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CASA 新任会长冯鸿辉

2018 CASA PRESIDENT'S REMARK 冯鸿辉会长 新年致辞

By Honghui Feng, M.D.

亲爱的美国华人麻醉医学会会员们，朋友和麻醉界的同仁们，新年快乐！

我代表我们美国华人麻醉医学会（CASA）执行委员会向大家致以最衷心的节日问候。希望各位阖家欢乐，事业蓬勃！并祝愿大家在新的一年里更上一层楼。

非常荣幸成为二 O 一八年度 CASA 主席，荣幸的同时也有惶恐。希望在新的一年里，我能在执行委员会成员、历任主席和全体会员同仁的帮助下承担重任。有大家的信任与支持我也充满信心，定不负使命。

CASA 起始至今从无至有到今天的壮大，承载着我们一代华人麻醉学者成长的艰辛、宽阔的胸怀和远大抱负。 忆当年我们怀揣梦想和对未来的美好憧憬，来到美国这个异国他乡寻求新的生活，探索一个未知世界。 我们来自中国，乃至世界的各个角落，因为共同的事业和理想走到一起。在 CASA 这个平台，茁壮、茁壮、迷茫、喜悦和成功铺垫着我们前进路上的每一方基石。过去十几年中 CASA 在学术和学术交流方面取得了很多辉煌的成就。在加强与国内同行交流合作方面也做出了诸多贡献。

是 CASA 把我们海内外华人麻醉学者志士聚集一堂，探讨学术知识、分享喜乐哀怒。

是 CASA 把我们海内外华人麻醉学者志士聚集一堂，探讨学术知识、分享喜乐哀怒。珍惜、共建这个平台是大家的共同心愿。让我们更加团结，群策群力使 CASA 更具吸引力、影响力和生命力。

新年伊始，我们的工作重点将继续放在扩大国内外学术交流 ，摒弃前嫌加强兄弟协会之间的协作，以及开展和开发新的合作项目上。让我们大家携手并肩，在 CASA 这个平台施展各自的才华、收获付出的快乐，为社会做出新的贡献。

在此特别感谢历任主席及协会骨干对协会发展付出的心血。

冯鸿辉会长个人简介：

1990 年毕业于上海第一医学院，1997-2001 年在哈佛医学院 Beth Israel 医学中心完成麻醉住院医和疼痛专科培训。2005 年起担任康奈尔州 Lawrence Memorial 医院麻醉科主任。

Inside this issue:

Quote of the Month

"Happiness is when what you think, what you say, and what you do are in harmony."

Mohandas K. Ghandi

艺海拾贝

CASA Bulletin 拟主办反映我们工作和生活的绘画，摄影，诗歌和散文比赛，请大家将个人原创作品寄到 *casabulletin@outlook.com*，今后每期会陆续发表。 2018 年底公布获奖者，请踊跃参加。

A hundred times a day
I remind myself
that my inner and outer life
depend on the labors of other people,
living and dead,
And that I must exert myself
in order to give
in the full measure I have received
and am still receiving

——*Albert Einstein*

CASA 新任主编曹锡清

编者按：岁末年初，庆祝接踵而来的圣诞节，元旦和春节，既是亲朋好友聚会和医院举办晚会之际，也是慷慨解囊捐钱送礼之时。我们给自己关心关注的慈善机构和公益团体发出一张张支票，给身边为我们工作的麻醉技工和手术室护士集资送节日购物卡，给一年中曾经帮助过我们的邻居，社区管理人员，同事， 秘书，老师，办公室清洁工，其他科室尤其是外科，妇产科，ENT 和 ICU 关系好的合作医生等等送上一份份感恩的小礼品，似乎要在此时此刻将一年中的恩惠通通的报答。我们给予，因为真诚的感谢，因为我们曾经获取的很多。

CASA 从２００２年底 PGA 会议酝酿成立至今已经走过了１５年，到了青春期。特别是有了微信，将过去不足百人的小团体迅速发展成包括全美华裔麻醉医生在内的三四百人的组织。中国经济的腾飞，麻醉事业的快速发展，信息的高度发达，百姓对医疗健康的追求和很多麻醉同仁的义医奉献促进了近年来中美麻醉交流的日益频繁和深入，过去单打单干的回国讲学越来越多的变成有组织有计划的多学科交流项目。我们感恩，CASA 前辈的辛勤播种耕耘使会员们得以在 CASA 的大树下乘凉。我们思考，在美国完成了住院专科培训，事业有成，家庭兴旺的同时，如何回报，如何给予，如何贡献。

岁末年初，继往开来。既是审时度势总结过去一年得失之际，也是乐此不疲写誓言定计划之时。估计我们都养成了写 New Year Resolution 的习惯，似乎要把过去所有的毛病和不留神多出的肥肉统统在新年里去掉，恨不得将信誓旦旦的决心和高大上的目标张榜示众以观后效。我参加的瑜伽健身馆甚至推出了元月 30 天挑战自我的比赛，有各种 workshops, 插五星，心理咨询，周日集中聊天等活动。其实后来发现很多事不以自己的意志为转移，或者意志本来就不坚定，计划赶不上变化诸

多缘由，很多新年计划最后不了了之。因此，我认为新年计划到底实现多少并不重要，顺其自然，向目标努力就可以了，关键是参与这个过程。毕竟“谋事在人，成事在天”,每个人尽己所能就足以了。

CASA 月刊在过去几位主编唐越，刘恒意和黄建宏领导下已经办的有声有色，小有规模，我新接手还有一段学习磨合过程，希望会员们有足够耐心并及时反馈建议，积极踊跃投稿，献计献策。

CASA 月刊的第一宗旨是真正成为每个会员发出自己声音分享自己故事的舞台，成为我们交流交友共同砥砺前行的平台。CASA 理事们会通过理事专栏向大家汇报他们的工作和正在实施的计划，请看新任会长冯鸿辉博士的新春致辞。前辈榜样们会介绍他们为发展所做出的首任会长海明医师时的成长，他了已故麻醉教授的家首次发表艾写的回忆了麻醉事业的的业绩，CASA 年资最深的王刻关心着 CASA 不辞辛苦找到学先驱艾世勋人。这期我们世勋教授生前录，帮助我们了解那段鲜为人知的麻醉历史，同时为有这样一位早于我们半个世纪的长辈和楷模由衷的骄傲。CASA 月刊将陆续刊登中年人充满汗水阅历的 “励志片”以及年轻人憧憬未来朝气蓬勃的“艰苦创业片”。既有脚踏实地的医教研，介绍新知识新技术的短讯，也有轻松愉快的小资和平衡搞笑的小品。盼望大家八仙过海， 各显神通，主动参与，既是读者也是作者，把咱们的杂志办的风生水起，生动活泼。

第一宗旨是真正成为每个会员发出自己声音分享自己故事的舞台，成为我们交流交友共同砥砺前行的平台

CASA 月刊的第二项宗旨是促进中美麻醉界的沟通和传播先进理念和技能。通过与国内同仁合作写文章，回国讲学，模拟培训，网络教学等各种方式，构建桥梁，互相学习，共同提高。每期我们会以一个亚专科为中心介绍美国的专业进展，热门话题和科研方向，也会选择发表中国的麻醉动态，国内麻醉专家和前辈事迹，麻醉研究等等，帮助 CASA 会员了解中国的近况，同时帮助国内同仁了解美国麻醉，并为来美参观进修解决问题和提供方便。

祝大家新年愉快，身心健康，家庭幸福和事业兴旺！

第二项宗旨是促进中美麻醉界的沟通和传播先进理念和技能

Trivia: Hot Topics

1. Medical drones geared for take off: https://www.healthcaredive.com/news/medical-drones-geared-for-take-off/512524/
2. General anesthetics mechanism of action: **How Do Anesthetics Work?** In Surprise, Research Suggests Lipid Bilayer Uninvolved, Challenging Prevalent Theory. http://www.painmedicinenews.com/Web-Only/Article/12-17/How-Do-Anesthetics-Work-/46508/ses=ogst?enl=true
3. The WSJ article on Motherhood in workplace: https://www.wsj.com/articles/if-you-want-women-to-move-up-you-have-to-accommodate-mothers-1515110849?shareToken=stade30d68e851400cbda3debdd870a1e1&reflink=article_email_share
4. Independent doctors get sweet deal under GOP tax cut bill: http://www.modernhealthcare.com/article/20171220/blog/171229992

欢迎大家对以上热点问题讨论，和同道们分享你的观点！来稿 *casabulletin@outlook.com*.

Person of the month: Professor Shih-Hsun Ngai(艾世勋教授)

美国华人麻醉学先驱 – 艾世勋教授

王海明 MD， CASA 首任会长 原稿 曹锡清 MD，改编

CASA 首任会长王海明

1945 年第二次世界大战结束，中华大地举国欢庆。但是，战后百废待兴，千百有志学子负笈西学，留学美国者众。

艾世勋（ Shih – hsun Ngai, M. D. 1920 – 1999）是位杰出的美籍华人麻醉学和药理学教授，可以说是麻醉界的泰斗之一。1920 年 9 月 15 日生于中国武昌，家中行八，为最小，父亲是高中教师。艾世勋毕业于中央大学医学院，1946 年考取官费奖学金赴加拿大多伦多大学学习麻醉。同期留美学习麻醉学的还有新中国麻醉界的三位鼻祖 Drs. Jones J Wu(吴珏),Deyan Shang（尚德延）和 Yung Shieh（谢荣），他们分别于 1949 和 1950 年回到新中国并建立了仿效美国的麻醉系和住院医训练体系。

艾世勋医生毕业后辗转从多伦多来到芝加哥，1947-1948 年在 Michael Reese 私立医院任麻醉医生，一年内做了 1200 台手术麻醉。那时他精力旺盛，对科研有浓厚兴趣， 上午做麻醉临床， 下午和晚上就到心血管研究所研究升压药的药理学。一年后又乘火车到纽约哥伦比亚大学医学院做住院医生，在享誉世界的 Apgar 教授指导下工作，是哥大也是美国的第一位 24 小时进驻产房的麻醉住院医生。期间继续科研，是哥大第一个研究去甲肾上腺素的研究员。1948 年与志同道合的王雪华医生喜结连理。1949 年七月留哥大成为主治医生。因为是外国医学院毕业生，尽管他通过了全美医生执照考试，但在纽约，之前没有先例允许外国毕业生在纽约行医。他历尽周折，最终 1953 年通过了纽约州的医生执照考试。

1953-1955 年他来到美国驻加勒比海的陆军医院任麻醉科主任，亲眼见证了原子弹实验。1955 年回到哥大长老会医院， 1965 年升任麻醉学教授。1969 到 1973 年任哥大的麻醉系主任。1974 年荣获药理学教授。 曾于 1968-1977 任十年 《Anesthesiology 》编委。1976 年刊文《科学》阐述一氧化氮镇痛作用。艾世勋教授研究兴趣广泛，同仁称颂他 “ His work always seemed a step ahead of his time.”

