University of Pittsburgh
SCHOOL OF MEDICINE
Department of Surgery

Supplementary Appendix #1 (Part A)
STARZL-Fung manuscript

September 16, 1991

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University of California, Los Angeles
Brain Research Institute
Center for the Health Sciences
10833 Le Conte Avenue
Los Angeles, CA 90024-1761

Dear Ms. Marshall:

I will try to touch on the issues in your letter to me of August 23, 1991. Please bear in mind that I have not worked in neurophysiology for more than 40 years. Consequently my perspective never matured beyond the primitive state of knowledge which existed in 1949-1951. I made no recent effort to educate myself about subsequent developments, fearing that this would change my memory of now-distant events. In addition, I would have to be re-educated to comprehend the more sophisticated later literature.

By coincidence, I wrote an autobiography last January which was written about transplantation, primarily for the lay public rather than the profession. I hope that the book will be published in 1992 in English, at the same time as translated Italian and Japanese versions. In it, Chapter 3 (pages 30-47) is concerned mostly with the time spent during 1949-1951) in Magoun's Northwestern and UCLA (Long Beach VA) laboratories.

If you are interested, Chapters 19-21 have the inside story of my near move to UCLA, 30 years later. Paul Terasaki and Jim Maloney were major figures during this latter time, and of course I have sent them the book for an accuracy check which it passed. Finally, almost all of Chapter 12 is about Paul Terasaki in the 1965-1970 era.

Magoun was a legendary figure at Northwestern where Neuroanatomy was taught separately from gross anatomy. The course was under Magoun's direct and very detailed supervision. Some of the men who later helped create your Brain Institute were at Northwestern at the time, although most of them did not play a large role in Magoun's neuroanatomy course and were not part of Magoun's nuclear research group. Included were Earl Eldred and Bill (Robert W.) Porter. Eldred and Porter did Ph.D.'s in the Department of Anatomy, but under the supervision of other faculty members.

Other than Magoun, the most senior person in the official Neuroanatomy Section was Ray Snider whose interest was highly focused (some said monolithically so) on the cerebellum. Eldred's main research was with him. Snider did not have the vast range of knowledge possessed by Magoun, nor the creativity. These deficits, and I emphasize that the term was relative only to the luminescence of Magoun, prompted invidious comparisons between the two which must have undermined Snider's self confidence and made his life miserable. He seemed easily irritated, always near the explosion point.

Bill Neimer was next in the seniority line. With his sunken eyes, raven black hair, and gaunt frame, he sometimes resembled a cadaver when he was immobile, or Boris Karloff (which is what the students nicknamed him) when he moved. The reality was that he was just about the kindest and most gentle man whom I have ever met. When Magoun went to California, Bill Neimer took a faculty appointment at Creighton University in Omaha --- about 90 miles from my home town of Le Mars, Iowa. Whenever I was in Omaha during the succeeding years, I visited him. He seemed very happy there.

Magoun's attention to his teaching responsibilities was greater than I have ever witnessed at any level of the education process. He gave most of the formal lectures in his neuroanatomy course, and each one was a masterpiece. Scheduled for one hour, the talks lasted exactly 55 minutes, and always were accompanied by beautiful visual aids. Until I worked with him in the research laboratory, I did not know that he wrote these lectures and memorized them with the same care as he might have taken for a plenary address at a major international congress. In addition, he always was present for the laboratory sessions, and his final "practical" examination, complete with dozens of specimens, was the supreme event of the semester.

I was a good student generally, and perhaps especially so in neuroanatomy. Because of his attention to the students, Magoun was aware of this. In the spring of 1949, toward the end of my sophomore year, he asked me if I wanted to be a summer research fellow. For the personal and consequent economic reasons which I described in Chapters 2 and 3 of my autobiography, I was looking for a job which would allow me to stay in Chicago instead of going home to Iowa. During the previous summer, I had worked as a copywriter at the Chicago Tribune where I was invited, and even recruited, to return for a much higher salary. Magoun's advice was never to make a decision about work which was based on money. As a consequence, I joined him to continue the work on the reticular formation which he had begun with Giuseppe Moruzzi.

