinavia connection began in 1958 with the Scandinavian Anesthesiology Congress in Gausdal, Norway. That initiated our lifelong relationship with Asmund Laerdal and his son Tore of Stavanger, Norway. My Pittsburgh collaborators (fellows, staff) from Scandinavia included Grenvik of Sweden, who joined us in 1968 and remained with us; Gisvold, who became chairman in Tondheim; Breivik, who became chairman in Oslo; Lind, Vaagenes, and Hoel, who became chiefs at Oslo's Akershus Hospital; Edgren, and Hedstrand of Uppsala (BRCT participants); Rubertsson of Uppsala; Aass of Norway; and Nikki of Finland. Visitors with us included biochemist Ernster, neuroscientist Siesjo, and anesthesiologists Ghord, Holmdahl, Wiklund, Savolainen and Airaksinen. We visited also pioneers Ghord, Norlander, Carlens, Nielsen, Ybsen, Secher, and others.

In the 1990s, I was invited to the 50-year celebration of the Laerdal Medical Corporation in Stavanger, and gave the first Asmund Laerdal Memorial Lecture at the Scandinavian Anesthesiology Congress in Trondheim. Out of Trondheim we went to the land nearest to the North Pole – Svalbard (Spitzbergen). I gave a Holmdahl Lecture at the Swedish Medical Society meeting in Stockholm. Almost every two years I participated in a special conference in Scandinavia. The first international guidelines meetings were those on CPR steps A and B in 1961 in Stavanger,[117] and on those on steps A, B, and C (as well as brain death) in 1967 in Oslo,[118] both sponsored and financed by Asmund Laerdal, and strongly influenced by Pittsburgh. I learned early to appreciate the catalyzing impact of the improvised dinner speeches and toasts so popular with the Scandinavians. Almost all Scandinavians that Eva and I have had the opportunity to know more than superficially, have been role models of honest, cultured, gentle, hard-working world citizens.

Our Belgium connection began in 1960 with our CCM fellow Jean Pennickx and our anesthesia chief resident Arsene Mullie. In Brugge, they introduced the world's first EMS system in which the same group of anesthesiologists controlled the continuum of care from the scene, via the ED, through the OR and ICU. The Belgian connection climaxed in the 1990s with my long visiting professorship in Brussels, hosted by CCM internists Huygens, Corne, and Idrissi and anesthesiologist Delooz, who pioneered EMS there. Huygens created a Safar fellowship. In the 1980s and 1990s our full-time research fellows included Sterz, Weinrauch and Behringer from Vienna, Ebmeyer from Magdeburg, Prueckner from Munich, and Kentner from Mainz.

Our Israel connection began in 1968, when anesthesiologist Pauline Lieberman sought a fellowship in CCM with us. She became a fine life-supporting physician for adults and children at Pitt and later became the first intensivist in Israel, as the initiating ICU director of the Tel Hashomer Hospital. Those who followed her included internists Lewinson and Leonov; pediatricians Goitein, Barzilay, Eshel, Bar-Joseph, and Barr; and neurosurgeon Pomeranz. The Israeli connection included Nancy Caroline, an American citizen who moved from Pittsburgh to Israel to improve EMS to ALS capability. Israeli visiting professors included Uri Frand (initiating EMS leader, killed in the Yom Kippur War), Jakov Adler (disaster medicine leader), and Shamay Cotev (pioneering anesthesiologist at the Hadassah Medical School). In 1978, I visited Israel for the first time; at the Anesthesiology Congress in Haifa, Mark Rogers, then the first successful chairman of anesthesiology at Johns Hopkins, remarked that I influenced him to become "international." Nagel and I were then hosted by Nancy Caroline for a tour through Israel including visits of the Magen David Adom (Israel's Red Cross) ambulance services for which she was medical director. During that visit in 1978, most memorable was the disaster medicine meeting in Safad, northern Israel. During that meeting we learned about the Camp David peace agreement between Israel and Egypt, mediated by U.S. President Carter. We all danced in the streets. Eva and I revisited Israel in 1984 and took a bus from Jerusalem to Cairo to "test" the peace agreement.

Italy, a beloved country from my childhood travels and my many professional visits since 1958, gave us collegiality with anesthesiologists Manni and Magalini of Rome, Novelli of Florence (and a unique conference in Erice, Sicily), IRRC fellow Cerchiari of Milano, BRCT co-investigator Bozza-Marrubini of Milano, and Gullo's APICE symposia in Trieste. Yugoslavia we visited three times after World War II.

Eastern Europe

Throughout the 1950s, our European travels were west of the Iron Curtain. In 1962, although overwhelmed with department-building at Pitt, I shared with many Americans hopes that Kennedy and Krushchev would defuse the standoff and the fears caused by the Cuban missile crisis in October. Ever since our Indochina experience, I felt that the East-West confrontation with the threat of nuclear weapons must and can be ended by peaceful competition between dictatorial communist and democratic capitalist countries. After World War II, Western and Northern Europe merged the good from both sides in socialized democracies, seeking "free enterprise with social conscience." This was accomplished under the protection of U.S. Armed Forces. How could I help in this direction? Maybe by bringing together physician scientists from both sides of the Iron Curtain.

In 1962, Otto Mayrhofer planned the first European Anesthesiology Congress in Vienna. He asked me to chair a panel on "controversial aspects of resuscitation." In a telephone conversation, Mayrhofer's associate Karl Steinbereithner and I agreed that Vladimir Negovsky of Moscow (fig. 13) should be invited to be on this panel.[140] By then I had glanced at only one English translation of Negovsky's work.[141] That historic panel discussion was held on September 5, 1962. Negovsky came with political chaperones. Anesthesiologists Hugo Keszler and Jiri Pokorny, of then-communist Czechoslovakia (the CSSR), came unchaperoned; they disagreed with Negovsky collegially on some scientific issue. My parents invited the panelists for lunch at their Viennese apartment. The panel discussion was published as the first volume of Anesthesiology and Resuscitation, the first of a long series of monographs published by Springer Verlag.[140] That Vienna congress, thanks to Mayrhofer, led to lifelong friendships among many anesthesiologists from East and West. Before Keszler and Negovsky left Vienna, they invited me to visit them in 1963.

The CSSR

In September 1963, after my last visit with my very ill father in Vienna (he died on November 22/23, 1963, when Kennedy was murdered), I visited Prague and Moscow, accompanied by hypothermia pioneer and Pitt neurosurgeon Hubert Rosomoff.[322,323] During our visit to Prague, we found wonderful people in a drab, poor atmosphere. Keszler, Pokorny, and Klain hosted us. (Klain, Keszler, Pautler, and Maivald joined Pitt's anesthesia faculty after the "Prague spring" which was followed by the Soviet occupation of the CSSR in 1968). In September 1963, doctors of the Czech Red Army greeted us with courtesy, because my CPR documentary film, which had been distributed to some colleagues abroad, was made by the U.S. Army. In Prague we also met Peleska, a physician and electrophysiologist. In 1962, he had developed the first portable battery-powered capacitance discharge DC defibrillator for use in ambulances. Peleska later visited us in Pittsburgh. Concerning resuscitation, our visits resulted in exchange programs with Prague. Keszler's first assistant, Stanislav Pautler, spent the year 1965 as research fellow in Pittsburgh.

The USSR

During our visit to Moscow in 1963, I met Guy Knickerbocker of Baltimore, the electrical engineer who had re-discovered external heart massage in Kouwenhoven's lab at Johns Hopkins in 1958.[67] Knickerbocker was then a visiting research fellow with Negovsky's associate Naum Gurvich, who had pioneered DC defibrillation in animals in the 1940s. In the U.S. during the early 1960s, Bernard Lown introduced these defibrillator developments of Gurvich and Peleska, with advanced technology, also for "cardioversion" of patients without cardiac arrest. Knickerbocker and I roamed the streets of Moscow, unchaperoned by Intourist, the ever-present political "hosts," who made sure that outsiders saw only what the government wanted them to see. I developed a feeling of friendship not only toward my medical colleagues, but also toward the Russian people in general. Other contacts made during our September 1963 visit to Moscow included professors of surgery Vishnevsky and Petrovsky (the health minister of the USSR whose sustained part-time work in surgery impressed me). Negovsky's associates included Alexander Gurvitch (neuropathophysiologist), Victor Tabak (cardiologist), Eugenia

Zolotokrylina (clinical pathophysiologist), Elena Damir (professor of postgraduate education in anesthesiology), Vladimir Kassil (anesthesiologist/intensivist), Tigran Darbinyan (president of the USSR Scientific Society of Anesthesiologists and Reanimatologists), Armen Bunatyan (chief anesthesiologist of the USSR Public Health Service), and George Andrejev (professor of anesthesiology at the University of Riga, Latvia USSR). Andrejev hosted the USSR Congress of Anesthesiologists in Riga in 1983, to which I was an invited speaker. Long train rides between Moscow and Riga provided opportunities to fraternize, philosophize, and drink much cognac, chased by vodka.

