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STORIES FROM  
A TWENTIETH-CENTURY LIFE



by  
*William Rees Sears*

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The cover illustration--a caricature of the author--was generously provided by George Reichart of Tucson.

Thanks are also due to Katherine Kac, who has read the manuscript and made invaluable suggestions to improve it.

Dedicated to Mabel Rhodes Sears



I wish thee as much pleasure in the reading, as I had in the writing.

Francis Quarles (1643)



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## INTRODUCTION

Isn't it presumptuous for one of your friends, namely me, to write his autobiography! But wait. Before you are completely turned off by the idea, let me explain what this modest volume is supposed to be--not an autobiography, in fact--and why I am writing it.

It has occurred to me that everyone really ought to write a kind of "memoirs", or at least a diary, as he grows old, if he has the energy, the leisure, and other circumstances that make writing a reasonable thing to do. How I wish my grandparents and my parents had left us their memoirs! I knew and remember both of my parents and three of my grandparents, but I realize that I am terribly ignorant of their childhood, their youth--and a great deal more.

Actually, there was one distinguished member of my mother's family, the Rees Family, who wrote his memoirs, an interesting book entitled *Rees Family History*, typewritten and reproduced by that process--thankfully no longer used--called "Ditto". He was my mother's uncle, Thomas Rees, of Springfield, Illinois, a state senator and publisher of a newspaper, the *Illinois State Register*. I'll have more to tell you about that book, later, since it contains a nice story about a relationship, not entirely friendly, between the Rees Family and the redoubtable Mark Twain.

There is also another memoir, of a sort, in Mabel's family history. After the death of her sister-in-law, Patricia Rhodes, in 1982, it was discovered that Pat had saved a collection of letters written to her and Mabel's brother Meredith ("Muddy") Rhodes by their father, William Maxim Rhodes, Montana rancher, between 1925 and 1935. Mabel, Muddy, and I put these together (with some photographs and a few other pertinent letters), being convinced that these "Rhodes Letters" constitute a remarkable record of one man's experiences and thoughts during the Great Depression, the New Deal, and America's preparation for war.

Mabel and I feel sure that our pleasure in these family records

is not unusual. Every family seems to have in it, in one generation or another, members who are fascinated by glimpses into the lives and personalities of their predecessors. Moreover, having been a college professor throughout my career, I am "father" and "grandfather", in the academic sense, to a lot of "children", most of whom are intellectually gifted and are attracted to the printed word, and many of whom have urged me to put together these stories. I hope they enjoy them!

I have already written, above, that this is not really an autobiography. It is, rather, a collection of anecdotes and some opinions. In the best cases an anecdote also makes a point! I propose to organize the volume, at least roughly, according to an autobiographical outline--recollections of my childhood and youth in Minneapolis, stories and characters from Caltech, the war years in the aircraft industry, then from Ithaca and Cornell, and finally Tucson--with recollections from the worlds of personal aviation and personal music sprinkled throughout.

I am calling it *Stories from a Twentieth-Century Life*--my life, of course, which I suppose is typically a 20th-century one, with its emphases on our technology, the First and Second World Wars, and aviation. I guess that even the re-emergence of the recorder as a musical instrument is a 20th-century phenomenon. Surely the universities that I tell about are 20th-century institutions, even if they were founded in the 19th.

Many of my dearest friends are not even mentioned in these memoirs! This strange state of affairs is caused, of course, by what I have tried to explain above: It is a collection of anecdotes, and the criterion for anyone's being written about here is "Is there a story about him or her?" There are infinitely many stories--some true--about Theodore von Kármán, and a lot, too, about such colorful characters as Hsue-shen Tsien, Arthur Kantrowitz, Hans-Wolfgang Liepmann, and others to whom pages are devoted here. If you do not find yourself written about, dear reader, that is surely not a measure of my affection or high regard!

Perhaps this is the place to issue another caveat: Everything I write will be, to my best knowledge, the Truth. Often, but not

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always, I will have confirmed my recollections with others who "were there". When I slip up, it will not be that I have evil motives but only a memory that's not as good as I think it is. I shall try to distinguish between stories that I remember first-hand and those (and they are often the best stories!) that were told to me by such master storytellers as von Kármán, Liepmann, and Mark Kac.

What shall I do when I want to tell an anecdote that might offend or embarrass one of the characters involved? One possibility is to tell the story without identifying the characters, but this would surely detract from the pleasure of some readers, especially my students, who recognize the names of famous teachers and scholars and love to hear of their wit, brilliance, or human failings. So I shall try not to use this way out of the dilemma unless it seems unavoidable.

No, I think, as I begin this writing, that I shall name names and places and tell the stories as I remember them, at least in this "first edition" of my memoirs, which is not intended to be circulated beyond the community of my family and close friends.

## CHAPTER 1

FAMILY CHILDHOOD  
HIGH SCHOOL

My father, William Everett Sears, eldest son of David J. and Naomi Everett Sears, made his whole career as a salesman and sales-manager of a sash-doors-and-millwork company. He should have been an engineer or architect--loved to make plans and layouts and, especially, to do the calculations that such designs require. He had a native talent for arithmetic, geometry, and algebra; his equations and drawings were beautiful and professional, done with hard, sharp pencils and meticulous engineers' lettering. He was also a born salesman, who liked people and conviviality--a Mason, a Shriner, an Elk, and a member of the YMCA and several fraternities of the lumber and millwork industries--including, believe it or not, one called "The Hoo Hoos."

He was born in 1875 in Oshkosh, Wisconsin, and raised in Rock Island, Illinois. The family's intention was to name him "W. Everett Sears", in honor of his mother's distinguished family, the Everetts. Family lore says that the minister, at the baby's christening, announced that the "W." had to stand for something, so they decided then and there upon "Will". But young W. Everett, as he

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grew up, didn't like the fancy name and called himself "William E." All his business and bridge-playing friends called him "Billy". So, throughout my youth, "Billy" meant him and "Bill" meant me--with, to be sure, some lack of logic and occasionally a bit of confusion.

He had a brief career as a student at the University of Illinois in Mechanical Engineering--apparently was doing very well, in spite of belonging to both Sigma Nu and the secret fraternity Theta Nu Epsilon--when along came an undergraduate marriage, a daughter, and, alas!, a divorce and the end of his college career.

He was a good dad--taught his kids habits of respect for their fellow man, the laws of the land, and learning. He and all his business associates were rock-ribbed Republicans, admirers of Herbert Hoover and haters of labor unions; but those convictions didn't "take" with either my sister Helen or me.

Neither he nor my mother was a church-goer, but they encouraged Helen and me to go (without them) to Sunday School, and they provided the quarters (maybe only one) that we dropped into the collection plate every Sunday. Church-going didn't seem to "take" for Helen and me, either. The church we first went to was The (Congregational) Church of the Open Door. Later we went to Park Avenue Congregational, because our high-school music teacher, Mr. Sydney H. Morse, was its musical director and we were very fond of him.

The Congregationalists didn't seem to be very big on theology. I remember one Sunday when the minister himself visited our class and remarked, in the course of his comments to us, "I don't suppose any of you believe that the Congregational Church is the only path to heaven." To his obvious chagrin, our class teacher, a young man, protested: "Yes," he said, "I certainly do!" I think he succeeded in messing up some important point that the minister wanted to make.

(Unfortunately, that same young man was romantically attracted to Helen, and paid a series of social visits to our house to see her. They also didn't "take".)

My father was, of course, delighted by Helen's scholastic successes and mine. I remember that once, probably when I was in junior high school, I got a report card that consisted of all A's except for an F in Penmanship and a D in Deportment. My father took

the card to his office and had a great time pointing out to his fellow workers that his kid couldn't write and didn't behave but could still get all A's! (In defense of myself I can only say that I was younger and smaller than my classmates and found it socially necessary to misbehave in junior high school!)

Dad was a great fisherman in the Minnesota tradition of those days; namely he fished in the Minnesota lakes, especially for black bass. He taught me to bait-cast and gave me the responsibility for keeping his tackle box and mine cleaned and in perfect shape. He taught me to drive a car, but that program began with a strange bit of confusion that must have made my father wonder about his son's alleged intelligence! The trouble was that I was so fascinated by automobiles that before these lessons began I had already sat in our Dodge touring car, parked at the curb, for hours, practicing gearshifting--with the engine off, of course. When the time came for me to learn to drive, we had a more modern car. I had "practiced" in the Dodge and the Dodge had an unconventional gear-shift pattern: "Reverse" was where more modern cars had "First". So when my father undertook to teach me to drive in the new car, a Pontiac, I shifted into Reverse when First was called for. He was sure that his son was a complete idiot!

I find it interesting--even historically interesting, perhaps--that my father, whom I have described as law abiding, patronized a bootlegger. I think my parents enjoyed a preprandial cocktail, though I am afraid it was a pretty poor one: Coca-Cola spiked with bootleg whiskey! There was a Sunday-morning family ritual that intrigues me now as I recall it: Helen and I would come to Mother and Father's bedroom and climb into their bed with them. Dad would get up and dress, standing modestly behind the foot of their bed, and then go down to the kitchen to mix drinks. What he brought to the bed was four glasses of Coca Cola. Theirs, I suppose, had a little booze added to it. (On Sunday morning? Before orange juice and scrambled eggs?)

There is a more disturbing memory of those days of Prohibition in America. I remember when I went to our front door one evening in response to a doorbell ring, and greeted a man who informed me that he was a Process Server delivering a subpoena(!) to William Everett

Sears, "Does he live here?" I don't think I ever saw my father so shaken. It turned out that the man was delivering a "summons" for my father to serve on the Grand Jury! Why was the incident so obviously frightening to him, unless he had visions of being in trouble for illegal dealings with a bootlegger? It reminds me that one of the costs of the "noble experiment" must have been the nagging fear it caused the many "law abiding" citizens who were flouting that particular law.

My mother, Gertrude, called "Trudie" by her close friends, was the daughter of Robert and Sabina Rees of Keokuk, Iowa; she was born in 1882. (How can you be more Middle-American than to be born in Minneapolis, as Helen and I were, of parents born in Oshkosh and Keokuk, respectively?)

I suppose no man can write objectively about his own mother, and I shall not try. Helen and I were very close to her, and so, after 1935, was Mabel, whose own mother died when Mabel was three. We said that if our marriage got into trouble there'd be a race to see which of us would run home to Mother first. I don't need to tell you that the love that grew between Mabel and Gertrude was one of the great joys of my life. (By the way, how do you like those two oldfashioned names, Mabel and Gertrude? Mabel's mother was also named Mabel; Gertrude's mother was Sabina Melissa.)

Mother's grandfathers were both immigrants: William Rees, a Welsh preacher, and Karl Otto Ulrich Klaus von Preen, a Prussian military officer. William Rees preached on the streets of London, but he was not Church of England, so he was put in jail. The family legend is that he was happy in jail, preaching to a captive audience, his fellow prisoners. So he was thrown out of jail, and, disgusted, he emigrated to Iowa, where he raised twelve children, including Mother's father Robert and her Uncle Thomas, mentioned above (Introduction).

Karl Otto Ulrich Klaus von Preen, eldest son of a well-to-do family of Rostock, elected a military career and was stationed in South America (Ecuador?) as some kind of military attaché of the Hanoverian Army, when his siblings went to court to have him disinherited from the family as "a wastrel, a drunkard, and not a gentleman". Two of his fellow officers came to Rostock with him for the trial, and succeeded in proving that he was, indeed, a gentle-

man. (Proof: He was insulted in a bar and did, as required, challenge the insulter to a duel! There is no record that the duel was fought.) But the other charges apparently stuck; he was disinherited, resigned from the army, and emigrated to Van Buren County, Iowa.

There, apparently, he became a strict disciplinarian. My grandmother, Sabina Melissa, one of his two daughters, lived with us in Minneapolis after Robert Rees died, and told us the following story about her father: There was to be a ball!--clearly not a common or trivial event in their rural Iowa community. The two girls sewed party dresses for the big occasion, and were fitting each other on the night of the ball when their father, Karl Otto Ulrich Klaus, appeared and announced, "You girls are not going to this party." Shocked, the girls wanted to know "But why, Papa?" As my grandmother remembered it, their father answered, "Because now you are grown-up ladies and you have to learn to take disappointments."

My mother was, like Mabel, a true intellectual without benefit of a college education: an avid reader and lover of classical music and opera. She was probably the ideal mate for Billy Sears--convivial and possessed of a wonderful sense of humor. When they had been to a stage play or a musical on a Saturday night, she would describe it all, magically, to Helen and me in their bed the next morning. She was full of great punch lines, malapropisms, mispronunciations, fractured French, and similar comical treasures from shows, books, jokes, and daily life! These became family treasures, and inevitably, unintentionally, they drifted over into her daily conversation. Some of our neighbors must have been convinced that she was abysmally ignorant! Her store of good jokes included some considered off-color in those days; I had to learn, after some incidents, not to repeat all my mother's anecdotes in more inhibited company.

For example, there was her story of the Cockney waiter who dropped a spoon down the décolletage of a lady guest. When she told him to retrieve the spoon and he proceeded to do so, he protested, "Oh I say! I feel a perfect ass!" "Never mind my ass," says she. "Just get the spoon!" I made the mistake of telling this at a nice birthday party at the Catlins' (our neighbors). I'm sure it damaged the Sears's reputation in the neighborhood.

\* \* \* \* \*

Mother had studied some German in the Keokuk (Iowa) High School, apparently enjoyed it hugely, and loved to use it all her life, although I'm afraid some of her pronunciations weren't very German. When she wanted us, her kids, to come to the table for a meal, she might say, "Sitzen Sie, bitte." But I'm told that wasn't really very polite!

The Keokuk High School must have been interesting. The principal was "Professor Marshall". (It was never quite clear to me, or apparently to Mother, why "Professor".) When one of the teachers caught one of the students reading a "dime novel"--the name seems to have referred to "trashy", paperbound books--she sent him to "take that disgusting thing and go to Professor Marshall this minute!" He was back in ten minutes, resumed his seat and his reading. The teacher was furious: "I told you to take that book to Professor Marshall!"

"Yes Ma'am," replied the boy. "I did, but he said he didn't want to read it."

On another occasion, namely the weekly Assembly of the high school, Professor Marshall waited impatiently for the student body to quiet down so that he could address them. When they finally subsided, he announced that there was great news: the high school was going to install electric lights! After the cheers, he explained: the problem had been the lack of power to drive the dynamos. "But this administration has just recognized that a power source is available: we are going to harness Miss Gertrude Rees's tongue power!"

Mother was a forgiving parent; she even forgave me when--and I think (hope) it was the only time--I hit Helen on the head with a book, in a quarrel. But I remember one exception: Buddy Olson and I experimented with tobacco, smoking discarded cigarette butts and even cigar butts--I can hardly believe it today, but we did!--and I came home deathly sick. I got no sympathy from Mother. She told me where I'd find the bowl to be sick into, and with a look of utter disgust turned her back on me.

One of my favorite memories of Mother concerns the day that she vacuumed the interior of the Pontiac, which by that time I had

inherited to transport myself and my drums to my dance-band jobs. In a pocket of one of the doors she found a pint bottle of bootleg alcohol. Those were the days when, for us college boys at the University of Minnesota, at least, the popular social drink was spiked near-beer. (Near-beer meant "less than one half of one percent alcohol",--therefore legal.) We added enough alcohol to fill the neck of the bottle.

On this occasion my mother confronted me, in the kitchen, with the alcohol bottle and a very stern demeanor: "What is the meaning of this?"

I assured her that I had not taken up booze, had only experimented a bit on a social occasion, and, in fact, would pour the alcohol down the kitchen sink--which I proceeded to do, then and there. She stopped me. "Well," she said, "it's perfectly good grain alcohol. There's no reason to waste it. I'll put it up here on the top shelf."

It must have been two or three months later, when I was ready to leave the house on a social evening, that I thought I ought to have something in my pocket to pep up the near-beer. I looked on the top shelf, where Mother had stashed the alcohol; the alcohol was there. I took it down and it served my social need very well.

Neither Mother nor I ever brought up the subject again.

I don't believe that any of us really remember things that happened in our lives before we were 5 or 6 years old. The rest is what we've heard from our families. I am told that in 1917, when I was four, I was dressed in a soldier's suit, including a "campaign hat", and was carried on the shoulders of a soldier dressed similarly, in a parade, in Minneapolis, to sell War Bonds.

Like all other patriotic families, we had a small picture of Kaiser Wilhelm II, on a toothpick in our sugar bowl. I was not at all clear about how that would help win the war "over there". I suppose the idea was simply to remind us to conserve sugar, so that more could be sent to the troops. We were also told that sauerkraut should be called "liberty cabbage", and there were other silly rules. People threw stones at dachshunds.

I am also told that my sister Helen (two years older than I), taught me to read when I was four, and that, therefore, I was put

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into first grade instead of kindergarten when I was five. But that was against the rules (!), so I had a "postgraduate year" in kindergarten. Then, the story says, I was skipped to second grade. I don't know how accurate these legends are. I also had a love affair in George Bancroft (Grade) School, they say. She was a pupil in Miss Lego's first-grade class. My mother, who sometimes indulged in a little pun, said that I walked hand-in-hand with my girl friend to Miss Lego's room, and there I would "leggo".

I really do remember--because nobody knows this story except me--that, having been taught "long multiplication", I figured out long division by myself. (To my mathematically inclined readers: Try it. You'll see that it isn't really too difficult for a youngster who is attracted by numbers!)

I don't remember much more about my days in the George Bancroft School. I remember that the smartest girl in our class was a pretty blonde, Luella Turner, and that I was as mean to her as possible on the way to and from school on 38th Street--probably because she was so pretty and so smart. I remember more about our neighborhood, a pleasant middle-class residential neighborhood near 38th Street and 10th Avenue South. The Olsons, Morrills, Hubers, Sinclairs, Murrays, Apelts, and Catlins were our neighbors, and they all had kids the ages of Helen and me. The Morrills and the Murrays were Catholics, so in our nasty moods we shouted "Cat lickers!" at them.

Mr. Mal Huber was a locomotive engineer, definitely a glamorous occupation! He invited me to ride with him in the cab of his engine from Minneapolis to La Crosse and back! I can still hear the deafening roar in that locomotive cab and feel its awful vibration. I still see the air-brake valve shaking madly at the top of its support as that train made its incredible way, at fantastic speed, along the Mississippi River! It is the kind of memory that remains vivid because it involves, to such extremes, all the senses.

Mr. Apelt was in the slot-machine business: not the kind you use to gamble, but the kind you put a penny into to get a colored ball of chewing gum. In the Apelts' basement were stored boxes of those gum balls. The Apelt children sometimes took their playmates into their basement and allowed us to sample the balls. Sometimes, I am embarrassed to say, we put some back in the box after sampling their flavors!

On Elliot Avenue, across the back alley from our house, lived a

very important family, the Solethers: they had five attractive daughters! The father of the family, Mr. Pliny Solether, was a lawyer. At a very early age, in grade school, I wrote an essay on lawyers, extolling their virtues as guardians of the law, especially their honesty! It seems that this evaluation of attorneys was a rather original one; it attracted some attention, and my essay was reprinted in some grownups' journals. Since Mr. Solether was the only lawyer I knew, the essay was recognized as a compliment to him, as my father pointed out.

The neighborhood was a pretty quiet one, I suppose. Most of the deliveries--ice, milk, bread--were made by horse-drawn wagons, and occasionally we had some excitement when there were runaways. The iceman threw a chain through the spokes of his wagon's front wheels when he had chopped a 100-pound cake of ice down to the 50- or 25-pound chunks that the housewives wanted for their iceboxes (and had informed him by putting the "50" or "25" of their "ice-card" uppermost in their front windows). The chain was supposed to keep the horses from going anywhere while he delivered the ice. Sometimes it didn't, and the neighborhood had some excitement. The young men who dashed out into 10th Avenue, grabbed a horse's bridle, and brought the wagon to a stop were heroes.

Besides horse-drawn icewagons, I remember some other common features of those days, which have disappeared over the years. We carried our garbage out to the back alley twice a week to be picked up, just as we do now, but we also had to carry out the ashes from the furnace. Our furnaces were all coal-fired, which meant that coal-dealers' wagons came to our houses, parked at the front curb or in the back alley, and the coal was carried by hand to chutes, usually placed in basement windows, which sent the coal--probably a one- or two-ton load--into a basement room reserved for that purpose. Our mothers were unhappy about the film of coal dust that appeared on everything in the house as a result.

My father knew how to "bank" the fire in our furnace at bedtime, so that the fire would smolder overnight but not go out. When he awoke in the morning, he opened the damper of the furnace by means of a chain that ran from the furnace, through two floors, to the hallway outside his bedroom. By the time he was dressed, the furnace was putting out maximum heat, and it was time for him to

shovel out its ashes and add coal. The time came, of course, when such chores became mine to do, but the time also came when we, like most of our neighbors, had an oil burner installed in the old coal furnace and an oil tank located outside; the basement coal room was assigned to other, cleaner uses.

I remember another characteristic of American cities of those days: Their nighttime sounds were different from today's. Of course we slept with our windows open, at least in the summer. Night sounds were dominated by the sounds of steam locomotives--the switch engines. It seems to me that I went to sleep every night with the "CHUFF, CHUFF, chuff, chuff, (chuff), ..." of the engines, accelerating, then dying away, a second or two of quiet, followed by a "bang!" Then the whole sequence repeated over and over as I slipped off into sleep. It was the sound of the switch engine putting a boxcar, or cars, into motion and the car banging into the rest of the train, as it was assembled for its journey.

It was the first thing I heard when I awoke in the morning, too, and it seemed especially loud on a frosty winter morning. Our house was miles from the freight yards where all this was going on. At seven a.m. the steam whistle of the farm-implement factory--also several miles away--blew, announcing, to the whole city, the beginning of a new day.

Down on Chicago Avenue, about 35th Street, was a station of the Minneapolis Fire Department, and in those days the engine that dashed out, with bell ringing, was a horse-drawn steam-powered pumper! Could anything be more thrilling to a 10-year-old?

Yes, perhaps, for it could and did happen that an airplane flew over us! I did not live in the days when people ran out to see automobiles, but we surely did run out to look at airplanes. And every year there was the Minnesota State Fair over in St. Paul. The Olson boys, Marvin, Leslie, and Vernon, always managed to get to the big spectacle in the grandstand, and sometimes I accompanied them. For several years a feature was the race between a racing car and an airplane. The airplane was a pusher biplane whose pilot--a lady!--sitting out in the front in the wind, could see her opponent just a few feet below her. Around and around the oval track they roared, neck and neck, until in the last lap she was able to eke

out a victory!

Another perennial thriller of the State Fair, even more exciting and incredible to the Olson brothers and me, was the head-to-head collision of two steam locomotives! A straight stretch of railroad track was built in the "infield" in front of the grandstand; an old steam locomotive was placed at each end of the track, facing one another. After getting their engines under way, with throttles wide open, the engineers leaped off to the side of the track, and the roaring locomotives, whistles screaming, charged toward each other. At the middle of the track, in front of the grandstand, they met in a spectacular crash. Fire, smoke, and billows of steam! The memory of the fantastic event was a source of wonder, conversation, and vivid description until the next year's Fair.

To a youngster growing up in a city in those days--at least to a youngster interested in machines and such things--a constant source of fascination was the streetcar system, which in the Twin Cities was an efficient and effective one. I always tried to sit right up in the front of the car where I could study the motorman's techniques. He had two controls, one operated by each of his hands, and a gong activated by his left heel. It became clear to me that his main control was a kind of throttle (actually a rheostat, I think), which he opened to put the car into motion or increase its speed and closed to reduce or cut off the power to the drive wheels. But if he opened this "throttle" too fast, I saw, a circuit-breaker was tripped, with a loud "bang", startling the passengers and obviously annoying the motorman. I also observed his operation of the brakes. They were air brakes, and it was clear that what he controlled was a valve that admitted compressed air into brake cylinders: not metering the braking force itself, as in an automobile's brakes, but the rate of increase or decrease of that force.

And, for even more entertainment, there was the little drama when the trolley slipper came off the trolley wire, as often occurred at intersections, cutting off all power, including all the lights, and requiring the conductor (no pun) to go to the rear platform of the car and coax the slipper back onto the overhead wire by means of the rope hanging from it.

In those days our mothers or our older sisters "called-for" us

to come in from play for supper, or when we were otherwise needed at home, by simply shouting for us from the front doors. And when we went out to play we stood in front of our playmates' houses and "called-for" them. I don't think these penetrating soprano calls are used in today's residential neighborhoods. Too bad. Each caller developed her or his characteristic style.

My mother said that whenever I was out playing in the neighborhood she could hear my distinctive, fortissimo soprano above everything else.

She also remembered hearing me and the other neighborhood kids come noisily home from school. We all agreed that we should play ball that afternoon, but her son's shrill voice announced that there would have to be a delay while he changed his clothes: "I've got my second-best pants on," he explained.

Our house at 3740 South 10th Avenue had been built by the Evans family, who lived at the 37th-Street corner. We were told that Miss Eliza Evans herself had nailed the shingles to the roof--and that was certainly eccentric, for women didn't do that sort of work then. One of my pleasant chores was to deliver my father's checks to the local merchants, and the mortgage check to Miss Evans, on the 10th of each month. The grocer usually responded with a piece of fruit or penny candy, and the druggist might even give me a soda!

When I delivered the mortgage check one day (in 1920, I suppose), my mother told me to ask Miss Evans whether she had voted. Mother was certain that she had; it was the first time that women were allowed to vote for President! Miss Evans answered, "You tell your mother that I certainly did, and now I'm waiting for the League of Nations." I delivered the message, not completely sure of its import. My mother's reaction was, with a little shock, "Oh, for goodness' sake! That means she voted Democratic!"

My grandfather, David J. Sears, once came to Minneapolis to visit us; I must have been about ten or eleven. I suppose it was at the time of his retirement. He had made his whole career in the lumber business; I presume he was an employee of some part of the far-flung Weyerhaeuser empire, because we received a message saying that Mr. \_\_\_\_\_ Weyerhaeuser, a young executive of the corpora-

tion, whose office was in St. Paul, wished that Mr. D. J. Sears would be his guest for lunch at the St. Paul Club, a big-businessmen's club in downtown St. Paul. My father was not available to take his father to St. Paul, and, after some discussion, the family decided that I should be given the responsibility. All I had to do was to get us on the streetcar at 38th St. and Chicago Ave., get off at Lake St., and wait for the right east-bound car, which would take us across the Mississippi River and, ultimately, into downtown St. Paul, close to the Club.

We did all this, and met the young lumber tycoon on schedule. I was properly impressed by the Club and by the young millionaire. He asked us if we wished to go to the men's room before lunch, which both generations of Searses did. As we three lined up at the urinals, Mr. Weyerhaeuser remarked, "Gee, Mr. Sears, you sure do piss a good stream for a man your age!" My grandfather was obviously embarrassed by such a remark, and did not answer. Whereupon his young host volunteered, "I used to be able to piss over an ice wagon when I was younger, but I can't any more!" Oh dear! It was an occasion for a youngster from a respectable, business-oriented, Republican family to be shocked and disillusioned. Mr. Weyerhaeuser's vulgarity is all that I remember about the occasion now, sixty-some years later.

My father's brother Roy also visited us once. That was a most impressive occasion, because he drove to Minneapolis from Rock Island in a beautiful new car that he had won in a raffle--the only relative of mine who ever won anything in a raffle! What's more, the car was an R & V Knight! (What, dear reader, you don't remember the R & V Knight? That's not surprising, because not many were built.) The name "Knight" in a car's name, such as "Willys-Knight" (much better known) signified that the engine was of the "sleeve-valve" type: each cylinder was enclosed in two concentric, sliding "sleeves" having ports whose coincidence provided valves for intake and exhaust. I think the sleeve-valve invention was a great one in several respects, but the sleeves had to be lubricated, made cylinder cooling difficult, and increased the cost of the engine. Anyway, Uncle Roy's splendid, expensive R & V Knight sat in front of our house long enough to make a big impression on Tenth Avenue.

But there were some occasions when I could make an even bigger

impression with the aid of an automobile. One of the couples who played bridge regularly with my mother and father were the Littlewoods. [Another couple were the Bushes; my father maintained that, somewhere in the past, the Littlewoods and Bushes must have been related!] Joe Littlewood was the local dealer for one of the luxury cars--Pierce-Arrow, I think. When it happened that I could ride with him in one of his cars, I certainly sat up straight and close to the window, hoping that our friends and neighbors would see me.

The other couple that made up the regular bridge game were the Harry Whites; they were older than my parents and I think Harry must have been retired. He was a wonderful old guy with twinkling eyes, quite fond of Helen and me. He always greeted me with a bit of doggerel from somewhere:

"Old Bill from Lemon Hill!  
He never worked and he never will!"

I can't remember just how the friendship started between my parents and a family named Sabom--perhaps it was just a merchant/customer friendship. Mr. Sabom owned and operated a retail business in window shades. The name of his company was "Mobas"--his name spelled backward--which may already suggest that he had a sense of humor--and it used a little panel truck with that name displayed on it.

But the panel truck also displayed, in a bold sign above its windshield, the message "THIS TRUCK IS DRIVEN BY A BLIND MAN!"

Apparently this joke worked so well that it frightened pedestrians and other drivers. Mr. Sabom was required, by the Minneapolis Police Department, to remove the sign!

My father, as sales manager of Curtis-Yale-Howard (later Curtis-Yale-Holland), traveled around the company's sales territory in Minnesota, the Dakotas, and Wisconsin about once a year, to cement relationships with the lumberyards that were the retail outlets for Curtis's sash, doors, and millwork. Some of the merchants he visited became his good friends. One example was the Gay family, proprietors of the lumberyard in Moose Lake, Minnesota. The family had three big, handsome sons: Chester, Clayton, and Kenneth, all several years older than Helen and I, and all--one at a time--linemen of

the varsity football team at the University of Minnesota: a guard, an end, and a tackle, respectively. In other words, there was a dynasty of 9 or 10 years when there was always a Gay brother in Minnesota's first-string line. My father, a loyal Gopher fan, was almost as proud of them as if they had been his sons. They were often--again one at a time--battle-scarred guests at our Sunday dinner after the Saturday game. I hardly need to point out that this made a major impression in the neighborhood. Once Chet, the guard, even managed to swipe a varsity football from the team's practice session and present it to me!

My father learned that their family name had originally, in France, been Gay de la Roux, which he translated, without benefit of any knowledge of French (or, apparently, its pronunciation), as "Gay of the Line."

Another family whose friendship embraced our whole family were the Norbert Borgerdings, who owned the lumberyard in Melrose, Minnesota, and later in Park Rapids, Minnesota. Norbert and his wife Tillie had a very pretty and charming daughter, Lenore, about the same age as I. Helen and I were invited to spend a week (or, perhaps, two weeks) with the Borgerdings at their lake cottage near Park Rapids one summer--had a wonderful time, and I would have liked very much to consider Lenore "my girl", but Tillie made it very clear that no Protestant could possibly be eligible for that honor.

I know which summer that was, namely 1927, because Charles Lindbergh returned from Paris after his famous flight and made a historic public appearance and speech in his home town, Little Falls, Minnesota. The Borgerdings and we, their house guests, drove to Little Falls, and I found a place to stand right in front of the speaker's platform, at Lindbergh's feet, to hear his speech and take his photograph! I was 14.

There was another Bill Sears in South Minneapolis, a pharmacist, who became rather famous in the newspapers. He kept a revolver under the counter of his drugstore, near the cash register. Occasionally his store was held up, and when he went to the cash register he also reached for the revolver. The robbers never got any of Bill Sears's money, but once they got away with his gun--I don't remember how. The Minneapolis police took up a collection and

bought him a new one. When Helen and I read all this in the papers, we rode out to his drugstore on our bicycles and introduced ourselves. He treated us to ice-cream sodas.

The name is not a common one, but I have run across a few namesakes. There was a rather well-known photographer, Bill Sears, in Tucson when we moved here. He has died, and so, just recently, has another Bill Sears, who lived nearby and was a beloved and world-famous writer of the Bahá'í faith. When I first came to the University of Arizona to stay (1974) there was also a young William R. Sears in the university's computing center. In a university of this type the administrative red tape is ponderous; I was not surprised to have to wait some weeks for my first paycheck, but after a month it became serious and I had to trace the problem down. It turned out that the paycheck computer had correctly printed his paycheck and mine, each with its proper Social Security number, department name, and amount. But a human being interfered, saw two paychecks with the same name, and tore up mine!