By the time I came in May 1949 to Magoun's 7th floor laboratory in the Montgomery Ward Building, his foremost collaborator, Giuseppe Moruzzi had returned to Italy. I had seen Moruzzi from time to time in the preceding year and was left with an impression (which probably is wildly at variance with the facts) of a youngish, slightly portly, very active and intense individual with hawk-like features and an army style haircut. Leon Schreiner, the budding neurosurgeon who did the chronic experiments of reticular formation ablation with Don Lindsley (motor and sensory both), was still in evidence, largely as an observer of previously operated cats which were a miserable lot.

Schreiner had the attributes of a movie star because of his good looks, long wavy hair, and a short but powerful build. He also had a dominant and engaging personality. His destiny was to be in University life, but somewhere along the way this was derailed. I think that he later worked at Walter Reed Hospital with David McRioch, but after that he went into the private practice of neurosurgery in Cheyenne, Wyoming. About 15 years later, when I was Chief of Surgery at the Denver VA Hospital I saw him again. He seemed bitterly unhappy, not only about his professional world, but also in his personal life. I was alarmed, but after this I lost track of him.

It was my misfortune not to have more than casual contact with Don Lindsley either in Chicago or California. Although I met Lindsley in Chicago, his work with Magoun appeared to have come to a hiatus at the time, and of course his arrival at UCLA was not long before the conclusion of my visit there in the spring and summer of 1951. Looking back on it, I fit into a hole between Lindsley's collaboration with Magoun at Northwestern, and resumption of these joint Magoun-Lindsley activities in Los Angeles. The consequence is that I undervalued Lindsley's two papers in EEG Clin Neurophysiol, although I always cited them. Last week, I reread them for the first time in 40 years and realized that they were magnificent.

There were other people at Northwestern whom I should mention. John Brookhart was in the Department of Physiology, where he taught neurophysiology as if it were in a different universe than Magoun's course. Here also, I failed to appreciate Brookhart's distinction until later when I spent the summer of 1951 with Brookhart's star pupil, Dave Whitlock.

I never met Bowden and Knowles who were the "mystery" authors on the three seminal papers from Northwestern which appeared in 1949 in EEG Clin Neurophysiol. Wendell Krieg, a neuroanatomist who published books with artistic stereoscopic reconstructions of the brain, worked on a higher floor of the Montgomery Ward Building, but he might as well have been on Mars. There was a very cool interface between Magoun's group and Krieg's.

My view of the magical environment of Northwestern in 1949 was that Magoun was the shark under whose fins we all swam. Magoun already was the master of the reticular formation because of his work on the extrapyramidal motor system. The classic monograph on this subject by Ruth Rhines and Magoun had been published recently by Charles C. Thomas. The frontispiece was a picture of a man with hemiplegia. Magoun used the tragic photo to begin his book, and the first line of the text (which I cite from memory, probably with minor accuracies) read "In an autumn that is appropriately sere . . . etc."

What followed was a description of the partially paralyzed patient, but what stuck was the first line which reminded me of the beginning of Stephan Crane's famous story in which the survivor described his first impression of seeing muddy waves against the yellow sky. I realized that Magoun was an artist. Thereafter, it was easy to recognize what he had written, no matter who the ostensible first author was. I remember resisting his editorial changes on papers which I wrote because I did not want to be a mere mimic. One of Magoun's stylistic quirks was to begin sentences with the preposition <u>for</u> ("For we now understand .

. . "). He slaved over manuscripts, eliminating redundancies.

To me, Magoun's 1949 paper with Moruzzi was the cornerstone of his monumental contributions toward an understanding of the behavioral significance of the reticular formation. The idea of an extraleminscal sensory system which ran through the reticular formation already was there from some of Magoun's own previous observations, those of Ransom, and more obscurely in the seminal observations of Bremer. However, these were patches in the crazy quilt until the paper of Moruzzi and Magoun made everything comprehensible. Moruzzi apparently had added a technologic component (electrophysiology) to Magoun's classic anatomic techniques which made it possible to give substance to the concept. I was led to believe, or perhaps I merely assumed, that in turn this technology had been imparted to Moruzzi by Lord Adrian (England).

Such a historical backdrop, if it is accurate (and I believe it to be), helps explain the idolatry for Magoun exhibited by those whose reflections later appeared in the history of the first quarter century of the UCLA Brain Institute. Like me, all of Magoun's co-workers appeared to see him as the locomotive of the train to which they were permitted and encouraged to attach and contribute great or small things in their own right. The article by Moruzzi and Magoun contained the synthesis of Magoun's life's work and in my opinion is one of the truly great articles in the history of science. I have been puzzled through the years why Magoun did not become a Nobel Laureate when other related work (that of Hess, for example, in the same field) resulted in this distinction.