Vladimir Negovsky, a pathophysiologist, was ahead of us[324,325] (fig. 13). In 1937, he had initiated a resuscitation research laboratory in the neurosurgical department of a large Moscow hospital. He and his associates documented a method of centripetal intra-arterial blood pumping, suggested by Russian physician Andreev in 1913. Negovsky used this method, combined with artificial ventilation and epinephrine, to restore vital functions in dogs after prolonged exsanguination cardiac arrest, without the need for heart massage. During the turning point of World War II, in the winter of 1941-42, working at the front line near Moscow, Negovsky's teams successfully employed the methods of centripetal intra-arterial pumping of blood and mechanical ventilation to resuscitate some wounded soldiers from terminal states or clinical death. Soon thereafter, Negovsky coined the term "reanimatology." He conceived and described the post-resuscitation disease, part of which was later recognized as "re-oxygenation injury."[232] He investigated protective hypothermia and the pathology of brain ischemia earlier than we and others did in the West. We, on the other hand, are credited for having first documented the modern first-aid, resuscitation and intensive care life-support methods, and for having assembled them into an effective system of CPCR (fig. 12). We influenced Negovsky to become more treatment-oriented, while he influenced me to also study pathophysiologic mechanisms.

Starting with our visit to Moscow in 1963, we exchanged abstracts and reprints. There has been continuous scientific cross-fertilization, fueled by yearly meetings at medical congresses in neutral countries, my subsequent visits to the USSR in 1973, 1983, 1986, and 1990 (and of my associates, in addition, in 1978 and 1989), and Negovsky's visits to Pittsburgh in 1976 (the tenth and last of our emergency and critical care medicine symposia), in 1981 (the Second World Congress of Emergency and Disaster Medicine of the Club of Mainz), in 1983, and in 1987. During the cold war, it was easier for Westerners to visit communist countries than vice versa. Some of my visits had romantic moments, like walking alone with Alexander Gurvitch or with an attractive female interpreter, without chaperon, on the snow-covered empty streets of downtown Moscow at night, speaking freely and without fear about the world and politics. While our Soviet colleagues impressed us as honest and trustworthy, I suspected that my hotel rooms had been bugged by the authorities.

In the memoirs of our trip to Prague and Moscow in 1963, prepared a few months after our return, Rosomoff and I said in the introduction: "We failed to obtain all-inclusive data on resuscitation and emergency care in Czechoslovakia and the Soviet Union. Nevertheless, the information and impressions gathered during this very concentrated tour may help in the evaluation of the status of resuscitation and other medical fields in these countries. We also feel that we can make recommendations concerning the exchange of scientists and clinicians interested in resuscitation, anesthesia, and neurosurgery."

Our Moscow/Prague memoirs include much humor, some related to the liberal narcotization of visitors with vodka and cognac. The memoirs were not published, however, because some of our comments about the communist system and their customs might have offended some of our hosts. Our memoirs ended with: "Getting into the country in Moscow was easy; leaving it, via Kiev to Vienna, was quite difficult, with impressions of suspicion and inefficiency. The strict political system of past years — undoubtedly undergoing liberalization — together with the cordiality and eagerness of the people we met, should encourage closer contact with Russian colleagues. The experimental work appears to suffer from lack of stringent controls. This is, in part, compensated by an imaginative and stimulating approach." Concerning anesthesiology in the USSR, we concluded in 1963: "The Russians are committed to anesthesiology as an applied science and a public health issue. They have decided that there should be one anesthesiologist for every 100 hospital beds throughout the country and are therefore fully supporting training programs. Although anesthesiology is further developed in the U.S. than in Russia at the present time, coverage by anesthesiologists in the U.S. varies tremendously among different areas and has not been surveyed. Also, criteria for optimal coverage have not been established. This should be done because of the great need for anesthesiologists in the U.S. and their potential role in resuscitation and allied fields."

At about the time of our visit with Negovsky in 1963, U.S. Senator Hubert Humphrey was also in Moscow. He subsequently recommended to our government to emulate the USSR's support of scientific research into the reversibility of acute unexpected dying. I supported this with recommendations to our government since the 1960s to create several reanimatology research centers in the U.S. This has not happened.

Although some may challenge Negovsky's thoughts about a separate specialty of resuscitation, his ideas have drawn attention to and support for this field in Russia. In the 1980s, when we were fighting for year-by-year funding of our IRRC, I envied Negovsky for his guaranteed government support of more than 50 salaries. Although meager, this salary guarantee allowed long-range planning. Unlike us, Negovsky was not plagued by having to drain enormous time and energy into (often futile) grant applications.

Among the spin-offs of the Moscow connection have been friendships with the distinguished American visitors to the

Negovsky Institute who followed me, namely Hugh Stevenson, professor emeritus of surgery at the University of Missouri, a resuscitation historian and pioneer for open-chest CPR, and participant in our BRCT project, and Robert White, professor and chairman of neurosurgery at Case Western Reserve University in Cleveland (a pioneer of brain protection and spinal cord resuscitation with hypothermia). We three made some visits together. Once I visited Moscow together with the late Ole Norlander, pioneering intensivist of Sweden. I made several visits with my Pitt associate and friend Miroslav Klain.

In 1986, anesthesiologist Victor Semenov became Negovsky's successor as director of Moscow's Reanimatology Institute. Semenov hosted me in Moscow in 1986, at the fiftieth anniversary of the Laboratory of General Reanimatology of the USSR Academy of Medial Sciences, the new name of Negovsky's laboratory.[326] It was moved from its original location near Red Square to the outskirts of Moscow. In 1990, still under Gorbachev, when Eva accompanied me to Leningrad and Moscow, their society was open, fearless, and safe. Since 1991, under "free capitalism," greed, poverty, street crime, and organized crime reportedly took over the country. Support for the Institute declined. In 1995, anesthesiologist Victor Moroz became director of the Negovsky Institute. In 1999 Klain and I returned to Moscow to honor the Institute and Negovsky on his 90th birthday. I am not sure that he recognized us. When Moroz, his impressive successor, showed us the new Institute, it was obvious that the torch had been passed on. We discussed possible collaboration in the future.

I credit Negovsky with having introduced definitions concerning the pathophysiology of acute dying. I reviewed and promoted some of Negovsky's publications in the West, both papers and books.[232] We communicated on philosophical and ethical issues. Our University of Pittsburgh presented Negovsky with a "special certificate of recognition." This was in lieu of an honorary doctor's degree, which Pitt gives only to the yearly commencement speaker. At the international level, we joined efforts to insist that resuscitation medicine include intensive (critical) care, and, since 1981, disaster reanimatology and "peace medicine." We worked for glasnost and the democratization of the USSR before these became official. My psychologist Eva wondered whether my friendship with Negovsky and my urge to help make peace were subconsciously influenced by gratitude to the Russians for having brought us peace when they conquered Vienna in April 1945.

During the cold war, anesthesiology and intensive care techniques, equipment, and supplies in Eastern Europe were about a decade behind those in the West, but their EMS systems and ALS ambulance services, at least in Moscow, were a decade ahead of us. In Prague and Moscow, physician-staffed ambulances were delivering intubation, defibrillation, medication, and infusion on the streets already in 1963, before the famous mobile cardiac care unit program in Belfast. We in Pittsburgh were ahead of Eastern Europe in education research, in programs for modern resuscitation by laypersons and nonphysician ambulance personnel, and in guidelines.

Negovsky's laboratory pursued many original ideas, but equipment-wise, his lab lagged behind the West. Until 1990, the resuscitation research centers in Moscow and Pittsburgh, were considered to be the only ones in the world with a global, multilevel, multidisciplinary approach. While free Europe and America had acquired an increasingly scientific, orderly approach to medical science and education, our Soviet colleagues, who since World War II had many imaginative new ideas, had trouble communicating with the West. This was mainly because some Soviet scientists presented data to support theories, without detailing how they had obtained the data. When they were asked for specific details, however, they provided honest answers. They just had a different way of thinking and publishing.

Hungary

Besides the USSR and Czechoslovakia (the CSSR), we visited other communist countries, such as Yugoslavia, Hungary, Poland, and East Germany. In Hungary, my main contact was professor of physiology at the Semmelweis University in Budapest, Aritzid Kovacs. In 1976 he was visiting professor in Pittsburgh. An artist in dealing with the communist regime, Kovacs somehow arranged for having had another position throughout the cold war at my alma mater, the University of Pennsylvania, with famous neurobiologist Britton Chance. Kovach and his Hungarian fellows worked in Philadelphia and Budapest simultaneously. I visited Hungary twice during the cold war, once before and once during Gorbachev's glasnost.

Poland

I visited Poland for the first time in September 1980, during the week of the Solidarity uprising. This peaceful revolt initiated a decade of pressure on the Soviet government, which ultimately achieved liberalization, democracies, and freedom from the USSR of most of the East European countries. In September 1980 I was hosted by anesthesiologists Sych of Krakow, Jurczyk of Poznan, and Kaminski of Warsaw. From Krakow I was treated to a helicopter sightseeing trip over and into the Tatra Mountains. In all three cities, talking about the German invasion of 1939 — followed by SS-occupation and retreat — revived horrifying memories. Visiting the area in Warsaw where once upon a time a thriving Jewish community existed, I mourned at the memorial monument which is the only reminder of the once-huge Warsaw ghetto. In that location there are now only new bland apartment buildings. Otherwise, the rebuilding of the centers of these East European cities was amazingly positive, in spite of the communist governments' nonacceptance of financial help offered by the West.

In Warsaw, Bogdan Kaminski became one of our seven European BRCT investigators for the NIH-supported 15-year study out of Pittsburgh. Although handicapped by the aftereffects of polio, Kaminski led an impressive life as a leader and teacher. Later I revisited Poland twice, once for a meeting in Lodz, and once for the European Anesthesia Congress in Warsaw. In September 1980, my hosts in Poznan drove me westward into East Germany to a historic symposium in Potsdam (near East Berlin), organized by anesthesiologist Manfred Meyer. He and his wife became our friends and were visitors with us on several occasions.