Later I began to get long-distance telephone calls, meant for the computer specialist, in the wee hours of the night. Here is a sample:

PHONE: Is that Bill Sears?

ME: Yes.

PHONE: Of the University of Arizona?

ME: Yes.

PHONE: Of the Computer Center?

ME: No! And when you call someone at 2:30 a.m., don't you think you should be sure you are calling the right number?

PHONE: (Pause) I don't know what you're getting so mad about. If it had been the other Bill Sears, he would have been up.

The logic still escapes me. I guess I should have said "Oh,

that's all right; I had to get up to answer the phone anyway!"

In 1939, when I became an engineering consultant to the Northrop company, I had to acquire a clearance for access to classified information, and that, in turn, required that I submit my birth certificate. I wrote to my mother, who still lived in Minneapolis, and asked her to get a signed and sealed copy for me. But when it arrived I was dismayed to find that I wasn't William Rees Sears but William Robert Rees Sears! I don't know what possessed my parents at the point of registering my birth, but they really had given me that four-cylinder name. I had never used the "Robert", so all my diplomas, licenses, insurance policies, and even a few published papers carried the wrong name. I asked Mother to make another trip to the Minneapolis Courthouse and to find out what I had to do to change my name. I supposed that I would have to hire a lawyer, go to court, ...

Mother went to the same window in the courthouse at which she had acquired the certificate and asked "What does this person have to do to drop one of these middle names?"

The official looked at her over his glasses and asked "Who are you?" "His mother," she replied, whereupon he handed her a blank form and a pen. "Write the new name here and sign your name."

That was all it took, but the resulting birth certificate, which I have used ever since, of course, tells a sad story: "Born (alive) 1 March 1913. Name supplied by mother 7/13/39." One can only surmise that the poor child was nameless for 26 years.

But to get back to the story: I went to Bryant Junior High School and there took Mechanical Drawing, as part of the required sequence of "shop" courses: woodworking, tin shop, print shop, electricity. The teacher was a Mr. Schimele, who, at the end of a term, told me I was cut out to be an engineer. Now, an eighth-grader has only the vaguest idea of what an engineer is or does, but it sounded good to me and delighted my father. Then and there I decided to be an engineer. I also fell in love with airplanes--built model airplanes and subscribed to Aero Digest--as did quite a few of my contemporaries. By the time the University of Minnesota established a program in Aeronautical Engineering in the late '20s, I was in high school, and it seemed clear to me that everything was

going my way.

But I had never yet had a ride in an airplane! So I went out to Wold-Chamberlin Field, Minneapolis's municipal airport and got a job, of a sort, handing out handbills advertising the sight-seeing rides that people could buy. These were the days when families spent their Sunday mornings or afternoons at the airport, watching the airplanes land and take off. My assignment was to walk back and forth along the fence where the spectators stood and to hand the advertisement to the latest arrivals. I did that for about seven hours; the crowd was thinning and the sun was low in the West when the entrepreneur said, "OK, kid, you go on the next flight."

The airplane was the famous Ford Trimotor; it carried about a dozen passengers in two rows of wicker seats. It droned down the runway, defied gravity, and flew! About then I noticed that the passenger opposite me--we were in the two farthest-aft seats--had covered her head in her arms and was moaning piteously, in abject fear. This she continued through the 15-minute flight, only looking up after we had landed. But I didn't let that spoil my thrilling first airplane ride!

My second airplane flight was just as thrilling. I had taken up the art of building and flying the delicate balsa-wood and tissue-paper model airplanes powered by twisted rubber bands. The goal was maximum endurance, so they were extremely light, their carved balsa propellers turned as slowly as possible, and their rubber-band motors were lubricated. They were flown indoors. As I recall, the city's championship contest was held in the National Guard Armory, and I won it! My prize was a flight, again at Wold-Chamberlin, in an open-cockpit biplane. The pilot was Fred Whittemore, chief pilot of Northwest Airways. Being such a seasoned, sophisticated veteran flyer (after my Ford Trimotor ride), I was truly appreciative of this glamorous flight in the slipstream and noise, wearing helmet, goggles, and (at least in spirit) silk scarf! The airplane was a Waco UFF-7.

Wold-Chamberlin must have permitted off-runway operations--or is it possible that it was an all-directional grass field in those days? Anyway, I do remember that Whittemore took off across the grass, gave me a beautiful ride, with graceful but not violent aerobatics, and executed a precision landing exactly on the center-point of the airport, where there was a concrete marker in the

grass. (It was some kind of sign or insignia, made of concrete, flush with the grass surface--perhaps it read "MSP" or was a compass rose.) The Waco had a tailskid. The approach to landing was quiet except for wind noise and the engine ticking over, but he landed exactly upon the marker, and the tailskid made a loud, startling noise when it hit the concrete! Except for the seat belt I might have jumped out of the cockpit!

Northwest Airways grew to become Northwest Airlines. Inc., of the present day, and Mr. Whittimore went on to become an executive there.

I had taken up drums and drumming very seriously, and played in both the orchestra and band in junior high school. My drum teacher was a handsome young university student, Wallace Lageson, recommended to me, via my father, by the great University of Minnesota band director, Mike Jalma. Wally was a candidate in dentistry, and would have been a wonderful dentist, but he was too good a drummer! Unlike most of the college boys who worked their way through school by playing in dance bands, Wally played in one of the Twin Cities' most popular professional bands, at the Marigold Ballroom. The famous orchestra leader and composer, Isham Jones, came to Minneapolis, heard the band at the Marigold, and took it over. The new Isham Jones Orchestra took its place on the national, coast-to-coast circuit. He didn't keep all the players he acquired in Minneapolis, but he did keep Wally, who assured his friends--and his mother--that he would go on the big circuit with Isham Jones "for a few years" and then get back to the university and dentistry. That's an old and familiar story that always seems to have the same ending. He had a great musical career as Jones's drummer, never returned to Minneapolis, and died young--probably in his thirties.

Upon arriving in Central High School I met Mr. Sydney Morse, who advised me to study tympani. To that end, and with Wally's advice, I became a student of Wilhelm Faetkenheuer, the redoubtable tympanist of the Minneapolis Symphony Orchestra. Mr. Faetkenheuer sneered at everything I had learned about playing the snare drum--called me "a chazz drummer"--and taught me, with strict, Teutonic precision and no mercy, the art of playing (and tuning) the tympani. Following a suggestion from my father, I found that his disposition (or my playing technique?) was much improved by my

bringing him a good cigar when I went to the MacPhail music school for my tympani lesson.

I think the musical education I received at Central High was good. Our symphony orchestra, under the direction of Mr. Morse, won highest honors in the state-wide competition, where we played "Finlandia" (which has a great tympani part) and the first movement of Mozart's G-Minor Symphony. The head of the panel of judges was Henri Verbrugghen, irascible conductor of the Minneapolis Symphony Orchestra. I overheard him needling Mr. Morse for having one of the parts of the Mozart played by a trumpet, for lack of a Central student to play it on bass clarinet or English horn or some such exotic instrument. What he said, with dripping irony, was, "I must say, Mr. Morse, until today I had not realized that Mozart used so much brass in the G-Minor."

When I repeated the story later, at the University, I was told the following anecdote about the same Belgian maestro: The University's concert band was scheduled to play the "1812 Overture" of Tchaikovsky together with the Minneapolis Symphony, as part of a big production held to open Northrop Auditorium or the new football stadium or something. Mr. Jalma reasoned, quite intelligently, I think, that Verbrugghen's professionals could transpose their parts to the band's key much more easily than Jalma's college kids could transpose the other way. When Jalma made this proposal to him, Verbrugghen replied, "Do you know something about harmony, Mr. Jalma?"

JALMA (puzzled): Yes, of course.

VERBRUGGHEN: Then you understand consecutive fifths, Mr. Jalma!

At which point he placed his two hands "in tandem", one thumb on his nose, and wagged the fingers of both hands.

We also played such classics as Wagner's "Rienzi", in which I had terrific tympani and snare-drum parts. When we played the "1812" and "Light Cavalry", I played (?) the gun shots! I found that my father had a .32-caliber revolver, and I borrowed it for musical purposes; the high school provided the blank cartridges. I am afraid that I must confess to brandishing the revolver in study-

hall, for the entertainment of my fellow students. Once, the study-hall teacher (poor girl!) figured out that I was the troublemaker and sent me to the principal. I was embarrassed. My father was the president of the Parents' and Teachers' Association that year. As I wended my way slowly toward his office I met my sister Helen on the stairway. She said, "The principal wants to see you." "Yes," I said, "I know that."

So I went to the principal's office. His secretary, when I entered the office, said, "Oh, yes, William, I have a letter here for your father," gave me an envelope addressed to my father--something to do with the P.T.A., no doubt, and thanked me for coming in. I returned to the study-hall and the glaring scowl of the teacher; I assured her that, Yes, ma'am, I had gone to the principal's office.

One year--I think it must have been my first or second year at Central--the big musical event of the year was a production of "The Mikado" of Gilbert and Sullivan. Every afternoon the high school was filled with the great sounds of the big choruses--especially the opening one: "We are gentlemen of Japan....." and the Finale--as Mr. Morse rehearsed his singers, and every morning our orchestra struggled with its accompaniment. What we didn't know, or at least didn't appreciate until the time came for the dress rehearsal one day before the big event, was that Mr. Morse had acquired the famous comedian De Wolf Hopper, as guest soloist, to play the part of Koko!

The dress rehearsal was puzzling to us kids. Hopper knew all the parts, and amazed us with his comical antics, but didn't finish any of his songs--just started them and had us move on to the next one. It also turned out that he had a lot of ideas about the drummers' contributions, beyond what Gilbert and Sullivan had written, and he cheerfully instructed me about them.

The public performance on the next evening was something different! He shouted, he grimaced, he threw himself about the stage--though he was an old man by then! I was not an experienced pit drummer, but I think I caught most of the drum rolls and cymbal crashes that accompanied his uninhibited, incredible swoops and pratfalls! At any rate, when the performance was over he signed his name on my drum head with a flourish and a friendly message.

In an intermission between acts of "The Mikado", Hopper recited his famous version of "Casey at the Bat". At its end, when Mighty Casey had struck out, tears were streaming down his face. The audience--and the pit orchestra--were standing and cheering. It was an experience I'll never forget!

Sydney Morse was not the only teacher whom I remember with great affection. We also had a mathematics teacher, Mr. Walter D. Rodgers, who volunteered to give courses in College Algebra, Trigonometry, and even Spherical Trigonometry, for the benefit of a little clique of us who were headed for college programs in math, science, and engineering. He was a tall, gaunt, scholarly, somewhat taciturn, gray-haired gentleman, who taught us to love mathematics. I remember that, to teach us Spherical Trig, he had a large globe painted with blackboard paint, which he held in front of him while he drew triangles on it with blackboard chalk. Since he always wore double-breasted blue-serge suits, and embraced the chalky globe three times a week, his dry-cleaning bills must have been large.

Our Physics and Chemistry teachers were also great. I think our Civics teacher was also very good, but unfortunately was very hard of hearing; he wore a hearing aid but didn't have any directional discrimination. We students were merciless, I'm afraid, the way we passed answers to his questions around the room, out loud, while keeping straight faces.

We had a term of Shakespeare from a spinster teacher named McDill. She and I didn't quite hit it off. Her method was to have her pupils read the parts, in the classroom. In "Twelfth Night" there is a scene where Sir Toby Belch is accused of lethargy, but he is a bit drunk and answers, "Lechery! I defy lechery!" (Act I, Sc.v). I was called upon to read the part of Sir Toby.

Now, in those days high-school students were not supposed to hear words like "lechery", so the book I was reading from--I think it was Helen's--had been bowdlerized and read like this:

"OLIVIA: Cousin, cousin, how have you come so early by this lethargy?"

"SIR TOBY: [Lethargy]! I defy [lethargy]!"

So, of course, I read it to the class as "Lethargy! I defy lethargy!" The class loudly protested; they and Miss McDill were reading books that had not been bowdlerized. My classmates were looking forward to a discussion of the naughty word, and she was probably hoping to slough over the matter--and here was the Sears brat bringing it to everyone's attention! She was furious. I suppose I ultimately explained my innocence to her.

I was a candidate for a scholarship at Harvard, to be granted by the Harvard Club of Minneapolis. Miss McDill was one of the teachers who was asked to recommend me. She "found" that she "couldn't recommend [me] as a gentleman". She told me this, and explained: She had come into her classroom one day, after we, her students, had already gathered there, and (horrors!) found me with my feet resting on the seat of an empty chair in front of me.

Actually, my parents were somewhat relieved that I failed to win the Harvard scholarship. It covered tuition only, and I would have had to struggle to meet living expenses. The Great Depression was upon us (1930). My life would have been changed greatly if I had gone to Harvard; if you read on, you'll find that my path from University of Minnesota to Caltech and von Kármán (and Mabel!) and Northrop and Cornell might have been all fouled up! I suggest that you not shed any tears for young Bill Sears, who didn't get to go to Harvard, with the help of Genevieve McDill, but had four good years at the University of Minnesota.

There were some truly remarkable young men and women in our class (of several hundred) at Central High. The one who became most famous, no doubt, was the tall, slender boy from North Dakota known to us as Arnold Severeid. He is better known to the world as Eric Severeid--the distinguished television newsman. The first of his several books, *Canoeing With the Cree* (MacMillan, N.Y., 1935), is referred to as "a juvenile"; he wrote it while he was still "Arnold", just out of Central High.

It is the story of a wonderful canoe trip made by Arnie and his good friend, the president of our Class of 1930, Walter Port. They undertook to paddle (and portage) from Minneapolis to Hudson Bay, and to write about it in the Minneapolis Star. They were so enterprising as to obtain financial support for the trip, which couldn't have been very much but was surely needed. They didn't quite make

it all the way to Hudson Bay, but they came very close, and Arnie's book is a first-class job.

His career as a newsman was brilliant and full of drama and danger, as many of my readers will already know. He established himself as a wise and articulate senior commentator. In 1946 he published *Not So Wild a Dream* (Knopf, N.Y., 1946), which is, in the form of a memoir, the story of his youth and wartime career. It is, in fact, an anecdotal autobiography like this one!--if I can be permitted to compare my modest account with the work of a distinguished professional! *Not So Wild a Dream* is a delightful book to read.

Arnold Sevareid didn't enter the University of Minnesota in 1930 as I did; he spent a year working to replenish funds, and entered in 1931. When the time came that I had the Pontiac for daily transportation, we made some kind of an agreement by which I picked him up every morning and drove us to the campus. We discussed world affairs, en route: he the student of journalism and political science, and I the model--perhaps a caricature?--of the student of engineering! I remember his asking me, once, "How can anyone so young as you be so conservative as you?"

This warm friendship with Arnold/Eric Sevareid was an important facet of my university years; his liberal views were an antidote to my conservative ideas, which, I'm afraid, were not based on much rational thought. I hope my readers will read *Not So Wild a Dream*. There they will learn how he helped organize a protest against compulsory military training (ROTC) at the University of Minnesota; for this and other anti-war activities he was denied the position of Editor of the *Minnesota Daily*, the student newspaper. And they will learn how he survived the crash landing of a military transport airplane flying "over the hump" in Burma. And much more.

I must go on to other matters, but not before telling a little story on Mabel. I was in Washington, D.C., on government business; I asked Mabel to wait for me at Butler's, the "business terminal" of National Airport. She arrived before me, a bit ahead of schedule, and found herself sitting beside Eric, who was on his way out of Washington and was awaiting his airplane and pilot. She knew she ought to introduce herself, but just couldn't bring herself to accost such a famous public figure, and only hoped I would show up in time to greet my old buddy. I didn't, and she was left to tell

me and to confess to her shyness.

When I was in high school, there was, in the class ahead of us --the Class of 1929--a lovely young lady, on whom I developed a wonderful "crush"---typical and traditional, I think, for a romantic teen-ager. She was Isobel Gregory, and my infatuation for her was fed by the fact that her picture appeared frequently in the newspapers. The administrators of Central High were much too progressive to permit a beauty contest, but they did allow the Physical Ed department to put on a posture contest, for which the girls wore (suitably demure) bathing suits. Isobel, of course! won the contest, and photographs of her in her bathing suit appeared in all the papers. I cut them all out and treasured them--but, of course, never had the nerve to ask her for a date.

In 1980 our Class of 1930 held its 50th reunion, in Minneapolis, and Mabel and I attended. I learned that Isobel and her husband, Pat Walling, having organized their 50th reunion the year before, had volunteered to assist in the planning and putting-on of ours. The first event of the occasion, albeit informal and unplanned, occurred when the attendees found themselves meeting at the hotel on the evening before the festivities. We took over a big table and ordered drinks. My spirits high, I told the gang at the table that Isobel would surely be around tomorrow and that I had been deeply in love with her, from afar, in 1929 and for 51 years. I think every man at the table said, "Of course, so was I, so were we all!"

Next day, sure enough, there she was, some gray in her hair but as beautiful as ever, and still having the prize-winning posture. So I told her what had occurred the evening before. She was absolutely delighted: "Oh, Bill, you've made my day!" And I was rewarded by a big hug and kiss!

One of the great things about the Isobel Gregory story is that she and Pat Walling played the leads in the Class Play of 1929. Pat was a handsome football star, with a winning personality, probably as attractive to the girls as Isobel was to us boys. I believe he became a police detective and chief of detectives of the Minneapolis Police Department, and, I am told, rose to the position of Chief of Police.

**CHAPTER 2****UNDERGRADUATE YEARS**

In the summer of 1930, between high school and college, I got a job as a stockboy in Dayton's, Minneapolis's leading department store. I was responsible for stocking the Drugs, Bedding, and Notions departments in the downstairs store. It was an interesting experience. The ladies who bossed those three departments were friendly; their immediate supervisor was the floorwalker of the downstairs store, a pleasant young man who became a friend of mine when he learned that I was a fellow tennis player. Two of my good friends at Central High were the Scherer twins, John and Paul, with whom I had played occasionally, but whose prowess on the courts was well beyond mine. He, the floorwalker, played "in their league" and was delighted that I knew the Scherers and appreciated their skill.

I learned a bit about the retail-store world that summer. Being a stockboy was a little like being a fireman: there were hours when my job was only to be there, in one of my stockrooms, on call if needed. Before and after store hours I was very busy, transporting the merchandise from the stockrooms in the sub-basement to the selling floor. Whenever I was called to the selling floor, I was told, I must walk briskly and purposefully--must look busy. To bring merchandise to the floor, I was to use a "truck"--a kind of

pushcart--bringing it up two floors on the freight elevator. I soon discovered that, although that was my job and the store's business depended upon it, it could only be accomplished by establishing the right, jovial relationships with the old Swedish fellows who operated the freight elevators, because all the stockboys were trying to get their trucks on at the same time. I had to ask about their families and join them in their hopeless pursuit, with flailing brooms, of the giant cockroaches that inhabited Dayton's sub-basement. I had to convince them that I was not some snooty college kid.

I think this reinforced my decision to be an engineer: an engineer, I reasoned, would be involved with machines, structures, the laws of physics, and wouldn't have to depend on personalities and personal relationships in order to get his work done! Of course I was quite wrong. There are probably no jobs in this world that don't involve, and depend upon, personal relationships. Some of my teachers and counselors at the YMCA and its summer camps had told us this, but their advice didn't convince me. I had to learn it by experience.

I also learned that I didn't like the "fireman" type of job where there were hours of nothing to do but wait, for either a summons from the selling floor or the arrival of closing time when I could begin stocking my departments for the next day. I didn't like to work in a job that turned me into a clock-watcher.

The only really unpleasant experience I remember from that summer at Dayton's occurred one day when the boss-lady of Blankets summoned me to her department, where she was selling an item to a customer who wanted blankets just like those she was being shown, but in a different color. "Run back down to the stockroom, Billy, and bring a set of them."

But I happen to be red/green colorblind, as are several percent of all males, so, of course, I brought from the stockroom green blankets (or brown, or tan, or something)--purely by guess and with optimism, when red (or pink, or rose, or something) was wanted. I probably cost Dayton's a sale--perhaps a customer--and I got a scolding.

Nevertheless, at the end of the summer my boss, the floorwalker, urged me to change my studies at the university from aero engineering to merchandising. As inducements he promised summer jobs

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(better than stockboy) every summer and at every Christmas vacation, and a permanent job upon graduation. I demurred, fully confident (the confidence of youth!) that there would be a career for me in glamorous Aviation. The fact was that, of all fifteen or twenty of us who graduated in Aeronautical Engineering in 1934, I think none went directly into jobs in the aeronautical industry. Three or four went into the military services, and three of us went into graduate schools.

So I matriculated at the university in the Fall of 1930. Tuition for engineering students was \$90 per quarter; for other undergraduates it was \$60. The College of Engineering firmly refused to give credit for Mr. Rodgers's courses in trigonometry and college algebra; I had to take these courses again. (I began to learn about university bureaucracy from my first day.)

In one of my first classes, one of the math courses, our teacher, a young Instructor, asked each student what he intended to choose as his major subject. Two of us replied "aeronautical engineering", whereupon the young Instructor stood in front of us, grinning evilly, and said, "Oh, aeronautical engineering! Ha ha! We'll see, when you get to *Aerodynamics*, whether you'll be aeronautical engineers or not!" The implication, of course, was that I and the other misguided student would find out, when we came to *Aerodynamics*, what a difficult profession we were trying to enter. I introduced myself to the other student, who turned out to be Benedict Cohn. We were classmates for the next four years, close friends, regular lunch-time companions, and fellow devotees of symphonic music and the Minneapolis Symphony Orchestra. Ben Cohn was a brilliant student and, after graduate study at New York University, made his career at Boeing as a key aeronautical engineer. I have no idea what became of the Instructor.

In that freshman year we had no aeronautical courses--just the basic engineering subjects: mathematics, mechanics, physics, chemistry, and English. This last was primarily a course in technical writing. I remember that one assignment was to "describe a process". I really guessed wrong about what the instructor wanted, and wrote the kind of instruction book that comes with assemble-it-yourself hardware: "Put machine screws M into tapped holes H and tighten with wrench W..." (I wrote instructions for replacing

broken strings in a tennis racket.) I think I made the instructor happy, because he selected my effort as a perfect example of what he did not want, and read it aloud to the class, in such an exaggerated style, and with such facial expressions, as to amuse everyone (except me).

I was embarrassed, but survived. A few days later, when we were being taught about colloquialisms, provincialisms, cant, etc., our textbook gave us examples. One of the colloquialisms listed was "went with" for "became of", and nobody in the classroom recognized that one, except me. It sounded very natural to me, and I said so. The instructor asked for an example and I offered "What went with the beer?" "Oh, I get it," said he, "and the answer is not 'Pretzels' but 'Grandpa drank it', eh?"

I tried out for the Marching Band, playing snare drum, and the Concert Band, playing tympani, and was accepted for both. Mike Jalma had just retired as Director, and was succeeded, temporarily, by Mr. Allen Abbott, band director at South High School, who loved to scold "you damn drummers". I found myself a member of an awfully good percussion section, one member of which was a big, Irish, aeronautical-engineering senior named Owen Cunningham. Owen had played under Abbott at South High and knew that his bluster and his prejudice against drummers were not to be taken seriously.

I hadn't had much experience with marching-band drumming. It is an art that requires physical skill and endurance, as well as the ability to master and memorize the parts. This is true for any band instrument, but, as my readers may or may not have noticed, marching drummers do not get any periods of rest between pieces. The drum--and in this case we are talking about a big military drum, 14 or 15 inches high--is swinging on a sling over your left shoulder and is supposed to ride against your right thigh and be played, continuously, by you as you march.

I'll bet there wasn't a better drum section in the Big Ten. Besides Owen Cunningham there was the "first chair" of our drum section, a feisty little medical student from Rockford, Illinois, named Bruce Canfield. Bruce and his identical twin, Burt (who played saxophone), finished medical school in about 1934 and returned to Rockford to set up their joint medical practice. When Owen graduated from Aeronautical Engineering in 1932 (or 1933?),

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there were no jobs. He had worked his way through college as a steamfitter, and upon graduation took a job in the Steamfitters' Union.

In those days there was compulsory military training (two years, i.e. six quarters) at the University of Minnesota. (Arnold Severeid had not yet organized his protests.) We who were members of the Marching Band were excused from this "drill", because the Marching Band was officially the band of the R.O.T.C. contingent--but only after one quarter of "Basic Military Training". So I, like all other male American students, marched and learned to salute, present arms, polish a rifle, smartly change it from one shoulder to the other, etc.

Then I discovered that the R.O.T.C. contingent also had a Drum-and-Bugle Corps, whose only function was to march in the Spring Review, and that this outfit was in need of a drum teacher. I applied for the job, got it, and taught its drummers--8 or 10 novices--how to march and play, in a room under the seats of the football stadium. I realized that my task was to prepare them for a march of several miles, during all of which, I hoped, they could beat out a simple, repetitive rhythm, without collapsing from the beating they would take from the swinging military drums.

Well, I think I succeeded, pretty much. They all made it through the whole parade! But what I didn't anticipate was the beating the drums took. (No pun intended.) It turned out that as my novice drummers got tired, on the long march, they became sloppy with the position of their drumsticks, and put the business ends of their drumsticks through the drumheads. Not all of them, of course, but several.

About this time I was trying to break into the dance-band business. I had acquired a set of drums and accessories and knew fellows who were members of various little combos; I let it be known that I wanted jobs. [I don't remember that we called them "gigs" in those days.] These were rather strange little bands, and we played some somewhat strange music, sometimes at strange places. One job turned out to be almost a steady one: at a sort of night club out on Lyndale Avenue, near a major east-west highway, probably about 80th, where I played with a small combo several nights a week all

one summer. But there were other nights when I found myself playing oldfashioned, honky-tonk jazz, and even music for square dancing, for special audiences--such as, at least once, a crowd made up entirely of Native Americans.

On a few occasions we didn't get paid, or got paid only part of what had been promised. I remember particularly the night we got paid at intermission time--10:30 or 11:00. When we asked why, the manager explained, "Things sometimes get kinda confused around here at the end of the evening." Sure enough, before 1:00 a.m., which was supposed to be our quitting time, there were several fights going on on the dance floor, and when these began to look ugly, with knife blades in evidence, we understood the manager's remark. We quietly packed our instruments and departed.

Happily, I was approached one day by Ed Fleckenstein, another medical student, saxophonist, and University Band member, who led a six-piece dance band that was rather popular among the fraternities and sororities for their weekend parties. "Fleck" needed a drummer and hired me. I was his drummer for about three years.

We were not the best, the most popular, or the most expensive of the four or five most visible campus dance bands, but we worked pretty regularly, at least during the school year. The other players were interesting. The pianist, Spence Brader, was talented, a great connoisseur of both classical music and jazz, and a man of great character and intellect. He later became the Director of Welfare for the City of St. Paul. The trumpeter was Stanley Kinyon, a Law student and important member of the University Bands. Stan was a brilliant student, was elected to Law Review, and finally became a professor and the Assistant Dean of the Law School at the University. There was also a trombonist, who joined us when a seven-piece band was wanted, a younger man who was also a university student.

When I moved up to Fleck's band, it was necessary that I become a member of the Musicians' Union. Having had some experience with nonunion bands, described above, I was ready to believe that the union could be an important aid, and I was right. We played for union wages, which required increments for week-ends and holidays, for wearing tuxedos, etc. If we demanded, we could require that the payment for our work be paid to the union instead of the band leader, and thence to us.

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My father was a bit apprehensive--never mind his congenital distrust of any labor union!--because he knew that the Musicians' Union operated a Musicians' Club, which he was not sure was a good environment for me. Once again he went to his friend Mike Jalma for advice. Mike told him, "Of course Bill's got to join the union if he expects to play professionally, but you tell him to stay away from the club." Which I did.

So I worked my way through the University of Minnesota, mostly as the drummer of Fleck's Dance Band. In the summer of 1932 we were hired to play regularly, several days of the week, at a posh club--I think it was the Minnetonka Country Club--on the shore of Lake Minnetonka. We were our regular six; we wore blue blazers, white flannels, and white shoes. I can still remember Stan Kinyon, when we took an intermission out on the lake shore on a balmy summer evening, saying, with his typical irony, how good it was to "get out and rough it". I also enrolled in summer school at the university and took a course in Differential Equations. [That subject was not included in the required engineering curriculum at Minnesota in those days!]

In the summers, we professional musicians were also hired by "big" dance bands--15 to 20 instrumentalists plus the leader and a vocalist--that drove out from the Cities to big dance halls in Minnesota and Wisconsin. These were "one-night stands"; we were paid pretty well, and I found that I really enjoyed that kind of drumming: good and loud! The percussionist was a key player, and I loved it! I loved to play loud, and I was good at it! But our hourly rate of pay was not so great if you counted it from "portal to portal"; we often drove two or three hours each way, in big seven-passenger cars, one pulling a trailer carrying our instruments. On more than one such occasion I saw that the driver's eyelids were becoming awfully heavy, and I took over the wheel myself. Typically we got back to our homes about the time that rosy-fingered dawn arrived.

I think it was the summer of 1933 when one of these big bands was hired by a hotel in Detroit Lakes, Minnesota, to play in their big dance hall six nights per week, all summer, and asked me to be their drummer. I was greatly tempted, but the job was to begin before final examinations at the university. I had a good scholastic record and had no trouble getting excused from the last week of

classes, provided that I would come back to Minneapolis for the week of Final Exams. The band leader was not happy about this plan, but finally agreed to it.

The first flaw in the plan showed up when I took the exam in a course called "Machine Shop". It turned out that the teacher, faced with the necessity of writing exam questions for a shop course, lectured to the class, during the last week of the term, on a completely new topic, namely how you deduce, by studying the three-view drawing of a part to be machined from metal stock, how to establish a flat plane or other reference from which all dimensions and angles would be measured, in the required order, in the machining process. The first step of the process would then be to produce that reference surface. I, of course, had never heard of this technique, and it was what the final exam was all about! I fully expected to get my first "F". In fact it turned out to be a "C", and it was the only one I got in the four years.

But the next blow was worse: I was packing up to return, by bus, to Detroit Lakes, when my fellow musicians appeared at our front door. The big dance hall was a big flop and the summer job was ended. What's more, they had brought my drums, tied on the running board of their car. (Bless their hearts! Some other instruments had been confiscated because the bill for our hotel rooms hadn't been paid.) My bass drum was badly damaged by gravel thrown up by the car's wheels en route. My mother helped me--she was adept with such projects--and we patched the bass drum with plastic wood and refinished it.

In those days the leader of a dance band, such as Ed Fleckenstein in the case of our campus combo, would purchase the musical scores of the popular pieces from a local dealer; they came with the individual parts for the several instruments, and sometimes we played them, with the customary "ad libbing" by the players, essentially as purchased. These were called "stock" arrangements. Sometimes, instead, he would purchase an arrangement of the piece from a local professional arranger. Usually these were much better than the stock arrangements. Our favorite arranger was James ("Red") McLeod, a young friend who was also a superb clarinetist in the University Bands. Every "name" band--i.e. the famous ones--has its own arrangers, and it is their skill and their originality that

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give such bands their distinctive styles of playing.

We especially wanted to play our own arrangements of the "old classics" of popular music--the perennial favorites like "Dinah", "Lazy River", "Star Dust", "Stormy Weather", and a dozen more. In comparison with McLeod's, for example, the stock arrangements of these were awfully uninteresting, prosaic,--"corny".

When a dance band plays from a stock arrangement, the drummer gets very little from what's written--usually just the general sequence of introduction, modulations, etc., and often not even that much; he is expected to "ad lib". But one's own arranger may really supply a drum part, since he is achieving a distinctive sound and style and the drummer is a key player.