In my autobiography (Chapter 3), I described the events in the summer of 1949 which permanently changed my life. With his principal collaborators gone or inactive, Magoun devoted much time to my training, beginning by showing me the neuropathologic techniques of brain preparation which he used to study the tracks left by stimulating, recording, and electrocoagulation needles which were inserted into the brain stem.

The map for anatomic localization was an atlas of the cat brain prepared by a Spaniard (or possibly South American) named Jimenez-Castellanos who had recently left Northwestern. The readings and drawings of the needle tracks were done in a tiny room which contained a microscope with an overhead projector, the images from which were traced on fine paper. During the summer, a young staff neurosurgeon named Charlie Taylor joined us on a sabbatical from the University of Toronto.

My original assignment was to systemically apply single shock and high frequency stimulation to areas in the thalamus and reticular formation, and to record the electrical changes in various cortical and subcortical areas. The objective was to determine how the impulses from the reticular formation reached their cortical destination. The conclusion was that there were both transthalamic and capsular pathways.

The basis for the second paper was Magoun's suspicion that the transthalamic route was the same as the "diffuse thalamic projection system" discovered several years earlier by Dempsey and Morison of Harvard. However, the Boston experiments had been performed under barbiturate anesthesia (Dial) which depressed the reticular formation fairly specifically and prevented delineation of the true character of this system or how it might modulate cortical or subcortical electrical activity. In both the first and second study, we used Bremer's encephale isole preparation. However, for the diffuse thalamic project we used repetitive slow stimulation to show a series of projections, primarily to the association cortices, from the medially located diffuse thalamic projection nuclei.

These experiments involved stimulation of subcortical areas with recording at a more cephalic level. They did not address what was feeding the reticular formation from below. One afternoon in June or July, 1949, before I was disciplined enough to refrain from deviating from protocol, a much more important observation surfaced. For no good reason, I switched the leads around so that the stimulating electrode was used for recording.

I was startled to see that substantial electrical activity could be picked up in the reticular formation and that this was significantly altered by noise such as that caused by a door slamming, a toy cricket, or an animal cry. I can remember bursting into Magoun's office, and how excited he was to hear the news. He came inside the wire cage with me where we spent a long time looking for artifacts. At first we searched for movement of the cat's ears, but the findings were unchanged by giving a large dose of a curare-like drug.

In Magoun's earlier paper with Moruzzi, conclusion 9 in the summary was "The possibility that the cortical arousal reaction to natural stimuli is mediated by collaterals of afferent pathways to the brain stem reticular formation, and thence through the ascending reticular activating system, rather than by intracortical spread following the arrival of afferent impulses at the sensory receiving areas of the cortex, is under investigation". The same hypothesis, and circumstantial support for it, was an important part of the two 1949 Lindsley papers. Now Magoun knew that these collateral pathways which had been very difficult to demonstrate with classical anatomic techniques, were susceptible to systematic electrophysiologic exploration.

The magnitude of the opportunity was stunning. Shortly afterward, I went to the Dean's office and gave notice that I was dropping out of medical school for at least one year. From this time until his departure for California in the late spring of 1950, Magoun and I worked all day, almost every day of the week, doing the experiments which we published together in the Journal of Neurophysiology in 1951. At noon, we invariably walked the several blocks to the Allerton Cafeteria on Michigan Avenue where we had lunch as I described in Chapter 3 of the autobiography. Because we were together so much, and because there was a physical resemblance, the preposterous rumor found its way back to me that I was Magoun's illegitimate son from some earlier youthful venture.

Quitting medical school was not easy to explain to anyone at Northwestern, or for that matter to my family in Iowa. Although I had discontinued all financial support from my father (see Chapter 3), I honored and respected him above all others. Magoun realized the quandary and wrote a touching letter to my father which he kept at his bedside for many years after he became invalided. The letter was lost when he died in 1976, but I remember it well and would blush to quote it. Magoun was determined that I should stay in research and explained why in his letter to my father. Eventually, I was half ashamed to follow a different pathway.