East Germany

The September 1980 symposium in Potsdam, under Manfred Meyer, was a gathering of many giants from East and West, including Severinghaus, Weil, and Negovsky. We debated anesthesia-related science right next to the Berlin Wall. Lodging was provided in the Cecilienhof where Winston Churchill stayed during the decision-making meeting about the future of Germany, right after the end of World War II. It was a historic meeting in a historic location. That Potsdam meeting of Meyer was followed immediately by the WFSA Congress in Hamburg. Lothar Barth and Manfred Meyer, the German pioneers of anesthesiology based in East Berlin right after World War II, became our friends. Barth escaped to the West, married anesthesiologist Mildred. Both practiced and taught in Bremen.

Our connections with East German colleagues went beyond anesthesiology. Anticommunist East German surgery pioneer Lembke (whose daughter is anesthesiologist Maria Lembke-Kampschulte of Munich and Pittsburgh) recommended to the internationally prestigious Academy of Natural Sciences Leopoldina (founded in the 17th century and based in Halle, then East Germany) to include anesthesiologists. Mayrhofer of Vienna, Benad of Rostok, and Safar of the U.S. became the first anesthesiology members of the Leopoldina. I remember with mixed emotions my initiating speech in Halle in 1984. I worked during nights in hotel rooms, translating my mega-talk into German. At the presentation, the absence of a carousel projector destroyed some of my cardboard slides. That visit opened a long-standing collegiality and then friendship between the Pittsburgh group and Wolfgang Roese, chairman of Anesthesiology at Magdeburg in East Germany. Roese's resident Uwe Ebmeyer (see Pittsburgh, IRRC) became chief research fellow with us in the early 1990s. He later returned to Magdeburg. I credit Roese and Lembke for one of the first physician-staffed mobile ICU systems in the world.

Mainz, Disasters, War, and Peace

My first meeting with German anesthesiologist Rudolf Frey was at a congress in Europe in 1958. He was then based in Heidelberg. He visited me in Pittsburgh during my first summer there in 1961. This was also his first summer at Mainz, where he had moved from Heidelberg to create the first free-standing department of anesthesiology in Germany. Mayrhofer's first European Congress of Anesthesiology in Vienna in 1962 and Laerdal's resuscitation researchers' conferences in Scandinavia deepened my friendship with Rudolf Frey.

In 1972, during a visit to Frey at the Gutenberg University in Mainz, Frey asked my opinion of his idea to create a "Club of Mainz" for worldwide disaster and emergency medicine. I encouraged this initiative, suggesting as a model the Club of Rome. This was a group of some 100 nonmedical gurus concerned about the ongoing self-destruction of our species on earth due to unchecked overpopulation, depletion of natural resources, overpollution, and resultant wars. The Club of Rome also feared that the availability of nuclear weapons could return us to the Stone Age. We talked more in autumn 1973, when Frey hosted the world's first international EMS symposium.[278] The Gudenberg University honored me with a doctorate degree for which I gave my first talk with double slide projection — with trepidation. This to me emotional symposium included my first Baltimore resident of 1955, Lourdes Aguto-Africa; Lothar and Mildred Barth; my Pittsburgh associates Benson, Esposito, and Sands; Eugene Nagel; and John Lane of Brazil. Family from Vienna came and my elederly mother (age 82) waltzed especially well with her son, historic German surgeon Killian and Frey.

Starting in the 1960s, I brought Frey, Negovsky, and Laerdal together. All four of us, having experienced or empathized about the horrors of man-made disasters in war, were primed for "disaster reanimatology." Until the 1970s, for natural disasters like earthquakes, disaster medicine and research had been the concern of epidemiologists, sociologists, engineers, and public health workers, all trying to help uninjured survivors, and help regions to recover. Nobody paid much attention to victims with lethal injuries. I suspected that among those counted as dead after major earthquakes, a significant proportion died slowly from repairable injuries, leaving time for resuscitation attempts.

Disaster reanimatology I introduced as a new field of research, for and by the "Club of Mainz," which Frey founded in 1976. After Frey's death, in 1983, the club was renamed World Association for Disaster and Emergency (WADEM). With Frey I met superb anesthesiology-based EMS leaders among his alumni, starting with Dick and Ahnefeld. Dick, then in Ulm, whom I promoted to become Frey's successor, has led since then anesthesiology's leadership roles in EMS in continental Europe, in an exemplary fashion.

Focusing on acute medicine and disasters, Frey assembled about 10 clinical leaders, mostly anesthesiologists from America and several European countries. We convened in Geneva in September 1976. We visited officers of the World Health Organization and the International Red Cross, including William Gunn, a recent president of the Club of Mainz, and then flew to Mainz to found the Club of Mainz.[279] I suggested that it should not compete with others but should fill a gap, namely, researching the potentials of modern resuscitation in mass disasters.[280-285]

With our help, Frey organized the first World Congress for Emergency and Disaster Medicine, held in Mainz in 1977.[280] The Second World Congress for Emergency and Disaster Medicine was held in Pittsburgh in May 1981.[281] I had already planned this meeting when, as a member of the Physicians for Social Responsibility (PSR) of Pittsburgh, I learned about the founding meeting of the International Physicians for the Prevention of Nuclear War (IPPNW) in Washington, D.C. at the same time. At our Pittsburgh congress, local PSR leaders Paradise, Michaels, and Rogers presented an estimate that the explosion of one hydrogen bomb in an urban area such as Pittsburgh would cause deaths and injuries of a magnitude with which no medical effort could cope. Any EMS plan for a nuclear war confrontation would be absurd and obscene. We brought this message to the 1981 Pittsburgh congress.[283,284] Negovsky, Semenov, and others from communist countries contributed significantly. I also brought together in Pittsburgh — for the first time — colleagues from various countries including the U.S., Canada, U.K., Israel, USSR, and CSSR, from disaster (resuscitation) medicine, military medicine, and peace medicine to discuss the role of the military for lifesaving in non-nuclear disasters.[282] Even nonmedical military leaders of the U.S. participated. We believe that this meeting had something to do with the subsequent change of U.S. military doctrine from hesitation to acceptance of its role in responses to natural or man-made (nonwar-related) mass disasters. In other countries, military disaster response has been a tradition.

Asmund Laerdal died from cancer in November 1981, soon after this historic Pittsburgh congress of May 1981. After Rudolf Frey lost his anesthesiologist son Martin Frey to an accident, Rudi died at Christmas 1981. These were deeply stirring events, followed by grieving, travels, and memorial speeches. When we initiated the journal *Disaster Medicine* soon after these losses, our co-initiating editorial assistant Nancy Kirimli-Linden, R.N., wrote moving poems in memory of Laerdal and Frey.[281]

Disaster reanimatology began with my anecdotal interviews of earthquake survivors in Peru[271] and Italy.[272]

I introduced the term "peace medicine" at the IPPNW Congress in Cologne in 1985, which was chaired by anesthesiologist Bonnhoefer. The IPPNW received the Nobel Peace Prize. Surgeon Martin Silverstein co-chaired several panels with me.[19] I addressed the IPPNW Congress concerning "roots of wars." For that document I consulted pioneering nuclear physicists in the U.S., namely, Hans Bethe (Nobel laureate) and Victor Weiskopf, both friends of Peter Winter and his family. They had contributed to the Manhattan Project.

I have been an active member of the World Federalist Association of Pittsburgh (WFAP) since our arrival in Pittsburgh in 1961, motivated by surgeon Harbison (see Pittsburgh), and a Federalist leader, MacLean McLean. This organization was founded by Norman Cousins at the end of World War II, under the motto "World peace through world law with justice for all." During the cold war we helped create a "peace coalition" in Pittsburgh, which including the PSR, religious organizations, the United Nations (UN) association, and others. During the early 1980s, still during ugly cold-war rhetoric by leaders on both sides (before Gorbachev), our peace coalition work in Pittsburgh brought famous peace philosophers to heavily attended public talks in Pittsburgh. I learned from preparing my introductions of our peace movement's guest speakers, Lewis Thomas famous for *The New England Journal of Medicine* essays and the book *Lives of a Cell*, and for "Listening at Night to Mahler's Ninth Symphony"; and Nobel laureate Linus Pauling. In 1991, the WFAP surprised me with the first Norman Cousins Award. Guest speaker at that meeting, chaired by our WFA Pittsburgh leaders Barner, Malmberg and Ketchum, was John Anderson, president of the WFA-U.S., historian, and former U.S. presidential candidate. He will deliver the Safar Lecture 2000 at the University of

Pittsburgh, where the present WFA Pittsburgh president, Burkart Holzner, is chairman of the University's Center for International Studies (UCIS).

My communication with leaders on both sides of the Iron Curtain included letters to President Reagan and Gorbachev. I read in detail Gorbachev's book *Perestroika*. If his advice had been followed, it would have led to a better world than we have now. In Russia, free enterprise would have developed gradually and with social conscience. Capitalist countries would become more humanistic. Murderous tribal conflicts fueled by nationalism and religious fanaticism might have been avoided through a strong UN sponsored "world police." Gorbachev wanted a joint peace-making leadership by the U.S. and the USSR. I consider his removal from power a great loss for the chances of world peace.