We were often in need of a place to rehearse. Sometimes we rehearsed at my house, but even a six- or seven-piece group tends to overfill the "piano corner" of one's home, and also our house was considered pretty far away (38th Street) in those days. I had an idea to solve this little logistic problem: there was a student radio station with studios in the Electrical Engineering building on the campus. I knew the students who ran that station and had no trouble talking them into letting us rehearse there. It was a perfect solution, until we packed up our stuff and were ready to go home, at about nine or ten o'clock one evening. All the doors of the Electrical Engineering building were locked, and we couldn't open them, even from the inside. [I thought there were fire-department safety regulations about such things!]

We solved the problem rather easily by climbing out a window, handing our instruments, in their cases, out to one another, and drove off to our respective homes. Our house on 10th Avenue was dark and quiet; obviously Mother had retired, and I proceeded to do the same.

But the doorbell rang--at that time of the night? I padded to the front door in my pajamas and bare feet, and was confronted by two uniformed policemen! Someone had seen suspicious characters stealing what must have been valuable electrical equipment by handing it out of the windows of the Electrical Engineering building and had gotten the license number of my old Pontiac as it carried the loot away. The cops--they were campus cops--had traced the Pontiac to its home very efficiently.

I invited them into the house, explained what had happened, and showed them my drums. Of course they had to write down quite a few names and facts, but finally departed, muttering that they would have to report the whole affair to the University authorities. The next morning I narrated the whole story to Arnie Sevareid as I drove him to the campus. With a true newsman's instincts, Arnie wrote it up for the *Minnesota Daily*, of which he was a leading figure, and I'm afraid he used his imagination in some details, to make it a cute human-interest story. I was a member (even President, I think) of the All-University Student Council at that time, so Arnie, in his story, had the campus cops threaten to report me to that body for my transgressions.

Later, below, I'll try to explain how I got mixed up in campus (undergraduate) politics and such a thing as the All-University Student Council. For now, I have more harrowing stories from the world of music, particularly the University Bands, to tell. When I look back on my four years at the University of Minnesota, I find, indeed, that my musical activities dominate my memories. Perhaps that only tells us that those activities were more interesting--more fun!--than my studies of aeronautical engineering. That is not surprising, I think, because my four-year undergraduate experience, like most, was one of preparation for studies of aeronautical engineering. My graduate-student years at Caltech, which I'll write about below, at length, were certainly interesting, and fun!

[This is actually a serious and perennial problem of engineering (and other) education--how to provide the necessary, strong, basic preparation and at the same time the desired motivation and dedication that are so important to the undergraduate.]

But now back to the world of music in the years 1930 to 1934.

The University of Minnesota Band was at once a musical organization, show business, and a kind of big club. In football season, of course, it was show business--we had to learn the marches and "fight songs", and we had to rehearse the extravaganzas that were presented between halves. Many cartoons have been drawn that lampoon the half-time shows, suggesting that, for the band members and their directors, at least, the football game itself is only a minor

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nuisance. The half-time extravaganza certainly does require an awful lot of time and energy.

In Minnesota it involves the additional hazard of very cold winter weather in October and November. Players of the brass instruments often had to use alcohol on valves and slides to prevent freezing; we never did learn to play snare drums very well while wearing gloves or mittens. Needless to say, the players often used alcohol internally as well. Our uniforms included warm, maroon mackinaws, but we didn't wear these in the half-time shows or when we marched dramatically into the stadium before the game started--comfort had to be sacrificed to style! The band's membership included some members, such as freshmen, who didn't march but were admitted to the game. They carried our mackinaws in while we marched. Perhaps unknown to them, they also carried our (illegal) booze in as well, in the pockets of the mackinaws.

Every year there was one trip, for the band, to another Big Ten university for an especially important football game. In 1932, I think, this was a trip to Ann Arbor for the Minnesota-Michigan contest. Such a trip, by Pullman cars, was a great event, inevitably accompanied by high jinks. In the middle of the night, enroute from Minneapolis to Ann Arbor, some of our band members decided that it would be hilarious to scramble the shoes of the bandsmen. They collected them from beneath the berths, carried them all to the men's lounge at the end of the car, and mixed them well. I really wasn't one of the perpetrators, but I was awake, watched the process (and hid my own shoes in my berth). I recall that the ring-leaders of the plot were, of course, extremely amused by their own joke, and were puzzled by the fact that the Pullman porter seemed only bored by it. One of the boys finally asked him if he didn't think this was a wonderful prank.

"Oh sure," he replied with a yawn. "But last week I had the Notre Dame football team in my car, and they also collected all the shoes in the middle of the night. And they threw 'em off the train."

An Aeronautical Engineering student, therefore a friend of Owen Cunningham and me, named Nienaber, was a junior officer, a pilot, in the Naval Reserve and was so enterprising as to arrange a training flight from Minneapolis to Detroit that coincided exactly with the Minnesota-Michigan game. His well arranged scheme worked out

just perfectly until he experienced some sort of airplane problem upon landing in Michigan. He was badly in need of transportation back to Minneapolis on Saturday night. (It was never clear to me why the Naval Reserve didn't provide it. I suspect that he had landed--perhaps at Ann Arbor--without proper authorization.)

At any rate, he needed transportation. Owen and I outfitted him in some of the essential parts of a band uniform, including the maroon mackinaw, and Nienaber rode back to Minneapolis in one of the band's sleeping-cars.

I had one dreadful experience as a member of the university's Concert Band. I was its tympanist, but when we played "The March of the Boyards", by Halvorsen, I moved from my tympani to a snare drum to play the four-measure solo that occurs (twice). My practice was to extend the snare drum's stand to stand-up height, so that I could move over gracefully from one instrument to the other. (I should explain, with due modesty, that the Director, Prof. Gerald Prescott, thought I played the solo better than my fellow drummers.)

On a certain occasion the Concert Band was scheduled to record a number of pieces, including the "Boyards". We moved to the rehearsal hall of the Minneapolis Symphony Orchestra to make the recordings. There I found the tympani (three) of my old teacher, Mr. Faetkenheuer, already set up; they would have to be moved to make room for mine. But I was (surely) one of his favorite students (remember the cigars!) and (surely) he would approve of my simply using his drums; it would save the heavy moving, and, moreover, using his three drums instead of our two would be a real pleasure. One of the pieces to be recorded required a tympano in F, for which his smallest drum was just perfect; it would sound better than ours would when tuned to the same pitch.

So I simply appropriated Mr. Faetkenheuer's tympani and proceeded to tune them as required for our first piece. You may have noticed that tympanists are always checking their tuning, with soft little finger taps on the heads and with an ear down close to the head. That is what I was doing just as the red light went on and we were recording. Damn!--the small drum sounded a tiny bit sharp to me, so I pressed down the middle of its head--a maneuver that lowers the tone perceptibly. Well, I must have done it carelessly,

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with a knuckle against the drumhead or something, because the head split with a great POW!

The recording engineer gave a cut-off signal, the red light went off, and the band stopped playing. To everyone else in the big studio it was only a nuisance caused by a "damn drummer", but for me it was a catastrophe! We replaced the broken drum with our own, started again, and got the first piece recorded. The next number was the "Boyards"; the red light went on again. I was trembling. I reached down to make sure that the adjusting screws of my extended snare-drum stand were tight--I didn't want any more catastrophes that day--and I turned one of them the wrong way!! The snare-drum came crashing to the floor, and then, to carry the tragedy to the ultimate limits of horror, rolled across the platform, "BANGETY, BANGETY, BANGETY, BANGETY, BANG ..."

Fortunately for me, Gerald Prescott was a man of great tolerance and mercy. He communicated with Faetkenheuer and even managed somehow to assuage his rage. It seemed that the Symphony was just embarking upon a concert tour, their first stop being Chicago. The best--perhaps the only--place to find a replacement for the broken drumhead would be the nation's biggest drum factory, in Chicago. He authorized Faetkenheuer to purchase the finest head in the factory and to charge it to the Band. He neither kicked me out of the Band nor demoted me in the drum section.

One of the most popular dance bands playing for campus parties and similar events in the Twin Cities in those days was Art Goldberg's. Art himself was a most talented pianist and arranger, and I remember also the tenor-sax man, the bassist, and Jimmy Robb, the guitarist, as being stars. Sometime in the late winter or early spring of 1934 Art's drummer left the band for some reason, and I got his job. This was a great break for me, since Goldberg's group got more work than Fleck's and paid more. Also, it was a real treat to play with such superior musicians.

Actually, Goldberg's musical style was something special, obviously copied from the style of one of the nationally famous recording bands, Eddie Duchin's. It was a style easy for dedicated jazz musicians to sneer at--they called it "rickey-tickey" and names like that--it certainly wasn't anything like the progressive jazz of the day. But it was popular!--especially with the sorority

girls, I think--and it was much in demand. Art himself was such a talented musician that he could have achieved whatever sound and style he wanted from that little combo.

Of course, I was scheduled to graduate from the university in June of 1934, so I was aware that my enjoyment of the Goldberg band was destined to be short-lived, but I was surprised when Art himself announced that he was leaving Minneapolis early that summer, for a new career in Hollywood. Jimmy Robb took over the band, and I was busy and happy with that group until I, too, left for California in October.

Art was apparently a success in Hollywood's musical scene, as a composer and arranger. Professionally he was known as Arthur Morton. One of his big successes was the tune "Pennies From Heaven".

The University Bands constituted, to a modest extent, a social organization as well as a musical one. There was an annual "Band Formal", a ball that was distinguished by the fact that we wore our band uniforms and had a Grand March. I was president of the Bands one year, and led the Grand March--didn't have any particular girlfriend in those days, so a pretty young lady from one of the sororities was chosen by my campus-political friends to lead with me. Unfortunately she had a steady boyfriend who was quite upset by the idea and hung around, moping, when we entered and when we left the party. I'm afraid she and I weren't much interested in each other; apparently she attended as my date only because her sorority sisters told her to do so.

There was also an engineers' honor society called "Plumb Bob", one of whose traditional chores was to keep and guard a huge boulder--the Blarney Stone--that was a traditional feature of the university's annual Engineers' Day (March 17). On that day each senior student in engineering was allowed to kiss the Blarney Stone--I forget what magic powers this conferred upon him. Our patron saint was St. Patrick, who invented the worm drive--when he drove the snakes out of Ireland. (Sorry, that's certainly a terrible joke, but it really was the tradition, so I am not personally responsible for it!) Our arch-enemies, the students of architecture, endeavored, every year, to steal the Blarney Stone. During the twelve months between March 1933 and 1934 the Stone resided in the basement of our house on 10th Avenue South, and my mother was

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warned that the architects might try to steal it. I remember it as a very large boulder and can't imagine how we ever got the thing down our basement stairs and back up a year later!

I suppose it was a result of such extra-curricular activities as these that I was elected to the All-University Student Council as a representative of the students of engineering. I wasn't much interested in campus politics, which was dominated by fraternity and sorority members. Somehow I was chosen to be President of that august body--I think it was a kind of fluke--someone else resigned or was ineligible and I was the vice-president and succeeded him. I can't remember anything the Council accomplished except to vote to award themselves "keys" identifying themselves as Council members! [Remember "keys"? In those days men wore watch-chains! and liked to have gold "keys" hanging on them--as many as possible.]

During my senior year, 1933/34, I also had a part-time job as an undergraduate assistant to our colorful department chairman, Professor John D. Akerman. This entailed spending a good deal of time in his office, and afforded opportunities for much interesting conversation, both technical and non-technical.

He was a Latvian, a jovial, outgoing person with a broad Slavic accent and twinkling eyes. His students loved him and his wife, "Buzzy" (which he pronounced "Bahzzy"), and it was clear that the love was reciprocated. He had served in the Czar's air force in World War I, then came to the U.S. and the University of Michigan, where one of the earliest programs in aeronautical engineering was offered. There he was a student of Professor Pavlovski, another colorful Slav about whom there are many anecdotes. One of my jobs was to copy pages and pages of exam problems and their solutions, which "Pavvy" shared with "John D". Pavvy's originals had to be sent back to him; I discovered that he was a paper-saver and that nearly all these pages were written on the backs of dunning letters ("We must call to your attention, Professor, that this is our ---th notification that your payment is overdue.")

My favorite Pavlovski story comes to me from the distinguished aeronautical engineer and administrator, Willis Hawkins, who was one of Pavvy's students at the University of Michigan. Once, after a semester in which Willis achieved much scholastic success, he was dismayed to receive only a "C" in Pavvy's course; so he went to

Pavvy's office to complain. "You are Mr. Hawkins?" asked the professor. "Then of course you get only a C. You did only half the homework and even missed some of the quizzes!"

But Willis, peeking over the professor's shoulder, saw that the class list included also the name "H. Willis", who was credited with exactly the homework and the quizzes that "W. Hawkins" had missed. When he tried to point this out to Pavvy, the professor turned to him impatiently: "You told me you are Mr. Hawkins; now you say you are Mr. Willis! Who are you?"

I regret to report that much of the technical education we received from John D was not up to the standards of the University of Minnesota. When I went to Caltech for graduate studies, I was required to repeat several subjects that I had studied at Minnesota; I was shocked to find that John D's subjects, which were Airplane Design and Applied Aerodynamics, had not been up-to-date or soundly based. There were other areas where our education was much better; the area of structural design was one of these; I should not have been surprised by this, since Professor Howard Barlow was undoubtedly proficient in his field, and Professor Joe Wise's course in Indeterminate Structures was understood, by all of us, to be the tough course that "separated the men from the boys" in the Aero.E. program.

Nevertheless, John D's students learned a lot from him, sometimes outside the merely technical. I guess he suspected that I would become a college professor some day, because he told me how to dismiss a student: "When you kick student out, always remember: kick gently. Student you kick out always returns some day, either as rich potential donor to university or as monitor of sponsored research project!"

I was appalled, one day, when an inventor came to John D for advice about his invention, and when he was told that it wouldn't work--it violated the laws of physics--he angrily stalked out, slamming John's office door and offering no word of thanks. I said to my professor, "Well, that's certainly a rude guy!" "Oh yes," John replied, "but at least he didn't jump on my hat." It seems that that is exactly what another inventor had done just recently, when John had told him his invention wouldn't work.

So I thought I was prepared when, some years later at the Nor-

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throp company, Mr. Northrop asked me to talk to an inventor who had come to him. Sure enough, the invention was an impossibility: it fell into a very popular category of fallacies, namely schemes based upon (almost literally) lifting oneself by one's own bootstraps. (It is a very old category; I have seen very old prints showing balloons or ships propelled by cannons that they carry and which shoot cannon balls into nets (sails) that they also carry!)

I told the inventor his device would not work, and why, whereupon he claimed that he had built a model and that, indeed, it worked. I told him that he was either a very bad experimenter or a liar. (Not very kind, was I?) The result was that he departed, but wrote to Mr. Northrop, "The spectacle of your Doctor Sears, in times like these [war times], using his brains as brakeshoes upon the wheels of progress ...." Some of my colleagues at Northrop, especially the test pilots, were so impressed by the inventor's grisly metaphor that they wanted to give me the nickname "Brakeshoes", but happily it didn't "take". [Well, there were two exceptions: both Johnny Myers, our experimental-test pilot, and his friend "Jeeb" Hallaby, who later became President of Pan-American Airways and father-in-law of the King of Jordan, thought "Brakeshoes" was a wonderful name for me, and they still use it whenever we meet or talk--which is seldom.]

As I look back on my education at the University of Minnesota, I conclude that it was strong in the fundamental areas like mathematics and mechanics--the subjects that we call "the engineering sciences"--so that we acquired a good foundation to build our professional careers on, in spite of some shortcomings in the Aero Engineering program itself. There was a period when that program was one of the biggest in the country, and people say that the great Boeing company was "built by" Minnesota engineers.

Benedict Cohn and I discovered that there was an OX-5 engine in one of the lab buildings of the College of Engineering and got the idea of disassembling and reassembling it. John D agreed enthusiastically, so we took it completely apart, spread all its parts neatly all over a kind of balcony in the lab, and then put it back together again. We certainly learned a lot about engines, all of which was useful in our automobile- and airplane-based lives. [To be sure, I am telling this here mostly to impress my readers, who

will have heard of the OX-5, if at all, as a famous engine of the old, old days!]

There was a flying club at the University of Minnesota in those days, but, in spite of my deep devotion to airplanes and flying, I was too poor to join it--there was no way that I could afford such a luxury--it was in the same category as joining a fraternity.

Among my classmates who did join the flying club was Thurman Erickson, a graduate of South High School and a young man with obvious qualities of leadership. Poor Thurm was prone to airsickness. He went to the airport as often as he could find the time and practiced making pull-ups and push-overs, being convinced that he could cure himself of airsickness by doing so. Unfortunately all he accomplished was to make himself sick again and again; he had to abandon his personal flying career.

When I went to Pasadena to begin my graduate studies at Caltech, Thurm was there to meet me and to drive me to the Caltech campus on California Street. Unable to find a job in aeronautics, he was embarking on a career in the construction business, which meant learning it from the ground up--at first by totally uninteresting, back-breaking labor on some mountainside. Later he did get an engineering job with Pan American Airways. On December 7, 1941 he was top engineer of Pan Am's Pacific Division and was captured by the Japanese in the Philippines. He was, of course, a prisoner until the war ended.

When the Winter Quarter began at the University of Minnesota in January of 1934, we students found that we had a new Instructor, Al Reed, in the Department of Aeronautical Engineering. He had interrupted his graduate studies at Caltech to fill a crucial vacancy at Minnesota. He was an attractive, able, and friendly young man, who first impressed and amused us by running at high speed through blizzards and snow squalls on the campus, to meet his classes. A real Southern Californian, he didn't own or believe in overcoats.

Al was a good teacher and developed, during his brief stay in Minnesota, a good rapport with some of us senior students (and also, I remember, with an attractive young lady who was an Instructor in Women's Physical Education). It was he who put the idea of graduate study into the heads of some of us. He also told us about Caltech--the California Institute of Technology--and introduced us

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to the glamorous names of von Kármán, Clark Millikan, et al. Graduate study was something rather new in engineering in those days. Jobs were scarce, and it sometimes happened that a graduating senior, unable to find one, would stay on at his university for another year, take some more courses, carry out a modest research project, and acquire a master's degree. A Ph.D. in an engineering discipline was rather rare in America, except at a few institutions like Caltech.

At least three of us graduating seniors in Aero. E., applied to Caltech for admission to graduate studies and for financial support. All of us were admitted, but only I was offered financial support--an assistantship that would cover my tuition fees. Ben Cohn, whom I have mentioned before, received the same kind of support at New York University.

I had corresponded with the Lockheed company about a job there and had written again to them, withdrawing my application, when Caltech gave me the assistantship. Mr. Hall Hibbard, who was, I believe, Lockheed's Chief Engineer then, wrote me a very nice letter congratulating me on my Caltech appointment and saying that they would have a job for me when I finished my studies. [In later years I met and worked with Mr. Hibbard, but I am afraid I never remembered to tell him how happy that letter made me.]

In October I shipped all my books, my slide-rule, and my tennis racket to Al Reed, who had agreed to receive them and keep them for me--but no drums! Fortunately, an elderly aunt and uncle of the Solethers had visited them that summer and needed a driver for their return trip to Southern California; thus I obtained transportation and had the unforgettable experience of driving across the Middle West and the West. It was not an unpleasant journey, although it was different from the same journey today. There were no interstate highways, of course, but we made the whole trip on paved roads, at speeds 15 or 20 miles per hour less than what we would use today. Hotels, motels, and gas stations provided much more modest services than we now expect.

I left Minneapolis on a bus, to meet my traveling companions in Spirit Lake, Iowa. Surely it was the beginning of the great adventure of my life. As I bade farewell to my native town, home, neighborhood, friends, the musical scene, and my mother and sister, I had no idea what successes or failures, or what adventures lay

ahead. I am sure that Mother feared she was seeing me for the last time--California was awfully far away. She was 52, and my father had died a year before, leaving her relatively secure financially by virtue of his life-insurance program. Helen, after graduation from the University of Minnesota, had become a teacher in Chaska, a suburb of Minneapolis. My Caltech assistantship was not adequate to cover all my expenses as a graduate student, but Mother was able to cover the discrepancy. It was agreed that that was a loan.

Shortly before leaving I underwent some physical examinations, both medical and dental. These confirmed that I was in good health except for some horrendous dental caries--like so many college students I had neglected my teeth for about four years. I can't remember just why I left the examinations until the last minute, but I must have, since it was decided that I should not try to have the dental work done before leaving and, instead, look for a good dentist in Pasadena. This meant asking Mother to advance a few more hundreds of dollars.

It was only at this point, ready to depart from Minneapolis, that I learned (I know not how) that Professor Akerman had recommended me for graduate study at Caltech by comparing me--favorably!--with Albert Reed. Since Al was obviously one of Caltech's bright stars, I was mortified; I thought that John D had done me an incredible disservice; I could imagine all of Al's professors remembering the name Sears with greatest skepticism.

I went to Professor Akerman and complained. "OK," he replied, "but I got you the assistantship, didn't I?"

Which brings to mind another story about Professor Akerman's letters of recommendation, which, I learned later, sometimes bordered on the bizarre; but I shall leave that anecdote to a later chapter.

At the end of World War II, the Aeronautical Engineering Department of the University of Minnesota was given a sizeable building in nearby Rosemount, which had been used during the war as a factory for some kind of munitions. It was a complex well-suited for conversion into research facilities--high-speed wind tunnels and the like. Akerman, I am told, went to Washington and entered the offices of the N.A.C.A., a principal sponsor of aeronautical research, waving large photographs of the Rosemount buildings and

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asking, in a loud voice, "Here are pictures of my white elephant! Who will give me some peanuts to feed my white elephant?"

We, his former students, were embarrassed by such unprofessional behavior on his part. But he did obtain support for his Rosemount laboratory, and in a few years it was the source of much good research in high-speed aerodynamics and a generation of well-trained graduate students who carried out the research.

In 1934 the nation and the world were still in the depths of the Great Depression. Franklin Roosevelt was elected President in 1932. I had the remarkable experience, before leaving Minneapolis, of walking into a bar, not slinking into a speakeasy, and buying, legally, a cocktail. It was a whiskey sour, about which I had heard but never tasted until then. I remember distinctly thinking that such a drink must be dangerous, tasting so good and obviously carrying such a kick.

The Depression, you would think, must have been a dreadful experience. It surely was; it ended my father's career and ruined his health; almost all of my contemporaries tell the same kind of stories--but many much more tragic--about their families. The sum total of the frustration and worry, the thwarted educations and dashed hopes, and the resulting impaired health, must be beyond imagining. But my contemporaries and I had the advantage of youth; we knew that conditions were awful and that our parents were sick with worry, but as long as we had three meals a day, however modest, we thought we were invulnerable.

My father, with both of his kids in the university, had to take salary cuts at the Curtis company and had to preside over the dismemberment of its little sales department. He had been the sales manager; now all but one of his traveling salesmen were laid off and he himself had to return to traveling throughout Minnesota, Wisconsin, and the Dakotas, spending his nights in small-town hotels. It is not surprising that the hypertension that had plagued him all his life worsened. Finally he was bedridden, laid off by Curtis, and dependent upon his insurance program, which fortunately included disability provisions. He died in the summer of 1933, with Mother, Helen, and me at his side.

The Minneapolis newspapers, in reporting his death, referred to "a daughter Margaret in Pasadena, California". Helen and I thought

it was a weird error, but Mother corrected us: she told us about his undergraduate marriage in Urbana. She and our father had believed that all that could be understood and accepted better by Helen and me when we were mature, and they were certainly right. Margaret's mother had reasoned similarly, so Margaret, who had always wished for siblings, first learned about our existence at the same time. She wrote to us, starting a correspondence that continued until her death in the '80s.

It was, of course, pure coincidence that, a year later, I was on my way to Pasadena to begin graduate studies. There I met Margaret, her husband Roger Brigham, and her mother. I even played tennis once with Roger, but it was a rather painful experience because he and his tennis pals were exceptionally skilled players--we played at the La Pintoresca courts, where such tennis greats as Don Budge played regularly. Roger, I now realized, was a terrific doubles player and played with all these stars every Sunday; I was hopelessly out of their class!

After the deaths of Margaret and Roger I received a heart-warming letter from their daughter Caroline Brigham Walmsley, asking me to tell her about her grandfather--my father. This has led to further correspondence and a delightful friendship between Caroline (and her son Lee) and Mabel and me. Caroline is the same age as our son David, and Lee the same age as our grandson Colin.

## CHAPTER 3

## GRADUATE STUDY AT CALTECH

Having arrived in Pasadena, I met my classmate Thurm Erickson, who drove me to the Caltech campus on California Street; I located the Guggenheim Laboratory of Aeronautics, opened its big copper front door, climbed the narrow stair to the second floor, and presented myself to the young lady in the little office at the top of the stair as a new graduate student. She was Miss Mabel Jeanette Rhodes, the department's secretary. We looked into one another's eyes, we gasped, bells rang, and the Guggenheim building trembled!

Well, not exactly. That is what should have happened. Neither of us can remember what really happened, but we suppose that she welcomed the new boy, made a note of his name, and directed him to Professor Clark B. Millikan's office.

Mabel had come to Southern California from her native Montana in 1929, for her senior year of high school. Her brother Meredith--called "Muddy" by everyone, in a nice departure from the popular custom of calling anyone named Rhodes "Dusty"--had come to Pasadena a few years earlier, as had a number of aunts and cousins also from Montana. Muddy got a job at Caltech as an electrical

specialist in the Construction & Maintenance Department. When she finished that senior year, Mabel too was hired by the C & M Department as their secretary; their office was located in the Guggenheim Building, where, in spite of its relative paucity of office space, the newly founded Guggenheim Aeronautics Laboratory ("GALCIT") had such space to spare. Within the next couple of years that situation began to change: Caltech's new-building program was less active and GALCIT was expanding. It was agreed that Miss Rhodes was needed more by GALCIT than by C & M, and she made the switch; her bosses became Professors Clark B. Millikan, Arthur L. Klein, and the colorful Theodore von Kármán, who had been made GALCIT's first Director and was in the process of closing out his responsibilities in Germany (Aachen) and moving himself, his mother, and his sister to Pasadena.

Mabel remembers how the smart-aleck graduate students teased her when Dr. von Kármán's arrival was anticipated: they assured her that he was a typical, dictatorial, Teutonic Herr-Professor, whose English she would never understand and whose expectations she would surely find difficult and demanding.

What actually transpired, of course, was quite different. He was a great boss to work for. He called her "Ma Belle", was interested in her family and background, and was quite conscious of his bad pronunciation of English. He asked her to correct any words she heard that were not correct. It is a favorite joke of mine to accuse Mabel of teaching him to (mis)pronounce English; he spoke several languages, all of them with a strong Hungarian accent. I think the only thing that annoyed her were the cigar butts that he sometimes left in her little office; she would come to work in the morning and her nose would tell her immediately that her distinguished boss had been in there--she'd find the telltale cigar butt somewhere.

But let us return to the arrival of the new graduate student from the University of Minnesota in October 1934. Making my plans for study at Caltech, I hadn't had any good ideas about living arrangements for graduate students there, and I wanted very much to have something arranged to move into upon arrival. I learned that a few grad students were housed in the undergraduates' Student

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Houses, on the campus, and thought that might be a good solution for my first year. Indeed it was, though a little expensive. I lived in Blacker House during my first two quarters at Caltech and will write a little more about that below.

I settled quickly into the ways of GALCIT. It was a small department, about six years old then, and its Director was Professor von Kármán. I propose to devote a chapter of these memoirs to him; it will suffice at this point to say only that GALCIT was his creation and that his personality, his philosophies of engineering and education, and his intellectual and personal standards defined and dominated the institution.

The Guggenheim building was unique. It was built around the ten-foot wind tunnel: the return leg, with the drive motor and fan, was at ground level, the working section, 10 feet in diameter, was at about the level of the second floor, and the balance room was at third-floor level. (There was a sliding-pole--like the ones firemen are traditionally supposed to use in their fire stations--from the balance room down to the first floor. I guess it was intended for use in some kind of emergency, but as far as I know it was only used for fun; all of us liked to see how far we could free-fall before actually hugging the pole!)

Everything else--offices, laboratories, shop, library, and the single small classroom--was distributed around the wind tunnel, comprising, all together, a rectangular, box-like building of dimensions a bit larger than the tunnel itself. I have mentioned, above, the narrow stairway leading from the bare little "lobby" to the second floor; the stair from the second to the third floor was even narrower and more spartan. It was clear that the architect (if there was one) was given a wind tunnel and asked to design a building around it, as a sort of afterthought. I know that some visitors making their first visits to the world-famous laboratory, entered at the big copper front door but decided then that they must have come in through a back door, and retraced their steps, hoping to find the main entrance.

The ten-foot wind tunnel was a busy facility; we carried out tests of scale models of all the airplanes being designed by the aircraft companies of the West Coast and most of the companies of other parts of the U.S. It ran two shifts--about 80 hours per

week--in those days, and there were always a few GALCIT research projects scheduled as well. My assignment as Graduate Assistant was as a member of the computing staff that reduced the measured data to approved nondimensional form and plotted them in graphs. When the test schedule of one company was completed, its model was removed from the working section and the next scheduled program was begun. The plotted curves were delivered to Clark Millikan, who proceeded to write the report that constituted our formal transmission of data to the customer--the aircraft company.

Clark's knowledge of aerodynamics and familiarity with all the test data generated in the testing of all the various models that came through made it possible for him to provide qualitative explanations of the plotted data and to catch (most of) the errors that had sneaked into the process by reason of faulty computing, balance-room errors, or discrepancies in the record of what was actually mounted in the working section. I can remember sometimes running to his office with a revised graph--somebody in the computing room had caught an error!--and finding that he had already figured out a good explanation of why the curves looked as they did--an explanation that now had to be replaced because the corrected curves behaved in just the opposite way.

I think there was one well-known airplane, manufactured in rather large numbers, that had (and still has, for some are still flying) a somewhat unusual empennage (tail) configuration, perhaps as the result of a balance-room error that someone in our crew made and Clark did not catch. The manufacturer's aerodynamicists probably didn't understand how the difference between two model configurations could have produced such a nice improvement in longitudinal stability, but there it was in our graphs, and with Professor C. B. Millikan's approval! Personally, I think one of our guys forgot to record a balance weight that he added at a certain point in the test.

Actually, they gave the new boy from Minnesota a different assignment, when he first reported for duty, before putting him to work on the computing staff. Several of the "GALCIT's-own research projects" that I mentioned above had to do with an airplane model with an electric motor in its fuselage driving a propeller; the major objective of such research was to study and to quantify the

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effects of the propeller and its slipstream on the longitudinal stability and control of the airplane. Professors Millikan and Klein had decided to run it with the horizontal tail surfaces at various heights (vertical positions). Apparently GALCIT was short of graduate students who could carry out the detailed design of such a mechanical device; it occurred to the professors that the kid from Minnesota had been educated in the kind of engineering college where that kind of thing was still taught.

They were right. I was given a drafting table and the detailed drawings of the powered model with its nominal, one-position, tail assembly. Professor Klein was also well equipped with those essential ingredients of any mechanical design, the catalogs of vendors of nuts, bolts, machine screws, ball-bearings, and all the other small parts one needs. What made the job interesting was that, with the horizontal tail in each of four different vertical locations, the wind-tunnel tests were going to be made with the elevators both fixed and "free"; i.e., statically balanced but free to be deflected by the airstream, and in the elevator-fixed runs the hinge moments were to be measured. So it was a rather challenging design job--given the tight limitations on available space.

Mabel Rhodes was a key member of the ten-foot wind-tunnel staff; she was responsible for the scheduling of the tunnel's tests--in communication with the airplane manufacturer's personnel and Clark Millikan. It was also she who typed the reports from Clark's long-hand version and finally billed the company for GALCIT's services. (In the middle 1930's they were charged \$200 per occupancy hour in the tunnel.)

Mabel was, until about 1936, GALCIT's only secretary and took care of all the faculty's correspondence, typed up Clark Millikan's class notes (textbook) in "Ditto", and operated the Guggenheim Building's telephone switchboard, in addition to her wind-tunnel chores. Caltech's meteorology program, under the famous meteorologist, Professor Irving P. Krick, was also part of GALCIT, and Mabel also did their secretarial work. In about 1936 a second secretary was hired.

When Mabel resigned from her job in 1939, Professor von Kármán asked her, "Ma Belle, why will you leave us? Are you going to have a baby?" "Oh, I hope so, some day," she replied.