It may be that I came to know Magoun better than almost anyone else. I learned from him firsthand the price of having a creative vision. He lived in a flat on the south side of Chicago where he tried to juggle the needs of his family with the pressures caused by his discoveries and work. His wife, Jean, already was chronically ill. A beautiful teenage daughter (and I do not use the adjective lightly) married someone of whom he disapproved. He yearned for a more tranquil life. Whether he found this peace in California, you would know better than I.

At scientific meetings, Magoun radiated charm and confidence, but behind the facade I thought that he was shy. During his Chicago period, he was the nuclear figure in a neuroscience brain trust which was interinstitutional. Weiner (the father of cybernetics) often came there from Boston. Others included Warren McCulloch, Perciful Bailey, Ralph Gerard, and several people from a downstate psychiatric institute in Mantino, Illinois. I am sure that they were all brilliant, but one thing I noticed in their meetings was that they fell silent when Magoun spoke.

I am sure that you will not want to include the following anecdote, but I will relate it anyway because it was the most devastating putdown I have ever witnessed in my life. One day, Magoun took me to a neurophysiology meeting at the University of Chicago, where Ralph Gerard worked. Gerard could scarcely be missed in a crowd because he was extremely overweight. Apparently believing that Magoun was sensitive about being bald, Gerard rushed over and greeted him by saying "Tid, your head has gotten as bald as my wife's ass". I remember how Magoun flushed, and I knew that he was genuinely offended. Gravely he examined his own scalp, as if comparing it to something, and then replied, "By God, Ralph, I think you're right". I never saw Gerard again, but I doubt if he forgot the riposte and the roar of laughter which followed. I still smile.

Magoun left Northwestern in May or early June, 1950. We were unable to finish our manuscripts because all of the experiments had not been completed. Throughout the summer, I worked on these and eventually sent drafts to Magoun in California which he revised them and added the often reproduced Figure 8 in the afferent collateral paper. Some time in the autumn, after I had returned to my junior year of medical school, Magoun wrote that he could provide a traveling fellowship for me if I wanted to come to UCLA the following May (1951).

My papers from the Northwestern period, and the later ones at UCLA were as follows:

1. Starzl TE and Carpenter W: Diffuse thalamic projections to the telencephalon. Anat Rec (Abstract) 106:250, 1950.

2. Starzl TE and Magoun HW: Organization of the diffuse thalamic projection system. J Neurophysiol 14:133-146, 1951.

3. Starzl TE, Taylor CW and Magoun HW: Ascending conduction in reticular activating system, with special reference to the diencephalon. J Neurophysiol 14:461-477, 1951.

*4. Starzl TE, Taylor CW and Magoun HW: Collateral afferent excitation of reticular formation of brain stem. J Neurophysiol 14:479-496, 1951.

5. Starzl TE and Whitlock DG: Diffuse thalamic projection system in monkey. J Neurophysiol 15:449-468, 1952.

6. Starzl TE, Neimer WT, Dell M and Forgrave PR: Cortical and subcortical electrical activity in experimental seizures induced by metrazol. J Neuropathol Exp Neurol 12:262-276, 1953.

*Of these, the one on collateral afferent excitation was the most important, or so I thought then and still believe. The introduction (next page) describes the ambiguous situation as we encountered it. The summary (the page after that) was so brief because the findings and implications were so clean. I carried the article in my heart for the rest of my life, never believing that I could do so well again.

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COLLATERAL AFFERENT EXCITATION OF RETICULAR FORMATION OF BRAIN STEM

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THE demonstrable capacity of afferent stimulation to arouse a sleeping subject, and the obvious benefits of reducing sensory inflow in predisposing to sleep, are in seeming disharmony with recently discovered influences for wakefulness exerted by the central reticular core of the brain stem. Direct stimulation of this part of the neuraxis reproduces the electrical pattern of wakefulness in the cerebral cortex (14) while at the same time it facilitates lower motor activity (16), and so arouses the nervous system generally (9).

The ascending course of this reticular activating system is distinct from that of afferent pathways in the brain stem (19) and selective destruction of its cephalic portion is followed by the EEG synchrony and behavioral somnolence, hitherto attributed to deafferentation of the cerebrum (7, 8). Such consequences do not follow selective interruption of ascending somatic and auditory paths in the midbrain and after this latter injury both somatic and auditory stimuli are still capable of awakening the sleeping animal and activating its EEG (7, 8).

It seemed likely that the apparent conflict might be resolved if evidence were forthcoming that collaterals from afferent paths turned into the reticular activating system in the brain stem and exerted their admittedly important arousing and awakening influences, indirectly, by modifying its activity.