Negovsky and I had agreed during the cold war that "people-pressure," predominantly by physicians with clout, was required to influence governments and the public to end the insane nuclear war confrontation between East and West. Frey and I had to make sure that the Club of Mainz was not misunderstood as an organization planning for medical response to nuclear war. Nothing was further from the truth. When in 1980, Negovsky and I were in Potsdam, East Germany, hosted by Manfred Meyer, several intensivists present at that meeting decided to work with peace organizations. In 1981, the founding of the IPPNW in Washington, D.C., and the simultaneous peace program at the Second Club of Mainz meeting in Pittsburgh, initiated correspondence on my part with the White House, the UN, and leading colleagues in the USSR.

WADEM members examined the results of studies by various organizations, added our expertise with EMS, and, at our third WADEM Congress in Rome in 1983 (under the auspices of Corrado Manni), agreed on a resolution that I had drafted. After input from Baskett and Negovsky, this resolution was published.[285] It concluded that any meaningful disaster preparedness for a nuclear holocaust is impossible and that such attempts represent an unjustifiable expenditure of medical and financial resources. It also said that all governments in possession of nuclear weapons should take initiatives to reduce and ultimately eliminate the nuclear arsenals worldwide.

In December 1983, Negovsky and I visited the so-called "Soviet Peace Committee" in Moscow, whose purpose was to try to dispel the West's fear of an aggressive military intention on the part of the USSR. Negovsky and I understood that it was important to separate paranoia from justified fear on both sides. We also met with Chazov, co-president of the IPPNW, at the Cardiology Institute in Moscow. At the 50th anniversary of Negovsky's laboratory in Moscow in 1986, Negovsky and I declared support of the IPPNW. In 1987, at Pitt's resuscitation researchers' symposium, Negovsky and I, joined by others, met with the Pittsburgh Peace Coalition, which was coordinated by the WFAP. We recommended that all peace organizations consider the Pittsburgh Peace Coalition model.

Concluding thoughts concerning "the world" might include the following: In my globe-trotting life, with exposure to the diversity of mankind, I learned that what unites us is far greater than what separates us. The greatest war-making poisons are fanaticisms about ethnicity, race, and religion. Empathy with victims of wars convinced me that national borders must not be changed by force, and that forced conscription into Armed Forces, by leaders, to kill and be killed, should be internationally outlawed. Developing an instantaneously effective non-lethal weapon is a research challenge for anesthesiologists. "Human rights" must encompass more than freedom of speech, voting, and laws. Societies should first provide for all people the "basics," i.e., safe water, public health (hygiene and vaccinations), first aid, basic food, shelter, and education.

I am disappointed that in spite of having learned from the most murderous twentieth century, our country is permitting an obsession with violence on TV, where the beauty of the nude human body is prohibited, while teaching violent problem-solving on TV is fostered. Availability of guns is rationalized by an erroneous concept of what gives security. Children snuff out lives on video games everyday. Our society's priorities are distorted. All this should be of great concern to physicians. Changes must and can start at home and in schools, where mediation of conflicts is taught.

Organization of and rapid action by the UN need reform. My obsession with the sanctity of each human life got me into dichotomies about the military — from the post-World War II belief that defensive wars are potentially "good," via Quaker-style absolute pacifism during the Vietnam War, to "qualified pacifism" enforced by international controls: immediate inactivating of all nuclear weapons by removing warheads from missiles; nuclear non-proliferation; no arms sales to non-democratic countries; an international criminal court; combating economic inequalities worldwide; promoting humanism; early prediction and prevention of genocide; and "neutralizing" malignant, murderous, narcissistic tyrants, if necessary by "surgically" aimed force, instead of killing their countries' conscripts and noncombatants. A rapidly responding international military force for peacemaking, under UN auspices, is needed. The U.S. should lead, at least help. All the above combined offer now a better chance than ever to persuade irrational "homines sapientes" to mature toward peaceful co-existence.

CONCLUSIONS

General Conclusions

My views expressed in these conclusions are biased by the events of the entire 20th century and my globally oriented professional life. Our generation has experienced before, during, and after the greatest mass murders in history, enormous technologic progress and scientific and medical advances, an end to colonialism, and a spreading of democracy. Lucky are those who survived. It is our duty to inspire those who follow us to protect humanity and the earth with a long-range view.

Personally, looking back, at age 76, I feel good about having been allowed to serve mankind for half a century. I also have regrets about mistakes I made. I am fortunate that most of the programs that I had the opportunity to initiate have continued to grow under new leadership and will probably survive into the future, although undoubtedly with changes. In these memoirs, I have tried to document events; my privileged survival; contributions by us and others; and my recognition of and thanks to my teachers, associates, and trainees.

For humanity: For my views on how we might achieve lasting world peace, see above (end of section "The World"). My devotion to lifesaving and world peace ran into the realization that there is a limit to the time and energy one can spend to influence national and world affairs. Nevertheless, if most professionals would spend a little time each week trying to influence others, including those with power over life vs death, at local, national, and/or international levels, the world would become a better and safer environment. I am concerned about current trends, throughout the industrialized world, toward "greed→money→power→laws supporting injustices" (e.g., the rich are getting richer and the poor are getting poorer) (adapted from Kurt Weill's opera "Buergschaft"). As an optimist, I believe that young leaders in the twenty-first century C.E., in developed and developing countries, have a greater chance than ever before to modify the above ills to the following:"love→ humanism in peace with nature→ethics→laws supporting justice." That requires free enterprise to be conducted with social conscience, and true democracies. It will be difficult to help young adults who want to make a decent living, to do so by ethical, hard work, when they see middle-men, ball players, and CEOs (even those of failing companies) cashing in millions for themselves.

For medicine: I personally feel good knowing that I have had the opportunity to touch the lives of over 500 students and other professionals and to have learned from many of them. The tables with the names of my residents, fellows, associates, and nonphysician helpers are deposited in the Wood Library-Museum. These tables include for Pittsburgh, in 1961-1979, 197 faculty members and physician trainees and 22 nonphysicians of the Department of Anesthesiology and CCM. During the IRRC-SCRR years 1979-2000, our programs included another almost 100 M.D. research fellows and 25 medical student research fellows, over 20 co-investigators and consultants, and over 20 nonphysicians. Among my physician trainees there were seven who became chairmen of university departments of anesthesiology (B. Smith, J. Smith, Wooten, Benson, Gisvold, Breivik, Vaagenes). Thirteen of our M.D. research fellows achieved a Ph.D. or its equivalency. Many of our groups' scientific findings and initiations of new treatments would probably not have occurred without us. Some would have happened, but different and later. "Nothing is as certain as change" (song by Paul Safar). Let us foster "good" changes while preserving eternal values and truths learned from history.

Some Conclusions Concerning Resuscitation Research

We heard the view expressed that "our groups' involvements outside ORs have been our major strengths and contributions to medicine and mankind" (J. Smith). Our teaching has been worldwide, with many time and energy-consuming missions, reviews, books, films, and conferences. Only a few of our review papers are listed in these memoirs.

On books: I had the opportunity, with the help of superb co-authors, to produce (or to help produce) the first textbooks on CPCR,[5] cerebral resuscitation (with Grenvik) (*Brain Failure*, Churchill Livingston, 1981); respiratory therapy;[9] and emergency medicine (as co-editor and 20% authored by us).[156] For the first textbook on CCM, edited by Shoemaker, Grenvik, and Ayres, over the years, I have been asked to contribute the first chapter on CPCR. I have made a commitment to produce in the near future a textbook *On Resuscitation Medicine in the 20th Century.* It is to be a single-author opus. I have begun working on it.

On films: Beyond our first documentary research films on resuscitation[61] and respiratory intensive care,[93] I helped Gordon with the first CPR training films (*Breath of Life; Pulse of Life; Prescription for Life*). In the 1970s, Grenvik and I spent much effort creating the first series of four CCM life-support training films (available from UPMC Media Production).

On conferences: I helped with the first international CPR researchers' conferences;[117,118] and co-initiated and co-organized the thus far five American "Wolf Creek Conferences"[326-330] and the first two American Heart Association Emergency Cardiac Care Guidelines Conferences, in 1966[331] and 1973.[332] The Wolf Creek Conferences are named for Jim Jude's[67,68] Wolf Creek Lodge in Georgia, where he hosted the first of the series.[326] In addition to the world's first ten International Emergency and CCM teaching symposia, which we organized in Pittsburgh (1967-76), my research associates and I conducted and hosted scientifically high-powered international researchers' conferences on dying and resuscitation, focusing on cerebral resuscitation — in the 1970s,[322] 1980s,[323] and 1990s.[287] We helped many researchers' conferences abroad, including the WADEM congresses, the cerebral resuscitation researchers' conference in Erice, Sicily,[333] and the European Resuscitation Council meetings.[334]

Some Conclusions Concerning Anesthesiology

My thoughts on anesthesiology concern the "complete anesthesiologist" who is also a reanimatologist and intensivist — and at least understands basics about pain control outside the OR. What anesthesiology's future will be is unpredictable, but each anesthesiologist has personal ideas about what it should be. As an idealistic old-timer, I realize that my views on this may be wrong, as I have not been in the clinical trenches since 1989.

For continued leadership by American medicine, anesthesiology and CCM, and for the sake of our patients, we should get the present "mismanaged care for managed profiteering by non-professional middlemen" (P.S.) replaced by a physician-guided national health care system for the "basics" of health care. All, including the poor, must have access to basic health care without becoming financially ruined. Knowledgeable and wise physicians (including wise anesthesiologists) should define "the basics" – to include hygiene (public health with vaccination, safe water and food, etc.); prenatal and perinatal care; regular checkups; potentially preventive or life-saving tests and operations; LSFA by the lay public; emergency, operative, and intensive care for critical potentially salvageable cases; and safe anesthesiology services for all operations. Frills (like cosmetic surgery) and unwillingness to wait for elective procedures, could be paid by patients or supplementary private insurance, to prevent the basic system from becoming abused and going bankrupt (the "pop-off valve"). Money might be saved by scrutinizing administrative costs (not having unproductive highly paid non-professional middlemen), and reducing defensive medical practice with obligatory "non-fault medical malpractice insurance" and mediation for damages.