VON KÁRMÁN: Then why do you quit now? Do we not pay you enough?

MABEL: Oh, yes, I guess so. But it does occur to me, since you ask, that I have never really gotten back the salary cut I took in the Depression.

(Caltech had made across-the-board wage cuts at the depth of the Depression.)

A young lady named Georgianna was hired to succeed her; I believe Clark Millikan had met her socially. Mabel undertook to instruct her in the intricacies of the job, but was disconcerted to find that Georgianna was not listening. She lasted in the job for a month or two but had to be replaced--I will describe her firing later--the story relates some of Dr. von Kármán's wisdom. What the bosses of GALCIT then did was to hire two people to replace Mabel, one an attractive young man, William Zisch, who took over all of the secretarial and fiscal details of the ten-foot wind-tunnel operation. He did a fine job and went on to new responsibilities, as I shall recount below.

My fellow students, beginning graduate studies in GALCIT in October of 1934, included five boys graduating from Caltech and Frank Malina, who came from Texas A. & M. The Caltech fellows were a very cocky bunch, clearly convinced that Caltech was the greatest and most demanding college in the world and that they, its graduates, must be the smartest students. Among the five were the Clauser twins, Francis and Milton, whose vociferous, gesticulating, finger-pointing lectures to Frank and me were especially merciless.

[There is a nice old joke about twins, which I learned from Phillip Morrison and which certainly applies to the Clausers: "Which Clauser are you talking about, smart Clauser or dumb Clauser?" To which the reply is, "Oh, it surely must have been smart Clauser; he was smarter than any of the rest of us."

And the punch line: "Well, that could have been either one." I've heard it told about the Panofsky twins, the Slawsky twins, et al.]

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Of course both Frank and I were aware that Caltech graduates were indeed a pretty select category, but we were brave enough to stay around and hope for the best. It was certainly true that the intellectual expectations of our courses and teachers were higher than what we were used to; we found it challenging and we loved it. The teachers were great. In that first year I studied under Clark Millikan and, as Al Reed had predicted, found him inspiring. It was not a hardship but a joy to take his courses in applied aerodynamics, subjects I had passed with high grades as an undergrad at Minnesota. I also had a course in applied math for engineers from Professor Fred Lindvall of Caltech's Electrical Engineering Department; the material, the nature of the subject, and Lindvall's way of presenting it were something new and thrilling to me.

That course of Lindvall's was attended by graduate students of other disciplines as well as by us from GALCIT, and some of those guys were terrific. One was Louis A. Pipes, born in Mexico, an exceptionally strong tennis player, who went on to become a distinguished professor at Harvard, at Rice, and at U.C.L.A., writer of famous textbooks in mathematical applications--I will tell more about "Luis Alfredo" later. Another was Simon Ramo, who became famous in industry. And still another was John R. Pierce, whose relationship to me is most amusing: In Minneapolis, when he and I were only one year old (before my family moved to the 10th Avenue house), so my mother told me during my Caltech days, she and John's mother brought their babies to a little park where they became friends and the two one-year-olds presumably beat each other over the head or threw dirt at one another. John R. Pierce is one of America's greatest engineer-scientists. We never happened to become close friends in later years, and I never yet have found an opportunity to tell John about our acquaintance in babyhood.

Two of the important features of GALCIT were the weekly Research Conference and the weekly Seminar. They were sacrosanct and were held even when Dr. von Kármán or other faculty members were away from Pasadena. At the Research Conference everyone, regardless of rank, was expected to report on his research activities. Kármán was the chairman--or Clark Millikan when the Director was away. Mabel

was on hand with her shorthand notebook. You could get away with "Nothing to report today" for two or three successive meetings, but about then the chairman would pause and peer at you, and either Clark or your thesis advisor might suggest that you meet with him to talk things over.

The philosophy of von Kármán's Research Conference was, clearly, that all of us were engaged in original research in engineering and "the engineering sciences", and that everyone's research was important. Everyone was expected to contribute, to the extent of his knowledge or experience, to everyone's research. That this was so successful and that personal ambitions or jealousies were seemingly non-existent, was due to the personalities of the faculty and the distinguished visitors who participated. The meetings were lively and often punctuated by the chairman's famous sense of humor.

Professor Arthur L. Klein, who was called "Maj" [pronounced like the first syllable of "major"] by everyone,--it is said that the nickname originated somehow in the days when he was a Major in the ROTC, or something of the sort--also had a lively sense of humor. He liked to give pet names to research projects. For example, there was an experimental project on measuring shearing stresses in liquid flows by use of a strange optical property of certain oils: under shearing stress they become bi-refrangent and the shear can be measured with photo-elastic techniques. One of such oils is a popular salad dressing, so Maj named the project "the mayonnaise experiment".

There was another investigation, this one in the realm of aircraft structures, carried out by a pair of students, Roland J. ("Jim") White and Hans Martin Antz. Maj immediately decided that that one must be "the termite problem", despite some entomological inaccuracy and the necessity of mispronouncing the German name Antz a bit.

Maj divided his time between GALCIT and the Douglas Aircraft Company. I was told that he had a bad habit of marking up the drawings of the engineers at Douglas as he wandered through the design rooms looking for errors or poor scientific principles--which was what he was being paid to do. The engineers, seeing him coming their way, learned to cover their drawings quickly, so that Maj's big black pencil marks were made only on a transparent overlay.

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He religiously read the journals of his fields, including especially the *Review of Scientific Instruments*, and he had a photographic memory. He was a stickler for "good practice" in design. I remember an episode when one of my fellow graduate students, W. L. Howland, designed a rig to make certain structural experiments; it involved what was essentially a balance: there were two knife-edges, one where a known load was applied to a beam and the other where the beam applied the (proportional) load to the test article. "Good practice" in such a design requires that both knife-edges be integral with the beam, so that their distance apart is precisely known and fixed. But Howland had attached one of the knife-edges to the loading structure instead of the beam, so that its position was not precisely invariant. Maj saw the apparatus only after it had been built, and roared with rage. When it was pointed out to him that the "wandering" of that knife-edge couldn't possibly be large enough to affect Howland's results, he replied angrily, "But it's a well-known matter of good practice! People will come through this lab, they'll see this blunder, and they'll say 'I thought Maj Klein taught here. He ought to know better than this!'"

Arthur Raymond, the distinguished chief engineer at Douglas Aircraft, told me that Maj was easily worth his (considerable) weight in gold to them, and I can certainly believe that. His course in Airplane Design was a somewhat disorganized pot-pourri of materials, economics of air transport, air safety, and other matters. We students didn't take it very seriously--I think all of us got A's--but I discovered later in my career that it was one of the most valuable components of my graduate education.

I decided to attend the Research Conference regularly, although I wasn't starting my own thesis research during that first year of graduate study. I thought, quite correctly, that I might learn valuable things about GALCIT's research programs and its faculty. I was amazed when, having asked the new attendees their names, Professor von Kármán recognized mine! and remembered what I had stated to be my research interest (namely indeterminate structures) when I applied for admission to GALCIT.

I was also impressed by the cosmopolitan nature of the attendees. There were two Belgians, Pol Duwez and Pol Genacht, two Germans, Antz and Arthur Ippen, Charles Sadron from France, and a

Japanese, Shoichi Atsumi. GALCIT had become world-famous in the brief time since its beginning.

Atsumi was an attractive, convivial young man. Already in the '30s the relationship between the U.S.A. and Japan was strained, and Shoichi was, of course, ineligible for work on the 10-foot wind tunnel. But the Navy and the Army (Air Corps) had instituted the policy of sending selected junior officers to GALCIT for graduate studies, and some of them got the idea of employing him to teach them Japanese--I think what they paid him was rather little, but important to him. A number of my American fellow students joined the Japanese classes. It is my impression that after about a year of instruction they were able to carry on simple conversations--to ask the way to the railway station or a physician.

Atsumi did instruct us, his classmates, in the basic sciences of Japanese eating and drinking; I remember an introduction to sake that rather overwhelmed some of my classmates.

In GALCIT there was a scale model--1/8- or 1/10-scale, perhaps--of the 10-foot wind tunnel and its fan, which had been used in the days of that tunnel's design. In the '30s it was available, as a most useful source of calibrated airstream, for use in research. Now, the laws of dimensional similitude require that the fan r.p.m. increase inversely as the scale decreases. So the fan r.p.m. of this little tunnel was pretty high--some tens of thousands--and the Millikan/Klein team decreed that the fan of the model tunnel should be surrounded by armor plate, to protect the tunnel's operators in case the high-speed fan came apart.

The little tunnel was assigned to Atsumi for research on some phenomenon that I cannot claim to remember, and he designed and built the enclosure of armor plate around the fan. He carried out his research successfully.

It was only much later, during Dr. Liepmann's tenure as Director of GALCIT, long after Atsumi had returned to Japan, that it was discovered that the armor plate, although it was neatly bolted in a position to enclose part of the tunnel, did not enclose the fan!

I remember when a student, Jack Dilworth, a big, somewhat loud lad from Louisiana, reported in the Research Conference that he was ready to carry out some experiments in water (or in some two-phase flow or something) but needed some way to record the time in the

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moving pictures of the transient phenomena: He couldn't put a stopwatch into the liquid. Chuckling, Maj pointed out that the stopwatch needed only to be encased in an ordinary condom; it would stay dry and could be read through the rubber and photographed! We all giggled, and Dilworth's instrumentation problem was considered solved. But for several weeks thereafter Dilworth had "nothing to report", and was asked to see Professor Millikan after the conference.

So he did, and when Clark asked him what was holding up his research he blushed, squirmed, and replied, "Aw, Dr. Millikan, I can't go into a drug store and ask for those things!" Clark was surprised, but told him, "You can't? Well, I can! I'll get them for you." And he did. (There are expanded versions of the story, involving a delivery boy bringing "Doctor Millikan's rubbers" to GALCIT, but this is the way Clark told it to me.)

Clark Blanchard Millikan, a graduate of Yale, received the Caltech Ph.D. in Physics in 1928. He and Maj Klein became interested in aeronautics during their graduate years, when Caltech's activity in that field consisted mainly of Professor Bateman's research and teaching in aerodynamics and some wind-tunnel experimentation carried out by Albert A. Merrill, a pioneer in American aviation, who was a Research Assistant. Merrill had designed and built a small, open-return wind tunnel, which still existed in the late 1930s and which I used in teaching a wind-tunnel course. The team of Millikan, Klein, and Merrill even designed and built an airplane--the "Merrill plane"--which embodied some of Merrill's novel ideas.

About that time funds were available from the Guggenheim Fund for the Promotion of Aeronautics, and Caltech had an enthusiastic nucleus to pursue the subject. The upshot was that Clark's distinguished father, the Nobel Laureate, Robert Andrews Millikan, whom we will call "R.A.M.", Caltech's "Chairman", agreed to a proposal to the Guggenheim Fund and GALCIT was born. Undoubtedly R.A.M. was convinced of the potential of the young aircraft industry growing in Southern California and the importance it might represent for Caltech.

Clark Millikan, whom we will simply call "Clark", just out of graduate school, was clearly the spark-plug of the effort. He was a

most impressive, brilliant, articulate young man. He had tremendous energy: an athlete, aviator, music lover, and a popular figure in Pasadena society. He was deeply devoted to his new field, aeronautical engineering, and became internationally recognized in it. He was a marvelous teacher.

It was easy for us, his students, to see him as the "real leader" of GALCIT, since he had great administrative abilities and obviously carried a lot of the day-to-day administrative load. The truth was, of course, that he and Dr. von Kármán constituted an unbeatable team: mutually confident, respectful, and dependent upon one another. Von Kármán had international stature and years of experience; Clark was a fast learner with the advantages of youth and energy.

Clark had a brilliant, logical, mathematical mind. Probably his best published works have to do with the turbulent boundary layer, a perennial problem area for fluid-mechanicists. But I really believe that his most important work was what he taught us in our courses in airplane aerodynamics--a couple of generations of us GALCIT graduates understood the subtle functional relationships of airplane performance, stability, and control better than our predecessors.

Clark was, of course, in continual demand as a member of our country's advisory boards and committees. He was a founding member of the National Academy of Engineering. He was a member of the advisory committee of the Naval Ordnance Laboratory. At one meeting of that committee the laboratory's director asked his advisory committee to endorse a certain resolution; the discussion indicated that the committee's opinion was the opposite. "Well," the director said, "I would like you to go along with me, anyway, and sign this resolution" [or something like that].

According to my informant, who was present, Clark packed his papers into his briefcase and walked toward the door of the meeting room. The director said, "Doctor Millikan, you aren't leaving us, are you?" Whereupon Clark replied, "Yes, I am. I was invited here to be a member of an advisory committee. Now I see that we are only expected to rubber-stamp your opinions--with which the committee members seem not to agree. I have no interest in being a member of such a committee."

Unlike Maj, Clark was a tireless traveler. He never missed the

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national meetings of the Institute of Aeronautical Sciences (which he had also helped to found), though I noticed that he rarely made comments on the papers presented. He sat in the front row at every GALCIT seminar, if he was not away from Southern California. I was told that, about a year before I came to GALCIT, Caltech and M.I.T. arranged a one-year (or was it one-term?) exchange: Clark spent the period at M.I.T. and one of their professors took his place at GALCIT. I was told by my Caltech colleagues that at the end of the period M.I.T. wanted to make the exchange permanent, but that the suggestion was not agreeable to either Caltech or Clark. I can well imagine that the story is true.

The relationship between Clark and Mabel and me was a close one. She was his secretary--and the wind tunnel's--and I was, for a few years, a kind of right-hand man to him. I think both he and we felt that he was a sort of father-in-law to both of us. We shared his love of music, the beaches, airplanes, and (I, anyway) tennis. We also shared his high regard for Dr. von Kármán and for the constant joy, amazement--and sometimes amusement--that our great "Boss" afforded us. Clark was very much at ease with being von Kármán's second-in-command. The same is certainly true of his position in Caltech: being the big boss's son obviously had nothing to do with his professional career, nor did it cause him any problems in personal or professional relationships; it was something that was seldom, if ever, in the minds of any of us.

Mabel and I also had a very warm personal relationship to Dr. von Kármán, as will become clear as this account proceeds--actually it became a closer relationship in the years after I left Caltech. But even in the GALCIT days we occasionally found ourselves the confidants of both men and sometimes the recipients of rather petty comments of one about the other. Von Kármán thought Clark's love of Bach's music was silly: You can't appreciate Bach, he told us, unless you are religious--and Clark was not religious. Mabel and I found that opinion itself pretty silly!

We also discovered that Clark would have liked GALCIT to be divided into two independent parts, one more scientific and the other more "applied". He pointed out that GALCIT's famous counterpart in Göttingen was so divided, one part headed by Prandtl and the other by Albert Betz. I wonder if Clark ever made that proposal to his distinguished father. He must have made it to Dr. von

Kármán, because he (Kármán) once told me that he knew Clark wanted it. His comment was, "But I am not Prandtl." Surely history suggests that GALCIT's organization has been a pretty sound one.

Clark actually had a personal life full of grief, though most of his students and professional acquaintances did not know it and would not have believed it--he was so vigorous and up-beat. His wife Helen, née Staats, suffered throughout most of her life from mental illness; she was in and out of such institutions as the Menninger Sanitorium. She and Clark had three children, but one, a girl, died in infancy from a crib accident. (Clark and Helen were traveling in Europe at the time, and Dr. von Kármán and his sister crossed the continent to join them in their grief.) One of Clark's sons also died young, killed in a weird accident. Clark was the oldest of three brothers, all brilliant teacher/scientists; one of them, Glenn, who married the daughter of the famous mountaineer, Sir Edmund Hillary, was himself killed in a mountain-climbing accident.

Besides the Research Conference there was a weekly Seminar in GALCIT--the same sort of one-hour lecture of general interest, delivered by a visiting expert or by local talent, which is called a "colloquium" in some other universities. The first Seminar speaker of the Fall Quarter of 1934 was Professor Harry Bateman, the great, British-born applied-mathematician, who had been a member of the Caltech Faculty before GALCIT was founded. He spoke on "The Magnus Force". (That is a somewhat obsolete name for the cross-wind force that develops on a rotating circular cylinder in a fluid stream.) He started out by describing the phenomenon in very down-to-earth, descriptive terms, then, suddenly, was deep in an abstruse mathematical argument whose relationship to the matter was unclear to me.

On the next day as I walked over to Colorado Boulevard for some reason I found Professor Bateman walking in the same direction. I summoned my courage and approached him; "I am, after all, a fellow scholar in the Institute," I told myself, "and it should be acceptable behavior to introduce myself to one of the professors!" What transpired--and my readers who knew the painfully shy professor will have anticipated it--was that he was extremely flustered and

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embarrassed by the encounter. In later years, having been his student, I enjoyed a warm relationship with him; I used to send him copies of manuscripts that I was preparing for publication--at least those with serious mathematical content--and would receive from him his comments written in a tiny script on three-by-five cards. His comments often concerned nomenclature and, especially, references to mathematical literature. I learned that many of the names we all use for certain equations, integrals, etc. are not really historically correct. But his names and attributions sometimes seemed a bit obscure; sometimes the material we used could only be derived from his references by means of some non-trivial mathematical labor!

In the Spring Quarter of 1935 I had a vacant spot in my program of courses. Professor von Kármán taught a three-part course in Elasticity in those days; I had not taken the first two parts in the Fall and Winter Quarters but was told that the third part was wholly devoted to Vibrations & Flutter and could be taken independently. I decided to do that, especially because I thought my preparation in the structures area was pretty sound.

I took the course and enjoyed it a great deal. Von Kármán was a marvelous teacher; I was delighted by his lectures: they were incredibly well organized and clear, in spite of his beautiful Hungarian accent and occasional Germanic pronunciations. When he was stumped by a derivation that didn't come out right, he chewed on his handkerchief--then gently chided us, his students, for letting him skip a minus sign or a factor  $\pi$  somewhere. I took copious notes in pencil and copied them meticulously at home, in ink.

[I must explain what I mean by "occasional Germanic pronunciations"--he certainly did not pronounce words in a Germanic way, except for German words and names. What I am remembering here is his pronunciation, in the continental way, of the Greek letters that are so common in mathematical work. In the theory of structural columns, for example, the combination " $\pi^2 P/EI$ " is ubiquitous. In Kármán's lectures  $\pi$  could be either "pie" or "pee", P could be either "pee" or "pay", E could be either "ee" or "ay", and I could be either "eye" or "ee". Most of us found it delightful, but I'm afraid that some of our fellow students, especially the military

officers, found it simply baffling.]

I learned to imitate him, to the joy of my fellow students. But someone reported that to von Kármán himself, and at some party or other he confronted me: He demanded that I demonstrate my imitation to him. I couldn't possibly! I refused and he was annoyed with me; nevertheless, he and I became warmest friends in later years.

Back to the course in Vibrations & Flutter: At the end of the quarter it developed that the final examination in the course would be an oral one. It was scheduled for a certain time in the single, third-floor classroom, and it turned out that everybody in GALCIT proposed to attend, to watch the fun.

When my turn came, Dr. von Kármán asked me a nice, straightforward question: He drew on the blackboard a sketch of a cantilever beam with a large weight fastened to its free end and asked me to estimate the natural frequency in vibration. I knew the formula for deflection of a cantilever beam due to a concentrated unit force at its free end; using that as a "spring constant" and knowing the weight, I had a first approximation to the frequency. He was happy with that, but it became clear that that was only the set-up for the real problem: How would I improve that estimate to account for the mass of the beam itself? I answered that the weight would have to be increased by some fraction of the beam's weight. He said, "Yes, could you estimate that fraction by energy methods?" I replied that I could if I knew the formula for the elastic energy of a deflected beam, but unfortunately I didn't know it. I panicked; my tongue stuck to the roof of my mouth as I stood there in front of all my teachers and fellow students. Von Kármán tried to help me with some hints, but I was hopeless.

When the exam was over, I found myself walking down the narrow staircase beside him; he said, obviously to cheer me up, "I guess you are not familiar with our custom of oral examinations." But I was sure that my career at Caltech was a complete failure, and wondered whether I hadn't better start packing to return to Minneapolis. And, sure enough, the next morning Mabel Rhodes told me, as I came up the stairs, that Professor Millikan wanted to see me in his office. "Here's where I get booted out!" thought I.

Instead, Clark Millikan informed me that the chief of our wind-tunnel computing staff, Dr. Roscoe Mills, was leaving Caltech and that he wanted me to assume that position. (This even after he had

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witnessed my disgraceful performance the preceding day!) The sun was shining again in Pasadena! It even occurred to me that my Caltech bosses might be willing to excuse Professor Akerman for his reckless recommendation of me a year earlier.

For the next two years, approximately, I held that job and enjoyed it greatly. It required, incidentally, a lot of contact and cooperation with Miss Mabel Rhodes, which I didn't find difficult.

I have carried my Caltech story, at this point, to the end of my first academic year there. During the Fall and Winter Quarters I had found life in the Student Houses convenient and pleasant. I made some good friends, including John C. Lilly, who lived next door to me in Blacker House and who became a rather famous zoologist--a specialist in communicating with dolphins and toothed whales--, Bill McLean, a brilliant Physics student who went on to become a national leader, and Warren Woodrich, a charming and most able young man from Minneapolis who stayed at Caltech only that one year and whom I completely lost track of thereafter.

But at the end of the Winter Quarter I learned that another housing arrangement was available to me that would save me a considerable amount of money. Five GALCIT students had rented a house at 346 South Michigan, just north of the Caltech campus, where each of them paid only \$35 per month, including breakfast, dinner, housekeeping, and laundry services, all provided by a young lady who lived next door. I couldn't pass up such a wonderful opportunity! I went to the appropriate secretary in Throop Hall--the administration building--and announced to her that I would be moving out of Blacker House.

She informed me, coldly, that that was not permissible; I replied that I would move nevertheless--in fact, had already begun the move. She drew herself up to a stern and severe demeanor--she was a battle-axe--and informed me that I would "lose my entire breakage deposit." "Yes, I replied, "I've counted on that." (The breakage deposit amounted to ten dollars.)

So I moved to 346 S. Michigan, and I told all my friends in Blacker House to bring their broken chairs, desks, lamps, etc. to my room and to exchange them for my unbroken items; and so they did. My room in Blacker was clearly the room of a bull in a china shop; everything in it was broken.

And at the end of the Spring Quarter I received from the Institute the "remainder" of my breakage deposit: the full ten dollars!

One of my housemates on Michigan Avenue was Jim White, who was struggling with the "termite problem" mentioned above. Jim was trying to work out the theory--it had to do with the transfer of shear from panel to panel of a structural aluminum sheet with stiffeners. He wrote the differential equations but couldn't solve them, so he resorted to energy methods and they didn't seem to work either. I found that he was taking long walks at night, through Pasadena and San Marino, hands clasped behind his back, pondering about his problem and muttering to himself.

But people do not walk the sidewalks of San Marino at night (or in daytime either, I guess), and he was picked up by the police. Jim says he tried to explain to the police about the differential equations and the energy method, but they only looked at him with great suspicion. They told him that they had recently picked up another walker who was peeking into the windows of the San Marino mansions and whose explanation was that he was interested in interior decoration and was looking for ideas. I guess Jim ultimately seemed less dangerous than the interior decorator.

Another housemate was Paul H. Dane, a GALCIT student and reserve officer in the Air Force. His fiance, Jean, a statuesque blonde, was a violinist and played in the Pasadena Civic Symphony Orchestra, and she suggested that I apply for the position of tympanist. So I attended a rehearsal of that orchestra and was delighted to be appointed to the "chair". It was a semi-pro sort of orchestra: young musicians of the Los Angeles area competed for places in it, and if they won they received free lessons from distinguished teachers as payment. And then there were amateurs like me, several of us staff members of Caltech, who played in the orchestra only for the love of it. One was Dr. Pol Duwez, the brilliant Belgian scientist of GALCIT in the area of materials, who was a cellist (and who later became a distinguished Caltech professor). When a concert was scheduled a few professionals of the Los Angeles Symphony were hired to fill in where the semi-pros and amateurs didn't fill the bill. As I recall, they were paid for the concert and one rehearsal. Mr. Reginald Bland, the orchestra's director, made me very happy by telling me that he much preferred me, as tympanist,

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to my professional counterpart from the L. A. Symphony!

I shall postpone to a later point of this chapter the sad story of the end of my happy relationship with Mr. Bland and the Pasadena Civic Symphony--and the end, alas, of my career as a tympanist.

I did get the dental work done that I have mentioned above. I think it was Al Reed who recommended a debonair young dentist, Eugene H. Dyer, who divided his time between Pasadena and Hollywood. Gene was a bachelor, about my age, and was interested in airplanes among other things. We became good friends. At his suggestion we spent a week at Christmastime traveling (in his big convertible) and camping out on the desert around Palm Springs. That was all pretty new to me, and I learned to enjoy the desert. We even crossed the border, visited a Mexican bar, and bought drinks (probably just colored water) for two pretty dancers there. They were American girls and they explained that they were getting valuable experience for careers as dancers--especially because the hall they danced in was lined with mirrors. They could watch themselves perform, they said, and that was supposed to be very important training.

I think Gene Dyer was a first-class dentist; he acquired a fine reputation in later years and had, as patients, some well-known Hollywood actors and actresses. He did dental work for both Mabel and me that has been admired by all of our dentists in Ithaca and Tucson. He seems to have been one of the first to recognize the dental problems associated with the "TMJ", the temporal mandibular joint. As I understood him, this joint, the complicated hinge of one's jaw, gets loaded unnaturally, especially as the result of orthodontia, and causes painful symptoms. He lectured extensively on this, using elaborate models. The "TMJ" became his speciality.

But when the Second World War came along and Gene was called to military service, he was determined, he told me, not to serve as a dentist. He chose, instead, to serve as an airplane pilot. (I suppose that he was a volunteer and therefore could make this choice.) I believe he flew C-47s in Alaska and the Aleutians. When the war was over he returned to his dental career, and in that profession was of important help to me, years later; but I think I shall postpone that story to a later chapter.

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Except for the trip to the desert with Gene, I stayed pretty close to the campus and my studies. I was having a great time and wondered why I couldn't make a permanent career out of being a graduate student at Caltech. As the Spring Quarter drew to its end in 1935, the owners of 346 So. Michigan Ave. wanted their house back, and most of my fellow tenants, master's-degree candidates, were preparing to depart from Caltech. I had found a kindred soul, one William W. Jenney, a fellow GALTIT student; we decided to join forces in looking for living quarters.

Bill Jenney, scion of a well-to-do family in San Diego, was a bright student and a fellow member of the wind-tunnel computing staff. He was an irrepressible, fun-loving companion, and when it transpired that he and I both got good grades for the Spring Quarter--in spite of my deplorable exam in Vibrations & Flutter--we decided to celebrate together: to get us dates and go out for a big evening. I had had one or two dates with a blonde from Minneapolis, named Virginia, so invited her, but Bill had a fiancee in San Diego who was not available for the date; he said he guessed he would invite Mabel Rhodes, the secretary. He did and she accepted; she knew him at GALTIT, of course, and was as charmed as I was by his great personality.

We did splurge: we went to the Biltmore Bowl in Los Angeles and had a wonderful time. We tried unsuccessfully to charge our evening to Bill's father's account, ran out money, and had to get Mabel and Virginia to pay for a snack on the way back to Pasadena. (The snack consisted of "size"--remember what that was?)

I really had a wonderful evening, especially because Mabel was so much fun; she was a great dancer and, what's more, she and I seemed to dance together perfectly. I began to wonder why our good, platonic friendship hadn't blossomed into something less platonic. On the next working day, at her little office on the second floor, I suggested that she and I rent a couple of bicycles on the next weekend and ride up into the foothills. She not only agreed to that but said she would pack a lunch. The result was that we rode up into Sierra Madre, consumed the sandwiches under a tree in the sunshine, and talked and talked and talked. On the way home she suggested that we ride past the home of her brother, Muddy, and his

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family. That was also very pleasant. I came definitely to the conclusion that our friendship should no longer be platonic! Mabel, it seems, came to a similar conclusion; she invited me, Bill Jenney, and her favorite girl-friend, Frannie Gottlieb, to dinner at her apartment. She forgot to serve the salad, but she did sit on my lap, and I kissed her.

That was in July 1935, and we've been together ever since!

I wrote to my mother and told her we wanted to get married--she and Helen had met Mabel briefly when they visited Pasadena earlier that summer. She fully approved of the match, but thought I ought to finish my education before getting married. (I also owed her a few hundred dollars, including the costs of the dental work, but she didn't mention that.) I wrote a most persuasive (I hoped) letter to her, pointing out that working for a Ph.D. is quite unlike studying for a bachelor's degree--it can't really be scheduled, and can amount to a kind of career in itself. I assured her that being married to Mabel would be a major help in such a career.

Mother wrote back; she accepted my arguments and agreed to our hopes and plans; she would come to Pasadena for the wedding, which we scheduled for March 20, 1936, the thirtieth anniversary of her marriage to my father. We had a modest little wedding in Muddy's house, with only eleven special friends, including Mother, Muddy, his wife (Pat), Frannie, Frank Malina, and Bill Jenney. The wedding was performed by Professor Theo G. Soares of the Caltech faculty--the professor of ethics. We went off on a little honeymoon at a big hotel on the beach near Santa Barbara, leaving from the railway station in Glendale. Frannie drove Mother to the station, where we would bid her farewell, but Frannie got lost in traffic somewhere and arrived just after our train had pulled out.

My marriage inadvertently ended my career as tympanist in the Civic Orchestra, because Mr. Bland scheduled a concert for March 21, with important rehearsals in the preceding week. I told him I couldn't participate because I was getting married, and he obviously found this to be a trivial and ridiculous conflict; he saw no reason why I couldn't postpone the wedding and honeymoon. I, of course, found his attitude quite unreasonable, and our happy relationship came to an end. [From a distance of 56 years I can easily understand his position, but it seemed utterly unconscionable at the time!]

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Mabel continued in her job for another three years; I began my thesis research on unsteady-wing theory, under von Kármán's direction and was appointed assistant to him, relinquishing the wind-tunnel job. What I had told my mother about marriage to Mabel being a help to my Ph.D. career was certainly true. (For one thing, her salary was greater than mine.) But I do remember times when we came home to our apartment in La Casa Bonita on Sierra Bonita Avenue, after a long day at GALCIT, but had to go right out again to the market to do our grocery shopping. And we didn't own a car yet. Then Mabel had to prepare supper--we were tired and edgy, and sometimes snapped at each other, I am sure.

[Having written that, I am appalled to remember how many single parents there are these days who, after a full day's work, have to come home to grocery shopping, cooking, housekeeping, and parenting. It occurs to me how "tired and edgy" they must be!]

How did I happen to be doing research on the aerodynamics of wings in unsteady motion? It wasn't a completely new topic in 1936; the first studies of the subject had been made by students of the great German fluid-mechanicist, Ludwig Prandtl, in the 1920s. But in 1936 one of these, a younger Göttingen professor, Hans-Georg Küssner, had published, in *Luftfahrtforschung*, a study of the case of an airfoil entering a "sharp-edged gust"; that is, an up-gust in the atmosphere, idealized by imagining it to have uniform upward velocity and an abrupt, step-like edge. If one could calculate the loading on the airfoil as it entered such a unit-step gust, he could calculate the loading for any vertical gust, such as more realistic ones. So Küssner's results had considerable practical importance--and they were very surprising! He concluded that the entry of a rigid wing into a gust was not related to the case of a wing that flies in uniform air but deforms so as to have the same relative-wind components--the same time-varying boundary conditions--as any fluid-mechanicist would suppose.

In other words, Küssner seemed to have discovered a new law of fluid mechanics; namely, that there was no reciprocity between the two cases--moving fluid and moving body (involving the same boundary conditions)--contrary to what all fluid-mechanicists had always

believed.

Dr. von Kármán visited Göttingen in 1936, and Küssner proudly told him about his discovery. Kármán didn't believe him--couldn't see any error in the traditional argument that led to reciprocity--but was worried. He was Director of the Guggenheim Airship Laboratory in Akron, as well as Director of GALCIT, and that lab was about to start an important series of wind-tunnel tests for the Navy, which were based on the reciprocity: they were going to test curved (banana-shaped) airship models in uniform, steady flow to determine the loads on airships in turning flight. (Certain catastrophic accidents involving the Navy's big dirigibles were believed to have resulted from excessive airloads in turning flight.)