1988 年退休后，艾世勋教授接连受聘于台大医学院，台湾荣总医院，帮助台湾创立医学科学院（相当于美国的国立卫生院 NIH），荣为台湾科学院院士。艾世勋教授品德高尚，热忱助友，潜心科研，堪为师表。他风趣幽默，关爱家人，有爱妻王雪华（哥大药理学终身教授），儿子艾约翰研究神经科学现任加州大学伯克莱分校神经生物学教授，二女习学历史和理疗。

艾世勋前辈为世界的麻醉事业做出了巨大贡献，他是我们美籍华裔麻醉医生的榜样，是我们中华民族的骄傲。感谢王雪华教授的厚爱，今天我们有幸和 CASA 会员分享他写的回忆录（由于罹患肺癌，回忆录只写到 1994 年），从中得以窥见美国麻醉发展的一段鲜为人知的历史，借此方式纪念和缅怀艾世勋教授。从连续椎管内麻醉到乙醚，铜壶的发明，再到氟烷诱发的肝炎，哥大的恶性高热死亡，阿片镇痛研究等等，麻醉科学从险象丛生到今天的安全质控，每一步的发展都经历了血泪斑斑的惨痛教训。掩卷沉思，我们任重而道远。麻醉药物的作用机制至今仍是谜团，中国的麻醉研究，麻醉安全和住院医生培训还与先进国家距离尚远，产科麻醉刚刚起步，新生儿麻醉还在摇篮。只有继承艾世勋教授的遗志，不断地为麻醉事业奋斗，积极回报祖国，我们才不愧于历史赋予留美麻醉医生承上启下的机遇和责任。

REFERENCES

Robert M. Epstein M.D. with Members of the Ngai Family: In Memory of Shih-hsun Ngai, M.D. (1920 - 1999). Anesthesiology February 2000 PP 310- 311

摘要：

“SHIH-HSUN Ngai, a distinguished anesthesiologist and pharmacologist and former Editor of ANESTHESIOLOGY, died of lung cancer on July 8, 1999, shortly before his 79th birthday. He was born in Wuchang, China, on September 15, 1920, the youngest and eighth child of a high school teacher. A graduate of China’s Central University Medical School, he spent almost his entire

professional career at Columbia University's Department of Anesthesiology after winning a competitive government fellowship to study anesthesiology abroad. At Columbia, he rose to be Professor of Anesthesiology in 1965 and of Pharmacology in 1974. He spent 2 years in the US Army (1955-1957) and chaired the Department of Anesthesiology from 1969 -1973. He was a distinguished clinician, scientist, teacher, and mentor.

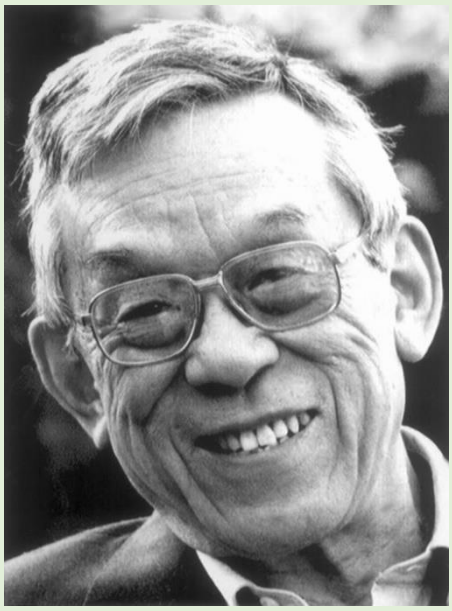


FIGURE 1: PROFESSOR SHIH-HSUN NGAI

Dr. Ngai began his research even as a resident, doing pioneering studies on central nervous system control of respiration with Dr. S. C. Wang, the renowned physiologist, that are classics. Later, at Columbia and with collaborators elsewhere, he studied the interactions of biogenic amines with general anesthetics. He also studied narcotics, narcotic antagonists, and the participation of opioid receptors in anesthetic-induced analgesia. His work always seemed a step ahead of his time. An article in Science in 1976 about the analgesic effect of nitrous oxide stimulated controversy, which persists to now.

For many years, Dr. Ngai was affiliated with the biomedical and anesthesiology communities of Taiwan. He was Visiting Professor at National Taiwan University in 1990, the Tri-service Hospital in 1992, and the Veteran's General Hospital in 1994. During these visits, he upgraded the practice, teaching, and research of anesthesiology in Taiwan. He also was a founding member of the National Health Research Institute, which is similar to the National Institutes of Health in the United States.

Dr. Ngai was a man with great integrity in both his personal and professional lives. He had a passionate curiosity informed by keen observation. His high standards, unwavering sense of fairness. and commitment to do the right thing, along with a quiet humor, touched everyone around him. Prestigious honors included the Commonwealth Fellowship (1964) , election to the Academia Sinica (Taiwan, 1972), the National Research Council Anesthesia Committee (1961-1970), and several National Institutes of Health Study Sections (1962-1982). Elected to the editorial board of ANESTHESIOLOGY, he served for 10 years (1967- 1977), having been asked to continue beyond the normal term because of his special contributions. Dr. Ngai is survived by his wife, Dr. Hsueh Hwa Wang, Professor Emeritus of Pharmacology of Columbia University; two daughters, Mae, Assistant Professor of History at the University of Chicago; Janet, a physical therapist in Massies Mill, Virginia; a son, John, Associate Professor of Neurobiology and Head of the Neurobiology Graduate Program at the University of California at Berkeley; five grandchildren; and memories of a joyous 50th wedding anniversary family reunion on Christmas, 1998."

Figure2：1998, with entire family.
Back row: Baikal, David;
Mid row： Lisa, John, Janet. Hsueh-hwa, Shih-hsun, Mae, Michael;
Front row：Julie and Stanphie with dog

《 Memoir of Professor Shih-hsun Ngai》王雪华教授提供，衷心感谢！

MEMOIR OF S. H. NGAI, M.D. （1920~1999）

Before Presbyterian

I graduated from the National Central University School of Medicine, China in 1944.

FIGURE 1： FORMER NATIONAL CENTRAL UNIVERSITY IN NANKING

As a clerk and an intern before graduation, I administered anesthetics. It was mostly open drop ether. During the winter months, we had to have open fire to keep the operating room warm. Luckily, we had no disaster. For two years, before I came to the United States, I was a surgical resident and continued to give anesthetics as well as to scrub and assist in operations. I remember we used isobaric spinal procaine, with barbotage, to provide analgesia for abdominal operations as well as mastectomy and thyroidectomy! Looking back, it does not seem possible.

As a senior intern in Toronto from 1946 to 1947 I administered anesthetics as well. In addition to spinal analgesia, we used closed circuit cyclopropane with "intercostrin." Then, on to Chicago at the Michael Reese Hospital from 1947 to 1948. If one was not on call, one got relieved at 1 or 2 p.m. I was not allowed to use curare during the first six months to learn how to give anesthesia. The surgeons there were so fast I did about 1,200 cases that year. Five or six tonsillectomies in the morning, or the same number of transurethral resections of prostate, each took about 30 minutes. We had no problem with water poisoning. One morning I remember particularly well. One surgeon did a colectomy, a cholecystectomy, a radical mastectomy and a gastrectomy, finished about 1 p.m. They did not have a recovery room then. I had to send the patients to their rooms and hurry back to start the next case. Everything below the diaphragm was taken care of by continuous spinal analgesia using a silver needle, bent to flush with the skin and protected with a cut out mattress. Since I took at most two calls per week, I got bored and asked permission to go to the laboratories. Dr. Louis Katz of the Cardiovascular Research Institute there took me in and let me work with a Belgian cardiologist. He came in the afternoons and worked into the evenings, which suited me fine. We used Langendorf's preparations to study the effect of methoxyephedrine, we got results which were never published. We also did clinical research, sort of. One afternoon, my chief, Dr. Bernard Stowski, and I proceeded to try to find out why repeated intravenous injections of methoxyephedrine loses its pressor effect. I laid on the operating table in the anesthesia room and he kept on taking my blood pressure. After 15-20 minutes, he told me to get up because my blood pressure fell to 70/50. I was relaxed but fully conscious. (Therefore, in later years, I kept telling residents: "do not reach for ephedrine every time the blood pressure falls a little.") Then, the chief laid down. His blood pressure did not fall, and I gave him methoxyephedrine intravenously. The next day, he looked tired. He could not sleep the night before because of the "high" from the drug. That was the end of the study. The experience at Michael Reese must have been my introduction to laboratory and clinical research.

Presbyterian Hospital from July 1948

After finished my last case at Michael Reese on June 30th in the afternoon, I got to New York City early in the morning on July 1 after an overnight ride on the Grand Central rail. I left my luggage there, came uptown and

reported to Dr. Apgar. She put me to work right away. I gave anesthesia for two tonsillectomies without endotracheal tubes in the morning and a radical mastoidectomy with Dr. Woodman in the afternoon. He took a long time. When I finally finished with the case late, Dr. Apgar told me that I was to take the obstetrical anesthesia call that night, the very first at Presbyterian Hospital. I said that I must get my luggage and check into Bard Hall. I could not take the call. As usual, she smiled and let me off. The obstetrical anesthesia call started on July 2nd, 1948, uninterrupted to the present. During my first year as a resident, I did about 1,100 cases. The schedule was such that one did gynecologic cases in the mornings and hurried over to GU, where Dr. Cahill or Dr. Fish, all scrubbed and waited for the anesthetist, or to the Eye Institute, where Dr. Dunnington or Dr. Reese waited. Then, when one was on call, there was only one resident in the hospital. The cases lined up and one did them one by one until the morning. We got the next day off though. After a few months, I asked Dr. Apgar for permission to do something in addition to clinical work. She got me to work with Drs. Goldenberg and Pines on norepinephrine. When I was ready to present the results to the staff, Dr. Goldenberg said he would do it. So, he did. I believe that was the very first clinical study of norepinephrine at Presbyterian.

FIGURE 2:1949, AFTER MARRIAGE TO HSUEH-HWA WANG IN NY CITY. THE GW BRIDGE HAD ONLY ONE LAYER THEN

The residents were paid every two weeks, about $50, with meal tickets but no room. I got married and lived in a room on West 164th street off Broadway. My wife, Dr. Hsueh-hwa Wang, after one year of fellowship with Dr. Paul D. White at MGH, was working as a medical resident in Hackensack Hospital. We were poor but had a great time. That was the year I learned to use the Flagg can and the "autoanestheton" to deliver ether vapor, among other things. I believe the autoanestheton is now in the Wood Museum. My first caudal analgesia using a catheter ended in disaster. I pulled the catheter back against the needle and sheared off the catheter. We had to get a neurosurgeon to remove it. It was done one evening with Dr. Jack Frumin on our own, without attending's permission. I do not remember we got scolded the next day or not.