Profiteering from curtailing care by insurance companies, HMOs, CEOs, administrators and investors is unethical. In the 1990s, physicians and nurses in the U.S. have been made into factory workers. The needed backlash has begun, as with the "call for action — patients not profits," spearheaded by my good colleague Bernard Lown (*JAMA* 1997).

In academic medicine, the clinician-scholar (teacher)-scientist (researcher), who is crucial for maintaining U.S. leadership in specialty care, is becoming an endangered species. Powerpersons must be forced to preserve academic values (curiosity, communication, search for truth, openness, shared governance, democracy, free speech, accountability, collegiality, loyalty, and others). How will teaching and research be financed? Academic leadership positions should be salaried by the Federal government. It is undesirable and unethical for university hospitals to give up their not-for-profit status. Excess revenues, which exceed costs, should be funneled back into improved services, teaching, and research.

Anesthesiologists will soon have to decide on our specialty's future in discussions with wise, leading peers of other disciplines, particularly the surgical disciplines. Broad values and goals should be similar worldwide. I have discussed medicopolitical issues concerning anesthesiology with respected colleagues worldwide, in the U.S. in particular with my friends Ephraim (Rick) Siker (former ASA president) and Peter Winter (chairman and CEO of our department in the 1980s and 1990s). Their pessimistic realism (pragmatism) and my optimistic idealism might, if combined, might help restore what the best of medicine can offer.

Rethinking answers to my younger colleagues' questions about the future of anesthesiology, I have, rather hastily, come up with about 10 topics:

1. *Physicians and nurses.* The medical profession is a calling, not a business. So is the nursing profession. The two should work together, helping individual patients and populations. I have always respected and worked with nurses and consider them colleagues. Because of their different backgrounds and education, physicians bring more scientific knowledge and nurses more

bedside caring. Both should become artistic practitioners. Much of what nurses use is what doctors invented. Physicians and nurses should teach each other and work as teams, fostering mutual respect. Incomes should be fair and adequate for both to live with decent, appropriate lifestyles. More important than wealth (money far beyond one's needs) are lifestyle and a meaningful life, i.e., "what you would like to see looking back at your life at age 80." Schooling of, and staffing by, physicians and nurses should be planned jointly, in response to nationwide needs.

Militant territorialism, which I understand is now exhibited by political leaders of organized anesthesia nurses, is silly, destructive, and uncollegial. The (in my opinion needed) supervision of nurse anesthetists by wise and skilled anesthesiologists is required abroad. If this requirement is lost in the U.S. we will be ridiculed for low standards of coverage. My alma mater, the Penn department of Anesthesiology, led by Dave Longnecker, provided data showing higher adverse outcome rates when anesthesia is given without supervision by an anesthesiologist (Silber, J.H., et al, *Anesthesiology* 93:152-163, 2000). Nurse anesthetists in solo practice in "undesirable areas" should not be guided by surgeons who have no experience in anesthesia, but by anesthesiologists, attracted into these (rural) areas as payback for financial support during training, or by other methods. All this requires anesthesiologists' activism with media and legislators.

Clinical anesthesiologists should not lose the artistry of compassionate care for the individual patient. I agree with Bernard Lown (Harvard cardiologist, co-initiator of external DC defibrillation, co-initiator of the IPPNW [Nobel peace prize]) that clinical medicine is ailing from a recent loss of "the art of healing." At the same time, however, life-saving technologies have increased patients' survival chances. New effective technology should be applied with compassionate communication and artistic anesthetizing, resuscitating, and life supporting. This includes (now rarely seen) the ear on the heart and lungs (esophageal stethoscope), the finger on the pulse, the hand on the breathing bag and the eyes on the surgical field. Taking time with pre- and postoperative visits has become difficult for anesthesiologists, as same-day surgery increases. The same has become a concern in intensive care. In the 1960s, we worked on ICU patients from the side or top of the head; we touched patients and talked to them. Now I feel uncomfortable seeing care decisions made on the basis of wall-to-wall data on computer-printouts, discussed during rounds at the foot of the bed or in the x-ray room.

2. *Public service and safety.* The ability to abolish all pain during surgical operations became possible in the mid-1800s with the first demonstrations of general analgesia by inhaling ether, nitrous oxide, or chloroform. Thus, providing freedom from pain became relatively easy. "Analgesia" did not threaten airway, breathing, or circulation. Chloroform induction, however, at times stopped the heart and breathing, as in the first such case in a doctor's office for a minor procedure, with the patient breathing chloroform from a cloth. That event in 1848 started a series of chloroform deaths and concern for the safety of general anesthesia. In the late 1800s, when antisepsis and asepsis called for deeper levels of general anesthesia to enable laparotomies, airway, breathing, and circulation were threatened. Many iatrogenic deaths occurred, and rudimentary attempts at resuscitation were initiated. Supporting vital functions proved more difficult. Modern anesthesia, therefore, did not come about until life-supporting methods were added, just before and during World War II on the allied side.

Increasing the safety of anesthesia has come a long way. Our wish that "no patient is harmed by anesthesia" (to quote J.S. Gravenstein of Gainesville, Florida, formerly Cleveland) has been anesthesiologists' goal since my time in the late 1940s. That was easier said than done. In the 1950s, Beecher and Todd, as well as my associate Otto Phillips and his Baltimore joint anesthesia study committee, reported an overall anesthesia-related mortality of about one in 2,000 anesthesias given in hospitals. This reflected a higher estimated casualty rate than poliomyelitis at that time. With polio conquered, we are now comparing anesthesia hazards with aviation hazards. In both, human error is the main issue. During my time as chairman in Pittsburgh we should have but did not conduct an ongoing registry of mishaps. I believe there were about one or two clear-cut anesthesia-related deaths each year in patients with relatively good physical status, among the over 50,000 anesthesias given each year. Now, the Anesthesia Patient Safety Foundation (APSF) of the U.S., founded by E.C. Pierce and led by Siker and now R.K. Stoelting, talks about one death per 2-300,000 anesthesias administered. Congratulations! This success, of which our specialty can be proud, particularly in the light of the recent report on medical errors published by the Institute of Medicine (IOM, National Academy of Sciences), I believe has been the result of a combination of factors: better trained anesthetizing personnel, guidelines, greater awareness, a few new agents (e.g., halothane replacing ether and cyclopropane), and more sophisticated monitoring devices. I happen to consider the last as least important.

Public service requires safety and cost-effectiveness. I could never understand how this could be provided by some of our colleagues doing fee-for-service practice in multiple hospitals, which required turning peri-operative management over to personnel not under your control. The one-hospital-based anesthesia team approach is more cost-effective and safer. Physician anesthesiologists are responsible for and can control all anesthesias and peri-operative care, each extending his/her impact, if necessary, to several anesthetizing areas with the help of nurses, trainees, and technicians.

3. Anesthesiologists' uniqueness. We must consider the whole patient, whereas many colleagues in other specialties focus on one organ system at a time. We focus on managing critical states. General anesthesia is coma, which is a critical state. So can be systemic effects of regional anesthesia. Our colleagues in general medicine try to prevent critical states. We have the unique expertise in providing life support during critical states until surgery, other therapies, and nature correct the underlying disorder. We are life supporting by moment-to-moment individually titrated management. This differs greatly from office practice and from making rounds in the hospital. It is good to learn that "without the advances in anesthesiology and the pioneering roles of some anesthesiologists in CCM, many of today's advances in surgery would not have been possible; modern anesthesiology synthesizes all aspects of modern clinical medicine" (J. Smith).

4. Recognition of our specialty. Physicians in general have lost much of their past prestige. Siker reminded me of the public perception that "my doctor is wonderful, but most doctors are thieves." Not only should we not be greedy, but we also should combat thievery by greedy doctors, administrators, CEOs, HMOs, insurance companies, and some industries. We should educate the public about health care as a calling and the unethical effects of making it a profit-generating business.

According to Siker, recognition of anesthesiologists would have to come from patients, surgeons, and the media. Some individual anesthesiologists have gained recognition at the national and international levels. Many practicing anesthesiologists have gained recognition at local community levels. Patients recognize our acting as good doctors, personally visiting them pre- and post-operatively, and communicating. Even day-of-admission surgery permits patient-anesthesiologist communication. You can make the patient recognize that you are the one who keeps him/her alive. Surgeons respect anesthesiologists for service (not having to wait), skills (providing good condition for the operation), gamesmanship, and safety. Media needs more input from anesthesiologists. Our specialty is less recognized in the U.S. than abroad, I believe, for the following reasons: a) American organized anesthesiology has failed to gain public recognition and has failed to educate legislators about our field of medicine, without which modern surgery, resuscitation, and intensive care life support would never have come about. b) Anesthesiologists' unique skills in resuscitation and life support throughout the EMS system have been applied abroad since the 1960s, while in the U.S. considerations of finances and lifestyle have kept most anesthesiologists in the OR. This has led to the assumption of these more glamorous roles by surgeons, internists, and emergency physicians. c) Although nurses may help physicians administer anesthesias in hospitals abroad, they are not perceived as being able to do it without physician anesthesiologists.