The present study has explored this possibility by probing the brain stem for alterations in electrical activity evoked by somatic and auditory stimuli. The findings establish the existence of collaterals from these sensory systems to the brain stem reticular formation, the rich wealth of which has never previously been suspected, though indications for it have been afforded by earlier anatomical investigation (2, 11, 13) and by study of the atypical route of conduction of the 'secondary response' to sciatic stimulation (5).

Though the results are presented here only with reference to the problem under discussion, it is felt that implications of these findings may be broad indeed, for they appear to enlarge outlooks in afferent conduction far beyond those which have been envisioned within the circumscribed limits imposed by classical sensory paths.

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Reprinted from
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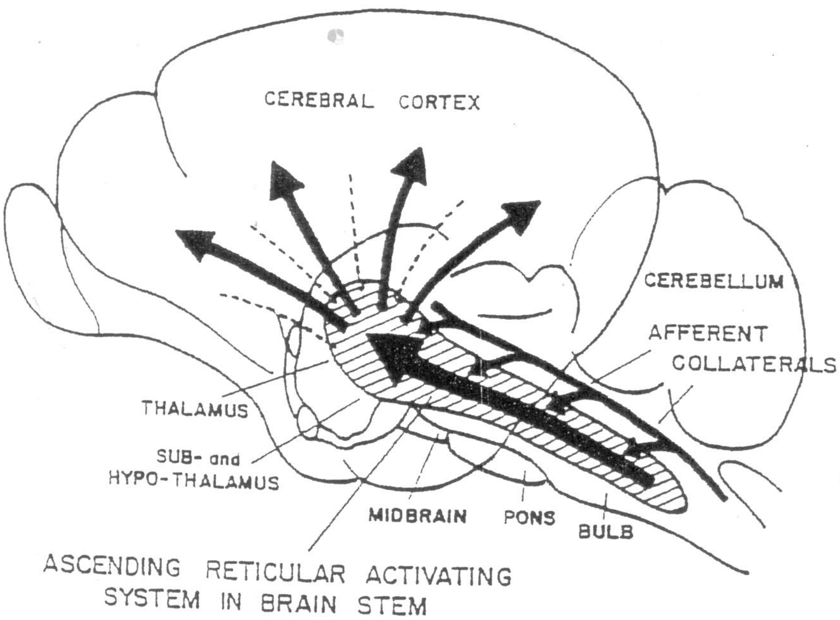

FIG. 8. Outline of brain of cat, showing distribution of afferent collaterals to ascending reticular activating system in brain stem.

SUMMARY

The distribution of afferent collaterals to the reticular formation of the brain stem has been investigated in the cat by probing for potential changes evoked by somatic and auditory stimulation.

In the case of each modality, a rich supply of collateral connections to the midbrain tegmentum, sub- and hypothalamus and ventromedial thalamus was encountered. These findings offer an explanation for a number of the generalized consequences of afferent stimulation which have been difficult to understand in terms of conduction within classical sensory paths. Specifically, they indicate that the arousing and awakening influences of sensory stimulation may be exerted indirectly, and at a subcortical level, by collateral excitation of the reticular activating system in the brain stem.

Reprinted from
J. Neurophysiol., 1951 *14:* 479–496

Throughout the entire period from 1948 until my departure for California, I had worked as an industrial surgeon at night, as I described in my autobiography. I quit this job, picked up my two sisters in Iowa, and drove to the Long Beach VA Hospital which had the only available laboratory facility. There I met Jack French and Mr. Edwards (the Hospital Director) and the other people who were laying the groundwork for the UCLA Brain Institute. There was no activity going on, but provisions had been made for monkeys and for operating facilities.

Dave Whitlock, a graduate student who had just completed his Ph.D. requirements, arrived from the University of Oregon with his wife, Peggy. I lived on the VA Hospital base. The Whitlock's had a small house in Long Beach where I spent many evenings. I tracked Dave from place to place after this, and eventually was able to nominate him for the Chairmanship of Anatomy at the University of Colorado where I was working myself. He accepted the job and we were reunited almost 20 years later.

My primary objective in Long Beach was to map the projections of the diffuse thalamic projection system in the primate, because the monkey had much more extensive cortical association areas than the cat. The results were largely confirmatory of those in the cat, although there was a much stronger localization of the medial thalamic nuclei projections to specific cortical association areas as well as an overall dominance in the frontal lobes and cingulate gyrus.