Looking back, the residents at that time were left pretty much alone on our initiatives, a swim or sink situation. There were no attendings in neuro, babies, eye institute, obstetrics or anywhere else except the 18th floor. Imagine that I did a case of tracheoesophageal fistula in the middle of one night by myself? The surgeon was Dr. Richmond Moore. For those who remember him, that was no mean trick, especially without an endotracheal tube. When the case was discussed during the next surgical round, I was praised by Dr. Moore for the wonderful job I did. I did not know what to say as I was a lowly resident. But a lowly anesthesia resident knew a few things. One afternoon I was giving anesthesia to a patient with acute abdominal pain. The surgeons, one since died and the other is now a senior attending, looked puzzled and stated that it might be military tuberculosis. I looked over the screen and said that it was acute pancreatitis. They looked at me and being polite, essentially said, "what do you know." But the surgical pathologist confirmed my diagnosis.

I became an "attending" and an Assistant in Surgery on July 1, 1949, proud of my long coat. Dr. Papper arrived soon after on September 6. His first years were rough going. The equipment at our disposal were primitive. After an explosion during cyclopropane anesthesia, for which the anesthesiologist was charged with manslaughter, every time we planned to use this gas, the floor had to be

checked for conductivity and one had to wear a metal chain connected to the patient, the table and the machine to prevent the buildup of static electric charge. There were no piped gases, no pin indices, the copper kettle was not designed (Dr. Lucien Morris did it for the use of chloroform, more about it later) and marketed until the mid-50s. We used in-circuit vaporizers. Because of the lack of safety measures and busy workload, we had a few disastrous incidents, such as empty oxygen tanks, misplaced tanks and ether overdose. In addition to carrying D tanks of gases from one end of 18th floor to the other, one had to change the soda lime in small cannisters frequently as the current form of cannister was not available until Dr. James Elam designed it. We had one Cambridge explosion proof electrocardiogram. It was heavy and clumsy, hard to push around. Soon afterwards, we had the Electronics for Medicine EKG monitors, nick named "bullets" because of their shape. But still we had to squeeze the rubber bulb every few minutes to take blood pressures and the rubber bag to breathe for the patient. We developed strong hands by doing these for hours day in and day out. Also, we were told to keep one finger on the pulse, facial or temporal artery, while holding the face mask with the other hand. Each of the residents was issued three endotracheal tubes, small, medium and large. We made endotracheal tube cuffs using pentose drains and thin urinary catheters. We washed our endotracheal tubes and carried them in our pockets. During those years, not too many patients had endotracheal intubations during anesthesia. A radical mastectomy could last 7-8 hours. If one was not careful, the patient could develop pressure sores over her face. Several patients did. Looking back, I do not know how we did it, but we did it. We did not have major malpractice suits either, except paying for broken teeth as we did not have succinylcholine either. One had to induce anesthesia using ether, deepen the anesthesia and hurry to intubate the trachea before the anesthesia lightens. My record time from induction to endotracheal intubation was about 10 minutes.

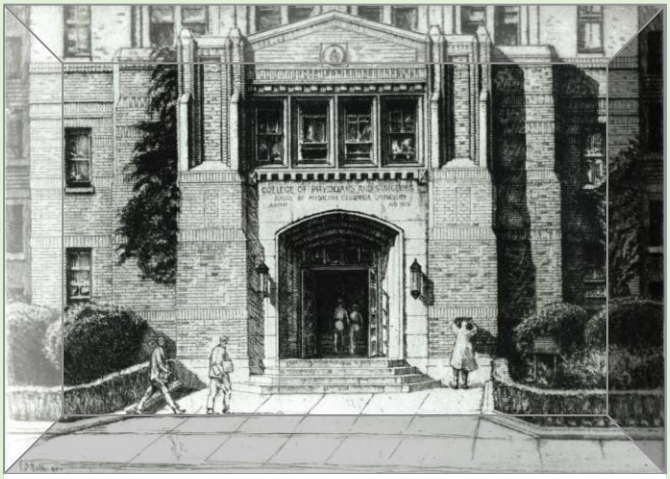


FIGURE 3: COLUMBIA UNIVERSITY P &S BUILDING

Starting in 1950, Dr. Papper arranged for me and Dr. Frumin to do part time research in the laboratories of the Department of Pharmacology. Dr. Harry van Dyke gave us about five feet of bench space in the students' laboratory, a mercury manometer and a smoke drum. He admonished us not to spill any mercury. The first work we did was to find out about the tachyphylaxis of ephedrine after repeated administrations in dogs, carried over from my Michael Reese days. We presented our work at one of the Federation annual meetings in Atlantic City. Dr. Papper sent us a telegram to wish us luck. Can one imagine that any chief would do such a thing nowadays? We finally wrote the paper. We fought about the writing, revised it about a hundred times. One day Dr. Frumin lost the original data and manuscript on the train going home. After that my rule is that no original data can ever be removed from the laboratory. Although we had the blessings of Dr. Papper, a few of the attending staff were not so kind. The saying was that "the sons of guns (the exact word is changed) are having fun on the payroll." Laboratory research by anesthesiologists at the Columbia-Presbyterian Medical Center started on this note and went on with the initial financial help of Mr. Wrightsman, who gave Dr. Papper $25,000 for research and development. That was the seed which later grew to fruition with grants from the federal government.

> As a foreign medical graduate, I had no license to practice medicine.

As a foreign medical graduate, I had no license to practice medicine. Soon the hospital authority caught up with my situation. I passed the National Board, but the New York State Board of Medical Examiners would not

recognize it. I went to see the secretary of the Board and was told that I must go back to school for two years. With the help of Dr. Papper, we bargained it down to one year, after which I took the State Board examination and passed it. A few years later the same secretary was found guilty of selling questions for a few thousand dollars a set, a lot of money at that time. He was sent to jail and I hope he is still there.

As soon as I got my license in 1953, I received greetings from the President of United States. I "volunteered," went to basic training in San Antonio. After that I was assigned as the chief of Anesthesia and Operation Section of the Rodriquez Army Hospital in San Juan. I had a rewarding year as the only anesthesiologist of the Caribbean Command, warmly received as they asked for but did not have an anesthesiologist for many years. Before I arrived, they did not even perform tonsillectomies, let alone craniotomies. With the help of two nurse anesthetists, we did everything, referred very few patients to the "mainland," as they called it. I was supposed to be there for two years. But the army ordered me back to the states and assigned me to the Walter Reed Army Institute of Research. I had three anesthesia residents after their first year of clinical training as research fellows. During those years, the country and especially the military were very much concerned about atomic bomb and mass casualties. I was assigned by the commanding officer, Colonel Harvey Slocum, to find a way to provide analgesia for people injured by the bomb, if one should be dropped by the Russians. As he was trained at Wisconsin, he believed that chloroform is the answer. With the help of Captain Henry Green and a specialist, drafted from DuPont, we started off by altering a Duke inhaler, designed by Dr. Ronald Stevens, then at the Duke University. The Duke inhaler, filled with trichloroethylene, was being used for the relief of labor pain, strapped to the wrist of the parturient. She would breathe from it during uterine contractions. When she had too much of the anesthetic, she would drop the inhaler and recover until the next uterine contraction. Using a mass spectrometer, we found that the vapor concentrations varied with ambient temperatures and the position in which the inhaler was being held. If the mass spectrometer was not working properly, the soldier would kick it to make it work. Anyway, we tinkered with the port to adjust for the difference in vapor pressures between trichloroethylene and chloroform. Then I met Mr. Frazer, his major business at that time was supplying cosmetics to beauty salons in Toronto. I asked him to alter a Tritec, a draw over, temperature compensated vaporizer for trichloroethylene. It was used in England by midwives for home deliveries and the British air force dropped them with paratroopers during the North African campaign, World War II. So, we had a Chlorotec, I think it is the only one in the world. We calibrated it with an infra-red analyzer and used it on dogs to test for analgesia and on ourselves. Even with analgesic concentrations, the dogs developed hepatic necrosis. When I breathed from the vaporizer, I was warm all over and felt that I was in heaven. Both Dr. Green and I had retention of rose Bengal afterwards. So that was the end of chloroform.

Pigs are difficult to anesthetize, and I could hardly see its larynx even with the longest Miller blade.

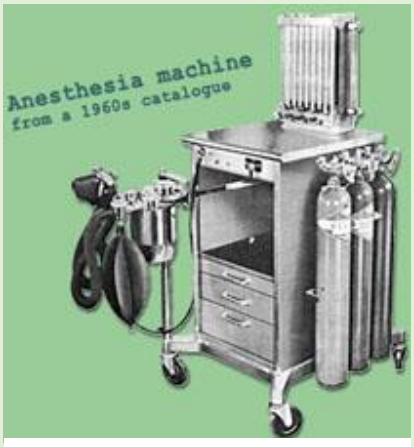


FIGURE 4: MORRIS COPPER KETTLE

During the last half year of duty in the army, the military decided to have a field test of atomic bomb using animals to simulate mass casualties. To use one thousand dogs was out of the question. So, we used pigs for practice and ordered piglets in March for use in May or June. Pigs are difficult to anesthetize, and I could hardly see its larynx even with the longest Miller blade. They die easily from pentobarbital as they have small lungs. But they are very resistant to infection. Anyway, a large team went to Camp Mercury, near Las Vegas, and set up a MASH unit for pigs. We waited for a few weeks for the right weather and wind. I was one of three anesthesiologists who witnessed an atomic explosion. It was awesome. That

ended my army career and I returned to Presbyterian in 1955.

The clinical scene did not change very much. We had copper kettles and started to use halothane. The first study I did of some significance was to measure ether vapor concentrations from the copper kettle, using a Beckman DB_2 oxygen analyzer, which we still have. It is of no surprise now that at room temperature (25° C), the ether: oxygen ratio was two-to-one. Only when the temperature of the kettle dropped to 15° C because of loss of heat from vaporization, the ratio became one-to-one, as published by Dr. Morris. One hundred percent more ether vapor was being delivered to the circuit, compared to what was thought to be delivered. Quite a few patients were being overdosed. I wrote a letter to the editor of Anesthesiology about my findings, it got published. Dr. Morris wrote to Dr. Papper and complained: who is this guy Ngai? Then I insisted that the out-of-circuit vaporizers made by Ohio and Forreger must have thermometers in the liquid phase. They thought I was crazy. We do not have to worry about ambient temperatures anymore as all vaporizers we use now are temperature compensated, just as the Tritec and Chlorotec. Recently, I heard Dr. Pierce pronounced that the kettle type of vaporizers is dangerous. They are if one does not know how to use them, just like everything else.