5. *Teaching and learning.* All four years of medical school should expose students to anesthesiologist-guided life-support practices and their underlying sciences. For future anesthesiologists, the rotating internship, with emphasis on surgery, should be restored. Trainees in surgery, in turn, should rotate through anesthesiology (as at BCH in my time) — for much more than just "learning how to intubate." We must get exposure to each others' problems, thinking, and reacting. I admire but cannot comment on chronic pain control.

I feel that apprenticeship is still the best way to become skilled and competent in anesthesiology and surgery. Although I have no experience with simulators and safety drills as used for aviation personnel, I would encourage education research to document the addition of these training modalities before and during apprenticeship on patients.

Teachers of anesthesiology (including chairpersons of departments) should be judged not only as lecturers, but also as clinical role models. In academic institutions, every time experienced physicians make a patient contact, they should be accompanied by someone who can learn from them (and often vice versa). The clinical trainee can be a physician fellow, resident, medical student, colleague from another discipline, nurse anesthetist, nurse, technologist, or other. Teaching and guidance in clinical anesthesia for residents and nurse anesthetists should include personal demonstrations by the teacher with the trainee helping, rather than the teacher just supervising. In OR anesthesia, to teach while moving the OR schedule along rapidly, without delays, requires that a responsible, skilled clinician move quickly between ORs, PAR, and holding areas, while always guiding with speedy action.

6. Resuscitation and intensive care. Every competent anesthesiologist is a reanimatologist and intensivist, even the one whose practice outside the OR is minimal. In terms of risk, there is no "minor" general anesthesia. The "complete anesthesiologist" concept of the 1960s can and should be revived.

For emergency resuscitation, anesthesiologists should be prepared to resuscitate not only in the OR. They also should participate (and, if skilled, also lead) in the necessary multidisciplinary team action for advanced resuscitation attempts hospital-wide. In the OR, need for CPR fortunately has become very rare. However CPCR skills can also be acquired in the animal laboratory and the ED. The anesthesiologist should feel insulted when called to emergency resuscitations merely "to intubate," which technicians can and have been taught to do. Particularly difficult airways are rare exceptions. The expertise of the anesthesiologist as reanimatologist ideally could include advanced and ultra-advanced titrated life support of the whole organism. This includes the knowledge and skills to perform open-chest CPR, emergency cardiopulmonary bypass, administration of sophisticated titrated minute-to-minute medications, and cerebral resuscitation.

For intensive care, prolonged life support needs to be carried out not only in ICUs, but also in EDs, ORs, PARs and sometimes elsewhere. It is our duty to apply our skills also outside the OR when we are needed and available. For complicated ICU cases with needs beyond CPCR BLS-ALS-PLS, we need help from our colleagues with backgrounds in medicine/pediatrics, surgery, and other disciplines. Since the 1950s, I have considered as ideal the hospital-wide 24-hour coverage of resuscitation and acute intensive care needs shared collegially among intensivists based in anesthesiology, medicine, and surgery who have special interest and expertise in resuscitation and intensive care. Subacutely, they would draw on each others' different backgrounds. Since the majority of anesthesiologists in the U.S. have withdrawn from the ICU and ED, the majority of intensivists now are not anesthesiologists. These often lack coached experience in OR anesthesia, an experience I consider essential for becoming an effective intensivist. All staff physicians in full-time ICU work should have spent several months with guided OR anesthesia experience, not only to practice intubations, but also to learn titration of life support in general. Anesthesiology is now attracting physicians, who have more sophisticated knowledge than we had in the old days. Many do not like long-term responsibility for ICU work. American anesthesiologists as reanimatologists and intensivists, however, do have a future, particularly when most specialists will become hospital-based and salaried, as is the case in other industrialized countries with national healthcare systems.

7. Emergency medical services. Leadership in the delivery of modern resuscitation and life support outside hospitals and in the ED could be improved by greater involvement of anesthesiologists. Anesthesiologists perform resuscitation attempts outside hospitals in about one half of EMS systems in central Europe and in almost all regions of Scandinavia. Most of these anesthesiologists work in the field with mobile ICU ambulance teams, and many are in charge of the EMS system. Many European anesthesiologists have led in EMS, not only in the past (e.g., Frey, Ahnefeld, Roese, Baskett, Lust, Delooz, Hugenard, Lund), but also at present (e.g., Dick, Sefrin, Lindner, Mullie, Baskett, Vaagenes, Gisvold, Edgren, Cerchiari, Ebmeyer). In the U.S., although we initiated in Pittsburgh the first guidelines for community-wide EMS organization and ambulance attendants' training, very few anesthesiologists have been willing to get involved outside the hospital. In the 1960s and 1970s they included Safar, Benson, Caroline, and Nagel. Now, Roger White of Minnesota may be the only one left. I doubt that fear of lawsuits is a major factor in traditional specialists' reluctance to work in EMS.

Since 1970, when traditional specialties abandoned EDs in major community hospitals, the new base specialty of emergency medicine has come about — founded by a few general practitioners. Since anesthesiologists, surgeons, and internists had failed to take on the leadership of prehospital CCM, I supported the initiation and growth of emergency medicine, primarily with the hope that it will fill this prehospital gap in CCM. The National Association of EMS Physicians (NAEMSP) (initiated and led by Pittsburgh's Stewart and Paris), and its parent organization, the American College of Emergency Physicians (ACEP), now seem to prevent anesthesiologists from performing EMS work, unless they complete an emergency medicine residency of three years. This is also territorialism and unwarranted. Emergency physicians should appreciate the fact that creation of their discipline was strongly supported by anesthesiologists, who were their teachers. Also, every general anesthesia is "critical," whereas 95% of emergency room cases are not critical. Anesthesiologists with competence and commitment to EMS should be allowed to contribute in EDs and in the field, even without another three-year residency.

Recently, some anesthesiologists founded the International Trauma Anesthesia and Critical Care Society (ITACCS), trying to lead the trauma surgery and ICU-related components of EMS. This is a laudable trend, provided the ITACCS stands for multidisciplinary traumatology. That would require collaboration with the SCCM, the American College of Surgeons Committee on Trauma, the NAEMSP (ACEP), the multidisciplinary WADEM (which was created mainly by anesthesiologists), and other related groups, including neurosurgeons and orthopedic surgeons.

8. Technology and industry. Do not sell your soul to commercial interests. Be open to, and supportive of, industries that help save lives, by pursuing "free enterprise with social conscience." These industries deserve our advice in jointly searching for increasingly more effective devices, drugs, and systems. Beware of venture capitalists who patent others' ideas and sell them without scientific documentation. Influence drug companies to look for breakthrough drugs, not for expensive modifications of what already works quite well, merely to be able to charge more as long as the patent lasts. Foster simplicity. Much money and effort has been spent on attempts to improve on "barb, arrow poison (curare), laughing gas (N_2O), and poppy-seed (opiates)," but only minimal added benefit for patients has come about. Learn to manage patients, if necessary, without expensive devices, using your senses and simple, inexpensive tools. Teach lawyers that the absence of a pulse oximeter, ECG, capnograph, or mass spectrometer is not negligence when there is an esophageal stethoscope connected to the ear, a finger on the pulse, and the other senses trained on crucial targets at crucial moments. When an alarm goes off, don't try to fix the monitor, but focus on the patient.

9. Research. Results are more important than grants funded and papers published. Research results should be judged on the basis of immediate or potential scientific, clinical, or (for a breakthrough) socio-economic importance. In the mid-twentieth century, anesthesia-related research was mostly about devices, techniques, and pharmacology. Some such research achieved important results. Recently, laboratory research by anesthesiologists has become increasingly sophisticated, much removed from possible clinical impact. To gain status as an academic anesthesiologist, one has to attract NIH money. This now requires molecular-level research. Grant proposals are mostly reviewed by committees of Ph.D.s and M.D.s focused on molecular genetics. Medicine is waiting for gene therapy to become a possibility. This might perhaps become important for rehabilitation, unlikely for resuscitation.

Although I am awed by the work of some molecular-level biologists, I consider outcome of the whole organism to be most important. Many breakthroughs in therapeutics that have had enormous life-saving effects, have come from treatments whose mechanisms are still unknown or unclear, (e.g., anesthetics, antibiotics, corticosteroids, and insulin).

Anesthesia techniques can still provide topics for breakthrough research. Needs and opportunities include simplification of general anesthesia, a search for general analgesia without loss of airway and breathing, the ideal blood substitute, and longer-acting regional anesthesia (nerve blocks) with easier localization of nerves. Search for a nonlethal weapon is a challenge.

Randomized clinical trials (RCTs) have become a fad for seeking acceptance of innovative treatments. RCTs may be the "gold standard" for evaluation of drug treatments for cases not requiring life support. In resuscitation medicine, however, RCTs (in which numerous factors cannot be controlled and initiation of patients must be done within seconds) have failed to statistically document overall differences in outcome between control vs experimental treatment groups, even when highly effective in controlled animal studies.

Anesthesiologists as humanists may search for better ways to help terminal patients with hopeless conditions have a painless, aesthetic departure. I do not mean active voluntary (requested) euthanasia. That opens the door for abuse. It differs from physician-assisted suicide in "who pulls the trigger." It also differs from passive euthanasia, i.e., "letting die" in futile situations. "Compassionate, titrated terminal care," using new physical, pharmacologic, psychologic, and logistic approaches for pain control, can learn from anesthesiology and intensive care.