In a second study in cats, we tried to determine if metrazol seizures were initiated in the subcortical areas and radiated to the cortex. This was an attempt to verify a hypothesis by Jasper about the deep initiation of seizures, but to our disappointment these appeared to start in the cortex and could be invoked best by stimulation of the classical sensory pathways. The seizures then spread across the cortex and antidromically to the diencephalon.

I cannot remember a more happy time than the summer of 1951. The place where I lived on the VA Hospital base was Spartan, but it was clear that French and Magoun had created an idyllic place to work. Play was not totally ignored. There was a small golf course on the station, and just across Highway One were numerous alluring places to go late at night. The Pike (an amusement park, now gone) was at the height of its popularity. Twenty miles to the south was

Christian's Hut where we had an overly hedonistic farewell party the weekend before I left.

Magoun was concerned at one time that I was neglecting my social life. It would have relieved him (or possibly the opposite) if I had shared all secrets. I did not know anyone of my own age when I arrived, and after a few days I drove up the Pasadena Freeway and on to the Los Angeles County Hospital where I conducted a survey of the first 8 or 10 interns and residents whom I encountered. I asked them "Who is the most attractive student nurse in the hospital?". All but 2 or 3 identified a young lady named Marilyn Conner. I tracked her to the ward where she was working the night shift, explained exactly how I had identified her, and asked her if she would join me for a snack after work (which she did). Unfortunately, she was in love with a medical student at the University of California, San Francisco, and soon was borrowing money from me to take the train on the weekends to see him.

During my last week in Los Angeles, she learned that her San Francisco friend had become engaged to someone else, emancipating her from further obligations and leaving her free as she saw it to join me in Chicago. Somehow, it did not fit my preconceived romantic scenario, and I never saw her again. Jack French met Marilyn at about this time, probably at the Christian's Hut party. He was unattached and I believe that they saw each other after this. Apparently, Jack was very popular, and was just coming off of a romance with Ava Gardner when I arrived.

About 35 years later, I was saddened to receive a letter from Marilyn who now lives some place in Oregon. She had developed renal failure, and was inquiring about the best place to have a kidney transplantation. In her long letter, she told me of her numerous adventures in life. Now she had grown old and sick. Even the most beautiful flowers bloom and wither like all the rest.

During the end of my stay in Los Angeles, Magoun approached me about taking a fellowship at the Karolinska Institute with Ragnar Granit, instead of returning to my senior year. I knew by this time that I wanted to practice surgery, primarily because the complex technical procedures required to do the experiments with Magoun had seemed so easy to me. Incidentally, Magoun himself was a master surgeon, more skillful in the performance of fine work, in my opinion, than any surgeon whom I have ever watched in the clinical operating room.

Until he left Northwestern, Magoun did all of his own experiments, and was an active participant in the smallest detail of the benchwork. He had an obsession for accuracy which caused some people to believe he had a fiery temper. His pursuit of an idea was so passionate that I believe he suffered intensely at the time of his highest creativity. I realized from talking to him that he did not intend to return to this way of life, and that probably I would be the last person he would work with shoulder to shoulder. He was only 43 years old. As it turned out, he had many other useful ways to serve out the remaining 40 years.

On the way back from Los Angeles, I drove to Salt Lake City, and gave a paper at the autumn meeting of the American Physiology Society. It was a sad departure, because I realized that I might never see Magoun again. In fact, I met him only once when he came to a neurophysiology conference at Northwestern in the late 1950's. He was grayer then, but otherwise much the same. Now at the peak of his prestige, he was surrounded by admirers who were speaking a language which I no longer understood.

I do not remember Joe Bogen. If you would like to know more, please write again. What I did in the rest of my life is in the book, and in the enclosed Chapter which I contributed to Paul Terasaki's recent history of transplantation (UCLA Press). It was all an anticlimax. At the back of the chapter is an abbreviated C.V.

In my book (pages 40-41) I tried to explain my debt to Magoun, in fewer words than in this letter, and possibly better. I was planning to send the book to him, but when I asked Don Lindsley's son about him last May, I learned that he had died 2 months before.