FIGURE 5: NEW YORK-PRESBYTERIAN/COLUMBIA UNIVERSITY COLLEGE OF PHYSICIANS AND SURGEONS

Soon, in a year or two after we started to use halothane, case reports of "halothane hepatitis" appeared in the literature. We had our share of cases. We had one resident who got ill with hepatitis every time he used halothane. He went to Yale University and Dr. Nicholas Greene administered halothane to him as a challenge. He developed hepatitis, confirmed by liver biopsy. That was when the National Halothane Study was initiated, sponsored by the National Academy of Sciences-National Research Council. We, Dr. Daniel Pino and I, started a pilot prospective study at the Presbyterian Hospital. All patients were randomized to be anesthetized with halothane or "other" anesthetics, according to the last digit of their unit numbers. Hepatic function was followed by measuring serum SGOT, manually. The technician, who has overseen the hospital's clinical laboratories for many years now, got cross-eyed. Ayerst bought us an automated analyzer, the very first in Presbyterian before the current SMAs. The staff were very unhappy because they no longer had the prerogatives of choice of anesthetics. Anyway, we proceed. I lost many nights of sleep over the study. Then, after about 400 cases, one patient died of massive hepatic necrosis within 24 hours following halothane anesthesia for a radical mastectomy, her second one within a week after a breast biopsy, also under halothane anesthesia. I talked with Dr. Papper and essentially said that we must stop the study. Our decision to stop the prospective study caused an uproar with the National Halothane Study Committee. The committee met in the Boardroom on the first floor of the hospital. After a few more meetings, sometimes in Washington, the retrospective study was designed. It was a compromise. The study was finally completed, and results published. We worked hard and spent a lot of money, but nobody was really satisfied. The study was and still is being quoted even we did not conclude that halothane is or is not hepatotoxic. The only eye opener was that the post-operative mortalities differed almost ten folds among participating hospitals, adjusted for risks and magnitude of operations. We considered to pursue this issue as to why there were such big differences. The proposal was dropped because the issue seemed "sensitive" at that time. Now, we have "peer review" and "quality assurance." But, the latest I heard is that there still are differences in post-operative mortality among hospitals in the United States and Canada which participated in the Anesthesia Outcome Study. Things did not change over more than 20 years, perhaps because we are doing more difficult operations in sicker patients.

In the laboratories, we are doing more "sophisticated" studies, progressed from using smoke drums to Grass polygraphs, measuring blood pressures with

transducers. But we always calibrated the transducers with a mercury manometer every day. Our studies on the respiratory centers, started before I went to the army, finally got published in the American Journal of Physiology, altogether four papers. They caused quite a stir. During those years, the word "center" was a dirty one. Eventually our concept was accepted and quoted in physiology texts. Later, Dr. S. C. Wang and I wrote a chapter on central control of respiration in the Handbook of Physiology, published by the American Physiological Society. Another version is now in the revised Handbook, written by Curt von Euler of Stockholm. I went on to study the effects of narcotics and anesthetics on respiratory control, neuromuscular transmission with Dr. Ronald L. Katz, respiratory effects of anesthetics in surgical patients with Dr. B. Raymond Fink. We used an Electronics for Medicine monitor to measure diaphragmatic muscle activity using shielded needles, inserted percutaneously. There was no Institutional Review Board, but we stuck the needles into each other's chest before we used them on patients. Looking back, it was very exciting and great fun, involving seven days a week of work. But, interesting as it was at the time and may be still is from the point of view of basic concept, respiratory control is perhaps of little practical importance now, as almost every patient is being mechanically ventilated during anesthesia.

FIGURE 6: PHARMACOLOGY RESEARCH LAB

With the financial support from the National Institute of General Medical Sciences, we had an Anesthesia Research Center and an anesthesia research training grant. Both lasted for many years. We were productive in terms of research in animals and in man, involving many areas of inquiry, such as acid-base balance, anesthetic uptake, regulation of circulation, circulatory effects of anesthetics, drug metabolism, obstetrical anesthesia, perinatology and many others. We trained residents in research after they finished two years of clinical work. A good number of our former research trainees are now prominent leaders in the specialty.

One study Dr. Robert M, Epstein and I started but never got any place was the measurement of MAC in mice. After reading and having heard Dr. Pauling about his theory of narcosis, we thought if anesthetics form hydrate with water, then anesthetic requirement should change if we exchange body water with heavy water, deuterium oxide. The energy of bonding between anesthetics with hydrogen and deuterium should be different. We got heavy water, quite expensive, and let mice drink it. A technician in Dr. Rittenberg's laboratory (Biochemistry) measured the deuterium oxide contents in these mice, using a homemade mass spectrometer. After about 60-70 percent of the body water was exchanged with deuterium oxide, the mice became aggressive and ate each other. We would find dead mice half eaten when we came to the laboratory in the morning. Then, we thought it might be easier to grow paramecia in heavy water. We had a hanging drop chamber with inflow and outflow ports made by Arnold Lee in the Medical Instrumentation Laboratory in Fort Lee. It is still somewhere in the laboratory (Black 711). But we did not get around to do the study as I went to Sweden on my sabbatical leave.

I took my first sabbatical leave in the Department of Pharmacology, Karolinska Institute. For eight months I worked with a medical student (a MK, Medical Candidate) when he was doing research toward his M.D. degree (equivalent to our Ph.D. degree). We implanted a catheter in the aorta to measure blood pressure and electromagnetic flowmeters for aortic and femoral blood flow in dogs. A probe, made in their own electronic shop, costed about five crooners a piece, less than a dollar. When the implanted probe did not work, the engineer was there in five minutes to fix it. We studied the sympathetic cholinergic vasodilation in the skeletal muscle and the "defense center" in the hypothalamus and rostral midbrain. When this area was stimulated electrically with a chronically implanted electrode, the hemodynamic responses were very much like what we see now during isoflurane anesthesia, increases in cardiac output and muscle blood flow. Using this model, we studied the effects of several general anesthetics on reflex and central control of circulation, published in the Journal

of Pharmacology and Experimental Therapeutics, and Acta Physiological Scandinavia. I planned to continue this line of work upon my return but never did. Instead, I started to learn about the biochemistry of monoamine neurotransmitters, norepinephrine and serotonin, from Dr. E. Costa, who came from Dr. B. B. Brodie's laboratories at NIH and joined our Departments of Pharmacology and Neurology, worked in the Parkinson Foundation laboratories.

Dr. Papper and I wrote the three-part series on Medical Progress -Anesthoesiology, published in the New England Journal of Medicine.

When I returned from Sweden in 1965, the Black Building was just completed. The anesthesia laboratories occupied half of the seventh floor, where we still have the same space as before, less because we lost a laboratory for survival surgery. My assigned space was not meant for biochemical work. So, we bought two heavy benches. It is still the same as when we did catecholamine and serotonin turnover in the brain and heart in rats, using radiolabeled precursors, as affected by anesthetics and other drugs. One study involved the effects of LSD in serotonin turnover in rat brains. We wanted to know why LSD makes people crazy. In the middle of our study, we run out of LSD. Sandoz, because of the bad publicity, gave all its stock of LSD to NIH. My collaborator, Dr. Costa, called his friends in NIH. Yes, they had it. But how to get it to New York was another story. I got Dr. Papper to be the unknowing courier as he happened to be in Washington at the time. He carried the little vial home and we completed the work, published in Science. I never told Dr. Papper about it.

We made some contributions in the management of Parkinsonian patients in the perioperative period. Dr. Yard, then the Director of the Parkinson Foundation, always stopped DOPA therapy before anesthesia. I anesthetized two such patients and observed that they were miserable after DOPA was discontinued. In the laboratory, Dr. Costa and I studied the kinetics of DOPA and dopamine in rats. With our results, we advised Dr. Yahr and neurologists not to stop DOPA before anesthesia and continue the medication as soon as possible after the operation. These patients had a much better perioperative course since then.

Clinically and academically, we made some progress soon after Dr. Papper's arrival. We built the recovery room from a surgical amphitheater which occupied the 18th and 19th floors. We started to have attending coverage in the "out services." It was during this period when **Dr. Apgar devised the famous "Apgar score**," Drs. Ernest Sananitre, Herbert Rackow and Robert M. Epstein pioneered the study of anesthetic uptake in the Babies Hospital, Drs. Herman Schwartz, Papper and I wrote the Manual of Anesthesiology for Residents and Medical Students, and Dr. Papper and I wrote the three-part series on Medical Progress -Anesthesiology, published in the New England Journal of Medicine. Just before he left, he laid the ground work for the renovation of PH 14th floor East to an intensive care unit.

During one of those years, we had our first recognized case of malignant hyperthermia. A young man developed high fever (42° C) at the end of an operation on his back. He died of cerebral edema and massive hemorrhage from coagulopathy. This was the time when the Toronto group published their first series of cases in the Canadian Anaesthetists' Society Journal, which was not in our hands yet. I was the attending and was sued by the widow. Many years later, the case was settled before a jury trial ended. It was the only time I was sued. To answer questions by the plaintiff's lawyers on the stand was not a pleasant experience. An unusual event happened during the weekend following the death of my patient. Five patients developed hyperthermia of various

FIGURE 7:SUED BY A WIDOW OF MH PATIENT.

magnitude in the recovery room. Dr. John Ryan and others looked for reasons of this "epidemic." They found none but proceeded to change all stocks of intravenous fluids and injectables throughout the hospital before the work started again on Monday, a tremendous task. I was away on vacation and did not know about it until I returned a few weeks later.

Dr. Papper left Columbia-Presbyterian in 1969. He asked me to go to Miami with him, but I did not. I acted as the chairman first and then became the chairman until 1973. I do not wish to recall events during those four years. I did not like the job in the first place. I had troubles with my back and eventually had an operation to remove two herniated disks with a spinal fusion. The first day I came back to work after my operation, I went to the Dean, Dr. Paul Mark, and told him that I wish to resign from the chairmanship. That was in 1972. The search committee found Dr. Bendixen, but he did not come until September 1973.

I was elected to the editorial board of Anesthesiology in 1968. I served for ten years

One thing during those years worth mentions. I was elected to the editorial board of Anesthesiology in 1968. I served for ten years, with changes of editors-in-chief from Drs. Leroy D. Vandam, Arthur S. Keats, Nicholas M. Greene to C. Philip Larson. Each time the board held election for the next editor-in-chief (in camera, like election of the Pope), I refused to be considered. Even so, two manila envelopes arrived on my desk every week on regular basis. We had six members, thus, the job of the editor-in-chief must have been tremendous, especially in answering the outraged authors whose papers were rejected. My additional duty as an editor was to screen and approve advertisement copies before they could be published. I had no problems with drugs, being regulated by FDA. But I had to ask for revisions of ads for equipment when unfounded or outrageous claims were made, the "best" or the "only." I would ask for supporting data. Without supporting data, the ads did not get published even the charges were high, an important revenue for the publisher and the American Society of Anesthesiologists. I did this anonymously through a secretary in the office of the publisher. I would never consent to deal with the companies or the advertisement agencies directly, no matter what. It was an interesting and challenging job, involved hard work but rewarding in terms of learning new subjects not in my own area of interest.