Researchers emerge because of curiosity (asking unanswered questions and having the urge to pursue them) and/or the requirements of academic careers. Those start with a mentored research fellowship. Distinctions have become blurred between basic and applied research, laboratory and clinical research, individual and team research, and research vs technology. There remains only good vs poor research. The tendency to possess a special device and then to search for its use is not good research.

Not every anesthesiologist-reanimatologist-intensivist in training should be required to be involved in a research project. Clinical training programs, however, should occur in an environment where clinical or basic research is ongoing and routinely communicated to the clinicians. Such exposure should teach the trainee to ask questions and how to critically review the literature. Full-time clinicians, who have spent 1-2 years in full-time laboratory or clinical research, have often stated that their research experience made them better clinicians.

10. Leadership and administration. I can make only general suggestions, based on history. I am not in touch with current challenges in OR anesthesia management and financing.

Chairpersons should lead by example, which includes being clinical role models and leading discussions of weekly or monthly conferences for the entire department. These are now increasingly less attended. Delegating some administrative responsibilities is now difficult when the chairman is expected to be a CEO.

Collegiality means that the department chairperson is first a faculty member and only secondarily an administrator. Integrity includes management with openness (including finances), to gain the trust of colleagues. Ruling by power and fear usually ultimately fails. Personal responsibility means not hiding behind committees. In critical decisions, the leader should seek maximal democratic input but not shy away from making authoritative decisions, while taking full responsibility for his or her actions.

Once the chairperson trusts a division leader, he/she should delegate not only responsibility but also authority. In large departments, delegated authority should govern groups not larger than about 12 staff physicians. With groups much larger than that, achieving the cohesive family spirit needed for strong group action is difficult.

I feel sorry for my clinician colleagues who are now forced into spending 20-30% of their time and energy to write notes for billing, not primarily for helping patients. Billing for anesthesia services has been made open and fair by the ASA relative value guide. For ICU work, average daily charges for life support would simplify matters. The simplest for anesthesiologists and intensivists in academic medical centers would be a return to hospital-paid salaries, if those were fair and openly negotiable between clinicians and administrators. With such an arrangement, physician groups, helped by nonphysicians as needed, could provide hospital-wide services for OR anesthesia and outside the OR, and for teaching and clinical research, unencumbered by considerations of billing for individual actions. The relationships between charges, collections and costs have always been a myth. Where does the money go? Accountability and open books should be required not only of physicians' practices but also of the finances by hospital administrations, insurance companies, and HMOs.

The needed changes in the health care system require strong physician leadership. Some anesthesiologists are exceptionally qualified to take on such roles. The future of anesthesiology must remain part of the future of medicine and health care in general. In academic institutions, the three-legged stool of patient care, teaching, and research must be preserved. That, according to present observations, requires that an academic medical center have a highly respected and trusted physician leader as dean/vice chancellor and hospital CEO.

Clinical outcome studies should focus on quality of survival, not ICU or hospital discharge rates, which consider survival as a triumph even if it leads to hopeless terminal suffering. Debates on ethical dilemmas should take into account the fact that the largest proportion of health care expenditures happen in the last decade of life. Prompt, effective emergency resuscitation is more effective than expensive and often too-late intensive care.

I see the greatest opportunities to maximize lifesaving and reduce costs in the following measures: 1. Reducing administrative costs. 2. A single payer system for the basics. 3. Defining and terminating futile but expensive treatments. 4. Physician leadership focusing on the needs of patients and opportunities to meet these needs, not on demands by the public or on profits by providers. 5. Regionalized centralization of highly sophisticated expensive care, as needed by major trauma cases. 6. Not-for-profit systems which channel excess revenues back into better care, research, teaching, and care for indigent patients. The noblest call of the medical profession is to help more and more people achieve complete, full life spans (fig. 14).

Table 1

Peter Safar's Professional Life

Born in 1924

1934-42	Piaristengymnasium, Vienna
1943-48	Medical School, University of Vienna
1948-50	Intern-Fellow in Pathology, University of Vienna in Surgery, Universities of Vienna and Yale
1950-52	Resident in anesthesiology, University of Pennsylvania
1952-53	Founder and Chief, Department of Anesthesiology, National Cancer Hospital, Lima, Peru
1954-55	Anesthesiologist, Johns-Hopkins Hospital
1955-61	Founder and Chief, Department of Anesthesiology, Baltimore City Hospital
1961-	University of Pittsburgh Medical Center
1961-79	Founder, Professor and Chairman, Department of Anesthesiology and Critical Care Medicine
1979-	Distinguished Professor of resuscitation medicine
1979-94	Founder and director, International Resuscitation Research Center (IRRC).
1994-2000	Principal investigator on traumatic shock and suspended animation; advisor, Safar Center for Resuscitation Research (SCRR)

Table 2

Peter's Laws*

For the Navigation of Life

The Creed of the Sociopathic Obsessive Compulsive

1. If anything can go wrong, Fix It!
2. When given a choice - Take Both!
3. Multiple projects lead to multiple successes.
4. Start at the top then work your way up.
5. Do it by the book but be the author.
6. When forced to compromise, ask for more.
7. If you can't beat them, join them, then beat them.
8. If it's worth doing, it's worth doing right now.
9. If you can't win, change the rules.
10. If you can't change the rules, then ignore them.
11. Perfection is not optional.
12. When faced without a challenge, make one.
13. "No" simply means begin again at one level higher.
14. Don't walk when you can run.
15. Bureaucracy is a challenge to be conquered with a righteous attitude, a tolerance for stupidity, and a bulldozer when necessary.
16. When in doubt, think!
17. Patience is a virtue, but persistence to the point of success is a blessing.
18. The squeaky wheel gets replaced.
19. The faster you move, the slower time passes, the longer you live.
20. Death is not the enemy, but occasionally needs help with timing.
21. When on thin ice, dance.
22. It is up to us to save the world.

*A joke — unknown author for "laws #1-19" discovered by Ake Grenvik. Law #20 by James Snyder. Law #21 by Peter Winter. Law #22 by Peter Safar, quoted by Peter Winter at P.S.'s 70th birthday.

Personal Conclusions

Looking back at my professional life,[335] it began during World War II in Vienna. The 1950s were for learning my base specialty, and for initiating and developing a new academic department of anesthesiology in Peru, and another one in Baltimore, with initiation of CPR research. The 1960s and 1970s were for initiating and developing a third academic department of anesthesiology, in Pittsburgh, which became, already in the 1970s, the largest in the U.S., and which helped initiate CPCR, EMS, CCM, resuscitation research, and biomedical ethics. The 1980s and 1990s I devoted predominantly to basic resuscitation research, in the multidisciplinary IRRC, disaster reanimatology, and to help "peace medicine." From the 1960s through the 1980s in Pittsburgh, I laid hands on patients in ORs and ICUs, first full-time and later part-time. I was ultimately responsible for the safety of about one million anesthesias in my life. I have been more effective in initiating than in maintaining programs. Colleagues and others have asked me —

"What do you want to be remembered for?"

1. For having followed my father's suggestion to make use of my talents (which were limited) and opportunities (which were considerable); and for having "found meaning" in life (V. Frankel, see Introduction). 2. For having touched many lives. 3. For having led from scratch the development of America's largest department of anesthesiology. 4. For having co-initiated new medical fields.

For medicine, some colleagues have called the following of my efforts important discoveries and documentations: 1. the life-saving superiority of head-tilt plus mouth-to-mouth ventilation over chest or back-pressure arm-lift ventilation; 2. assembly of the CPCR system (fig. 11); 3. cerebral resuscitation with mild hypothermia; 4. preservation potentials for traumatic hemorrhage (hypothermia and suspended animation); and 5. co-initiation of multidisciplinary CCM, EMS systems, and disaster reanimatology. To people who have called me "the father of modern resuscitation" I said that no single person invented or discovered modern CPR or CPCR. We were merely the first to document steps A, B, G, H, and I; added pre-existing steps C, D, E, and F; and assembled them into a system (fig. 11).

In 1998, reporters for Mr. Peter Jennings' ABC television news came to interview and film in our laboratories, asking, "What do you consider to be the most important advances in resuscitation medicine in recent years?" My answer: 1. The system of CPCR BLS-ALS-PLS (fig. 11); 2. external defibrillation; 3. improved fluid resuscitation for severe hemorrhage; 4. therapeutic (controlled) hypothermia; and 5. the delivery systems of EMS and CCM. "To which of these five have your teams significantly contributed?" Answer: "All except number two." This was possible only because of collaboration by many colleagues and friends, and because Eva mothered our family. Off-and-on I freed myself to also experience some of the beauties of this world, including family, friends, nature, sports, music, arts, and travel.

"What are your professional regrets?"

1. I wish I had taken a long, full-time research fellowship at the end of my residency at Penn, rather than learning research part-time from collaborating scientists while accepting clinical chief positions. 2. I wish I had not seriously hurt two patients. 3. I wish I had been less idealistic and more realistic in financial matters. 4. For research money, I wish I had not disregarded the molecular orientation of our "National Institutes of Mechanisms." 5. I wish I had better understood the psychologic suffering of our daughter Elizabeth, and had not (unintentionally) abandoned her when she needed me for survival.

"Which values would you like to pass on?"