Unfortunately the state of the theory of bodies in unsteady motion was rather confused in those days: several investigators of the subject seemed to be using different formulas for the local unsteady pressures, which were, of course, basic to the calculation of forces and moments. Dr. von Kármán came home from Europe and called me to his office; "Are you still looking for a thesis topic, Mr. Sears?" I certainly was--a good thesis topic is a most rare and valuable property! He explained the problem and even offered me, on the back of an envelope, a new formula for the local unsteady force, which he thought might be useful.

I plunged into the research with greatest enthusiasm, and before long I had elegant new formulas of considerable generality. My professor was delighted; I seemed to have cleared up the confusion and obtained useful new results--but what about the controversial reciprocity? "Oh," I replied, "Küssner is all wrong about that; he was led to his startling new law of nature by a simple mistake in sign in one of his equations."

Dr. von Kármán couldn't believe me! "Now, Bill," he protested, "even in Nazi Germany they still referee manuscripts. I don't believe Luftfahrtforschung would let his paper get by with a simple error of sign!" But I was right.

It is difficult to see how Küssner's mistake got by the referees, but it is even harder to understand Küssner's behavior when he discovered his "new law of unsteady flows". His blunder was to believe his own result when it seemed to say that everyone else was wrong. This brings me to "Sears's First Law of Science":

Whatever you have just discovered is wrong!

Of course that seems, at first glance, to say that you can't ever discover anything that is right. That, of course, would be nonsense--I've overstated my "law" to startle you and attract your attention. The operative word in the "law" is "just". Here is an expanded version that explains what I really want to say:

When you find that you have a new result that contradicts what everyone else believes, you must, first of all, assume you've made a mistake. Check all your work; if you haven't found an error, assume again that you've made a mistake. Check your radical new result against all the simple or not-so-simple cases for which you know the answers. If you still haven't found your mistake, find exactly their mistake. Then, and only then, present your new result to your friends.

[The last three words remind me of "Sears's Second Law", which tells you what your friends will say when you tell them what you have discovered. There are only two real possibilities: (1) They will say that you are just wrong or (2) they will say "Yes, of course." Don't expect them to say something like "Oh, that's wonderful!", because they (almost) never do. No, the answer you must hope for is the first--that you are wrong--because (a) you might be wrong, and if you are you surely want to know it!-- and it's better to be told so by your friends than by anyone else. And, what's more, (b) if you're right, which we hope you are, you have learned that your new result is not trivial or obvious! The second possibility--"Yes, of course"--implies that it is trivial, obvious, or not new.]

We made some wonderful, long-lasting friendships in those GALCIT days. I have already mentioned Frank Malina; he received his Ph.D. in 1940 and stayed on at GALCIT even longer than I. Frank was a pioneer in rocket technology and was actually the founder of the Jet Propulsion Laboratory, which later became so famous and so important to the space program. He was also an amateur (and later a professional) artist. He was born in Brenham, Texas, of Czech-

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American parents who moved back to Czechoslovakia--so his boyhood was mostly spent there. He was a gentle, soft-spoken man, whose accent was pure Texan.

The GALCIT rocket research program was taken very seriously by Professors von Kármán and Millikan. One of its first practical products was the "jato" rocket where "jato" stands for jet-assisted takeoff. Frank and the two professors were so far-sighted as to form a small company to manufacture jatos (and, later, many other rockets); its name was "Aerojet", and in the beginning its personnel were essentially all from GALCIT. Its Office Manager was Bill Zisch, the young man who was hired at GALCIT to replace Mabel in 1939. Bill went on to become the Business Manager of Aerojet and, as the company grew in size and importance, its President.

During our years in Ithaca, Bill came to visit us. When only Mabel and I were in our living-room with him, he asked, "I would only say this to you two: Don't you think it's pretty good that I first came to Caltech as a secretary and have now been made a member of its Board of Trustees?" The answer was "Yes, Bill, and an honor well deserved!"

After World War II, during the era of space vehicles, Aerojet prospered; Frank Malina, Clark Millikan, and Dr. von Kármán, who had founded the company, became wealthy. Frank was able to move to Paris, grow a beard, and pursue his interest in avant-garde art. Among other forms he produced kinetic paintings; typically his consist of (i) a stationary ground-glass screen at the front, (ii) a "stator" on which is painted a design, and (iii) a "rotor", which rotates slowly and on which is painted a simple, periodic pattern. Behind the rotor are small light bulbs, so that the pattern seen on the ground-glass screen periodically changes. The result--the "painting" on the screen--changes gracefully in colors and pattern and is most pleasing. Frank also founded the art journal *Leonardo*, which is devoted to "art and science", and is now in its 25th year and is edited by his son, Roger, a space scientist who lives in San Francisco.

Among our close friends in those days were a delightful young couple, Andrew and Edith Fejer, who came to Pasadena from Czechoslovakia. Andrew, whom we always called "Andris" because he was a Hungarian and "Andris" is the Hungarian equivalent of "Andy", came to GALCIT as a graduate student; Edith was Czech. He worked under

my direction as a master's-degree candidate and went on, after my departure from Caltech in 1941, to the Ph.D. He became a very successful and well-recognized professor at the Illinois Institute of Technology, from which he has recently retired. The Fejers had a special love for dachshunds, and when they decided to acquire one they convinced Mabel and me that we ought to have one too. They knew where there was a family that bred dachshunds and we bought litter-brothers, "Peter" and "Braunie".

Two more GALCIT graduate students of that period who became buddies of ours were bachelors: Hsue-shen Tsien and W. Duncan Rannie. Tsien arrived in 1935; Duncan the next year.

Tsien (whose name is transliterated differently in present-day China), was a brilliant student. Early in his Caltech career he impressed his professors, including both Fritz Zwicky and Paul S. Epstein, both of the Physics Department, with his abilities. Epstein had read that a community of Chinese had been found in China who practiced Judaism and presumably were descendants of Jews who immigrated to China long ago. He told von Kármán that he was convinced Tsien must have come from that background, he was so smart!

Tsien became known throughout the world of aerodynamicists by being co-author of a famous paper by von Kármán and him that presented a remarkable approximate method to "correct" the aerodynamics of wings and bodies for effects of air compressibility in the transonic regime.

["Transonic" is the word used to denote the regime where the speed of flight is near the speed of sound. For low enough speeds the effects of compressibility can be ignored; for high enough speeds--the "supersonic" regime--these effects dominate and the idea of "correcting for" them must be discarded. Dr. von Kármán and his friend Dr. Hugh Dryden apparently coined the word "transonic". Dryden thought it should be "transsonic", which seems eminently correct to me, too, since it combines "trans" with "sonic". We don't say "trancontinental", "tranatlantic", or "tramit". "But," Kármán told me, "I thought we don't always have to be logical."]

I happened to be at his house when he and Tsien met to work over their manuscript wherein the to-be-famous Kármán-Tsien theory was first presented. There seemed to be some confusion between them about the mathematical details; they had each written it up and

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there was a difference, seemingly non-trivial, between their analyses. Naturally I pricked up my ears, wondering whether the professor or his brilliant student had erred. They went back to the beginning and compared line by line; they were both right! As can happen when an approximate theory is constructed, they had taken different paths at a certain point and had arrived at results that were equivalent within the scope of the approximation. At this late date I cannot remember whose formulas were chosen for the manuscript.

Tsien remained at Caltech after getting his Ph.D. in 1939. He was attracted by Frank Malina's rocket research, and I recall that they wrote papers together on rocket propulsion and space flight. Tsien's talents were much in demand, especially as the American armed services found themselves in the transonic flight regime and began to develop rocket-powered missiles and jet-propelled airplanes; he was asked for assistance in classified military projects that involved "clearance". He was, of course, a Chinese citizen, and China was an ally of the United States; moreover, Professor von Kármán vouched for his good character.

Thus Dr. Tsien served the United States as scientist, consultant, and teacher through World War II; at its end (1946) he was made a professor at the Massachusetts Institute of Technology, a position he filled with distinction until, in 1949, he was called back to Caltech as Professor and Director of the new Daniel and Florence Guggenheim Jet-Propulsion Center. From 1946 to 1949 he was a member of the U.S. Air Force's Scientific Advisory Board.

In the meantime, of course, the political situation in his country had changed--China was no longer America's ally but was seen as a potential enemy. The American military establishment, especially in the person of Navy Secretary Dan Kimball, canceled Professor Tsien's clearance for access to secret work. Thereupon, Tsien decided to return to his native land; he reserved space for himself and family on a transpacific steamship and actually shipped books and reports to China. Kimball and the Navy panicked; they intercepted his books (which they thought included secret data!) and incarcerated him in a kind of detention apartment in San Pedro. I think that must have been in 1950.

Tsien was furious. I asked him why he thought he needed access to classified material; he replied that he couldn't fulfill his

responsibilities as Guggenheim Professor at Caltech without such access. I think that was an exaggeration and that the clearance matter had injured his pride. After a week or two the government released him from the detention center but placed him under a "deportation order", which, in spite of its name, forbade him from leaving Los Angeles County! This he saw as another insult and he was determined to leave the United States. I saw him and his little family in Pasadena during the summer of 1955; he assured me that he had been meticulous in all his handling of classified information and was sure that he would ultimately be allowed to go to China. I told him that I surely hoped so, whereupon he smiled, thanked me, and said, "Of all my American friends you are the only one who has said that." All the others, it seems, had told him that he was making a terrible mistake--that he "didn't know what he was getting into".

As we bade him goodbye, leaving Pasadena to return to Ithaca, he predicted that we would meet again, somewhere, in twenty years--or perhaps he said twenty-five. And so we did: Mabel and I were in China briefly in 1979 and had several pleasant meetings with him, his wife, and their daughter.

Quite outside the field of aerodynamics, I must acknowledge how Tsien Hsue-shen made a wonderful, major contribution to my life! He came to me one day at Caltech--it must have been in 1938 or 1939--and announced that he had decided to learn to play the recorder--the predecessor of the modern flute--which was having a popular revival at that time.

The recorder--Blockflöte in German or flûte à bec in French--is a woodwind end-blown through a whistle-like mouthpiece. Its great virtues are that it is relatively easy for a beginner to learn to play, it has a very pleasant, dulcet tone, and there is an immense literature of music for it, coming from the Medieval, Renaissance, and Baroque periods (as well as a few notable modern compositions). Its shortcomings, on the other hand, are its restricted range--about two octaves--and that the player has limited control over precise pitch of its tones. Its dynamics are also limited; an attempt to play loudly usually results in a tone rising in pitch (sharp). The first of these faults has been compensated for, over the centuries, by the use of a family of recorders: bass, tenor,

alto, soprano, and sopranino, each of which has a range of a bit more than two octaves, overlapping. The limited dynamics means that the recorders are best suited to chamber music and are not often used in symphony orchestras or dance bands.

Tsien had bought an alto recorder and proposed that Mabel and I should buy a tenor and a soprano, so that we could play together. We agreed, bought recorders, and undertook to learn to play them. I can't remember how long that trio lasted, but when I left Caltech and we moved to Inglewood in 1941 I was the only one of the three who was still practicing. I decided to make the recorder my musical instrument; I acquired a full complement, bass, tenor, alto, soprano, and sopranino, which required me to learn both the C and F fingering and to read treble, alto, and bass clefs. At Cornell and here at the University of Arizona my recorder playing, both in informal quartets and in early-music ensembles has been a great joy. I have learned to love the old music--which has increased in popularity over the years, especially, I think, because there are so many small recording companies these days, making music of all the eras available to the music-loving public.

When we saw the Tsiens in China in 1979 I found that Hsue-shen [or, in deference to his age and exalted stature, perhaps I should call him "Lao-Tsien"] had long ago forgotten his brief love affair with the recorder. I myself became a member of the Collegium Musicum of the University of Arizona in 1973 and have been a member ever since.

This might be an appropriate place to tell a few more anecdotes about Tsien. One, in fact, has also to do with music. As my readers have probably guessed, Mabel's and my musical tastes are indeed catholic; we are lovers, to the point of rapture, of music from the Medieval to Bartók and Stravinsky, and including Dixieland and Modern Jazz. (Needless to say, we have our favorites and our un-favorites, in all the eras, more-or-less.) During our Caltech days we began to collect records, and some of these were boogie-woogie and the blues.

Since Tsien was an admirer of Western music, I thought I could teach him the virtues of these modern, Western musical genres. I put some wonderful examples on the turntable. He listened quietly to one or two sides, but when I proceeded to put on another record

he asked politely, "Don't you think I've had a long-enough lesson for today?" I'm afraid my program to educate Tsien to jazz music ended at that point.

There was an application from a Chinese student in Toronto who wanted to come to Caltech. Our faculty had his Chinese transcripts and letters of recommendations; they were superb! Dr. von Kármán called Tsien to his office and presented the whole folder to him. "What do you advise, Tsien?" (Tsien's name, like Tony Biot's typically Gallic name, was converted by von Kármán to Hungarian: viz. "TSEE-en" and "BEE-oh".) Tsien's conclusion, after studying the papers was, "Is a scholar. Nevertheless may be a good graduate student." Von Kármán was not sure what Tsien meant, and asked for clarification. Tsien repeated: "Is a scholar. Nevertheless may be a good risk." It became clear that he was referring to the frequent discrepancy between formal scholarship and the ability to make useful, original contributions to the sciences.

The applicant was Chia-chiao Lin; in his case Tsien was eminently correct; Lin came to Caltech, was a brilliant student and research worker, and went on to be a distinguished professor of applied mathematics at the Massachusetts Institute of Technology and a world-famous applied-mathematician. He has made important original discoveries in fluid mechanics, in the understanding of the dynamics of tornadoes, and in a host of other physical sciences.

Tsien did get his wish and was allowed to return to China shortly after our 1955 conversation. There he established, under the aegis of the Chinese Academy of Sciences, an Institute of Mechanics, in Beijing. Its mission was to carry out research in solid and fluid mechanics and to advise the Chinese government in related matters. He became an important advisor to that government in many areas of engineering and applied sciences; I am told that he was the highest ranking scientific/technical civilian in the government. As such, of course, he became a political personage, and his relationships to many old friends, both Chinese and American, were complicated. His distinguished former student (at Caltech), Dr. Joe Charyk, went to Beijing as head of an official American delegation on satellites and, when he was introduced to his former teacher, was told that "Professor Tsien [would have to] speak to him through

an interpreter".

It must have been the official nature of the occasion that led to this occurrence, because neither Professor Frank Marble nor I encountered anything similar when we met him in Beijing. (In a somewhat analogous exchange, Professor von Kármán wrote to Tsien, inviting him to a meeting, and said he looked forward to greeting Tsien's wife and meeting his children. When Tsien replied that the children didn't speak any English, Kármán wrote that Tsien "must teach them enough English so that I can tell them that I am their father's teacher.")

Tsien became so famous in China, and therefore in the pages of Chinese-language papers in this country, that he became the subject of several articles in popular, English-language, American newspapers and news-magazines as well. His name and picture appeared whenever there was a story about Chinese intercontinental rockets or nuclear weapons. Some of us wondered if our former colleague was really so universally involved or whether the papers reached for his file mainly because they had one; it made good copy, especially because he had been allowed to leave the U.S.--or "deported"--with American secrets! We didn't suppose that he really knew a lot about nuclear bombs.

But there was one M.I.T. graduate, a former student of Tsien's, who wrote to the *New York Times* to say that if Tsien was China's expert the West had nothing to worry about anyway. Tsien was a terrible teacher, he said, who came to class unprepared and whom nobody could understand, who gave a passing grade only to the best student in the class--a present faculty star at M.I.T. I got a call from another of Tsien's students, who was upset about the article; he told me that Tsien always came to class meticulously prepared and was a superb teacher. His exams were very tough--the sort where you're only expected to solve one of the four or five problems posed--or perhaps to make good starts on more than one--and where you'll get an A if you do that and do it well. Several of our best Caltech professors wrote exams of that kind, including Bateman, so it's not difficult to imagine where Tsien learned it. I agreed to call Professor Holt Ashley, another distinguished Tsien student, and to discuss whether we should prepare a rebuttal in the interests of truth and accuracy. Ashley was much amused by the story; he confirmed that Tsien was a great teacher; he believed that he

himself was, in fact, the "best student" (the one who got the passing grade), but it was an A and was certainly not the only passing grade. Nevertheless, we decided that the *Times* letter did not merit a rebuttal.

Another Tsien story: During the years (1946-1949) that Tsien was a professor at M.I.T. I had a Chinese colleague in my department at Cornell, namely Dr. Yung-huai Kuo. As a matter of fact Kuo, in 1946, had recently completed his Ph.D. at Caltech, was looking for a job, and decided to ride East with Tsien when the latter drove from Pasadena to Cambridge; they stopped overnight in Ithaca and Tsien suggested that I consider Kuo for a teaching position in my new department. We hired him as Assistant Professor; he was my colleague until he returned to China a decade later.

One day he came to my office with the good news that the Tsiens (married in 1947) had a son! Sure enough, within a day or so Mabel and I received an announcement of the happy event; the boy's name would be "Yukon". We were delighted by the news but puzzled by the name. I went to Kuo and asked him what kind of Chinese name that was--it seemed more Canadian to us!

Kuo's reaction to my question was equally puzzling: "Why? Whose name is that?" "That seems to be the Tsien baby's name," I replied. "Surely you must know that!"

"Aha!" he exclaimed. "That explains everything." I told him it didn't explain anything to me, and he proceeded to clear up the whole matter: "Yukon" is short for "Yung Kong", which must be the boy's full name. Now, since "Yung" was Kuo's given name, it was not polite for the boy to be given that name: it put him and his father's good friend Kuo in the same generation! (It had only happened because the baby was named by his grandfather in Shanghai, who didn't know that his son had a close friend with that name, way over in America.) Well, the Chinese may have such fine old customs, but they are also a very practical people. All that was necessary to resolve the dilemma was for the Tsiens to avoid using the boy's name in any of their communications with Kuo; he, in fact, had already noticed that the boy was nameless--"our son", "the baby", etc.--in such communications. Now it was clear why.

And while we are being amused by Chinese names and customs, here is another little East-meets-West anecdote: One of my fellow stu-

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dents in GALCIT was a Chinese fellow named Shih-I Pai; In the late 1940s he was a temporary colleague of mine at Cornell; much later he became a professor at the University of Maryland, had a distinguished career, wrote several fine textbooks in aerodynamics, and has now retired.

His surname, Pai, is a bad transliteration--as usual!--of a Chinese name that should be pronounced something like "buh" (or perhaps "boh" in his local dialect), but is, of course, rendered as "pie" by his Western friends. [I cannot resist telling you that "Lao Pai", which is my affectionate name for him ("old Pai") also means "old uncle" in vernacular Chinese.]

During his tenure at Maryland he was granted sabbatical leave, and his office was occupied by a visitor from the U.K. This courteous Scotsman, when he arrived in College Park, asked the departmental secretary what office he would occupy; she replied, with equal courtesy but not quite correct Chinese pronunciation, that it would be Professor Pai's ("pie's") office, Room 314.

The visitor thanked her but added that "surely you are pulling my leg! The office of Professor Pai ( $\pi$ ) isn't really 314!" Apparently the coincidence had never before been noticed by Lao Pai's colleagues.

Sometime in the academic year 1937/38 I took my final exams, which were oral, for the Ph.D. One was the comprehensive exam which was supposed to confirm the candidate's general knowledge in the field. What I remember particularly was Dr. von Kármán's question "Can you obtain the formula for induced drag of a lifting wing by means of a momentum balance?" The point was that we always use a momentum balance to get the formula for the wing's lift, but we always resort to an energy balance when we derive the induced drag, because it's easier. It's easier because the energy is second-order (small) like the drag, while the momentum and lift are first-order (much bigger). If you try to get the drag by momentum survey, I thought, you have to take into account all of the first-order effects, to get the second-order result right.

So, in the oral exam I started bravely to do that, and got bogged down. Kármán seemed disconcerted and showed us (the examiners and me) how he could do it easily--by a calculation that I

thought was a bit of a swindle (ein Schwindel was one of his favorite German expressions) since he neglected the (first-order) deflection of the wing's trailing-vortex wake and got the right result anyway. It wasn't until much later, at Cornell, that I figured out how to do it correctly. It turns out that when you account for this deflection you do indeed get some new second-order terms and everything is altered--for example, the static pressure in the wake is found to be above atmospheric pressure instead of below--but all this cancels out and, sure enough, you get the familiar result for the drag! Unfortunately the great Professor was no longer around; he would have enjoyed it.

After the exam and after my thesis was accepted, I was promoted to Instructor and given the responsibilities for two courses, Vector Analysis and Wind-Tunnel Methods. I, of course, was delighted; the GALCIT faculty numbered only five of us (plus Professor Bateman, who was really in Mathematics), and even I, definitely the most junior, felt that I had been awarded a Nobel Prize at least! My students included some who became GALCIT's most distinguished alumni: generals, admirals, professors, and captains of industry!

I shared the office just beyond Clark Millikan's; my office mates were, successively, Carl Gustav Rossby, the eminent meteorologic scientist, Lesley Howarth of Cambridge University, and Sydney Goldstein--these were all visitors to GALCIT--, Duncan Rannie, and Homer Joseph Stewart.

Those may have been the happiest days of my life. While I sat at my desk--as the senior (!) permanent tenant I had the desk next to the window!--Professor von Kármán shuffled in to ask, "Bill, what will you do this summer?"

I answered, pointing to the papers I was filling with right (or wrong) equations that had to do with unsteady-airfoil theory, "Keep on working on this, I hope." "OK," he replied, and he shuffled out. No proposal, no contract. I was hired for the summer.

Dr. von Kármán and I collaborated on a paper on airfoils in unsteady motion, presenting his novel formulation of the problem and my solutions and new results; it became a kind of classic in the published literature of the subject. Professor W. F. Durand, one of the grand old men of the profession, was editor-in-chief of a definitive series (six volumes) entitled *Aerodynamic Theory*,

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published under a grant from the Guggenheim Fund for the Promotion of Aeronautics. He and his committee decided that a seventh volume on unsteady-flow theory was needed, and chose Professor Küssner and me to be its joint editors. I was flattered and thrilled, and I assume that my bosses at Caltech were also pleased. Unfortunately the war clouds of the Second World War loomed in the late thirties and the seventh volume of "Durand" never materialized.

There was, nevertheless, a Fifth International Congress of Applied Mechanics in the Spring of 1938; it was held in Cambridge, Mass. (For some of my readers it may be well to explain that "applied mechanics" is a term used to define a broad scope of applied physics that concerns materials, structures, fluids and fluid-dynamics, combustion, and such things--in other words, the broad field of sciences that underlies mechanical engineering and its offshoots such as aeronautical and heat-power engineering and naval architecture.) The International Union of Theoretical and Applied Mechanics was founded in the early 1920's by practitioners of the field, including Dr. von Kármán, in countries that had fought against one another in the First World War.

My paper on unsteady airfoils, including both my thesis work and some extensions of it, was accepted for the 1938 Congress. Mabel and I drove our Plymouth to Cambridge; it was our first international scientific meeting and very thrilling to us. There we met applied-mechanicists from all over the globe, including Sydney and Rosa Goldstein, from Cambridge University, who became our dearest friends. They were on their way to Pasadena to spend the academic year '38/'39 at GALCIT. We advised them about driving across America; we found that they were already fascinated by the U.S.A. and the strange language and customs of its inhabitants: they could hardly believe that a sign reading "Flats Fixed" meant a tire-repair shop and not a real-estate office. But when we told them it also (in the case of a store in Cambridge, Mass., at least) advertised custom-made brassieres, they were just delighted!

Perhaps the most thrilling event of the Fifth International Congress, for me personally, was to be introduced by Dr. von Kármán to his great teacher, Professor Ludwig Prandtl of Göttingen as "Eines von Ihren Enkelkindern"--"one of your grandchildren". Prandtl greeted me with old-world courtesy but suddenly remembered that he had left his hat in the lecture hall where he (and I) had

just delivered our papers--exclaimed "Ach, mein Hut!" and started back into the hall to retrieve it.

At that point another man came up to Professor Prandtl to compliment him on having delivered his paper in English!--it was something new, for Prandtl had always lectured in German, in the past. The elderly professor acknowledged the compliment in a rather halting accent, "Ja, ja, I can shpeak the technical English, but I have great difficulty when I try to make intercourse."

[Well, dear reader, there's nothing wrong with that, except that there's a "Gresham's Law" of word-meanings. See the dictionary.]

## CHAPTER 4

## TEACHING AT CALTECH

Having been granted the Ph.D. in 1938, I was entrusted with teaching more challenging courses, including even the one devoted to theoretical aero- and hydrodynamics, which traditionally Clark Millikan had taught--one that I had enjoyed so much when I was one of his students. It was a rather mathematical course; it was our conviction at GALCIT that fluid dynamics is a discipline based on observation--not a mathematical discipline in which you can deduce profound results by purely mathematical argument--but that, nevertheless, it is one in which mathematical descriptions have such utility and power that they become essential tools for those who propose to work in the field. It was my responsibility to convince my students of this and to provide them with such tools. I was given some very good students to work with, like Homer J. Stewart (whom Mabel and I christened "Homerjoe", a name that he has carried through a long and most distinguished career) and Duncan Rannie. I learned to watch the subtle facial expressions of such auditors; if either of them--they were my students in different years--frowned as I lectured, it meant that I should go back over the point being made and make it clearer.

Our style of teaching at GALCIT was very informal--I think Clark

Millikan set the mode: neither necktie nor jacket--and I remember him, a six-footer, sitting, with both legs drawn up, on the little table that stood near the blackboard in the classroom, smoking his cigarette through a rather long holder, as a student presented his solution to an exercise. But I guess I must have been more of a martinet than he, for I actually kicked a student out of the classroom for misbehavior!

He was the unforgettable Jack Dilworth, whom I have mentioned earlier. He came late to my class--after I had begun my lecture--and had stopped enroute to pick up a Coke from the vending machine in the wind-tunnel balance room. So he came in late, with one arm laden with notebooks and the other carrying the Coke bottle. The chairs in the classroom were movable and the floor was bare concrete. Jack decided to move the chairs around with his feet: one chair to sit in and one to put his bottle on; I had no choice but to stop talking--the noise was too much!--so I stopped and glared. When he was finally seated and had opened his notebook I said something like "Can I continue this class now?" His response was, *sotto voce*, for his fellow students' ears, "Geez, he must have had a bad night last night." So I told him I would only continue the class after he left the room--"And don't come back!" The room was silent as he gathered his books and Coke and departed. Nothing like this had ever happened in GALCIT.

Of course I had to let him come back two days later--it was a course required for graduate students--but I required that he remain absolutely silent in my class: no comments, no questions.

My senior colleagues undoubtedly knew all about the incident, but never said a word about it to me. When I recounted it to my good friend Hans-Wolfgang Liepmann, who was then a newly-arrived post-doctorate fellow in GALCIT (and always a wonderful purveyor of academic yarns) he pointed out that the silent, outraged glare--a traditional professor's weapon against bothersome students--can sometimes misfire. He remembered a case in Zürich when a professor was lecturing to a big class in a lecture hall; the entrances to the hall were at the front. A very late-arriving student came in after the lecture was well started and tiptoed up the aisle to find a seat. The professor stopped talking and employed, as I did, the classic, silent, outraged, academic glare. The student located an empty seat in one of the upper rows, and painstakingly made his way

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to it. The professor continued to glare and was about ready to resume the lecture when, incredibly, the young man stood up, made his painful way back to the aisle, and tiptoed to the door.

The professor could hardly contain his rage. He remained silent (and glaring) until the student reached the door. Then, "May I ask what you think you are doing?" he shouted. "Oh, I'm sorry," replied the student politely, "I came here under the impression that there would be a lecture."

This may be a good place to introduce my readers to Hans-Wolfgang Liepmann, native of Berlin, son of a famous physician who was director of a medical institute there. Director Liepmann was deposed from his position and actually imprisoned by the Nazis sometime in the 1930s. When his family were allowed to visit him there, he told them to get out of Germany. As I understand the story, he was released soon and joined his family in Zürich; he then went to Turkey. Hans-Wolfgang and his wife, Katja, stayed in Zürich and there continued their interrupted studies--both were physics students. The elder Liepmann devoted the rest of his life to public health in Turkey, traveling through the country, setting up public-health facilities, treating the sick, and delivering babies.

His son, H-W, finished his doctorate at Zürich University and himself proceeded to Istanbul to join a brilliant group of applied mechanicists, German refugees, including such famous names as Richard von Mises and Willy Prager. It became clear that H-W, though he had remarkable talent in theoretical work, really wanted to do experiments. Prager and von Mises thought he ought to be with their old friend von Kármán in Pasadena and wrote to him to say so. The upshot was that H-W, and, later, Katja, made the long journey to Pasadena. I think I must have been about the first friend he made there, and the Liepmanns became important members of our little clique. We remember how they wrestled with the English language and some strange American customs.

Not long after his arrival in GALCIT, H-W was asked to present a talk about his studies in fluid mechanics. It went very well, although there were a couple of linguistic idiosyncrasies: several French words (such as carré for "squared") crept in, and he carefully Anglicized his pronunciation of all German names such as Blasius and Mises. He now claims that I said "Somebody will have to

teach this guy how to pronounce German."--and that that was only the beginning of years of abuse of him by me!

For example: I received a phone call one evening from Professor Paul S. Epstein, one of Caltech's eminent physicists. [This was a few years later, when both H-W and I were junior faculty members.] In his characteristic, strong German accent he invited me to join a small group who were going to meet a gentleman who could perform amazing numerical calculations in his head, and witness one of his demonstrations. I accepted the invitation, but before ending the call Epstein asked me for H-W's phone number.

I provided it, waited an appropriate length of time for Epstein to call Liepmann, then called him myself, imitated Epstein's voice and accent, and proceeded, with apologies, to cancel the invitation. H-W was completely fooled; he very politely assured "Epstein" that no apology was necessary--but then suddenly was silent. After a few moments he said, "Sears! Epstein always speaks German to me!" I had not thought of that, but my hoax worked anyway because H-W temporarily forgot it too.

The story of my great-grandfather, Karl Otto Ulrich Klaus Preen, which I have recounted in Chapter 1, was discovered by the Liepmanns in a packet of old letters inherited by my mother. None of my family or other friends could decipher the German script in which the letters were written. H-W was overjoyed to learn that I was descended from "a wastrel and drunkard". He also concluded that my inheritance from a Prussian autocrat explained my continual cruelty to him.

I believe that I was the first in GALCIT to recognize H-W's scientific talents--at least I would like to lay claim to that distinction. I clearly remember telling Clark Millikan that this new German boy (same age as I, minus one year) seemed to know a lot and to have a lot of ideas, and Clark was impressed. The German boy did indeed rise to worldwide distinction in fluid-mechanics research and teaching, and was successor to Clark (who succeeded von Kármán) as Director of GALCIT.

The marriage of Katja and H-W was a bit of a mystery to Mabel and me. Katja, a physics student, seemed to believe in all kinds of folklore and "old wives' tales"--she believed that sleeping in moonlight would drive one mad, etc.--while H-W was an epitome of

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hard modern science--a wonderful skeptical observer of the world and of his "scientific" contemporaries.

It was depressing, but not surprising, therefore, when we learned that they had separated. By that time we were in Ithaca. Katja went back to Zürich, to the Jung Institute, where she studied psychology and became a professional practitioner in that field. H-W married Dietlind, the ex-wife of another fluid-mechanicist, Peter Wegener. H-W became the step-father of two Wegener boys: one was Paul, named after his famous grandfather, who was Germany's greatest Shakespearian actor--it is said that Paul Wegener, the actor, was a continual thorn in the side of the Nazis: he was too famous and too beloved to be abused by them.

H-W loves to tell the story of his introducing Dietlind to Professor Theodore Theodorides. The courteous Theodore asked her, "And Madam, are you also a scientist?" "No," she replied, "I studied Byzantine art and architecture."