The first thing Dr. Bendixen did upon his arrival in 1973 was to remove all flammable gases and vapors from our stock. Ethylene is a gas even less soluble than nitrous oxide, with a blood/gas partition coefficient of 0.17, and slightly more potent, a good analgesic as well. I missed it but the progress in patient safety and monitoring was more important. As our chairman, Dr. Bendixen did many things to improve patient care in the operating rooms, intensive care units, teaching of medical students and residents, recruitment of better staff and research. Unfortunately, when we applied for the competitive renewal of our Anesthesia Research Center grant, our fourth five-year period of support, the National Institute of General Medical Sciences changed gears and formulated new policies and guidelines for research centers, as well as the review process. There had to be a "theme." As our research program was and still is involved with many different areas, we could not come up with a "theme" good enough to qualify for funding. We had approval and funding to continue our research training program with a priority score less than 110. NIH changed its rules also, a research trainee had to spend two years in a basic science department. Even with a joint appointment in the Department of Pharmacology, I was told that I could not sponsor a trainee. We had the grant, but at that time no one who finished two years of clinical training wanted to spend two more years in the laboratory. We had to return the money to NIH after two years.

It was not until the spring of 1975 when the department became truly independent, after one and one-half years of hard work by Dr. Bendixen. Many of our residents completed their training in medicine, pediatrics or surgery before they started their anesthesia training. Research trainees are now willing to spend two years in the laboratory. Everything seems better than ever before.

Our collaboration with Dr. Costa's group continued after he went back to the National Institute of Mental Health. Dr. Finck and I would do some ground work in our laboratory in the Black Building and went to his laboratories located in the St. Elizabeth's Hospital, Washington, D.C., to do the finite experiments. We infused deuterated phosphocholine intravenously in rats to measure the acetylcholine turnover in the brain as affected by general and local anesthetics, using gas chromatography/mass spectrometry. I ordered an ounce of cocaine and had to wait for six months before I received it. It costed only $22.00. Dr. Finck and I made about five or six trips a year to Washington for almost two years.

FIGURE8: ROCHE INSTITUTE

During Dr. Papper's and Dr. Bendixen's tenure as our chairmen, I was involved on and off with the NIH as a consultant. My first assignment was to review research training grant applications. A committee was formed to review applications from departments of anesthesiology, surgery and radiology. Each application required a site visit. So, I was travelling a lot, in addition to visiting departments of anesthesiology, I also visited departments of surgery and radiology. It was a ludicrous arrangement. What do I know about radiology, let alone research training in that field. I travelled frequently with Dr. Edward Churchill, then the Chairman of the Department of Surgery at MGH, a gentleman in the truest sense. One time, we visited the laboratories of the Department of Surgery at the University of California, Los Angeles, where a Japanese, Dr. Terasaki, was working on tissue typing using human hair. That was the beginning of research laying the ground for human tissue and organ transplants. Later, Dr. Terasaki did a booming business in settling paternity suits.

After I finished my term on the editorial board of Anesthesiology in 1978, I was invited by NIH to become a member of the Surgery, Anesthesiology and Trauma study section. For four years, the study section met three times a year, each time we reviewed 60 to 100 applications, all in two days. Unlike reviewing papers, the work was even more demanding. The primary and secondary reviewers sealed the fate of the proposal. The responsibility was much greater than that of accepting or rejecting a manuscript. A rejected paper always got published somewhere else. In the case of a grant application, someone's career was at stake. As with the editorship, I learned a lot too. This time, it was learning about things people are planning to do, not that I understood everything, such as genetics and immunology for tissue and organ transplants, infections and other subjects.

I took another sabbatical leave at the Roche Institute of Molecular Biology in 1973. I started to learn new research technics all over again, such as assay for catecholamine synthesizing enzymes, radioimmunoassay for narcotics and narcotic antagonists. Our collaboration during and after my tenure at the Roche Institute resulted in many publications. Our paper published in Science in 1976 on the analgesic effect of nitrous oxide provoked a great deal of controversy, existing to this day. Our collaboration with Dr. Barry A. Berkowitz continued until he left Roche to become the Vice-President of the Smith Kline and French Laboratories.

I went to the Roche Institute again for my last sabbatical leave, to learn how to measure plasma catecholamine concentrations using radioenzymatic assays. After I learned how to do it, I decided to do something else. To measure catecholamines using this method, with standards of norepinephrine, epinephrine and dopamine starting from 5 picograms up to 5,000 picograms, unlike the commercially available kits which have only one standard, it took two whole days, nonstop, before I could put the vials in the counter. Another day for counting and at least half a day to analyze the data using a computer. The technician had to help me with the computer. What I

Our paper published in Science in 1976 on the analgesic effect of nitrous oxide provoked a great deal of controversy

did learn eventually was how to study receptors, the grind and bind. I continued to work in this area with Dr. Finck after my return. We were successful in getting further indirect evidence to support our hypothesis that anesthetic-induced analgesia is the result of direct action of drugs, such as ketamine, on the opioid receptors, or indirect action of drugs, such as nitrous oxide, through the release of endogenous opioid peptides. Recent literature, published or in the process of being published, supports our hypothesis, inspite of many other papers refuting our original report published in 1976. Dr. Finck and I started to look for direct evidence that anesthetics with good analgesic property release endogenous opioid peptides. We decided to use the dog as our model. We obtained cerebrospinal fluid from the third ventricle with great difficulties after chronic implantation of catheters, with the dogs trained to lie quietly while breathing air or nitrous oxide. We have eight pairs of CSF samples. The six endogenous opioid peptides (met5-enkephalin, leu5-enkephalin, met5-enkephalin-arg6-phen7, dynorphin A, dynorphin B and beta-endorphin) were first separated using a high-performance liquid chromatograph. Concentrations of these peptides were then determined by radioimmunoassay using commercially available kits. We found that nitrous oxide markedly increased the CSF concentrations of met5-enkephalin, to a less extent, met5-enkephalin, and borderline significant increase in that of leu5-enkephalin. Concentrations of dynorphin A, dynorphin B and beta-endorphin did not change significantly. Thus, we have some direct evidence indicating that nitrous oxide-induced analgesia is related to the opioid receptor-endogenous opioid peptide system.

FIGURE 9:1986, AT SON'S WEDDING

In the meantime, I was involved with life science programs in Taiwan. I was elected a member of the Academia Sinica, Republic of China, in 1972, an honorary position for life without stated duties. The members meet every two years in Taipei. The major task each time we met was to elect new members according to a complicated procedure. In 1980, academicians in the biology group proposed to plan and start a new research institute of biomedical sciences. Other members, whose disciplines varied from physical sciences and humanities, agreed. The building of the Institute of Biomedical Sciences broke ground in 1984 and was completed in 1986. It has six floors above the ground, four of which are research laboratories. The building was designed without knowing who are going to work there and in what areas. We actively recruited oversea Chinese as well as American scientists. As a member of the planning committee responsible for equipment acquisition, I had to work with returning scientists to find out about their needs. During the summer of 1986, after the regular biannual meeting of the Academia, I stayed for some time to help to assign laboratory space to different groups. Their areas of research varied from oncology, virology, cell biology, cardiovascular physiology, blood coagulation, prostaglandins, neurosciences to epidemiology. I walked up and down the floors many times a day to find suitable space for each group and space for core equipment. Without knowing or worrying about the budget, I must have ordered 1.5 dollars (US) worth of major and small pieces of equipment, including an electron microscope and a GC/MS. Now we are talking about getting three nuclear magnetic resonance devices. The Institute started with 25 people in the summer of 1986. As of now, it has more than 200 people. More scientists are being recruited. The Institute is running out of space, so the plan is to build another ten-storied building right behind the existing one with flying bridges to connect the two. It was quite an experience and gratifying to have seen a big hole in the ground and a few years later, to observe scientists working with state-of-the-art technologies.

When Dr. Bendixen assumed his present post as Dean of the College of Physicians and Surgeons and Vice-President of Columbia University, the search committee was successful to recruit Dr. Miller as our new chairman. Dr. Miller arrived in September 1986. He came to the Presbyterian Hospital with a rich background of patient care and

research. With the new hospitals soon to be opened and the increasing governmental interventions in the delivery of health care, Dr. Miller and the department will have a bright future as well as challenges.

For some time now, I feel that I could not keep up with the science anymore. I no longer understand what are being published in Science, Nature and the New England Journal of Medicine, although sometimes I get little tips from other journals, such as Trends in Pharmacological Sciences, Trends in Neurological Sciences. With the patient help of Dr. Finck, I finally learned how to use the computer to write, but still know nothing about the spread sheet. It is time to retire but hopefully I will be permitted to come to this great Medical Center from time to time. It has been my home for forty years.

I wish to sincerely thank all the people I worked with. The chiefs were and is always supportive. My collaborators, there are many throughout the years, are my dear friends even we sometimes argued about different matters. Mrs. Erlinda Samaniego helped us faithfully for many years in doing everything from cleaning up after us to doing complicated analytical procedures. Before I learned word processing, Mrs. Margaret Reihl deciphered my scribbles as no one could, and in addition, corrected my English. Again, my deep appreciation for the privilege of working here.

Life After 1989

After my retirement, I was not idle either. In 1990, I was a Visiting Professor at the National Taiwan University Hospital as a Special Chair, sponsored by the National Science Council (of Taiwan) for six months. I lectured on history of modern anesthesia, managements of various disease states, as well as medical ethics and medical economics, comparing the states of situations in various countries, economics and available manpower in the specialty of anesthesiology. I proposed to establish a training course for future leaders and teachers in anesthesiology, versed with the latest advances in cellular and molecular biology. The response from the Department of Health was positive, but those from the major teaching institutions were less than lukewarm, except that from the IBMS (Institute of Biomedical Sciences). Thus, I could not do anything worthwhile. It is not until I went to Taiwan again in 1992 for a 3 month stay as visiting professor at the Tri-Service General Hospital, also sponsored by the National Science Council, when I found out that each teaching hospital has its own plans and services to send their junior staff for advanced training either in clinical subspecialties or research on subjects on subjects related to the practice of anesthesiology in Taiwan or abroad. So, the problem does not lie with the lack of planning on the part of leaders in major teaching hospitals, rather, on how to further cultivate these freshly trained faculty members to further their academic careers. Opportunities must be provided for these individuals for further academic development to become future teachers of modern anesthesiology and leaders for cultivation of leaders of the specialty for years to come.

So, the problem does not lie with the lack of planning on the part of leaders in major teaching hospitals, rather, on how to further cultivate these freshly trained faculty members to further their academic careers

The quality assurance as discussed recently in Taipei during the Symposium on training of anesthesiology residents, anesthetic equipment, monitors and quality assurances can only improve the quality of anesthetic care to minimize anesthetic morbidity and mortality, most of which are preventable.