Sincerely,

Tom Starzl

Thomas E. Starzl, M.D., Ph.D.
Professor of Surgery

TES/ps

Supplementary Appendix #1. (Part B): Starzl-Fung manuscript

UNIVERSITY OF CALIFORNIA

LOS ANGELES 24, CALIFORNIA

Department of Anatomy
School of Medicine
December 20, 1951

Dr. Alfred Blalock
Johns Hopkins Hospital
Baltimore 5, Maryland

Dear Doctor Blalock:

I am writing you in connection with the application of Mr. Thomas E. Starzl for an internship at Johns Hopkins Hospital in the year 1952 to 1953. Mr. Starzl will graduate in medicine from Northwestern University School of Medicine in June, 1952, and will receive his Ph.D. at the same time. I have been closely acquainted with Mr. Starzl during three of his years at Northwestern and again last summer at UCLA.

Mr. Starzl is a fine-appearing, clean-cut, personable young man who is one of the most outstanding medical students with whom it has ever been my pleasure to be acquainted. He has constantly stood at or near the top of his class, and he is a prodigious worker who has far exceeded the ordinary accomplishments of medical students.

Mr. Starzl has spent each of his summers and a year between the sophomore and junior years of his medical program in research work in the experimental neurology laboratory under my supervision. I have never known a young man who showed such capability in rapidly grasping the background of a problem. His imagination in conceiving new ideas for exploration is outstanding, and he possesses a remarkable ability to design the experimental approaches to test such possibilities. In each research team with which he was associated, he rapidly assumed leadership and carried the main burden of the work. His research accomplishment for a young man of his age is, in my opinion, unique. He is the chief author of three published papers and one additional paper now accepted for publication. Two other major projects to which he contributed heavily are now being prepared for publication. In all of this experimental animal work Mr. Starzl has been constantly alert to the implications of the program for clinical medicine. In his present clerkship at Passavant Hospital in Chicago, he writes of the clinical investigative activity which he is undertaking in addition to the regular program.

Mr. Starzl is intent on entering surgery as a career, and I am confident that he will become one of the outstanding figures in this field in the future. I would rate him as absolutely the top man that I have encountered during some twenty years of association with medical students. I recommend him to the attention of your internship committee in the very highest possible terms.

Very sincerely yours,

H. W. Magoun

H. W. Magoun
Professor and Chairman
Department of Anatomy

HWM:d:l

Medicine/Current Affairs Supplementary appendix #2 Starzl-Fung manuscript

Thank you for sending me the advance copy of Scott McCartney's outstanding book. I would not be surprised to see it show up on the bestseller list. It has dramatic inherent interest, and in addition it could not have appeared at a better time in the political process of policy determination.

The patient histories are touching, but similar accounts are in other books on transplantation—some written by organ recipients. The unique feature of Mr. McCartney's narrative is his subplot exposing how the transition of a new technology occurs from its developmental phases to commercialization. As Mr. McCartney has frankly stated, no advance, however promising, can be diffused into our health care system unless it is economically advantageous to the involved institution and to the medical personnel whose livelihood and variable lifestyles depend on cash flow.

In *Defying the Gods*, the transplanted organ under the journalistic microscope is the liver. However, in my book *The Puzzle People* (1992), I wrote, "It was uncanny how much the liver transplant gold rush of 1984 resembled that of kidney transplantation twenty years earlier. As before, there was a shortage of gold miners . . . The fresh crop of youthful men and women inherited the earth, or at least that part of it where they landed and staked their claims, hard-eyed now and determined to limit the numbers of new intruders who came close behind."

Thus, not far below the tragic surface of patient illness can be found the war for turf. In this case, the ultimate coin of the marketplace became the organs without which the services that generated cash flow could not be rendered. The battlefield was governed by the United Network of Organ Sharing (UNOS), whose directorship was made up not by the patients who needed the organs but by those who aspired to transplant them. Mr. McCartney has looked at the mercurial combatants in these struggles with a balanced and generally kind eye. All the while, he has made clear the entrepreneurial drives of a whole range of health care providers who frequently could be seen to change sides when their own supply of the precious livers expanded or retracted as rules of organ allocation changed.

Seemingly forgotten by many was the simple principle that organs must go where patients wait—or die while waiting if the principle is abrogated. With Mr. McCartney's help, it may still be possible to have equity.

—*Thomas E. Starzle, M.D., Ph.D.* *Professor of Surgery, University of Pittsburgh School of Medicine*
Director, Pittsburgh Transplantation Institute

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