Theodore was delighted. "Really? Do you know that Mrs. Peter Wegener is also a student of Byzantine art and architecture? You must meet her!" Dietlind looked to H-W for help, and he came to her rescue: "Oh, Theodore, you didn't understand. This is the same lady."

But this only confused Theodore more. "But I thought you said she was Mrs. Liepmann. Now you tell me she is Mrs. Wegener!"

H-W studied under some famous physicists in Berlin and in Zürich and did not fail to come away with stories about them. I believe it was Walter Nernst, who propounded a theorem so important that it is called "the Third Law of Thermodynamics", who was one of H-W's professors. H-W needed to confer with him about something and ran up a flight of stairs after him. Catching him on a landing he said, politely, "Excuse me, Herr Professor ..." But that was not good enough for Nernst, who was the President of the *Physikalische Gesellschaft* that year, and who wanted therefore to be addressed as "Herr Präsident". He said, sarcastically, to his student, "Why don't you just call me 'Nernst'?"

Another story that has come back to me several times--occasions when it was pertinent to my own situation--concerned another great physicist, Wolfgang Pauli. He presided over a weekly seminar, where he sat in the front row and dominated. He introduced a speaker at one of these meetings; the speaker announced that he was honored to

be invited and would speak on the problem of [so-and-so] and would show how the theory of [such-and-such] applied to this problem.

Professor Pauli interrupted: "Do I understand that you will apply the [such-and-such] theory to the [so-and-so] problem?" The speaker replied, "Ja, ja, Herr Geheimrat, exactly!"

Whereupon Professor Pauli arose and said to the audience, "There will be no seminar today," and proceeded to walk up the aisle, departing. He was heard to mutter, "I will not have such nonsense presented in my seminar."

[I regret to say that I have been in almost the same situation with my seminars in Ithaca and Tucson, and I have remembered H-W's story. But it has never been possible to be quite so brutal as Wolfgang Pauli.]

Changing the subject only slightly: H-W and I attended a meeting on magneto- and plasma-dynamics at Wood's Hole, on Cape Cod, sometime in the '60s. One of the speakers was the astrophysicist Eugene Parker, of the University of Chicago. He presented a paper on turbulence in space. Of course there is practically nothing out there--almost no molecules--so the nature of the "turbulence" is moot, to say the least. Parker believed there was random propagation of electromagnetic waves, and bravely carried over to this case the known phenomena of turbulence in air and other gases on Earth! He ended his talk by comparing the predictions of this theory with measurements by James Tuck and others in the "Zero Energy Thermal Assembly", a nuclear reactor in Great Britain. The agreement was very good. But Dr. Tuck was in the audience. He rose to ask, "Are those numbers supposed to represent my experiment?"

"Yes," replied Parker. "Why, have I got something wrong?"

Tuck said Yes, that all the numbers were pretty far out, and proceeded to correct them, one by one; they were off by large amounts. Parker studied the new values, pondered, turned to the audience, and assured us that his theory would still agree well with the measurements!

Liepmann was convulsed by suppressed laughter and whispered to me a story about the famous British physicist Paul Dirac: He was asked to review a paper; he discovered that the author had put  $c$ , the speed of light, into the denominator of a fraction when it should have gone into the numerator. That would make his calcula-

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tions wrong by factors of about 10 to the 17th power. Still he got "good agreement" with experimental observations. Dirac's straight-faced comment was that this author had accomplished what scientists had sought for so long: he had produced "a theory that is invariant to error".

There are many stories about errors in science and scientific papers--I have discussed the subject in a preceding chapter. Mabel, in her careers as secretary to great scientists and as wife to a modest one, heard so many such tales that she came up with "Mabel's First Law", which says, simply,

"All research papers are wrong."

She explains that there are, nevertheless, two categories of papers: those whose mistakes have been found and those whose have not. I have remonstrated with her, claiming that the two categories are those whose mistakes are serious and those whose are unimportant. [Some people believe that the genius of great men like von Kármán is just that their papers always fall into the second category.]

There is, I know, a quotation from some famous scientist who made a disparaging remark about another's work. The person to whom he was talking asked, "Oh, is his work wrong?" The scientist replied, "Oh dear! It's much worse than that!"

The point he was making, of course, was that "trivial", "stupid", "not original", or "pedestrian" would be more serious than "wrong". In fact, a piece of work that is simply wrong can be great. Consider the Theory of Continuous Creation originated by Gold, Bondi, and Hoyle as a replacement for the theory of the "big bang"; the world of physicists has decided (proved?) that it is wrong, but it remains a most ingenious, original tour de force of human intelligence.

One of the cases that led Mabel to her First Law was a GALCIT Ph.D. dissertation, written by one of its smartest students. It involves the numerical calculation of a certain class of fluid-flow problems. The writer found a family of elementary solutions (singularities) with which he could satisfy the boundary conditions much better than with the usual sources and sinks. It was not until long after he had been awarded the doctorate and gone from GALCIT to a

distinguished career that somebody discovered that these singularities are not "harmonics"; that is, they don't satisfy the right differential equation. So they don't represent fluid flow at all!

An example of a dissertation that is "wrong" in quite a different sense is one where the authors carried out a beautiful mathematical study of an important, timely, aerodynamic-flow problem. But when they had finished it they failed to recognize the significance of one of their results: They rejected it because it had singularities in it, not seeing that these could be eliminated by interpreting the flow field and its boundaries correctly. That was very bad luck; another author in another institution made the same study at about the same time and made the right interpretation.

A lot of papers and dissertations based on experimental work are also "wrong" because experimental techniques used are found, too late, to be faulty.

Besides the theoretical fluid-mechanics course, I also taught the course in airfoil and wing theory. It was pretty difficult to constrain myself: every young faculty member has full respect for the great professors who taught him, and their fine courses, but is also convinced that his students should be taught not only all that great stuff but also everything else that he, himself, has learned since.

I decided that GALCIT needed a course in propellers and rotors, so I made such a proposal to von Kármán, Millikan, Klein, and Sechler. They agreed, but it was a Caltech rule that each proposal for a new course had to be presented to the assembled Institute Faculty for approval. So I prepared a syllabus for the propellers-and-rotors course and attended my first (and only) meeting of the Caltech Faculty; Dr. von Kármán accompanied me. My proposal was accepted without any difficulty. The only controversial matter before the Faculty seemed to be a proposal that all the chairs in all the classrooms of the Institute should be screwed to the floor and numbered, so that the teacher could "take attendance" by simply noting the numbers of empty chairs. The Faculty debated this proposition vigorously for nearly an hour.

As we left the meeting Dr. von Kármán asked me how I had enjoyed my first Faculty Meeting; I told him that I was dismayed to hear the famous Caltech Faculty arguing such a stupid question; I point-

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ed out that I had never yet attended any class at Caltech in which any teacher had "taken attendance"!

"Oh, Bill," he replied with a twinkle in his eyes, "you don't seem to understand the Caltech Faculty. It is composed of some scientists and some pedagogues. Today you saw and heard only the pedagogues."

I guess this is common to all universities. One of my friends from Stanford tells a better story. At one of the meetings of the Stanford Faculty a professor introduced a motion that would require every teacher to clean the blackboard at the end of each class, so that the next teacher wouldn't have to do it. The debate raged. Finally Professor Willy Flügge rose solemnly to say that he firmly opposed the motion "on the ground of high educational standards". It was not clear to his colleagues how educational standards would be jeopardized by the requirement to erase the blackboard, but Flügge explained: If the incoming teacher does not have the interlude of blackboard erasing during which to prepare his lecture, when, he asked, will he prepare it?

As a member of the faculty one also has the responsibility--and sometimes it is a pleasure--to take part in oral exams, including oral final exams for the Ph.D. There are many anecdotes about these exams, some true and some apocryphal.

My good friend Louis Pipes got his degree in Electrical Engineering but was really a talented applied-mathematician. Some of his professors in Electrical Engineering were unhappy with the amount of time and energy he put into his mathematical studies, and one of them--a distinguished senior member--apparently decided to be difficult with Louis in his final oral exam. When it was his turn to question the candidate, he asked him if he knew the resistance of #00 copper wire in ohms per foot. Louis answered that he knew just where to look it up if he needed it--which was a pretty brash answer for a final oral exam. Professor Eric T. Bell, the famous mathematician, was also a member of the committee; he turned to his colleague from Electrical Engineering and remarked, "I must say, Professor, that that's the stupidest question I've ever heard on one of these exams!"

Another candidate seemed to be weak in mathematics; he was asked an easy question, the answer to which was "the exponential func-

tion"--the function that has the property that it is always proportional to its derivative. The candidate couldn't come up with the answer. Finally the questioner, Professor Morgan Ward, asked, "Haven't you ever heard of the exponential function?" The candidate said that he had, but he didn't seem to relate it to the problem at hand. So Ward asked him to sketch the function:  $\exp(x)$  as a function of  $x$ . But the poor fellow had seen somewhere a graph of the exponential where the abscissa scale was folded:  $\exp(x)$  and  $\exp(-x)$  were both plotted against  $x$  to save space on the printed page. So that is what he sketched. The exam committee were nonplused; Ward said, "That seems to be a double-valued function--two values of the function for each value of  $x$ ." The candidate pondered, studying his sketch, and agreed that the exponential must indeed be double-valued. [My non-mathematical readers may not quite appreciate this story. Let me explain that not to know the exponential function, in any scientific or technical field, is like being a business man and not knowing about compound interest.]

For that and a lot more sins he was failed on his oral exam. Unfortunately he had a very bad idea of what to do then. He visited his committee's chairman and made a proposition: He had important contacts in a local industry. If Caltech would award him his doctorate he could acquire for Caltech a very nice contract for research. That was the end of his scholastic career at Caltech.

My good friend Professor Conrad Rawski, the musicologist, has a favorite oral-exam story: One of the members of an exam committee was an elderly emeritus professor; the chairman politely invited him, as senior member, to start the questioning of the candidate. The emeritus agreed, and asked a most ambitious, far-ranging question--something analogous, in our field, to "Would you explain to the committee the influence of Isaac Newton in Western science." As the candidate took a deep breath and began his response, the emeritus turned to Conrad, sitting next to him, and whispered, "Would you listen to this, please? I took a pill this morning and it's starting to work,"--and tiptoed out of the room.

The first exam that I attended as a committee member was for a candidate in another field of engineering--I guess Aeronautics must have been his Minor Subject. I thought he was very weak, but the committee passed him without argument. As we left the room and walked toward the Athenaeum for lunch, Dr. von Kármán asked me,

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"Well, Bill, how did you enjoy your first final oral exam from the other side of the table?" I answered that I was deeply disappointed that Caltech would pass such a weak candidate. Von Kármán stopped walking, faced me, and said "Oh Gott! was it so bad?"

"Well, he didn't answer your questions, nor mine, nor Morgan Ward's, ... and when it was over the only debate in the committee was whether or not he should get *cum laude*!"

"Oh my," says my boss, "I'm afraid I didn't listen. Why did you not protest?" "Because," says I, "as you have just said, it was my first final oral exam from the other side of the table."

I was working at my desk one day, with Homerjoe Stewart working at his desk, when the door opened and in walked Professors von Kármán, Clark Millikan, and Ernie Sechler, all obviously highly amused by something. They wanted, they said, to show us, Stewart and Sears, what our former professor, John D. Akerman, really thought of us. They had received from him a recommendation of one of his Minnesota students who had applied to GALCIT for graduate study. Here is what the recommendation said, in part:

This man is of the same high quality as Mr. Sears, whom I recommended to you in 1934, and Mr. Stewart, whom I recommended to you in 1936, but this man has high ideals and will not be content to remain a college professor.

Homerjoe and I tried to explain that John D's mastery of the English language was not quite perfect and that "ideals" probably meant "ambitions", and so forth, but our bosses were having a lot of fun at our expense and departed, chuckling and making wise remarks about whether or not GALCIT ought to admit the candidate. We did, and he turned out to be a good grad student. He took a job in industry, but has not yet become president of the company.

Clark remained behind when the others departed, and asked Homerjoe and me, "O.K., so he means 'ambitions' instead of 'ideals', but, still, why does he say 'will not be content to remain a college professor'? Isn't he a college professor?" I think the anecdote tells us as much about Clark Millikan as about John Akerman.

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One of the interesting friends that I made during the time that I ran the 10-foot wind-tunnel tests for the aircraft companies was Karl D. Wood, an aerodynamicist, erstwhile head of a program in aeronautical engineering at Cornell and, later, Purdue, who came to us as consultant to Consolidated Aircraft of San Diego. "K D", as he was known throughout the industry, was the author of important books on applied aerodynamics and airplane design. He had a fascinating hobby: binocular photography. He owned a camera with two lenses, which made two not-quite-identical pictures of whatever scene or object was photographed.

The intended use of this two-lens camera was to make photos that, when viewed through a binocular viewer, were seen in startling three dimensions. But K D had learned to view such photos in three dimensions without a viewer, and he taught me to do the same.

This is a subject that fascinates me, and I must try not to go into it here at such length as to bore my readers. It must suffice to state that the binocular "focussing" that we do, seemingly intuitively, when we look at something with our two eyes, and the true optical focussing of our eyes are not the same. We learn, from infancy, to use these two processes together. The only people who learn to separate the two processes seem to be those who, in their youths, had the "wandering eye" syndrome or crossed eyes, and were made to do exercises to cure such faults. This separation of lens-focussing and binocular-focussing is what K D taught me; you have to learn to look at the two pictures, one with each eye, and then to focus your eyes' lenses to make the pictures sharp without changing your binocular focussing. There are two ways to do this--one requires crossing your eyes so that your left eye looks at the right-hand photo and vice versa.

K D and I had great fun with the pictures his "cross-eyed camera" took, and we also drew diagrams and colored figures that became three-dimensional when we viewed them with our technique. [Incidentally, the colors you see when your two eyes look at different colors are sometimes wonderful: iridescent, changeable.] There is a mathematics book (*Ordinary Differential Equations* by Morse & Feshbach) in which all the diagrams of the iso-lines of complex functions are presented, each in two plots, so that they

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become three-dimensional when viewed this way. I am delighted by the authors' footnote referring to these figures: they explain that they have done this for the benefit of their readers who know the art of stereoscopic viewing, and that other readers, who are not so clever, simply have to accept that the authors have (it seems) put two pictures of each of the complex functions into the book.

The trouble with this hobby is that so few people know it. I went to an international meeting on fluid dynamics, somewhere in the NATO countries, and listened to a most interesting paper on cloud formations and cloud dynamics by a distinguished Brit by the name of Scorer (he says "Skaw-uh"). He showed, on the screen, beautiful pictures of cloud formations, and each seemed to be duplicated. He explained to his audience, "These are stereoscopic photos. I hope most of you will be able to view these, by the cross-eyed technique, in three dimensions."

I did just that, of course, and the cloud formations and their dynamical significance, were impressive. I went up to him after the lecture and said "Your stereo photos are terrific. Tell me, Dr. Scorer, do you find that your audiences know how to view these, cross-eyed?"

"Oh, heavens, no!" he replied, "I think you are the first person who has ever told me that he sees my photos in three dimensions!"

[But my vision in my right eye has degenerated, and I can no longer see my "K D Wood pictures" in three dimensions.]

One of the pleasures of graduate study in GALCIT was to be a fellow student with the young military officers who were assigned there by the Navy and the Army (including the Army Air Forces). The naval officers, in particular, were well prepared for our strenuous scholastic program; they had been posted to the Navy's Postgraduate School--which was in Annapolis in those days--for two years, before they came to Caltech. Several of the Army officers, including Col. Donald Putt, were also very capable graduate students, but it was interesting to us that they were products of civilian universities, rather than West Point.

One of our Army-officer students was Capt. Howard McCoy. I noticed that he carried with him a notebook in which he frequently made notations during our conversations. When I asked him about the notebook, he answered, "Well, Bill, I keep notes there on the inter-

esting subject of why a graduate of West Point, like me, isn't prepared to handle graduate study at an institution like this."

I met McCoy years later when he was a colonel and had responsibility for "technical intelligence" at Wright Field. I was a member of the Air Force Scientific Advisory Board at that time, and was asked to meet with Col. McCoy at his office to discuss that interesting subject. We lunched together and repaired to his office; but it turned out that he had locked his keys therein. I was a bit amused by the fact that the expert in technical intelligence had locked himself out of his office. "Mac" was a big, heavy fellow, and decided to get into his office by brute strength: He backed up a few feet and went at the door with his shoulder. Splinters flew and the door was on the floor. "Hell," said Mac, "these damn doors aren't very secure, are they?"

Later, he was made the leader of an Air Force effort to find the answers to the mystery of the "flying saucers".

There is another story about an Army officer whose scholastic career at GALCIT seems, to me, remarkably impressive. He was Captain Ted Bolen, assigned to Caltech to work for a master's degree in meteorology. Several young officers were sent to that program each year; they reported to Pasadena in the early summer and were put through a strenuous review of engineering fundamentals and mathematics before regular classes began in October. As the "refresher course" began, Ted Bolen realized that an unfortunate error had occurred: He was not a college graduate; all the stuff that was being "reviewed" was new to him!

He explained his difficult situation to his teachers, of course, and the necessary administrative steps were taken to extricate him. But Army administration is ponderous; the letters and forms--in eight or ten copies--would need time to produce a new assignment for Capt. Bolen. Meanwhile, he continued to attend the review classes. At the end of the summer he was still at GALCIT, and to everyone's amazement he had passed all the final exams of the refresher program.

So the requests for his reassignment were cancelled, and Ted went on to complete the meteorology program on schedule.

The clouds of war were gathering, as the cliché says. Clark

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Millikan was commissioned an officer of the U.S. Naval Reserve. The Navy had come to realize that, in case of war, people like him would be needed; they invented a category "AV(S)" that would take civilian specialists like him directly into their engineering laboratories, etc. Clark urged me to apply for the same kind of commission. I did so--I think it must have been in 1939.

As a result of my application I reported one day to the Naval Reserve building on Alameda Street in Los Angeles, for a physical examination. The building was teeming with naked young men being examined for enlistment in the Reserve, and I joined them. I recall sitting in the office of a Navy doctor who, among other things, shouted at me, "I'm supposed to test your hearing, but can't do it in this madhouse. Ever had any hearing problems?" I was tempted to shout back, "What? Can't hear you!", but resisted the temptation. I said I hadn't.

The next were eye tests; I could read the charts all right, and the doctor couldn't find his color-vision book. His young assistant, an ensign, remembered that he had the book in his car, down in the parking lot, and would fetch it. The doctor thought this was unnecessary: "You ever had any trouble with color vision?" he asked me. "No, nothing like that, sir," I fibbed. The ensign insisted that he would run down and get the book; the doctor insisted that it was unnecessary; the ensign ran down and got the book; and I could only read the first page. I had flunked the physical exam.

Our naval-officer students at GALCIT--we had four or five that year, all pilots, students of mine in a couple of courses--were disappointed by this outcome. They asked me to go with them to the Naval Reserve Air Base at Long Beach Airport, where they kept up their flying proficiencies one day every week. There they introduced me to the Medical Officer of the Squadron as "one of our teachers at Caltech". They told him the story of my physical exam at the Los Angeles headquarters; then, "Does Dr. Sears look colorblind to you, Captain?" The doctor decided that "He doesn't look a bit colorblind to me."

The result of this high-handed business was that I was commissioned Lieutenant, Junior Grade, AV(S), and proceeded to buy uniforms and to study Naval Regulations--to learn when to salute whom and what, whether to carry a suitcase (no), or an umbrella (no), when it is OK for an officer to enter "Enlisted Men's Coun-

try" (never), and such important things.

The consequences of all this were important, for me, as time went on. At first it was great fun. In the olive-drab naval-aviators' uniform I went to Long Beach with my naval-officer students and we went flying. They were so kind as to sign out for their weekly flying in N3Ns--that's a biplane primary-trainer--and to take me along and teach me to fly!

They taught me to take off and to land the N3N. I graduated to the (informal) rank of copilot of the SB2C-4 (if I remember its designation), a Curtiss biplane dive-bomber that retracted its wheels into the fuselage. My student (the pilot) would take off from LGB, put his seat into a comfortable position, and turn the airplane over to me. I can remember dive-bombing the nudist colony at Lake Elsinore--even diving so steeply that I got excruciating pains in my ears!

War seemed to be coming closer. The commanding officer of the Reserve Squadron at Long Beach Airport was another of our GALCIT students, an interesting fellow named C. Fink Fischer, who will come into these memoirs again, later. Son of an Admiral, Fink Fischer was a graduate of Annapolis who had resigned his regular-Navy status and worked for some years as an airline pilot before coming to GALCIT as a graduate student. He had retained his connection with the Navy as a Reserve officer, and when he came to Pasadena for graduate study he was affiliated with the Long Beach Naval Reserve Squadron. [Our regular-Navy students, mentioned above, were not affiliated with the Squadron except for being provided with airplanes to fly, once a week.]

The Squadron was actually having a difficult time just then. Nearly all of their personnel--pilots, gunners, aircraft-and-engine mechanics, etc.--were employees of the various West Coast aircraft factories. It seemed likely that the Squadron would be called to active duty almost any day, whereupon all those young fellows would be paid Navy wages that were much less than what they were earning in industry, and which their families needed. The Reserve Squadron had procedures for training courses and examinations that would raise these fellows' ratings and pay rates. Until now the men had been very lax about taking advantage of these procedures, but with call-ups imminent the Squadron's training and upgrading programs were suddenly overloaded. Fischer, who took his

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responsibilities and the welfare of his men seriously, asked me if I would agree to become the Squadron's Educational Officer and supervise the classwork and examinations of the men. I agreed to do so.

Actually I was somewhat dismayed by how little some of the enlisted personnel knew about their specialties--about the workings of aircraft carburetors, propellers, electrical and hydraulic systems, etc. I think that Fischer, his Chief Petty Officers, and I did a lot of good for a few months there--we were running a good-sized school--but the Squadron was called to active duty. Fischer and I were all that was left in Long Beach. When he finished his studies at GALCIT he too was called up and assigned to a job at the Naval base in Johnstown, Pa.

But the Naval Reserve and my AV(S) commission were to play another role in my career a year or two later, as I will recount.

Before we come to that story, however, I have more to say about how things were going at GALCIT as preparations for war cranked up.

Perhaps the Massachusetts Institute of Technology also felt the need to crank up for a possible war. At any rate, at about that time I received an invitation to join their faculty. Looking back fifty years I cannot now understand how I could be so blasé about an offer from such a distinguished institution; I guess I was just completely sold on Caltech, GALCIT, von Kármán, C.B.M., et al.

Anyway, having received the offer from M.I.T., I asked Dr. von Kármán about the prospects for a promotion at Caltech. This was in the academic year '38/'39, when I was sharing my office with Sydney Goldstein. Sydney and Rosa were dinner guests of Dr. Robert Millikan and Greta. Apparently their host asked Sydney, after dinner, whether he knew this young fellow Sears in GALCIT. He had on his desk a recommendation to promote that young fellow, but he, the Chairman, was worried about it. It seemed to him that Sears might be "one of those who wanted to sit on their hindside and write mathematical papers".

(Let's skip over the fact that this was a somewhat strange thing for the Chairman to say to Sydney, to whom there was surely nothing more virtuous than to sit on one's hindside and write mathematical papers.) Sydney, usually so discreet about such things, told all this to me. "All right," I told myself, "I'm going over to Robert Millikan and tell him what I think." I called the Chairman's secre-

tary to ask for an appointment; I was told that he would see me "right now". I went down the stairs to the big copper front door, on my way to the Chairman's office in Throop Hall.

But there, by remarkable chance, I met my great boss, Dr. von Kármán, just coming in. "Oh, Bill," he said. "Where are you going?" "To see Dr. Millikan."

"You will go to see Old Millikan? And what will you say?"

"I will say that I am told he thinks I want to write mathematical papers, and I want to know whether I'm going to do that at Caltech or at M.I.T."

He gently knocked some ashes off his cigar. "Ja, but I would not say in those words."

"Oh? What words would you use?"

"You tell Dr. Millikan that it is true that you are mostly interested in the theoretical aspects of aerodynamics, but always guided by and in contact with experimental results."

I balked. "I can't say that to Robert Millikan," I protested.

"Bill, you do as I say", said Dr. von Kármán.

So I proceeded to Throop Hall, where the Chairman was waiting for me. I had at most five minutes in which to decide whether I would follow Dr. von Kármán's advice.

Dr. Millikan leaned forward over his desk. "Tell me, Sears," he said, "What is it that you want to do?"

I took a deep breath and replied, "I am especially interested in theoretical aerodynamics, but of course always guided by experimental results and in contact with experiment."

He smiled, leaned back in his chair, and put his fingertips together. "I think we have a place for you, Sears." I was promoted to the exalted rank of Assistant Professor and my salary was increased to \$2500 for the academic year.

At the same dinner with the Goldsteins Dr. Robert Millikan told Sydney, "You mathematicians talk about your 'research', but there is really only one kind of research, and that is experimental research. Theoreticians only codify and generalize the results."

This sweeping generalization was quoted to Dr. Valentine Bargmann by his friend H-W Liepmann. Bargmann was a right-hand man to Professor Albert Einstein. He said that "the big shots" liked to make such silly statements; he could easily imagine Dr. Einstein

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saying exactly the opposite!

One of the measures that our government took in preparation for a possible war was the Civilian Pilot Training Program. This was a program to provide primary flight training to large numbers of young men (and, later, young women as well). Colleges and universities were asked to administer the plans and to teach the ground-school courses in basic flight mechanics. The actual flight training would be done by local flying-schools.

Caltech was one of the institutions asked, and I, as the junior member of the Aeronautics Faculty, was asked to be the Director. It was not an onerous assignment. Fink Fischer was available to teach the classes, and the fixed-base operator ("FBO") at Monrovia Airport, about ten miles east of Pasadena, was given the contract for flight training.

Both did their jobs very well. My job turned out to be principally to preside, as judge and jury, when we had to decide whether to dismiss students who couldn't pass the ground-school course or couldn't learn to fly, or both. At first the students were Caltech undergrads, and they had little trouble with the ground school, but as the program went on the admission requirements were relaxed and we began to get youngsters who were less well prepared: I remember Fischer trying to tell a young lady what a right angle is. He had her march out of the room into the hall and turn to the right. "There! You have made a ninety-degree turn!"

Those who couldn't learn to fly seemed to be those who failed to respond to the many kinesthetic cues their eyes and ears received.

The flight instructors thought it would be a good idea for me to go through the CPTP training course with the students, but I was ineligible according to the government's rules: I was too old! (I was twenty-six in 1939.) They offered to put me through the training for half their regular price, and I couldn't resist that!

The great thing about the CPTP program was that it consisted of a well-planned regimen of dual and solo flying. The student flew several times per week, starting with dual instruction, of course. He could solo after a few hours--my log book shows that I soloed after eight hours. But then he continued in a program of dual instruction and solo flight, including dual and solo cross-country flights, before taking the flight test for the Private license. I

went cross-country to Vail, Ontario, Pomona, and Fullerton, and took the test on 21 March 1941. At that point I had forty-four total hours, equally divided between dual and solo.

I am sure that I had no idea how important my private flying would be in our lives. I got a little flying during the years we lived in Inglewood and I worked for Northrop, but we worked very long hours during the war and there were lots of restrictions. It was after we moved to Ithaca, a town not very well served by public transportation, that we discovered the utility of personal flying, as I shall describe later.

Another big government-sponsored program came along to keep us busy at GALCIT. People responsible for military production became alarmed by a shortage of aeronautical engineers, and Lockheed Aircraft set up a program to convert a lot of engineers of other specialties to aeronautics. GALCIT was asked to undertake this. My assignment was to prepare a course on "The Airplane and Its Components"--it was the first of a series of courses these engineers had to take as they undertook to be "retreaded" as aeronauticals. Clark taught them "aerodynamics of the airplane", by which we meant mechanics of flight, and there were courses in airplane structures, etc.

They were large classes--several hundred students in each--given in lecture style in one of the big lecture halls. What I remember especially is that quite a few of the students were University of Minnesota graduates, and that a half dozen were old classmates of mine who had studied aeronautical engineering but couldn't find aeronautical jobs in 1934. Some of these fellows were hired by Northrop Aircraft when they finished the "retread" program and were there when I joined that company in 1941, and some of these were holding jobs of considerable responsibility. So I conclude that the program was a valuable one.

My fellow teachers and I wrote up the content of these courses and they were published by John Wiley & Sons as the first volumes of a "GALCIT Series", which later included material not involved in the "retread" program.

After Mabel resigned from her Caltech job in 1939 we moved out of the Casa Bonita apartment house on Sierra Bonita Avenue--we had

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taken over her two-room apartment when we married--and rented a bungalow on Fay Place in Pasadena. Just East of Fay Place there was a church, and shortly after we moved there we found that the church, just on the other side of our small backyard, was a center of anti-war propaganda. On several nights each week, anti-war films were shown; they were sound films, of course, and on the warm summer evenings we had our windows open and could not avoid hearing the shrill pacifistic message, the gunfire, the screams of wounded men and animals.

It was painful to Mabel and me. We were convinced of the fallacy of war as a way of solving anything, but were, at Caltech, personally in touch with refugees and their families, victims of Nazi terrorism. We had to believe that America had to join the Allies and fight this incredible, inhuman scourge.

Dr. von Kármán, a Hungarian who had made his whole career in Germany--Göttingen and Aachen--before coming to America, retained his position and responsibilities at Aachen for a few years. I understand that when the Nazis came to power Hermann Göring urged him to return to Aachen full-time, but that von Kármán's response was to sever those ties completely.

Stories were coming to us in America about mistreatment of Jews and other minorities in Germany--how they were mistreated, robbed of jobs and careers, and their property destroyed or confiscated, how those who wanted to emigrate were made to pay exorbitant fees for exit permits that usually failed to materialize. I had the distinct impression, working closely with Dr. von Kármán in those days, that he found all this almost incredible. He grasped every opportunity to question those successful refugees who came to Pasadena. And after hearing their stories he would say, "So it is all true, Bill! The stories are all the same! The Nazis really are devils!"

America was deeply divided. There was a strong anti-war movement. About this time Clark Millikan was invited by the German aeronautical society, the Lilienthal Gesellschaft, to visit Germany and deliver a paper about GALCIT's research. His friends and associates were divided about whether he should accept, but he did: He read his paper, which covered a wide range of GALCIT's theoretical and experimental work, in German. He was greeted personally by Göring. It is interesting to remember that Dr. von Kármán did not

presume to instruct Clark in reaching his decision, although he surely must have been consulted.

Of course the anti-war movement ended on December 7th of 1941, but some of us learned, much later, that it was no disgrace to have a history of anti-war sentiments in one's record. In fact, it turned out that there was such a category as "premature anti-Nazi" for people who correctly understood the situation before Pearl Harbor, and it could be a handicap to be in that category!

John K. Northrop, the well-known aeronautical engineer and airplane designer, had set up a new aircraft manufacturing company bearing his name in 1939. He had begun his career at Douglas Aircraft when that was a fledgling organization--he and Donald Douglas had carried out the design and stress-analysis of the biplanes with which the Army Air Corps made history by flying them around the world. Later he had joined the Loughhead brothers in their company, which bore their name, spelled more phonetically as "Lockheed". That company became famous for its sleek, handsome, plywood-skinned airplanes, including the "Vega" and "Orion". Those were products of Jack Northrop's design skills; he thought that an airplane, to be a good one, ought to be beautiful.

Still later Jack founded his own company, which was ultimately merged with Douglas Aircraft as its Northrop Division. When Northrop Aircraft was founded in 1939, the Northrop Division of Douglas became the El Segundo Division, named after its location in the city of El Segundo, California.

The new company received Army Air Corps support in the form of some modest production contracts, but Jack's ambitions were much greater: He was convinced that the future of large, long-range airplanes lay in the "flying-wing" concept--airplanes consisting only of monoplane wings, without tail surfaces or fuselages. And he was convinced that the power plant of the future was the gas turbine. He got tangible encouragement for the former from the Army Air Corps and for the latter from the Navy.

He had long been an admirer of Professor von Kármán and asked him to become a consultant, in both of these challenging technical areas, to the new company. It turned out that the professor found both subjects interesting--I think he was inclined to agree with Jack's convictions in both cases. At any rate the two men liked one

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another and a consulting agreement was drawn up; Dr. von Kármán asked that I be included, which suited me very well.