Just three weeks after my return from Taiwan in April 1992, I had to go there again. This time for the Advisory Committee meeting of the Department of Health there. The department is planning to establish a National Health Research Institute, something like the NIH in USA, but in a

much smaller scale. The proposed budget for the next five years is twenty trillion NT dollar. While the planning goes forward for research buildings and recruitment of scientists, pending approval by the Executive Yuan and the Legislative Yuan, a small budget is being provided for establishing research centers and laboratories in different medical centers, amounting to NT $128 million. The Committee met twice already in January for preliminary screening, it had to meet again in May to make the final decision as to whom the fund should be awarded. It was like the study section of NIH, except the Committee of 15 served both as an advisory board and study section. I suppose that as time goes on this arrangement would have to change.

In July 1992 I had to go again for the biannual meeting of the Academia Sinica. In addition to the usual proceedings, as advisory committee had to meet to evaluate the progress and achievements of the Institute of medical Sciences (IBMS) since it began to function in 1987, like the five-year review of NIH laboratories.

The repeated long air rides were tiring. It took 14 hours to fly from Los Angeles to Taipei, and seven and half hours from New York to anchorage with an additional nine hours plus from there to Taipei. After four such flights, I am getting used to it. I am doing these as long as I can manage physically.

While not traveling back and forth to Taipei, I was not idle. The Advisory Council to the Department of Health met in New York airport and San Francisco to screen and to make recommendations. But before each meeting, all the grant applications had to be read and evaluated. These grant applications vary from research in biomedical sciences, public health, pharmacology and biotechnology. I do not presume that I know these, but by reading research proposals, I get to learn a lot outside the field of anesthesiology.

Figure 10. 1998, 50th Wedding Anniversary

In September and October this year (1993, editor's note), I am scheduled to go to the Veterans General Hospital in Taipei as visiting professor. In 1994, there will be a meeting of the Academia Sinica in July and the Advisory meeting of the National Health Research Institute Planning Group in September. In the meantime, there are other matters, such as the Federation of Chinese American Medical Society, the Society of Chinese Biologist in America (the Tri-state Chapter) and the Chinese American Medical Society (CAMS). Most of the CAMS matters are taken care of by my wife, but I must proof read all her publications, such as Newsletters, Directories, etc.

-The end

附录：王海明寄给艾世勋教授遗孀王雪华教授和世交傅传孝老师的信函原文

尊敬的王雪华教授和傅传孝老师:

您们献出了金矿， 功德无量！

艾世勋教授的回忆录生动， 明快，幽默, 极好！

我一口气拜读嘉文，仿佛回到半个世纪前， 跟随着艾教授学习。 他一边走， 一边对我讲说他那看似一般，其实是非凡的丰功伟绩！

艾世勋教授的故事，中文版我至今未见 。我曾在 《 Anesthesiology 》 看过一篇纪念艾老的英文。后来，我到纽约上州 Kingston ， NY 12401 工作，仍不甚知艾教授的生平。

2002 年底，我去曼哈顿参加纽约麻醉年会，巧遇另九名大陆来的青年麻醉医师。大家一见如故， 相见恨晚。一起去附近一中餐馆 （至尊宝），上到二楼，见无其他食客，于是便兴高采烈地中文交谈起来。相互介绍后，我竟是年资最高者。责任感和使命感油然而生。

我提议：组创 《美国华人麻醉医学会， Chinese American Society of Anesthesiology , CASA 》。众人皆推举我担纲首任会长， 我未推却，马上任命了副会长陈国纲，秘书李迺曦，秘书助理王长征， 司库谢燎阳。大家情绪高昂，十分齐心。当晚，我回到家起草“ 创会宣言”。

一激动，我写出 《留学生之歌 》。请学音乐的留学生谱了曲，传给《光明日报 》。2003 年 1 月 1 日中国新华社发出新闻： “《留学生之歌》唱响美国纽约！ ”歌词还先后刊于巴黎的 《欧洲时报》， 美国的中文报刊和数个大陆媒体均报道了《留学生之歌》。

CASA 成立大会在法拉盛 Sheraton Hotel 举行。与会者 50 余人，每人交会员费 $ 50, 用仅有的经费通过律师注册了学会，建立了网站。CASA 的宗旨有二： 1. 帮助会员学习提高麻醉水平； 2. 促进中美麻醉学交流。

首先，我和周海峰，李迺曦帮助其他会员练习备考麻醉口试。同时，我联系上北京中华医学会麻醉学分会。不久，我也联系上台湾的麻醉领导王志中教授和香港的麻醉领导 Tony Jin 教授。我还试着联系新加坡麻醉领袖。我聘请了几乎所有美国大学麻醉突出的华人教授们为荣誉会员。

2003 年 10 月我去旧金山参加美国麻醉医师（ASA) 年会， 周六晚，在“羊城茶室中餐馆” 举办了“首次美国华人麻醉医学会晚会 ”。邀请到 ASA 主席 James Cotrell 教授到会致贺， 中国驻旧金山总领馆王勉烁科技参赞发言庆祝，美国华人麻醉前辈 Philip Liu 教授即席发言鼓励，

大陆麻醉学会 CSA 主席罗爱伦教授和岳云教授赞扬 CASA 的成立并希望以后多合作。台湾王志中教授也称赞。聚会顺利举办得益于王清，李鸥夫妇（ 均毕业于北京协和医学院八年制 ）和两位学长楼燕勤医师和汤革新医师的帮助。美加来的麻醉医师每人付 $ 50, 大陆麻醉医师们均免费。王清和李鸥与我合作订制成 CASA 会旗。

CASA 成立已经 15 年了，在 ASA 年会期间举办了 14 次美中交流聚会。过去 10 余年中，CASA 组团去大陆讲学， “为中国大陆麻醉医学现代化贡献巨大 ”， 前任中华麻醉学会主席吴新民教授如是说。

过去 15 年中，我的工作经历三次大合并，是当初入伙医师唯一幸存者。周海峰 MD, PhD 从 NYU 副教授进入新泽西一高尚生活区医院任职，李迺曦 MD, PhD 现任纽约 Scarsdale 劳伦斯医院麻醉科主任， 王长征医师在纽约市创立了麻醉公司 “ 8th Avenue Anesthesia ”.

CASA 有三位会员担任 《 Anesthesiology 》 Associate Editors (艾世勋教授一定会欣慰地微笑)。CASA 继任会长冯鸿辉医师在康州 New London 医院任麻醉科主任；汪红教授在西佛吉尼亚大学医学院任麻醉科副主任。 CASA 许多会员积极参加 ASA, CSA, 和中国医师协会麻醉医师分会活动。我相信 CASA 将与时俱进，奋创辉煌， 再创辉煌 ！

以上，可视作是向王老师和傅老师的简单汇报。 艾世勋教授听之也会为我们骄傲。您们是我们的楷模，我们将沿着您们的足迹继续向前 ！

希望王教授和艾教授的家人尽可能多提供有关艾世勋教授和您们的生活故事刊登以益后人。

我代表美国华人麻醉医学会祝您们欢乐，安康，幸福 ！

致礼

晚辈 王海明 敬上
2017 年 12 月 13 日凌晨

中美交流合作新动议：ASA 和新青年麻醉论坛（NYAF）签署协议

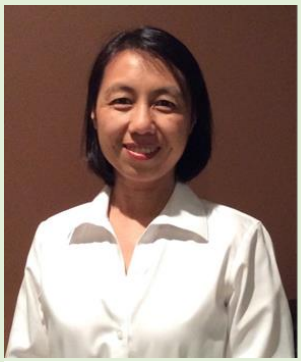
CASA 候任会长汪红教授

新年伊始，美国华人麻醉医学会（CASA）与国内最具影响力的麻醉学网站“新青年麻醉论坛”（NYAF）再次合作，”共同启动了《ASA 年会知识更新精选系列翻译项目》。。 此项目由新青年倡仪，CASA 候任会长汪红与 ASA 协商得到美国麻醉医师协会（ASA）的大力支持，ASA 已于近日正式签署了合作协议，ASA 也表达了未来长期合作的积极意愿。此项目旨在将 ASA 年会中最新、最热点的麻醉学观点以最快的速度传递给国内的各级麻醉医生，全面提高国内麻醉医生解决各种麻醉相关问题的综合能力。该项目选取每年 ASA 年会知识更新中最有价值的部分内容，先由新青年麻醉论坛文献编译组中的优秀青年麻醉医师进行翻译，然后请 CASA 相关领域的专家进行审校，最后经过编辑后在 NYAF 上向全国麻醉医生推送。该项目将由新青年韩宁老师和 CASA 会员王濛医师共同协调。

回眸专栏:

体验义工文化

刘宇燕 MD，美国新泽西州 Raritan Bay Medical Center 麻醉科



产科麻醉责任编辑刘宇燕

最早接触到义工是二十多年前刚到美国时，为加强英语听说能力，我参加了晚间英语培训 班。银发苍苍的老师 70 多岁了，来自教会，免费为我们教课。她的友善耐心与热情无私，不仅让 囊中羞涩的我如愿以偿地得到学习机会，也帮助我打开了认识美国义工文化的窗口，让我感受到 人与人之间互助的善意和雪中送炭般的温暖。

美国医院里到处可见义工活跃的身影，从高中生到退休老人。做住院医时，一次收急诊病人，需要到病案室调出老病历，我忙得焦头烂额，分身乏术。一位年过花甲的义工，毫不犹豫地揽 下这份差事。他虽然行走不便，却没有自怜自艾，其乐观自信令人尊重。当他将病历交给我时， 那份满足与自豪感令我感动震惊与难忘。即使身有残疾而心无拘绊，也可坦然自在，乐施好善。

我当初做义工完全是无心之为，小学生班级出游，我去做家长陪游。女儿拉着我的手向同学 骄傲地介绍说“今天我妈妈带队”，顿时我的自豪、被信任之感油然而生。活动结束时老师和孩子 们溢于言表的感激让我快乐无比。在孩子接受中小学教育的十余年中，各式各样的校内外义工活 动不胜枚举。工作之余加入义工行列，活动中可以与孩子们近距离接触，观察同学之间的互动， 体验孩子的教育环境，见证他们的成长，结识其他有共同兴趣的学生家长，还可让儿女感受我们 亲力而为的支持关爱，何乐不为！

一双儿女逐渐长大成人离家，空巢后的我一度有些失落。 两年前因机缘巧合了解到无痛分娩 中国行。这项广益于母婴的公益活动，将我曾在国内做过的妇产科专业，与现今的麻醉专业，完 美地结合起来，激发起了我的好奇心与热情，让我很快便全身心地投入。从未料到，这项活动会 对我以及家庭产生如此巨大的影响。

自从在美国执业行医，这 16 年来一直是一个普通的临床麻醉医生，同事间无论是初入茅庐的 新人，还是学识渊博、经验丰富的资深医师，无论年资、职位，均以名字相称，即使带教学生， 也不过是以 " Dr. Liu " 代替了名字而已。初次回国参加中国行，突然被国内的同仁尊称为“刘老 师”、“刘教授”，让我在惊异不安与不习惯中感到了压力，也感到了责任。这促使我对相关学科及 领域更深入地关注学习了解，以此融会贯通，教学相长。