Concerning medicine, I expressed suggested values above, under 10 topics on the "future of anesthesiology." Concerning the world and us, I start with suggesting that religion and political orientation should be personal philosophies. We should demand tolerance and separation of church and state and should despise religious fanatics, fundamentalists, moralists, and church-going hypocrites; they make a civilized society impossible. Overriding ethical values should be practiced worldwide as humanism. Crimes against humanity should be addressed by law, not by seeking revenge. Free enterprise should be pursued with social conscience, as intended by the socialized democracies of America's New Deal, Vienna in the early 1930s, and Western and Northern Europe's post-World War II governments. Manchester-type (19th century) capitalism is cruel, unfair, and undemocratic. Business should seek reasonable profits and be fair to people. Absolute dictatorship, be it monarchic-aristocratic, communist, fascist, or capitalist (corporate) can be prevented by Jeffersonian democracy which I consider the greatest gift of America to the world. I am for maximal democratic input, but at times I see the need for wise, benevolent authoritarian decision-making. I am for "world peace through world law with justice" (WFA). I am a world citizen who loves the U.S. for having liberated Europe and for now defending human rights world wide. "It is up to us to change the world" (Table 2).

"What are your plans for retirement?"

Not to "retire" as long as my brain can function. Now, at age 76 (2000 C.E.), to have a brisk walk into the sunset, not to sit at the golden pond. To consider each additional day I can live with an active mind and in reasonable fitness, as a bonus. My immediate tentative plans until at least age 80, look like this:

1. As lab researcher, having helped extend the "golden hour" of tolerating traumatic uncontrolled hemorrhagic shock (HS) in animals, through our team's work in the 1990s, I have turned the leadership of this program over to Tisherman and encouraged him to initiate a clinical multicenter study of mild hypothermia for HS. I remain available to help.
2. To lead our suspended animation (SA) research program to fruition, with the goal of effectively reversing presently

unresuscitable conditions. Encouraged by our successful DOD funded research in 1997-2001, to prepare a proposal, with the help of many smart associates/colleagues, for starting in 2002, for another 3-05 years of systematic research in the form of a multicenter "Mini Manhattan Project." That should have components of basic science (how cells die), clinically relevant large animal outcome studies, development of clinical devices and methods, and multicenter clinical trials. To promote the start of clinical feasibility trials for currently unresuscitable exsanguination cardiac-arrest in EDs; and to clarify, at least with laboratory research, SA potentials for temporarily unresuscitable normovolemic sudden cardiac deaths.

3. To help Kochanek acquire more secure long-term funding of the SCRR. Long-term secure support of this center is warranted because of the large proportion of humans who die before their time has come from acute potentially resuscitable emergencies. These random chances of nature will always be with us.
4. To remain available for advice, with historic perspectives and global views, and to stimulate serendipitous discoveries.
5. To complete the textbook *Resuscitation Medicine in the 20th Century*, contracted with Springer-Verlag of Vienna. This will close my family's professional historic circle, because Springer bought the Josef Safar medical publishing house in Vienna in 1924, the year I was born.
6. To help make a training film on "the art of anesthesia," using video footage made of my OR anesthesia teaching in the past.
7. To travel (with family) to places missed so far.
8. To continue promoting "world peace through world law with justice" (Norman Cousins) through a reformed United Nations, working with the World Federalist Association (WFA)

and the International Physicians for the Prevention of Nuclear War (IPPNW), i.e., the PSR.

9. To begin putting my things in order (voluminous files, books, reprints, slides, photos, movies, audiotapes, and paraphernalia) — in home, office, library and laboratories, in order not to burden family and colleagues after my departure.
10. Most important — to devote more time to our five grandchildren.

To conclude the conclusions of these memoirs — I am deeply grateful and consider it a privilege to have lived beyond age 70, relatively fit, still a workaholic, and still able to enjoy some of the beauties that life on earth can offer. I always think of the millions who have not been so fortunate. I am grateful to those who founded anesthesiology before me, the specialty that enabled me to contribute to new fields of medicine. I am deeply grateful to my roots, my teachers and my associates. I am very fortunate in having had a great lady as a lifelong partner (Eva), and having fine children and grandchildren, and some real good friends. I am proud of our two sons (Philip of Wenatchee, Washington and Paul of Eugene, Oregon) — their integrity, professionalism, personalities, families, and parenting. Whenever my earthly life will end, I am prepared to accept it with equanimity, being grateful for the time that fate has given me on this earth. Hopefully it will end without a brain-crippling interlude, with a "good death," without burdening others. Will there be nothingness or an afterlife of the spirit? I am curious to find out.

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(*) = articles considered by others as important and/or "firsts."

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ABBREVIATIONS

ABC	Airway – breathing – circulation (BLS steps of CPR)
ACEP	American College of Emergency Physicians
ACTH	Adreno-corticotrophic hormone
ACGME	American Council for Graduate Medical Education
AED	automatic external defibrillation
AHA	American Heart Association
ALS	Advanced life support
APSF	Anesthesia Patient Safety Foundation
ARC	American Red Cross
ARDS	Adult respiratory distress syndrome
ARI	Acute respiratory insufficiency
ASA	American Society of Anesthesiologists
ATP	Adenosine triphosphate
AUA	Association of University Anesthetists
BCH	Baltimore City Hospital
BLS	Basic life support (CPR steps A-B-C)
BRCT	Brain Resuscitation Clinical Trial
CA	cardiac arrest
CBF	Cerebral blood flow
CCM	Critical Care Medicine (specialty; journal)
C.E.	Common era
CEM	Center for Emergency Medicine (Pittsburgh)
CEO	Chief Executive Officer
CHP	Children's Hospital of Pittsburgh
CIA	Central Intelligence Agency
$CMRO_2$	Cerebral metabolic rate for oxygen
CMU	Carnegie Mellon University of Pittsburgh
CO_2	Carbon dioxide
CPB	Cardiopulmonary bypass
CPCR	Cardiopulmonary-cerebral resuscitation
CPPV	Continuous positive pressure ventilation
CPR	Cardiopulmonary resuscitation
CRNA	Certified registered nurse anesthetist
CSF	Cerebrospinal fluid
CSSR	Czechoslovak Socialist Republic
CV	Curriculum vitae
CVRI	Cardiovascular Research Institute (University of California, San Francisco)
DOD	Department of Defense

Dr.h.c.	Doctor honoris causa
ECC	External cardiac (chest) compressions (step C of CPCR)
ECG	Electrocardiogram
ECCM	Emergency and Critical Care Medicine
ED	Emergency department
EEG	Electroencephalography
EEH	Eye and Ear Hospital of Pittsburgh
EM	Emergency Medicine
EMOC	Emergency Medical Operations Center (Pittsburgh)
EMS	Emergency medical services
EMT	Emergency medical technician (= EMT I)
ENT	Ear, nose, and throat
FASEB	Federation of American Societies of Experimental Biology
FDA	Food and Drug Administration
FHE	Freedom House Enterprise Ambulance Service
GASP	Group Against Smog and Pollution (Pittsburgh)
GBI	Global brain ischemia
HMO	Health maintenance organization
HS	Hemorrhagic shock
HUP	Hospital of the University of Pennsylvania (in Philadelphia)
ICA	International Cooperation Administration
ICP	Intracranial pressure
ICRC	International Committee of the Red Cross (Swiss)
ICU	Intensive care unit
IOM	Institute of Medicine
INEN	Instituto Nacional Enfermadades Neoplasicas (Lima, Peru)
IPPNW	International Physicians for the Prevention of Nuclear War
IPPV	Intermittent positive pressure (controlled) ventilation
IRB	Institutional Review Board (for reviewing clinical trials)
IRRC	International Resuscitation Research Center, University of Pittsburgh
ITACCS	International Trauma Anesthesia and Critical Care Society
JAMA	Journal of the American Medical Association
JHH	Johns Hopkins Hospital
LRCS	League of Red Cross Societies (international)
LSFA	Life supporting first aid
MAP	Mean arterial pressure
MEDICO	Medical International Cooperation
MMV	Mouth-to-mouth ventilation (step B of CPCR)
MOH	Montefiore Hospital of Pittsburgh
MWH	Magee-Women's Hospital of Pittsburgh
N_2O	Nitrous oxide

NAEMSP	National Association of EMS Physicians
NAS	National Academy of Sciences
NIH	National Institutes of Health
NRC	National Research Council
O_2	Oxygen
ONR	Office of Naval Research
OR	Operating room
PAR	Postanesthesia recovery room
PDM	Prehospital and Disaster Medicine Journal
PEEP	Positive end-expiratory pressure
PI	Principal Investigator
PLS	Prolonged life support
PM	Paramedic (= EMT-II)
PSR	Physicians for Social Responsibility
PUH	Presbyterian University Hospital of Pittsburgh
RCT	Randomized clinical trial
RN	Registered nurse
SA	Suspended animation
SAEM	Society of Academic Emergency Medicine
SCCM	Society of Critical Care Medicine
SCRR	Safar Center for Resuscitation Research, University of Pittsburgh
SNANSC	Society of Neurological Anesthesia and Neurologic Supportive Care
TBI	Traumatic brain injury
UAEM	University Association of Emergency Medicine
UHCP	University Health Center of Pittsburgh (= UPMC)
UK	United Kingdom
UPMC	University of Pittsburgh Medical Center (= UHCP)
U.S.	United States of America
USAMRDC	U.S. Army Medical Research and Development Command
USSR	Soviet Union
VAH	Veterans Administration Hospital of Pittsburgh
VF	ventricular fibrillation
WADEM	World Association for Disaster and Emergency Medicine
WFAP	World Federalists Association of Pittsburgh
WFSA	World Federation of Societies of Anaesthesiologists
WLM	Wood Library-Museum (of the ASA)
WPIC	Western Psychiatric Institute and Clinic (of Pittsburgh)

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