We both became involved with the flight tests of a small, two-engine, flying-wing airplane, which were carried out on a dry lake on the Mojave Desert, about which I shall write more later. We also became involved with the preparation of a proposal to the Air Corps for design of a large flying-wing bomber--which became the XB-35--and I accompanied Mr. Northrop on a trip to Wright Field and Washington when it was presented.

In the Spring of 1941 he made another proposal: He proposed that I join his company as Chief of Aerodynamics, Flight Test, and Wind Tunnel. At that time it seemed likely that the United States would soon be at war; it also seemed possible that we would be waging intercontinental war from bases in the Western Hemisphere; the development of long-range bombers appeared to be ever more vital. It seemed ever more doubtful that an assistant-professorship at Caltech would be considered essential to the Nation's survival.

But I had to point out to Mr. Northrop that I was a commissioned officer of the Naval Reserve and might be called to active duty any day. He broke into a big smile. "Suppose," he asked, "that the Navy Department would say that your most important assignment was to be Chief Aerodynamicist at Northrop. Then would you join us?" Of course I would. He talked to his friends in the Navy Department and, as he described it, they moved Lt. (j.g.) Sears's file down to a bottom drawer, where it would remain.

I went back to Dr. Robert Millikan's office in Throop Hall to resign from Caltech. He appeared to be a bit unhappy about this for a moment, but soon smiled and reminded me that "We [Caltech] pride ourselves on our close relationships to industry." Mabel and I began house-hunting in Inglewood and bought a brand-new one on the brow of the hill over Manchester Boulevard, from which we could see the Northrop plant about four miles away.

[We thought our new house was the height of luxury: It had two bedrooms and a study, "one and one-half baths", a two-car garage. But when we proudly showed it to Dr. von Kármán he was greatly unimpressed: he thought it was so small--that his dear Ma Belle should have a bigger one!]

Mabel and I moved into the new house in Inglewood when I fin-

ished my teaching for Caltech's Winter Quarter, April 1941. During the Spring Quarter I continued teaching--probably only one course--by commuting. It was about an hour's drive from our house on West 85th Street to Pasadena in those days before freeways. Dr. von Kármán held a seminar at his house on South Marengo one evening each week, in which I was a regular participant, and some of my Northrop colleagues and I occasionally went over to GALCIT for the Tuesday seminars.

I remember that the evening seminars at the von Kármán house were usually very inspiring and informative. Malina, Liepmann, Rannie, Stewart, Tsien, and Fejer were regular members, of course, and so were Maurice Antoine (Tony) Biot, Chia-chao Lin, and others whom I have forgotten. Tsien and Biot (who was von Kármán's co-author in the famous textbook *Mathematical Methods in Engineering*) didn't get along very well, and made a practice of sniping at one another during the seminars. [Example: In the discussion of some difficult or obscure point Biot says "Yes, I thought about that once, and wrote a short paper on it." Which elicits from Tsien, with deep sarcasm, "I'm sure if you ever thought about it you wrote a paper about it!"]

[Incidentally--about that famous book--Rannie, Stewart, and I were asked, by our beloved boss, to work all the problems that appeared at the ends of the chapters, before it went to press. They are mostly non-trivial problems. We found that Tony Biot, whose job it was to provide the answers, had a lot of them wrong. Also Dr. von Kármán was unhappy about the chapter on Operational Calculus, Chapter X, and I took on the task of rewriting it; I hope that those of you readers who have studied it are happy with it!]

I think Mabel and I were starting out for Pasadena shortly before noon on December 7, when we heard, on the car radio, the awful news of the Japanese attack on Pearl Harbor. It wasn't too long after that that gasoline rationing was introduced and our trips to Pasadena were curtailed.

## CHAPTER 5

## WAR YEARS AT NORTHROP

When I joined it, Northrop Aircraft Inc. was two years old and had just moved into a big, wooden, factory building (government property) in Hawthorne, a suburb of Los Angeles. The company's buildings extended from Prairie Avenue on the West to Crenshaw Boulevard on the East. On the North side these buildings faced a mile-long East-West runway that is now called the Hawthorne Municipal Airport. The big wooden building was at the East end and housed the Engineering Department, upstairs, and the facilities for construction of experimental airplanes, downstairs.

As Chief of Aerodynamics, etc., I was given an office on the South side of the building, with Mr. Northrop's office on one side of me and on the other side the office shared by Walter Cerny, Dick Black, and "Duke" Schwartz; Walt was Chief Design Engineer and the other two were generally responsible for structural design. It was my younger colleague and former Caltech student, John Wild, who first noticed that all three names meant "black"---in Czech (or any Slavic language), English, and German, respectively---and proposed that their office be called "The Black Hole of Calcutta" (which was the title of a movie of those days).

While we were being silly I noticed something else that was

special about their office: Just as those three guys were always ruining the airflow in various ducts of our airplanes by putting their ubiquitous structural members into them, so the architects of the big wooden factory building had put two enormous wooden beams across the windows of the "Black Hole", crossing in a big X and surely ruining its ventilating airflow--there was no air-conditioning in those days, certainly not in a wooden, wartime, government-issue factory building. I claimed that was simple justice.

When I moved into the Northrop Company I inherited a small, high-quality aerodynamics group. Besides John Wild I found several other former students, and managed to lure Irving Ashkenas away from North American Aviation, where he had done beautiful work in the aerodynamic design of the famous P-51 "Mustang". Irv was (and is) one of the great aerodynamicists and took the position of Aerodynamics Group Leader. He had been a student of mine at Caltech and had worked for me on the ten-foot wind-tunnel staff. He was recognized in GALCIT as an especially able student.

I inherited not only the nucleus of an outstanding aerodynamics group but also a ten-foot wind tunnel, similar (at least aerodynamically) to GALCIT's but with less power, and the responsibility for the company's experimental flight-test program. The major programs of the company were the P-61 "Black Widow" night fighter and the XB-35 "Flying Wing" bomber, about both of which I shall write in some detail below. Together these constituted a considerable workload for our group and we had to add to our personnel. It was not a deliberate policy, but we seemed naturally to acquire GALCIT's products. Few of our tasks were routine; we were continually facing new aerodynamic requirements and devices, pushing into higher realms of airspeed and Mach Number. The young men we hired out of GALCIT seemed better prepared for this kind of thing. The aero-thermodynamic design of propulsion systems, for example, had suddenly become a major responsibility of my aerodynamicists. Irv Ashkenas had been a key man (at North American) in the design of the P-51's beautiful engine-cooling system, which was an exemplary design.

It was unusual in the aircraft industry for a company to have one person, namely me, in charge of aerodynamics, wind tunnel, and experimental flight testing. (There was, of course, another department, not part of Engineering, responsible for the flight testing

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of production airplanes.) Many of my most interesting--and sometimes most difficult--problems arose in the area of flight testing, and certainly some of the most colorful characters I worked with at Northrop were the flight-test personnel whom I shall tell you about.

But first I should describe another, perhaps the most interesting of all, the President and Chief Engineer, John K. Northrop. "Jack" was certainly unique among the presidents and chief engineers of the major aircraft companies: a quiet, soft-voiced, somewhat shy, scholarly man. His morals and his ethical standards were impeccable. He was not a graduate engineer but one largely self-taught, whose innate engineering judgment was remarkable. He was one of the most original and imaginative aeronautical engineers in the history of that profession: He was the father of the stressed-skin type of structure that is almost universally used in modern airplanes, the only exceptions being the "monocoque" structures that he also originated. He made inventions, and held patents on them, in such fields as prosthetics (artificial limbs), light-weight anchors, and welding techniques, as well as aviation. Donald Douglas once said, "There is some of Jack Northrop in every modern airplane that flies."

Dr. von Kármán used to say that throughout his career he had met many engineers who told him, "I don't understand your higher mathematics, Professor. I myself am a practical engineer."--which, of course, was somewhat impolite to say to the famous engineer. Von Kármán told us, "You know what a 'practical engineer' is? It's one who perpetuates the mistakes of his predecessors." By that definition Jack Northrop surely didn't fall into that category: he was a born inventor. Faced with any engineering problem, his fertile mind seemed immediately to produce new and better solutions. Often they were also the right solutions; if we, his younger co-workers wanted to convince him that older, tried-and-true solutions were more practical--as they often were for a variety of reasons--we had to convince Jack. I think he understood all this: He knew that in those cases we could be depended upon to argue for the conservative and practical approach, and he saw it as his duty to point out more original possibilities.

Behind his back we told each other that if Jack had his way our

airplanes would all be aerodynamically novel configurations, powered by unconventional propulsion systems, and constructed of new and original materials in new and original structural elements. (Some of them almost fit this description.) Our customers, the military services, as my readers will know, tend to be conservative, so there were plenty of reins on Jack's imagination.

His relationship to me and to his other young assistants was one of respect and courtesy. He would sometimes come to my office and ask, "Bill, do you plan to be here tomorrow?"-- when "tomorrow" was a regular workday, there was a World War going on, and I wouldn't even have thought of being absent. He had great faith in us, his young assistants; it was clear to us that he was going to support us loyally in any of our professional statements or opinions.

I am reminded of one comical little episode: The company bought a series of full-page magazine advertisements, including one that pictured a page of my mathematical equations (which really were related to aerodynamics--they were not faked). I had some nice big definite integrals of trigonometric expressions, which were expected to make a big impression on the readers of several national magazines. The ad showed a photograph of a page of this stuff plus pencils, a slide-rule, and some boastful copy about the Northrop Company's scientific prowess. Well, after this ad appeared Jack received a letter from some young fellow who wrote, "I hope your airplanes are better than your mathematics."

Jack, unperturbed, brought the letter to me and asked me to drop the young man a line and straighten him out. To my consternation, I soon discovered that indeed the ad's math was nonsense! An exponent in the integrand had somehow crept up into the upper limit of the integral, so that the wrong function was being integrated over the wrong limits. The advertising agency had recopied my equations, of course, and introduced the error. When I reported that back to Jack he said, "Oh dear. Write the boy a nice letter. Tell him that both our math and our airplanes are more reliable than the agency's proof-reading. And ask him if he wants a job."

As I have said, Jack's moral standards were high. He did not enjoy off-color jokes. The aircraft-company presidents of the Los Angeles area--they were all engineers in those good-old days--met for lunch once a month, and apparently amused themselves on those occasions with dirty stories. Poor Jack came back to the plant

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after those lunches in a gloomy mood, shaking his head sadly over the bad taste of his contemporaries. Apparently, to make things worse, some of the stories were even directed to him, teasing him about his inventions.

I think all of the other aircraft-industry tycoons had shed the wives that they had married back in their youths, and had married younger women, sometimes their pretty secretaries. In this respect Jack was again a bit different. He and Inez Northrop did divorce in the late 1940s, and Jack did marry his secretary. But she was Mrs. Margaret Bateman, a lovable, motherly, gray-haired lady of the same age as he. All of us young fellows who knew Mrs. Bateman and worked with her loved and admired her, for her intelligence, efficiency, and patience. [Among ourselves we made rude jokes about how Jack hadn't quite understood the popular custom of ditching one's wife when one becomes a big shot, and marrying one's secretary.]

The truth is, apparently, that Jack and Inez had not been compatible for some years but had postponed their divorce until their children were grown. Jack and Margaret were marvelously happy together.

I must recount here an anecdote from a somewhat later era. Mabel and I were temporarily back in the L.A. area in a summer of the '50s, with our two children, David (born in 1942) and Susan (1945), and were invited by Jack and Margaret to have dinner with them in their home in Pacific Palisades, overlooking the blue Pacific. The Northrops were very proud of the beautiful view, toward the West, from their living room, but that night it was solidly fogged in. So Jack provided a color-slide photo and had us stand at the window with the slide viewer in hand, to enjoy the view!

After dinner Jack and Margaret drove us home to our motel. Jack drove, with Mabel and the kids in the back seats. On the way, we made a left turn at an intersection where there was a traffic light of the sort that only changes if a car runs over a bar in the pavement--then and only then it changes to permit traffic to cross or enter the main street.

Well, there was some poor fellow who wanted to cross the intersection but whose car had not run over the bar and who was therefore destined to sit there all night, waiting for the light to change. So good-hearted Jack, as he made his left turn, drove over

toward this fellow, rolled down his window, and said, "You have to drive over that bar!" The guy rolled down his window and shouted back, "Aw, f--- you!!!"

There was deep silence in the car, except, I regret to report, for the uncontrollable giggling of David and Susan (the brats!). Then Jack sighed deeply and asked, "What are people coming to?"

His political views were very conservative. He was dismayed to find that so many of his younger colleagues and protegés, including Irv Ashkenas, John Wild, Walt Cerny, and me--and even Dr. von Kármán himself!--were politically liberal and admired Franklin Roosevelt. He was sincerely interested in the welfare of his thousands of employees, but this was a very paternalistic interest; the idea that they might form a labor union was anathema to him.

In fact, I never did really understand Jack's political convictions. Once during the war, when I was conferring with him in his office and we were using the leather-covered sofa that he had there, I had to move several boxes of buckshot cartridges from the sofa.

[The story of what he and I were trying to do--namely weigh Jack's head!--which required moving the boxes of buckshot is amusing but will have to be postponed.]

I had heard of buckshot many times but had never seen any before that. I exclaimed, "What are you going to shoot with buckshot?" With a grim expression on his face he replied that the purpose of the ammunition was to defend his home and family!

I was baffled. I asked him if he meant to defend his family against Japanese soldiers. The grim look on his face became grimmer, and his answer was, "I don't know whether I shall have to shoot Japanese or Americans, but I propose to be ready."

When I recounted this to Mabel she recalled that Inez Northrop had apologized for not having invited us up to the Northrops' mountain home at Lake Arrowhead. She explained that their cottage up there wasn't really very comfortable, because they weren't connected to the public electric-power system. "Jack and I think that if there is a revolution in this country the power plants are the first thing they'll take over."

We are left with a puzzling impression of an exceptionally brilliant and successful American family, in the prime of their

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lives, haunted by fears of some ill-defined perils. Is that the fruit of our capitalistic system?

Before the war, during the days of the Northrop Division of the Douglas Company, that Division designed and built, for the Navy, a beautiful little single-seat fighter called the XFT. In the process of flight testing over the Pacific near Southern California the XFT disappeared! Military and civilian members of the "conspiracy theorists" category, surmised that our Japanese potential enemies had somehow captured the XFT!

As the war in the Pacific got going, Jack received a phone call from a high-ranking naval officer in San Pedro. The U.S. forces had succeeded in capturing a Japanese "Zero" fighter. The naval officer on the phone assured Jack that the Zero was, indeed, "a carbon copy of the XFT!", and insisted that Jack come down to San Pedro to see it.

We talked to Jack when he came back from San Pedro. "It's a single-engine, low-wing fighter with a radial, air-cooled engine. Otherwise it bears no resemblance whatever to the XFT," he told us. He was, of course, referring to the engineering details: structural design and materials, engine and airplane control systems, and all such things; apparently none of these reminded Jack of his XFT.

I also remember when Jack was invited to visit the Hughes plant while they were building the big seaplane, more lately called "the Spruce Goose". [This is the big, eight-engined, wooden flying-boat that has recently been a museum and/or a restaurant in the harbor at Long Beach. Apparently it made a first-and-only flight, to an altitude of a foot or so.]

Jack returned to our plant, much impressed with what he had seen. When he entered the big hangar where the boat was being built, he said, he could hear nothing but the chirping of birds. There was no sound of rivet guns! The big flying-boat was being assembled by dozens of "little old men" climbing up ladders to put into place small pieces of wood. There were three kinds of glue used to fasten all the pieces together, he was told, one for above the water line, another for below the water line, and the third to be used "at the water line".

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That the test pilots of the Northrop Company were a colorful fraternity actually became clear to me before I joined the company full-time. Dr. von Kármán and I, as consultants, were slightly involved with the flight testing of a small flying-wing airplane, the N1M, designed and constructed a couple of years earlier. It had two aircooled engines, submerged in a very thick wing; it was underpowered and plagued by problems with its engines--which was most unfortunate since the little airplane was intended to demonstrate stability and control characteristics of the tailless configuration, and the constant engine problems made this difficult.

Its flights were carried out on a dry lake on the Mojave desert at dawn; Mr. Northrop and I, and sometimes Dr. von Kármán, drove out to the test site with the test pilot, Moye Stephens, leaving Pasadena about one a.m. We went by way of Mint Canyon, stopping for breakfast at a roadside restaurant well before dawn.

Moye was a big, handsome, deep-voiced man, a rather famous pilot, actually, because it was he who flew author Richard Halliburton around the world. That achievement was well publicized by Halliburton's book *The Flying Carpet*. Moye was also an officer (Secretary) of the new Northrop Company. When Dr. von Kármán and I first visited the company it was housed in the former Hawthorne Hotel; von Kármán had heard that the hotel was previously a brothel, and asked Moye if that was true. "Well, yes," replied Moye pompously, "but it's been well fumigated."

There wasn't much to talk about at 2:30 or 3:00 a.m. while we ate breakfast out there in Mint Canyon. I had noticed that Moye invariably ordered cornflakes, and for lack of a better topic I said, "You're really a fan of cornflakes, aren't you?"

Moye put down his spoon. "No, dammit, Bill. What I really like is Rice Krispies. But dammit, Bill, a grown man can't go into a restaurant and order 'Rice Krispies'!"

When the first flight of the first Black Widow (XP-61) came up, it turned out that--well before my arrival as Chief of Flight Test etc.--the company had contracted with a test pilot, Vance Breese, to make the first flight. He was a test pilot of the Old School and had no respect at all for the photo-recording flight-test instrumentation that we were developing and installing in our experimental airplanes in those days. He called them "mechanical stool

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pigeons" and refused to turn them on (!) when he carried out test flights. [This is, of course, incredible today, when any such experimental flight test is characterized by elaborate recording instrumentation and telemetering of data.]

It seems that Breese had been hired by other companies (he was a free-lance test pilot) including Bell Aircraft. For Bell he carried out dive-pullout tests of a new fighter. But a former student of mine, Bob Stanley, was one of Bell's test pilots at that time, and he questioned whether Breese was actually reaching the speeds and the pull-out accelerations that he claimed. So Stanley cranked up another of Bell's fighters and flew in formation with Breese, and found, indeed, that the required Vs and Gs were not being attained. Breese, of course, was furious.

And Mr. Northrop had contracted with Breese to make the first flight of the XP-61 night fighter. He made the flight, and everything went well with the big, new, twin-engined airplane, except for some unacceptable stick forces. My smart aerodynamicists and I, carried away by concern for the very powerful elevator effects needed to counteract the XP-61's full-span landing flaps, had designed an asymmetrical elevator without worrying enough about its hinge moments--which translate, in flight, into stick forces. So Breese had a legitimate squawk after the first flight, and we had to replace the elevators with more conventional, symmetrical ones, less powerful, to be sure, but having stick forces more palatable to the pilot.

Meanwhile, with Mr. Northrop's help, we had hired some colorful experimental-test pilots of our own, and became independent of Vance Breese. One was John Myers, son of the Chief Justice of the California Supreme Court and himself a graduate of Stanford's Law School. John was (and is) both a superb pilot and a big, extroverted man, very sure of his own abilities and convictions. Sometimes, when he phoned me from down on the flight-line to express his opinions of our airplanes or my instructions, my colleagues and I thought he needn't have used the telephone--we could have heard him without it.

John is the husband of Lucia Myers, a beautiful lady who, it seems, is an heiress. The Myerses have a mansion in Hollywood, a mountain estate at Big Bear Lake, and a big ranch home somewhere

farther north in California. John loves machines, such as his Bentley, his boats on Big Bear, and his jet-airplane (a Citation). He told us about the time that a motorboat passed him on the lake; he followed the boat to its destination, pulled up alongside it at the dock, and bought the boat from its owner. He wasn't going to have less than the fastest boat on Big Bear!

Lucia pointed out to Mabel and me one evening, over dinner, that she really couldn't understand the feminists' complaint that women are victims of prejudice in the business world. She has served on boards of directors and boards of trustees, she said, and has never detected any such prejudices. It did not seem polite, to Mabel and me, to point out that heiresses who serve on such boards are, to be sure, less likely than some other working women to encounter discrimination and chauvinism.

Besides Johnny Myers we also hired another experimental-test pilot, of quite a different temperament, Max Stanley (not related to Bob), who became the leading pilot of the big Flying-Wing bombers, the XB-35s and YB-49s. Max is a quiet, soft-voiced, studious type; his contributions to the Flying-Wing program, and to all of the Northrop experimental flight programs, were invaluable. At least twice during my years at Northrop, while carrying out maximum-speed, maximum-pull-out tests in the P-61 program, he experienced structural failures: on one of these occasions the airplane was destroyed and Max was saved by his parachute; in the other case he was able to retain control and land the airplane.

Mr. Northrop met a famous stunt pilot from the Continent, Alex Papan, a Romanian, obviously a very talented young man in need of a job, and believed that Alex could assist us with our flight-testing of the N1M and its successor, the N9M. [The latter was again a twin-engined, one-seat, plywood, Flying Wing; namely a one-third-scale flying model of the XB-35, about which I will have more to write below.] He hired Alex, possibly impelled by charitable motives, but it turned out not to be one of his better ideas. For all of Alex's great skill as a stunt flyer in such aircraft as the Bücher "Jungmeister", he was not a test pilot, nor did he ever, in the months he spent with us, acquire the ability to master big, powerful airplanes like the P-61s.

On a test flight of the XP-61, with one of my flight-test engi-

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neers, Hugo Pink, aboard, he actually ran out of fuel and had to make an emergency, dead-stick landing across traffic at Los Angeles Airport (then called Mines Field). Hugo had realized that the fuel tanks were near empty, had warned Alex, and was about to resort to his parachute when the engines quit. The airplane was destroyed but nobody was hurt.

Mr. Northrop agreed to let Alex go to someplace in Texas to carry out the first flight of some small experimental airplane. When he returned he found that his girl-friend, the young lady who kept up the Northrop test pilots' log books and sent their parachutes out for regular repacking, was absent, and so was Northrop pilot Charley L. There was even a rumor that they were vacationing together. To my dismay, I was informed that Alex had bought a gun and was lying in wait for Charley.

I went to our personnel specialists with the story, and was somewhat reassured. The general opinion was that a romantic Romani-an with a gun was unlikely to hurt anyone. But they were wrong. While we were worrying, Alex went out into the California desert, parked his car, walked to a place behind a big rock, swallowed poison, and shot himself! His body was found the next day.

And we also hired Harry Crosby, a well-known racing pilot who, I suppose, comes closest to the layman's image of a test pilot. Harry was practically nerveless, a lovable, happy-go-lucky young man, an incredibly skillful but utterly nontechnical daredevil.

Harry always wore his sunglasses. Once, somewhere, Harry and I went together to see a movie, and I discovered that even there he wore his sunglasses. I asked him, "Don't you want to take off your sunglasses?" He chuckled. "Hell no, Bill, I can't see a damn thing without these specs." The sunglasses had a big correction in them; he needed glasses all the time, but as a glamorous racing and test pilot he couldn't be seen with glasses, could he?-- except, of course, sunglasses.

Harry's favorite (and only?) tune was "Red Wing". ("My pretty Red Wing, ...") What I didn't know, but later found out, was that the only words he knew for "Red Wing" were off-color words.

Now, to tell you about Harry Crosby I must tell you a rather long story about Northrop airplanes. Mr. Northrop conceived of a flying-wing fighter with a prone pilot. In the prone position

(lying on his belly) the pilot would not only fit within the lines of a flying wing--no fuselage or other deviation from the clean lines of a thin wing--but would be able to tolerate accelerations up to perhaps 12 g's in turns and pull-ups. Now, at last, you will understand why I was trying to weigh Mr. Northrop's head (!) on the couch in his office, earlier in this chapter: we were designing a counterweight system to support the pilot's head in such accelerations.

Mr. Northrop's concept intrigued the Army Air Forces, and they gave a contract to a new group, Avion Inc., to design and build such a prone-pilot flying-wing fighter--they even conceived of the fighter as a "ram"--it would be flown into the wing or fuselage of an enemy bomber and would slice through it, presumably with minimal damage to its own welded magnesium-alloy structure! Avion Inc., you will be interested to learn, was headed by a former Caltech character named W. Curtis Rockefeller and my old friend Albert C. Reed--remember Al Reed, back in Chapters 2 and 3?

So Avion Inc. designed the airplane, the XP-79. Meanwhile we at Northrop built a plywood replica of it--a glider that incorporated the prone pilot and control system (including the head support). We took it to Muroc Dry Lake, and with Harry Crosby as the pilot we towed it into the air, first with a tow-car and then with an airplane. The purposes of the glider were, of course, to try out the flying and handling characteristics of the design and especially the completely unconventional controls, at least in gliding flight.

On one of the car-tow tests the glider got out of Harry's control and cartwheeled on the sandy surface of the dry lake. Harry emerged from the glider, which was not badly broken up, dusty and laughing, ready to try it again. After repair and some successful launches Harry thought he had the little glider under control, and we proceeded to tow it to altitude behind an airplane--probably a C-47. But on one such flight, when he had been towed to 4000 or 5000 feet above the dry lake, Harry undertook to pull the cable that would release him from the tow-plane, and apparently pulled the wrong handle. The result was that his emergency-escape system was activated: the bottom panel of the pilot's compartment, upon which he was lying prone, was jettisoned, and Harry with it!

As he emerged--he was not ejected, as from a modern fighter with an ejection seat--the glider, not being controlled, rolled over on

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its back, and Harry found himself sitting on its bottom. The tow-plane crew had quickly released the glider and, incredibly, it was in a steady, stable, upside-down glide. Harry told us later that he slid himself toward the leading edge in preparation for slipping off and opening his parachute, but found his ride comfortable and steady and decided to stay with it for a while. Below him we could scarcely believe what we were seeing: our little flying wing in a smooth, steady, inverted glide toward the desert floor with Harry sitting on its leading edge. Its flight path was nearly circular. At a comfortable altitude above the flat lake he did slip off and opened his 'chute. The glider continued its circling glide into the hills nearby. Harry, characteristically, was in fine spirits and laughing when we picked him up. He said he had no idea what had happened with the tow-release, but I think I detected in his voice a natural concern lest he be accused of pulling the wrong handle.

The glider was rebuilt; it flew very successfully. Harry explored its stalling (low-speed) characteristics and found that, as expected, it "fell off" to one side when stalled, into a nose-down spiral that was not difficult to handle. We were especially interested in this, which we interpreted to mean that the XP-79's behavior at the stall would also be relatively benign.

We even installed a liquid-fueled Jato rocket in the glider. We towed it to altitude, where Harry turned on the rocket, streaked across the sky over Muroc, and made aeronautical history in a prone-pilot, all-wing, rocket-powered airplane! His light-hearted descriptions ("... with a firecracker up my ass ...") were not recorded in aeronautical history, but were authentic Crosby!

Before the XP-79 was built, the responsibility was transferred back to Northrop by the Air Forces--I suppose Avion Inc. was not equipped to build it. It was more-or-less the ultimate Jack Northrop airplane that we, his young colleagues had joked about: It was unconventional in every way. Besides being a flying-wing with a prone pilot, it was constructed of welded magnesium alloy, powered by two Westinghouse turbojets, and intended to be used as a ram. Its flying qualities had been determined in the flights of the glider, especially at low speeds, but even, by means of the Jato flights, at higher speeds. Its pilot, of course, would be Harry, who had mastered the unconventional control system. It was trucked to the dry lake.

The takeoff was perfect--but frightening, when an Air Force ambulance drove across the lake in front of the XP-79 just as Harry began his takeoff roll! Another pilot might have been sufficiently shaken by the occurrence to abort the takeoff, but not Harry; apparently he could see that he and the ambulance were not going to collide, and he rose beautifully into the clear, still, desert sky. Mr. Northrop and I and a small coterie of Air Force and flight-test personnel, having recovered our composure after the harrowing takeoff, watched with joy as he roared back and forth over the desert; it became clear that he was enjoying the flight--the XP-79 was performing perfectly!

He came back over us, flying gradually more slowly, presumably feeling out the low-speed behavior of the airplane--more and more slowly, until I said to Mr. Northrop, "Jack, he shouldn't bring it into the stall on the first flight!" Just about then, the unbelievable occurred: the XP-79 performed a slow roll. Someone said, "My God, he's so happy with it that he's making a slow roll!" I knew that wasn't the truth. The airplane must be out of control. And as we watched in horror we saw the escape panel fly off and the pilot with his parachute emerge. The airplane continued its "slow rolls", but the pilot and his parachute fell to the desert floor.

The "rolling" airplane had struck Harry after he left it, and had killed him instantly.

I have had plenty of time to reconstruct what I think happened on that first flight of the XP-79. My conclusion is that the benign stalling qualities of the plywood glider were quite misleading. The glider was the same size as the airplane but much lighter; it was not a dynamically similar model. If it had been,--that is, if it had been heavier and/or smaller--its stalling behavior would not have been what I have described above ("... a nose-down spiral ...") but, instead, a motion resembling a slow roll. In other words, I believe that the rolling motion of the XP-79 was related to the "benign" motion of the plywood glider but was transformed by the laws of mechanics into a motion that Harry did not recognize.

If I am right, Harry could have stayed with the XP-79 as it slow-rolled over the desert, pushed the nose down, recovered from the stall, and continued his flight. And if I am right, the XP-79 was a great concept and an airplane of great potential. It was we

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aeronautical engineers who missed the boat. [There was, of course, an Air Force investigation of the XP-79 accident, but I don't think its conclusions agree with mine.]

At Harry Crosby's funeral a pretty soprano sang what Harry's young wife knew was her husband's favorite song, "Red Wing". She did not know what we, his flying colleagues, knew about the naughty words that Harry had sung to that tune.

After the demise of Avion Inc., Al Reed joined a group of aeronautical engineers in a highly classified study, organized by the military services, called "Project Vista". It was a name derived from the project's mission, which was to look into the future of warfare, and the project's location, which was the Hotel Vista del Arroyo, on Colorado Boulevard in Pasadena. I was not in touch with Al in those days and was astounded to read in the newspapers, some time later, that he had "disappeared". He was, of course, described as a brilliant aeronautical specialist, privy to all of America's and the Allies' secrets; there was at least a suggestion that he might have been kidnapped by some enemy nation--or defected to them! I learned that, since the Avion days, he had married and fathered a child. His wife was in financial need and was not well, the papers said. I also learned that Al had spent some years, during childhood, in Mexico, and was fluent in Spanish.

I took no stock whatever in the "defection" theory, or even the "kidnapping". Some of us wondered if he had had an unhappy marriage, and there was a rumor, among his acquaintances, that his wife's illness might have been psychosomatic and had been annoying to him. But sometime after his disappearance the lady died of a heart ailment and the child had to be taken in by her mother. Still no sign of Al.

After a year or so of disappearance, Al was found by the F.B.I. by a strange fluke. He was working as a groom of the racehorses at the Santa Anita Racetrack, under an assumed name, living in the rude quarters provided for the grooms. There was some scandal about the horses' being doped, and all the employees were being fingerprinted. He was unaware of his wife's death.

I have never seen Al or heard from him since those days, except that Bob Stanley, whom I have mentioned earlier in this chapter, called me one day to ask whether I thought he should hire Al as an

engineer in the company that he had recently founded. [Stanley Aviation specialized in the design and manufacture of ejection seats for military aircraft. Bob Stanley died, some years ago, in a flying accident.] I thought Bob was very astute to think of hiring Al. His professional career was surely jeopardized--so far as access to confidential matters were concerned--but Al was a very competent, experienced engineer.

I am told that he is now a schoolteacher and lives in Santa Monica.

Another colorful character of those days was a former GALCIT student, Roy Marquardt. He was an able student and a productive member of my ten-foot wind-tunnel crew. His talent as a kind of "operator" became clear to me during those Caltech days. Roy had a wind-tunnel assistantship and put in his required hours-per-week in return for a tuition grant like the rest of us. But he was approached by a fellow student who had a teaching assistantship, found teaching onerous, and proposed that Roy take it over. Roy must have felt that teaching would be more fun or more educational--or would take less time--than the wind-tunnel work, so he agreed.