因中国行而认识了众多志同道合的义工，从朝气蓬勃的大学生到六七十岁德高望重的学者，专业不同，各有所长，人才济济。一次全美产科麻醉及围产医学学术年会上，来自哈佛医学院的 教授，授课培训老师，当得知我在做中国行时，她会意地说“我也在做”，使我倍受鼓舞激励，也 倍感荣幸。他们的品行修养、学识才智令人肃然起敬。

组织开展这样一个公益活动历尽曲折艰辛，如果没有对生命之痛本身 悲天悯人的关怀与理解，没有为之努力改善的热忱、信念与百折不挠的勇气、坚韧，没有智慧、 学识与专业素养的铺垫，就不可能号召吸引全美几百名相关领域医者志愿加入，并且十年如一日 地坚持不懈，也就不可能在实施之地将理念推广与付诸实践，让无痛分娩-产科麻醉-现代产房理 念的种子深植于土壤并生根发芽、开花结果。

深度参与后我才认识到，组织开展这样一个公益活动历尽曲折艰辛，如果没有对生命之痛本身 悲天悯人的关怀与理解，没有为之努力改善的热忱、信念与百折不挠的勇气、坚韧，没有智慧、 学识与专业素养的铺垫，

就不可能号召吸引全美几百名相关领域医者志愿加入，并且十年如一日 地坚持不懈，也就不可能在实施之地将理念推广与付诸实践，让无痛分娩- 产科麻醉-现代产房理 念的种子深植于土壤并生根发芽、开花结果。

在频繁的互动交流、推动项目开展过程中，结识的同道逐渐变成良师益友。为赶上他们的脚 步，我拼命努力，也开始涉足我以前望而生畏的领域，比如电脑技能。每一点滴进步都令我欣喜 若狂，也意识到，只要心不老，还能学会新东西。令人意外的收获是重拾中文说写能力。记得两 年前回国首次讲课，中文表达常常词不达意、卡壳，以前的国内老师听完后直言不讳地说“刘宇燕 ，你都不会讲中文了” 。我虽然羞愧难当，但谢谢这么坦诚，让我知耻而后勇。两年后，初次相识 的国内同仁与我交流后对我说“你的中文超好” 。这段经历使我牢记：做个称职的义工也需要不断学 习，与时俱进。

后来受命管理一个无痛分娩中国行项目。在忙碌中，感受到了无以言状的心灵自由与充实快 乐。一向自认为个性内向，不擅言辞，在管理团队、与各方沟通协调过程中，逐渐开朗自信起来 ，有时都不敢相信，我也有这种组织能力，也可以接受挑战，化解危机。这两年的经历，无论酸 甜苦辣，开阔了视野，增加了阅历，丰富了人生。 为我的转变感到欣慰的，还有我的先生。他言行中对我极尽赞许、鼓励、支持。我为活动出 行，时时能感受到他关注的目光，看到他不遗余力的宣传。先生作为肿瘤学与血液病学临床医生 ，这些年，无论在美国还是在中国，都非常专注和享受临床实践与教学。我现在终于能与他并肩 而行。

令我意外的是对儿女的影响。女儿上高中后开始做义工。曾在中文学校当老师助教，在图 书馆儿童绘画班兼任老师及项目策划主管，在体操馆里带班示范等。当时以她繁重的功课，还有 每周 20 小时的高强度体操训练与不定期比赛，我担心她是否有足够精力兼顾应对。但她却兴致盎 然，干得有声有色，因这些都出于她的兴趣爱好。当初或许是为了满足学校与申请大学的义工需 求，但后来逐渐变得习以为常，自觉、自发、自愿，到了大学还保持了这种热情和习惯，并且结合 了自己的志向去探索。助人的机遇源于生活点滴，只要有心有意，便可从小从简做起。

女儿暑假去南非做义工，报名时她跟我说， “妈妈，我要做的项目与你做的中国行类似，是属 于全球健康计划活动的一部分” 。这让我感到了无言身教的力量。不久她也加入了中国行活动，发 挥兴趣爱好所长，参加英文网站的建设管理，为杂志插图作画，在团队中翻译导向。看到我电脑 操作为难，会伸援手相助。她也会向我求教中文与医学相关信息，彼此交流中有了更多的共同语 言。她有时兴致勃勃与我分享义工经历，那份见识让我惊讶。我们之间的相处越来越像好朋友。 共同的义工体验，增进了我们的感情，融洽了家庭气氛。无痛分娩中国行活动，使我产生的自信及带来的能量，是我始料未及的。我深感幸运，能在 后半生找到新目标，促使自我完善，还能惠顾我的家庭，并有机会如烛头之火，为他人带来光与 温暖。

来美二十多年，有感于义工文化如此自然而然地濡染了人们的生活习惯，让我看到了人心之 美、之善。社会文明的建设人皆可为，勿以善小而不为，勿以恶小而为之。一己之力微不足道， 却可日积月累，携众人之力聚沙成塔，积水成渊。更有意义的是，在追求善与美中，营造耕耘与 丰富了我们的精神家园。这一行动有益又有力，不仅成就了自身，温暖了他人，也潜移默化地影响着后代所思所行。

热衷公益的幸福之家：刘宇燕，黄夷伍医生夫妇和一双儿女

刘宇燕医师简介： 1986 年毕业于北京医科大学。曾在北京协和医院任职妇产科医生。在美国德州大学达拉斯西南医学中心从 事过细胞与分子生物学的基础医学研究。于纽约麦摩尼医疗中心完成麻醉住院医培训，随后在新 泽西医学与牙医大学接受了心脏麻醉专科培训。2002 年至今，在纽约州和新泽西州医院做临床麻 醉医生，现任职于新泽西力登湾医疗中心。ASA 会员。无痛分娩中国行高级产科麻 醉 123 项目主管及领队。

科研动态：　　静脉携氧微粒

编译：曲歌 MD　　北京协和医院麻醉科

北京协和医院 曲歌医师

由波士顿地区的一个研究团队开发了一种新的快速治疗低氧血症的方法，通过静脉注入预先携带氧气的微粒能够使窒息的兔子的血液保持充分氧合状态，防止器官损伤和死亡。这种方法可能帮助治疗急性气道梗阻或损伤导致的低氧血症，但尚需进一步研究该携氧微粒的安全性。

该研究团队的队长 John Kheir 是波士顿儿童医院心脏重症监护室的一名医生。他们首先开发了能够携带游离氧的磷脂共聚物壳组成的微粒并配制在 Plasma-Lyte 中。在离体脱氧的人红细胞中，容积含氧量 70％的微粒悬浮液所携带的氧气能够在 4 秒内对血红蛋白进行再氧合。在低氧空气中呼吸而导致中度低氧血症的家兔模型中，该研究组将微粒悬浮液静脉输入低氧家兔 2 分钟，在此期间，家兔的动脉血红蛋白含氧饱和度从 65％-70％上升到接近正常水平的 90％以上。之后，在气道阻塞引起的窒息家兔模型中，与输入未携氧微粒对照组相比，在 15 分钟窒息期间连续输注含氧量为 70%的携氧微粒能够使氧合血红蛋白水平正常化，并且减少了酸中毒的、肝损伤以及心脏骤停的发生，家兔存活率增加。该研究未观察到肺栓塞、心动过速或低血压等并发症。研究结果提示，该技术可能为急性呼吸道阻塞或呼吸衰竭患者提供快速，短时间的氧合作用，或在耗时的插管过程中防止器官损伤和心脏骤停。

该技术可能为急性呼吸道阻塞或呼吸衰竭患者提供快速，短时间的氧合作用，或在耗时的插管过程中防止器官损伤和心脏骤停

Kheir 团队的研究，在窒息兔子模型中，携氧微粒以 4ml/kg/min 输送氧气，相当于平均成人 200-250ml/min。 Kheir 说：“微粒的主要优点是它们不与一氧化氮系统相互作用，因此不会因为清除太多或太少的一氧化氮来影响血压。”另外，微粒在身体里的分布位置，以及它们的清除时间尚不清楚。即使在解决了安全性的问题之后，该微粒将只适合于为窒息病人提供短期给氧。但是并不适合于窒息超过 15 分钟的病人，原因是无法清除二氧化碳蓄积。

目前 Kheir 的团队正在进行安全性研究，以及测试携氧微粒在心脏骤停动物模型中的作用。该技术也可用于将其他气体输送到血液中用于治疗或诊断目的。该研究结果已申请专利, 该团队计划通过一期试验开发微粒，然后寻求合作伙伴。

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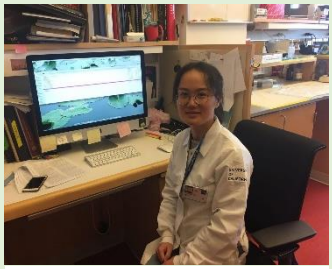
中美交流

UCSF 访学见闻

刘红菊 MD 北京协和医院麻醉科



北京协和医院　刘红菊医师

2016 年咋暖还寒的初春，我从北京出发来到加州大学旧金山分校麻醉及围术期医学科进行为期一年的访学。 年少时听过、看过很多关于美国留学的故事，毕业时也亲身经历了同窗好友渐行渐远奔赴美利坚的怀抱。一直好奇，这个国度为何有这么大的魅力吸引如此多的人努力接近。 刚到旧金山的一段时间，常常清晨醒来我迷迷糊糊不敢相信自己已经身在美国。

UCSF 的麻醉科对现代麻醉学有着卓著的贡献。吸入麻醉 MAC 概念的提出者、肌松药的创造者、血气机的发明者、CPAP 在新生儿和重症患者应用的研究者、大出血输血策略的倡导者、右旋美托咪定的发明者，以致影响中外一代代麻醉医师的《米勒麻醉学》主编都曾在这里工作奋斗，作出对麻醉事业的巨大贡献。

我这次进修学习的重点是疼痛学，在位于 Mt. Zion 的疼痛门诊（PMC）见习。作为一所教学医院，除了我这样的访问学者外，还有医学生、麻醉科住院医和疼痛 fellow 在这儿一起学习。他们并不能独立诊疗患者，需在主治医师的指导下完成临床工作。不同级别的医师有不同的职责权限。 例如：fellow 和住院医可以单独问诊病人、书写病例，然后向主治医师汇报病史、阐明自己的诊断思路和治疗计划。最后由主治医师向患者解释病情和治疗计划，并最终签署医疗文书。除此之外，Fellow 还可以在上级医师指导下进行疼痛介入操作，住院医却少有这样的机会; 而医学生们连独立问诊及书写病历的权限都没有，需要 fellow 或住院医的陪同；像我这样的访问学者身份，由于没有美国职业医师执照，只能见习。严格的准入和监督制度，保证了医疗服务的安全性和教学的可行性。

除了临床工作，每周有讲座、文献阅读等教学工作。每周一次全科大查房，每月一次科研讨论会。也是很好的学习机会。虽然身份不同，但大家一起参加讲课、查房和病例讨论， 互相交流气氛融洽。刚到门诊工作的时候，我在电子病历使用、语言等方面存在很多困难，不论住院医还是 fellow 都给了我很多帮助，感觉非常温暖。在和这些年轻人一起工作中，我看到他们的勤奋努力、自信，良好的时间管理、自身管理能力。在和病人接触中，他们态度友善、语言亲切并耐心细致；在和上级医师汇报的时候，思维缜密、逻辑清晰、表达明确；当门诊患者较多出现等待的情况时，他们会主动替同事分担工作；而偶有短暂门诊闲暇时间，他们也会充分利用进行笔记整理、病例总结或数据采集。大多数医师在住院医阶段就开始培养自己的科研兴趣，积极收集有意义的病例并练习论文书写。住院医轮转出科时可根据自己兴趣选题，与大家分享相关文献的最新信息,锻炼住院医的演讲能力。一名大学在读的男孩在 7 月来到疼痛门诊见习。这一个月中，他每天同我们一样早到、晚走，一起参加晨课、讲座，一次都没有缺席。在和他聊天中我才了解到：这个男孩 19 岁，还没有完成本科学业；他立志毕业后申请医学院继续学习，为了更多、更真切地了解医生们的工作日常，他利用暑假时间来到门诊见习。这样一个年纪尚轻的孩子，就开始懂得计划自己的学习生活，知道获取信息的途径，并且有如此毅力坚持做好。实在让我心生敬佩！

门诊工作通过转诊和预约制实现，没有经过 primary care 的转诊患者不能来到疼痛专科门诊就诊。PMC 的工作量为平均 5～6 个患者 / 门诊单元，每个病人的就诊时间 30～60 分钟。如若患者需要进一步的介入治疗，则需要另外预约时间同时就所需治疗向医疗保险机构提出申请，申请批准后才可以进行下一步治疗。 医院和患者对医疗保险的申请都非常重视，如果保险拒绝负担治疗费用，那么就会出现患者自掏腰包或医院无法收费的情况。美国的医疗费用非常昂贵，因此这种情况可能导致患者损失巨大或医院倒闭。医疗保险机构会根据患者的诊疗记录判断下一步治疗的合理性，从而给予批准或驳回。因此，在医疗流程规范化过程中，医疗保险结构也起到了一定促进和监督作用。例如，计划接受神经毁损治疗的患者，必须有诊断性阻滞的结果，且达到神经毁损适应症的情况下才能接受治疗。

患者的隐私非常被重视。每个患者到达门诊后被指引到一个独立诊室，在诊室内等待医师的访视。整个就诊期间不会被其他患者或家属打扰。这样的环境更容易让患者放松，对医师产生信任。记得一名普通的慢性疼痛患者。她专程从别的城市转诊到我们医院就诊。在门诊时，她向我们讲述了自己的想法：“教学医院把每一位患者当作教科书，从刻苦好学的医学生，到训练有素的住院医师，甚而严谨认真的主治医师都会仔细询问病史、详细记录病例和系统分析病情。我信任教学医院！我宁愿远道来就诊，希望得到最合适自己的治疗。”良好的医患关系为学生们带来优越的教学环境。大部分患者会在接受住院医或医学生问诊后，才能见到主治医师，再次补充病史采集和重点查体。大多数患者不厌其烦、理解并配合。

另外，完善便捷的电子病例系统也是一大亮点。电子病历系统记录了患者在整个相关联医疗系统里所有的

就诊记录和用药情况，包括其他医院、其他科室以及 primary care 处的医疗记录。其便利之处尤其体现在阿片类药物的管理上。由于这类药物的特殊性，医师在每次开具处方之前都会回顾每位患者的阿片类药物使用时间、剂量、种类及处方医师。这些信息都被电子病例系统记录在案，并可以随时查阅，大大方便了医师对患者药物使用情况的了解。

美国为期一年的学习收获颇丰。能够有机会见习发达国家的专科 fellow 培训体制，体会他们的培训内容和方式；同时也是帮助自己把知识进一步系统化、全面化更新的机会。我也看到了很多可能性，明白自己需要努力的方向。异国他乡的生活是孤单的，幸而有家人的陪伴让我不那么孤独；认识了很多来自五湖四海的朋友，得到了导师、fellowship 和同行者的关怀和帮助，也离不开远在祖国的朋友们对我的支持。 即将离开这座城市，我会怀念这儿安静的街区、自信满满的医学生、实验室里忙绿奔波的学者……离开自己熟悉的舒适区求学需要勇气，我圆了一个梦。爬上了高高的山岭，看到山脉绵延、天地辽阔；最终了悟故土难离。回家，真好！即将，再一次在路上……

创新发明 声门上喷射通气提高丙泊酚镇静下的上消化道内镜检查时的氧合

原文由 Chase Doyle 为 Anesthesiology News 撰写

成都华西医科大学麻醉科邓晓倩医生翻译

最近的研究显示， 在上消化道内镜检查过程中，使用丙泊酚镇静辅助以声门上喷射供氧通气（SJOV）可减少低氧的发生率。和鼻导管吸氧相比，经鼻放置魏氏鼻咽喷射管道实施声门上喷射供氧通气可使低氧发生率从 8.93%降低到 2.53%（P<0.0001）。 此外，对 1781 名患者的研究显示 SJOV 只有轻微的一些不良反应，如口干，咽喉部疼痛，没有一例发生气压伤。

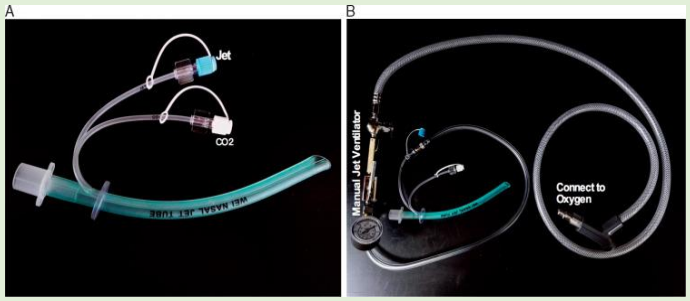
魏氏 SJOV 发明者：宾夕法尼亚大学医学院麻醉系魏华锋副教授

美国宾西法尼亚大学魏华锋教授指出，在上消化道内镜检查过程中使用丙泊酚进行镇静的情况越来越多，并伴随着越来越多的肥胖患者，这与缺氧发生率和死亡率的“惊人上升”相吻合。受迈克尔·杰克逊输注丙泊酚致死的启发，魏华锋教授发明了“魏氏鼻咽喷射管道（WEI NASAL JET, WNJ）”，即使在俯卧位，经鼻声门上喷射供氧通气也能保持肥胖患者的充分氧合。

中国泉州解放军 180 医院的吴志平，谢平等医师的临床研究表明在使用纤支镜行支气管镜检时联合使用 SJOV 和瑞芬太尼-丙泊酚-利多卡因，较传统的瑞芬太尼镇静辅助以表面麻醉的方法能明显减少低氧血症的发生率且降低呼吸、循环系统的应激反应，并提高外科医生的满意度。SJOV 显然具有良好的风险受益比，能提高患者的安全性。鼻导管组的患者较 SJOV 组有较高的高血压、心动过速、及咳嗽发生率（41% vs. 0%），（47.9% vs. 0%），(62.5% vs. 6. 5%)。鼻腔喷射组患者满意度明显增高。虽然气管内喷射通气是抢救困难气道患者的最后一招，但是难以在患者身上进行示教和练习，同时该技术发生气压伤的风险较大， 且应用往往太迟。相较而言，利用魏氏鼻咽喷射通气实施的声门上喷射供氧通气简单易学，很多手术都可使用此种方法，并且由于声门上通气的特点使之很少发生气压伤。

魏教授强调“通气不足，是上消化道镜检镇静过程中导致低氧血症的主要原因”。

“预防低氧血症的关键在于检查过程中维持正常的通气量。 ”



试验设计与结果

中国上海仁济医院的苏殿三教授等进行了一项多中心、随机、单盲的研究，将 1781 名常规行上消化道镜检的患者分成三组（均使用丙泊酚镇静）。 第一组，行鼻导管吸氧（2L/min）； 第二组，经 WNJ 供氧（2L/min）；第三组， 经鼻腔行声门上喷射供氧通气（SJOV 组）。

试验的主要结果为低氧发生率，定义为外周脉搏氧饱和度（SpO_2）低于 90%并持续至少 10 秒。同时记录其它不良反应。该实验从 2015 年 3 月起至 2016 年 7 月止。

正如苏殿三教授在 2017 年气道管理大会上的报道，使用 SJOV 后能使低氧的发生率从 8.93%降低到 2.53%（P<0.0001）.

除此之外，SJOV 组没有一例发生了严重的低氧血症，而在经鼻喷射通气组有 1 例发生了低氧血症，经鼻导管吸氧组有 2 例发生了低氧血症。研究发现， 术后 1 分钟 SJOV 相关轻微不良事件发生率增加，30 分钟后又下降。术后 1 小时，鼻腔喷射组口干的发生率增加，但无其它严重的不良反应，比如气压伤或者是气道受损。

此次大会的主持人， 加拿大多伦多大学麻醉科的黄大卫教授问及魏教授如果将经鼻喷射通气和临床上常用的高流量鼻导管吸氧进行比较，结果会怎样。魏教授回答道： “我认为两种方法的机理可能是类似的”“通过鼻导管进行高流量的吸氧，能得到 100%的氧气，增加了氧气的输送，和呼吸暂停时的氧合，并且能维持氧合很长一段时间。经鼻喷射通气是高压供氧因此也具有通气功能。当声门上喷气装置恰好对准开放的声门时，为呼吸暂停的患者同时提供了有效的供氧和通气。”“经鼻喷射通气能够对呼气末二氧化碳进行监测，为其又添加了一安全特性，便于早期诊断和处理呼吸抑制，避免增加低氧血症带来的病死率和死亡率。

黄教授问：“你碰见过胃胀气么？”

魏教授回答道：“当我们在设计这个研究时，这是我们重点考虑过的一个问题，也是我们为什么要选择行胃镜检查的患者进行研究的原因，但是，胃胀气很少发生”

“虽然绝大部分患者并没有发生胃胀气，但是我们仍然要弄明白为什么这一小部分人会发生”魏教授补充道“也许是因为特殊的解剖结构，导致胃胀气发生在这一人群中。在实施声门上喷射供氧通气时， 建议将胃部暴露，便于观察是否有胃部胀气并及时处理”

备注：宾夕法尼亚大学医学院麻醉和重症监护系魏华锋副教授是魏氏鼻腔喷射通气管及魏氏喷射性气管导管的发明者，也是中国 Well Lead 公司的顾问。

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