He announced to me that he wouldn't be working on the wind tunnel anymore, but he didn't inform anyone else in the Caltech administration; he apparently reasoned that, since the pay (the tuition grant) was the same, it shouldn't concern the Institute. He met the class--albeit at a different hour and in a different classroom than scheduled--and, incidentally, he changed the textbook of the course (to von Kármán's and Biot's famous book, mentioned above.) I guess it all worked out all right for all concerned, but caused some understandable confusion when the administrators disentangled it all at the end of the quarter.

I hired Roy to work in our Aerodynamics Group at Northrop during the war. He was a man of great energy and productivity, and in spite of the long hours we all worked in those days he taught courses in night school at the University of Southern California. I remember his telling me that such teaching paid so well that he "simply couldn't not do it". He and his buddy, Bob, another GALCIT graduate, also undertook to do some consulting in their free time (!). I had a heart-to-heart talk with Roy about his responsibility

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to devote his full time and energy to his Northrop job, but I could not be awfully firm about it, since he was doing a very respectable job for us; moreover he could argue that the teaching and consulting were also valuable to the war effort.

The consulting, he said, was being done for Aerojet, the rapidly growing rocket company founded by Frank Malina, von Kármán, et al. Aerojet had hired Roy and Bob to study the concept of a helicopter whose blades are driven by rockets mounted at their tips. It turned out that the studies seemed to say that it was a great idea; Roy told me the results he found were "much too good to give to Aerojet!" I thought he was obligated to deliver his results to Aerojet, but he disagreed. He was proposing to start a new company of his own, to develop and exploit the idea.

And so he did. He arranged for an engineer of one of the military services to come out to Los Angeles to discuss contractual matters and government support of the new company. Roy asked for time off from Northrop during which he would meet and confer with the fellow in an office on Leimert Park Boulevard in L.A. "Whose office is that?" I asked naïvely.

"Oh, we've rented it for the day," replied Roy. "We'll have our company's name lettered on the door and scraped off when the guy goes back to Washington." So that is how Marquardt Aircraft was born. It seems that they got contractual support, but not, I think, for the rocket-powered helicopter. Marquardt Aircraft became an important company in rocket and ramjet propulsion; it later attracted the financial support of Laurance Rockefeller.

But, in its beginning, the Marquardt company aroused the ire of Dr. von Kármán, who was a consultant to Northrop and came over to Hawthorne quite regularly, about once a week during the war years. He learned the story of Roy's decision not to give the helicopter results to Aerojet, and was shocked. He told Roy that he was an unscrupulous consultant. Roy argued that neither he nor Bob had received any money from Aerojet and simply wouldn't submit any invoices for their services.

We also learned that Roy had received financial support, or promises therefor, from Johnny Myers and perhaps from some others at Northrop, and this angered Dr. von Kármán too. He told me, "Roy may be a naïve engineer, but John is an experienced business man and knows that what Roy is doing is unethical!"

But there are some anecdotes from the Northrop years that are more pleasant.

Mabel and I wanted to have a family, and after several years of consultation with experts in the matter of fertility, specifically in late 1942, we turned to adoption as a solution and acquired a beautiful Christmas present, a three-month-old, blond baby boy, whom we named David William. David's weight record is amazing: The graph of his weight shows a very modest rate of increase during his three months in the adoption agency, followed by an abrupt increase of rate ( $dW/dt$ ) when he came to us and to the loving care of Mabel. No baby could possibly have been a greater joy to his parents than David. Of course he inevitably became a teen-ager (!) and even an uncommunicative undergraduate (!). But sometime later, as often happens, his parents acquired unsuspected wisdom; communication, and even the sharing of important moral, political, and intellectual convictions became possible.

We attacked the adoption process again in 1945, as the war began to draw toward its end. This time we were involved before the birth of the baby, and we acquired Susan Carol in November of 1945 at the age of two days! Of course there is sure to be a great difference between one's first and subsequent children: they grow up under completely different conditions. Nevertheless, both of ours have given us incredible love, joy, and pride. They are fiercely loyal to each other and to their aging parents. Both are in professions where they serve their fellow men, and we are proud to see that they do it with compassion, skill, good judgment, and impeccable honesty.

When I joined Northrop I acquired not only an Aerodynamics Group but its secretary, Mrs. Ila Poudrier, a striking brunette beauty. When I asked her if that wasn't a French name, she replied in a deep-South drawl that it was, but that the only thing French about her was her husband's name. She was a very good secretary, but as our group grew it became clear that she was overloaded, especially with the typing of reports, and would need some help.

My smart (smart-alecky) young colleagues informed me that they already had Ila's assistant picked out: the shapely young lady whose job it was to deliver blueprints from the blueprint room to

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the various engineers and draftsmen in the Engineering Department. In those days blueprints were an essential input for every designer in any of the groups and the ultimate output when he finished any job, large or small, so there was a constant flow from the blueprint room. The girl upon whom the aerodynamicists had their eyes was one whose regular journeys down the long aisles caused a hundred or so heads to turn in unison--it was a phenomenon almost as fascinating to watch as the girl herself.

I replied that I believed I knew which blueprint girl they referred to, but what made them think that she had any secretarial skills? Their answer (which they probably had rehearsed) was, "Look, Bill, if that girl can read or write, we want her for Ila's helper."

Well, we were fortunate; the attractive young lady, Kay Bissell, could not only read and write but also type. So I got her transferred to our group. She didn't know shorthand. [Remember shorthand?. Such persons as Mabel Rhodes, Ila Poudrier, Dolores Lewis (Ila's successor), and Toni Anthony (See Chapter 6) were skilled in shorthand, but nobody seems to know it anymore.] So Kay took over from Ila the typing of our reports, and did a fine job.

Our reports were highly technical and required quite a bit of mathematical jargon, including Greek letters. Kay asked me if I would teach her the Greek alphabet. I would, of course, but I had a little handbook from Pratt & Whitney that listed the Greek alphabet, and I simply loaned it to her. Some time later I found Kay diligently working with it at her typewriter, and I asked her what she was doing. Her reply delighted me: She was putting it into alphabetical order! "Kay," I told her, "the Greek alphabet that I gave you was in alphabetical order when your ancestors and mine were still living in trees!"

Although my Aerodynamics Group included so many attractive young men--even Caltech graduates--lovely Kay married an aeronautical engineer of the Hughes company. It seemed, to us, a traitorous act.

Moreover, one gloomy morning, Ila Poudrier came to my office to tell me how difficult her life was, with her working at Northrop and her husband, Roy Poudrier, working at another aircraft factory on the night shift. "We only see each other at breakfast," she said, "and we think we ought to have a second child."

I understood their problem, and I was sympathetic. My heart sank as I contemplated her quitting, but I had to accept it. I expressed this and suggested that she give me a couple of weeks to look for a replacement. "Oh dear!" she gasped. "Roy is waiting for me right now in the parking lot!"

Our most pressing jobs in those days had to do with the P-61 series of airplanes. They were big, heavily-armed fighters, initially designed as night-fighters, they were painted dull black and were given the name "Black Widow" in honor of the rather large, black, poisonous spiders that householders contend with in Southern California and other semi-tropical locales. As I have mentioned, we gave our P-61s full-span--or, rather, almost-full-span--wing flaps, to facilitate their operation at night from short fields. We gave them upper-surface, "spoiler" ailerons, just forward of the flaps, and small, conventional ailerons near the wing tips to give the pilot acceptable hinge-moments. The night fighter carried 20-mm. cannons in its belly, aimed by the pilot, and a big machine-gun turret, midship, that was aimed and fired by either the front-gunner, who sat behind the pilot, or the rear-gunner, who sat, facing aft, in a compartment behind the turret. The nose of the big fighter was devoted to radar.

The P-61's features were the result of collaboration between our Army Air Forces and their British counterparts, who were fighting the Axis's night bombers over London. Some of our airplanes got to England in time to help, but by the time we were in full production the "Battle of Britain" had pretty well been replaced by defense against the "buzz bombs"--pilotless, jet-propelled vehicles. Most of the Allies' fighters, including the P-61s, were not fast enough to shoot down the missiles; the only effective defense was anti-aircraft artillery combined with radar. P-61s were flown to the war in the Pacific, where the machine-gun turrets were removed and external fuel tanks were hung under the wings to give the airplane the additional range needed for that operation.

The Black Widows were unusually ugly airplanes: big, black, with windshields for both the pilot and his gunners, the turret, the 20-mm guns, and the tail assembly supported by two tail-booms that permitted the rear gunner a wide field-of-fire rearward. But they were beloved by their pilots because of their equipment and their

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docile flying qualities. They could be flown safely at speeds so low that the wing was "in stall". The pilots were aware of other high-powered twin-engined airplanes that were not controllable if one engine failed--there was even an American fighter that was so dangerous in that condition that its pilots were instructed to take to their parachutes immediately if either one of its two engines lost power, at any flying speed.

But the P-61s were quite otherwise. Johnny Myers took one of them to the Pacific and demonstrated its remarkable properties at American air bases. We were told that he even made takeoffs on one engine, followed by acrobatic maneuvers! I think his demonstrations must have been very important to the military personnel, especially since the Black Widows were so unusually big, as fighters go, were high-powered twins, and even had unconventional ailerons.

Actually their unusually good flying characteristics had been confirmed by a program of handling-qualities tests carried out by test pilots of N.A.C.A. at Moffett Field; such tests were made of all new military types, by N.A.C.A. under contract with the military services. I was amused--but also chagrined--to learn that the highly skilled N.A.C.A. test pilots, when they tested the P-61, had assumed that it couldn't be flown inverted--couldn't be slow-rolled, for example--and had deleted such maneuvers from their test program for this airplane! Their reasoning was that the upper-surface ailerons were "spoilers" that worked by "spoiling" the lift of one wing without, of course, increasing the lift of the opposite wing. So, they said, if the airplane were to be flown upside-down--with the wings producing lift in the negative sense--the ailerons would "spoil" that negative lift and produce a rolling moment opposite to that desired.

Even my readers who are aeronautical engineers may be fooled by this seeming logic. (Those who are not have probably been lost already, back in the preceding paragraph, in the world of positive and negative lift and rolling moment.) The mistake in the argument is that the "spoiler", when the wing is inverted, acts just like a flap; it doesn't "spoil" but increases the (negative) lift, thus maintaining the usual relationship between control-wheel deflection and rolling moment. Too bad that Johnny Myers didn't buzz Moffett Field in a P-61, upside-down, before I explained their error to the N.A.C.A pilots. (One of whom was a Minnesota Aero.E. buddy of mine,

Larry Clousing; it was he who told me about their theory regarding the P-61s in inverted flight.)

From those big night-fighters with (some people said) three fuselages, two engine nacelles, two tails, and three windshields, we turned our attention to the Flying Wings, which had no fuselage, no nacelles, no tail, and no windshields. I think Mr. Northrop's attraction to the all-wing configuration had a considerable esthetic component in it; he often said that, to be a good airplane an airplane ought also to be good-looking. His track record was certainly one of handsome airplanes, from the Lockheed days of "Vega" and "Orion" to the Northrop "Alpha", "Beta", and "Gamma", and even the XFT. He considered the fuselage, nacelles, and tail surfaces to be unnecessary appendages and therefore unbecoming. But every engineer, including Jack Northrop, knows that any good engineering design, be it an airplane or a bridge or a television set, is the result of a long series of compromises--by a process called "optimization"--that undertakes to produce the best possible system to meet all the requirements of the job.

In the early 1940s the Allies seemed to be facing the prospect of waging intercontinental war against the Axis powers from bases in the Western Hemisphere. They needed airplanes to carry large bomb loads over ranges of several thousands of miles, and so the Army Air Forces contracted for the development of the B-36 series: very large, six-engined airplanes. It was becoming clear that a Law of Diminishing Returns was at work here: historically, maximum ranges of airplanes had always increased as the airplanes got bigger, but the increase achieved by going to behemoths like the B-36 seemed to be woefully small.

The ability of an airplane to carry a useful load over the greatest possible distance--its maximum range--is determined by its aerodynamic efficiency, its load-carrying ability, and the characteristics of its propulsive system. There is no direct relationship between size and range if the airplanes are required to use airfields of given size--that is, to maintain given takeoff and landing performance.

Mr. Northrop was convinced that by going to the flying-wing configuration he could improve the aerodynamic performance to the extent that airplanes of more modest size could achieve the re-

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quired improvement in range and useful load. The A.A.F. was so well convinced that they contracted for the XB-35, which would have the same range and payload characteristics as the B-36 but would be smaller and more economical--four-engined.

As our project progressed it became clear that the biggest advantage offered by the Flying Wing, given the takeoff and landing constraints, would not be in its aerodynamic performance but in its load-carrying ability; i.e., in the area of structural-weight reduction. Simply stated: a Flying Wing having the same aerodynamic efficiency and using the same airfields as a conventional airplane could carry a greater proportion of its takeoff weight in fuel, and therefore fly farther with the same bomb load. This advantage results from the substantial reduction of its structural weight in an airplane whose load is distributed along its wing instead of being carried in a fuselage.

In other words, in the inevitable game of optimization, the best strategy is to exploit the Flying Wing's advantage in structural weight while holding the aerodynamic efficiency to (at least) the level of conventional airplanes. This emphasis on the structural-weight advantage has led NASA engineers to call flying-wing airplanes "span loaders", a name that does emphasize the important advantage of distributing the load along the wing-span. (It also has the virtue, for NASA, of avoiding the use of a registered trade-mark, "Flying Wing".) NASA's studies of "span loaders", showing substantial advantages over conventional designs, were published and made available to Mr. Northrop before his death in 1979. He liked the new name. All of his design studies, dating back into the 1930s, had predicted the improvement of structural-weight ratio, although he seemed to emphasize aerodynamic refinement when he wrote about Flying Wings, perhaps because he thought it was easier for non-specialists to understand.

The N9M airplanes were one-third-scale, dynamically similar models of the XB-35. I must emphasize once again the importance of the dynamic similitude; it meant that all of the motions of the N9Ms, such as responses to controls and atmospheric turbulence, behavior at the stall, etc., would be exact replicas of the motions of the XB-35, but measured on different time and distance scales. For a one-third-scale model the conversion factor for distance is

1/3, and for time the square-root of 1/3, which is about 0.58, so the N9M carried out maneuvers that were the same as the XB-35's but just scaled down like its size. That seems intuitively right, doesn't it? But it carried them out about 40 percent more quickly. Its flying speed in any such maneuver would be reduced about 40 percent.

In one respect we had to relax the requirements for a true one-third-scale model: the engine power required for similitude is very little, but there were no such little engines available that would fit into the N9M's wing, so we used only two--one on each side--to represent the four engines of the full-scale airplane. This made the N9M a rather dangerous little twin in case of an engine failure: it represented the XB-35 with two of its engines, on the same side, inoperative. [And that did happen in our N9M flights, as I shall recount below.]

Emergency escape of the pilot from the N9M would require him to jettison his bubble-canopy and go back between the propellers. They were driven through hydraulic couplings designed by Mr. Northrop himself, and, to make the pilot's egress possible, Jack designed spring-loaded brakes that would stop the propellers; these were activated from the cockpit by pulling handles located on the dashboard.

The N9Ms were an outstanding success. They may have been rather underpowered and underloaded, with dangerous single-engine performance, as 60-foot-span twins go, but they were designed to be dynamically similar scale models of the XB-35s, and as such they were absolutely successful. And they performed another function--perhaps not anticipated: they were flown by a dozen or so AAF pilots whose opinions might matter. One of these big-shot test pilots was the colonel who was the Chief Test Pilot of Wright-Patterson Air Force Base. In a flight in the N9M at the AAF base at Muroc, he became so thrilled, I suppose, that he forgot to lower his landing gear. This held up our development program for at least a few months, but it was some kind of historical first, and proved, to anyone who doubted it, that a Flying Wing could make a wheels-up landing.

The N9M had a gentle, lateral-directional oscillation--a kind of "Dutch roll"--that worried Johnny Myers, which he wanted me to see. So he and I arranged a way that I could sit behind him, facing sideways, in the single-seat airplane and observe the oscillation

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in flight. We were sure that Mr. Northrop would not approve of this, so we undertook it without bothering him about it. We took off from Northrop Airport; sure enough, in cruising flight we were in a mild yawing-banking oscillation with a period of about 5 or 6 seconds. With a little skill Johnny could stop it. In the XB-35, the period would be 9 or 10 seconds [as you will surely recognize from what I have taught you about dynamic similitude, above.]

So I was not seriously worried about the "Dutch roll", and we flew back toward our airport--but when John lowered the landing gear only the nosewheel and the right-hand main wheel came down! He made several attempts to correct the situation. We flew over the runway, so that our mechanics could see what our problem was. The situation became comical: We saw that they had written a message to us on the runway, but the N9M was not designed to give its pilot downward vision. John said, "I'll come back over the field and make a steep banked turn, and you read the message." He did his part, but when he made the steep bank and asked, "What's it say?" I had to say that it was upside-down to me and I couldn't read it.

He said he would come back the other way and bank the airplane again--but just then one of the engines started missing and making an ominous popping sound. We gave up reading the message, turned to a downwind direction, and began an approach to landing. We knew that any kind of a crash landing would seriously delay the Flying Wing project; we wanted especially not to damage the propellers or engines, so John instructed me to pull the handles to actuate the propeller brakes "When I say 'Now!'"--which would be just over the highway at the east end of the field. They were two-blade propellers, so there was a reasonable probability that they would stop turning in a horizontal position and not be damaged by the one-wheel-up landing. But I couldn't reach those handles from my makeshift seat behind him.

Fortunately, we wore neckties in those days; Johnny quickly tied his to one of the handles and mine to the other, and handed the other ends to me. He lowered the landing flaps, cut the power, and shouted "Now!" He made a perfect one-wheel landing and proceeded to hold the left wing off the ground as we rolled down the runway, until finally, as our airspeed diminished, the left wing-tip gently settled on the concrete and the airplane executed a graceful turn to the left to where our line-crew and the fire engines stood

waiting. And there, too, was our boss, Jack Northrop, and he learned for the first time that I had been on the flight.

When they tell this story, my Northrop colleagues always end it by pointing out that my sweaty hands ruined both neckties.

It turned out that the message on the runway read "Make belly landing". It's just as well we didn't read it, because we would not have done that anyway. Neither of the propellers was damaged. Besides the neckties the only damage was to the wing-tip, which was just a formed piece of sheet-metal; it was replaced and the N9M was back in flight in a couple of days.

The XB-35 made its first flight in 1946, with Max Stanley at the controls. Its flights confirmed our claims about large airplanes of the Flying-Wing type. Its tendency to oscillate slowly and gently in a lateral-directional mode--the "Dutch roll"--was easily eliminated by an autopilot designed for the job by the Honeywell company, working with my aerodynamicists. Still it was not a very good airplane in some respects, having to do with the complexities of the propulsive system. The four engines were buried in the wing, as were cooling fans, oil coolers, intercoolers, long drive-shafts and gear boxes driving eight-blade counterrotating propellers. The various air ducts required for all this had to be provided with gates, valves, and their controls.

But, while the 35s were being built, a major development took place in the world of aeronautics: the turbojet engine came upon the scene, by virtue of the genius of my good friend Hans-Joachim Pabst von Ohain and, independently, Frank Whittle. John Wild was one of the first of our group to learn about the characteristics of these engines and to recognize how well they suited the needs of our Flying Wings. Air Force engineers came to the same conclusions, and the YB-49 was born. The 49s were 35s with all the components of their propulsion systems eliminated and replaced by eight turbojets, four on each side, buried in the wing. They were fast, efficient, and nimble airplanes, well appreciated by their pilots, both military and civilian. They set records for speed and range and were said to be able to out-maneuver the fighters of the day.

Nevertheless, they exist today only in some very impressive cinema records and in the memories of some of us old-timers.

The sad story of the major production contract for B-49s and how

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it was canceled by the Air Force has been written and presented in television documentaries. It is a complicated story involving corporate ambitions. We can only look back on the Flying-Wing project as one that was highly relevant in its day and generated some impressive engineering facts and figures, which remain available in aeronautical history. Many of the radical features and concepts of the project have become commonplace in the intervening decades and are being utilized--or will be.

The most important wind-tunnel tests in the XB-35 design program were carried out at Langley Field, near Norfolk, Virginia, where the N.A.C.A. had its major experimental facilities, so I made a number of trips to that establishment and met its personnel, some of whom were remarkably colorful. One was Robert T. Jones, surely one of the world's greatest aerodynamicists. Bob never had the benefit of a college education, but made major contributions, nevertheless, to his profession--and to grinding lenses for telescopes and building violins--and has been honored by medals, prizes, honorary degrees, and elections to academies. His colorful, incredible career was told when NASA published, in 1976, a big, fat volume, *The Collected Works of Robert T. Jones* (NASA TM X-3334, Natl. Tech. Info. Services, Springfield VA 22161).

Bob and I had actually met before, back in the '30s when we both worked on the theory of wings in unsteady motion and wings entering atmospheric-gust boundaries. His most famous accomplishment was that he discovered the effect of wing sweepback in delaying the appearance of shock waves and all the undesirable phenomena they bring with them. It is a fascinating story: Jones's theoretical argument is so simple that his contemporaries at Langley Field and elsewhere just couldn't believe it:

Suppose a cylindrical wing (constant chord, incidence, etc.) is placed in an airstream at an angle of yaw--i.e., it is swept back. Now, even if the local speed of the air on the upper surface of the wing becomes supersonic, a shock wave cannot form there because it would have to be a sweptback shock--swept at the same angle as the wing--i.e., it would be an oblique shock. Such an oblique shock cannot form until the velocity component normal to it becomes supersonic.

This means that for such a yawed wing all the deleterious effects of local, embedded, supersonic regions of flow are postponed to flight Mach numbers that are greater (by the factor  $1/\cos \beta$ , where  $\beta$  is the wing's sweep angle) than for the same wing, unyawed. [No wonder that the wings of all of today's high-speed airplanes are swept back!]

This discovery, of inestimable importance in modern aviation, was actually made first by Dr. Adolf Busemann, in Germany in about 1935, but was not recognized in the Allied countries. When Jones independently discovered it, in the '40s, he met with skepticism. When wind-tunnel tests confirmed his predictions the skeptics said that the tests were faulty, and N.A.C.A. started a program to test the theory in flight.

Before these tests could be completed, the war in Europe came to an end, and a number of teams of aeronautical engineers went into Germany to find out about wartime German progress in the field. One member of one such team was the eminent Boeing engineer, George S. Schairer, who reported back to Seattle that all of Germany's latest airplane designs--experimental airplanes and wind-tunnel models--looked as though they had been pulled through a too-small doorway--their wings were all swept back!

In later years George said that, as far as he could make out, only Robert Jones and Bill Sears understood the "Simple Sweepback Theory"--the argument I have quoted above. He gives me too much credit: The truth is that I found it impossible to believe that a local supersonic flow could decelerate to subsonic without a shock, --until Bob Jones straightened me out.

There was also, at N.A.C.A.'s Langley Laboratory, a distinguished aerodynamicist, Doris Cohen, who was a co-worker with Bob Jones. Much to my surprise, one day I learned that Miss Cohen, whom I knew, had applied for a job at Northrop. I quickly made contact with her and learned a romantic tale: A romantic relationship had developed between her and Bob, and she proposed to test it by resigning from N.A.C.A. and moving away from Hampton, Virginia--apparently to see whether they could get along without one another!

Knowing something of Doris's abilities, we naturally arranged a job for her at Northrop. Dr. von Kármán was spending one day per

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week with us in Hawthorne as a consultant, and was pursuing some interesting new ideas about unsteady-wing theory for wings of finite span in compressible flow; it was my bright idea to assign this talented lady to him, to assist in the new theory.

So Doris came to work at Northrop. I recall that she didn't own a car and had to cope with the appalling lack of public transport in the Los Angeles area. Nevertheless she was happy there and made new friends. To work with Dr. von Kármán was a great thrill for her. I don't remember how long she stayed--it must have been less than a year--but she came to me one day, agitated and embarrassed: she said that she thought she must be insane!--to work with Dr. von Kármán was more than she had ever hoped for; nevertheless she was going back to Langley.

It became clear to me that her and Bob's test program had been a success: they had found that they could not live apart! She wanted to steal away, but I was cruel enough to say that she herself had to tell her great boss that she was leaving. He told her that it was a blow to him to lose her assistance but that he "was sure that Bob Jones could give her what he (Kármán) could not". [He was so proud of this clever remark that he quoted it to me, Jack Northrop, et al.]

When, a year or so later, we learned that Bob and Doris Jones were the parents of a son, Dr. von Kármán was even more happy about his clever remark and reminded all of us about it.

During the war the Los Angeles region was deemed to be a potential target for enemy attacks--infiltration by enemy agents, perhaps--because of the great amount of war materiel produced there. We also were taught blackout procedures. Mabel and I remember walking down from our house on 85th Street to see a movie at a theater on Manchester near Crenshaw--having found a baby-sitter for little David--and finding, when we left the theater, that L.A. was undergoing a blackout. An air-raid warden instructed us to get off the sidewalk, to take shelter in a store or other public building, until the "all clear" sounded. But with our house and infant child only a few blocks away, we opted to wait until the warden turned his back and then to hurry home, up the hill.

World War II buffs will remember that there was a period, prior to the Battle of Midway, when Allied intelligence "lost track of"

the Japanese Navy. Our military authorities decided to move all flyable aircraft from the West Coast to inland airports such as Phoenix and Tucson. The young Army Captain who carried this "evacuation" order to us commandeered all of Northrop's pilots to move the P-61s inland. There were more airplanes than pilots. It just happened that Johnny Myers's friend "Jeeb" Hallaby [mentioned in Chapter 2, above], was visiting us that day. The Captain asked him, "Who are you?", and when Jeeb replied that he was a Lockheed test pilot, instructed him to fly one of the P-61s to Phoenix. Jeeb, of course, had learned a lot about the P-61 from Johnny, but it must have been the first time he had ever flown one.

I think it was that same night when the "air raid" occurred. We were awakened by the sounds of explosions--bombs? anti-aircraft artillery? I said to Mabel, dramatically, something like "Well, here it is! We're in it!" We wondered whether we shouldn't bring little David from his crib and shelter him in our arms.

From our bedroom window we could see the flashes of explosions (bombs?). All around us we could see the beams of searchlights shining up into the night sky. On our knees, we could see the sky, and we realized that each explosion on the ground was followed by one in the sky.

I noticed something peculiar: The searchlight beams all converged at a point in the sky; the explosions in the sky were all occurring at that point; there was a cloud of smoke at that point, which drifted slowly over the city; the searchlight beams followed the drifting smoke. "They're shooting at their own smoke!" I said to Mabel.

We were very skeptical. When morning came, our skepticism was not allayed. The morning paper reported that citizens had seen enemy airplanes, had heard them and counted them ...!

It was some years before we learned the truth: The Japanese (we were now told) had released incendiary balloons, out over the Pacific, that they hoped would drift with the prevailing westerlies and start forest fires in the Western U. S. These were the targets, drifting with the smoke, that our anti-aircraft batteries were shooting at!

In 1945, when the war in Europe ended, the several American military services sent panels of civilian experts to survey Axis

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(German) aeronautical developments, as I have reported above. One was a Navy effort called, in inimitable Navy style, "NAVTECMISEU", for "Naval Technical Mission in Europe". I am told that its personnel gathered in Paris one evening, and someone said "Where's Bill Sears? He ought to be here." So they got the Navy to request my services. I was excited by the prospect and told Mr. Northrop so. He thought it was a perfectly stupid idea; he assured me that if I stayed in the aircraft industry with him I would have many opportunities to visit Europe--and with Mabel. But I was rather stubborn, and he finally agreed to let me go with the Navy, as Civilian Technician, for a month in the summer of 1945.

It was a most interesting experience. I was a little worried lest the Navy, if they realized that I had a Reserve commission, would call me to active duty and send me to Europe as an officer, and who knew when I would be allowed to come back? My draft status was that of an inactive member of the armed services, and when the lady Lieutenant (j.g.) in Washington found that out she scolded me: I should have made that clear from the start. I confessed that I didn't want to go as an active officer; she was disgusted! "Do you think we would send you into Germany as a lousy Lieutenant (j.g.)? We are sending you as a big-shot Ph.D. of the Northrop Company!"

I had to buy uniforms--yes, in war-time Civilian Technicians are in uniform--including duplicates of garments I already had back home, and dozens of black socks, suits of underwear, another Navy "raincoat"--the kind that policemen and hotel doormen always wear, etc., and had to get all new shots. Finally I was off to Europe in a C-54 (I forget its Navy designation) by way of Patuxent River, Newfoundland, the Azores, and Paris. Civilians with the Ph.D. were ranked "with (but after) Navy Captains", but there were a half-dozen comfortable seats for Navy Captains and none for civilians with the Ph.D.: we sat in bucket seats.

It was a pleasant trip. The highest-ranking officer was my old friend, Captain Walter Diehl, eminent aerodynamicist and writer of a famous textbook. He was visibly embarrassed to be sitting in the comfortable chair, and moved over to sit with me in the bucket seats as soon as we were airborne. At Patuxent, Newfoundland, and the Azores, the local commandant met the flight and invited Captain Diehl to join him in his quarters for a drink and a meal. The Captain accepted politely and added, "Oh, and I hope your invita-

tion includes my old friend Dr. Bill Sears, who's traveling with me." It did, of course. Diehl liked to introduce me as his friend "who thinks you can fly around in pieces of airplanes"--an allusion to the Flying Wings.

The U.S.Navy had taken over the luxurious Hotel Royal Monceau in Paris, near the Place d'Etoile; a string quartet played for us in the dining room each evening, and wine was served. Paris, in the summer of 1945, was thrilling to a Minnesotan from California. I found that my friends of NAVTECOMISEU did not have anything special in mind for me to do, except that there were a lot of data on sweptback wings, and I was the only American who had had any experience with them. It was agreed that I should visit the Messerschmidt plant near Munich and another research group at Prien-an-Chiemsee. I was fitted with still another uniform and assigned an olive-green Jeep and a driver, Lt.(j.g.) Mendelson. The uniform, too, was olive-green, with high, laced boots and a jaunty "overseas cap".

We drove into Germany, which was occupied by General Patton's 1st Army. None of his soldiers had ever seen a green Jeep or the olive-green uniform, and General Patton had ordered his troops to wear their helmet liners at all times. [Not the helmets themselves, but the handsome, shiny, plastic liners.] We were stopped at every checkpoint. A typical exchange was "Now who the hell are you guys?" "We are Naval officers" (firmly, with dignity).

At one checkpoint the response was "Yeah? And what Navy is dat?"

At another it was "Naval officers, huh? Ain't you guys gettin' pretty far from da ocean?"

Of course the missing helmet liners were a source of trouble; we had to convince the soldiers that General Patton's edict didn't apply to us.

But we did get to Munich, which had been reduced to rubble by the Allied bombing. There the Navy, characteristically, had taken over a handsome mansion and was known to serve the best food in the city. My old classmate and housemate, Paul Dane, whom you met in Chapter 3, was a colonel of the USAAF, and frequently found it convenient to deliver messages, in person, to the Navy mansion just about mealtime. I'm afraid I teased him about colonels carrying messages, and he blushed.

My traveling companion, Lt. Mendelson, was mainly interested in

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buying valuable stamp collections from the Germans. We would look up the addresses of stamp merchants, make our way there in the Jeep, and invariably find bombed-out buildings. The local people were eager to tell us where the stamp dealer could be found. Nevertheless I don't think I saw him consummate any big deals that would make him rich.

At Messerschmidt I found some sweptback-wing data that seemed to confirm ours (Northrop's), but the project at Prien-an-Chiemsee was not interesting. They were building a full-scale but light-weight glider representing a very-high-speed airplane of the "space-plane" type. The project was apparently begun at Peenemunde and was moved to Bavaria when the Peenemunde research station was getting bombed. The glider would definitely not have been a dynamically similar model!

Some American "experts" had recommended that the glider project be transported, with personnel, to the U.S. for continuation. [Perhaps these "experts" included those who proudly drove back to Munich with their Jeeps full of telephone directories, which they thought were tables of technical data.] It turned out that one of the things that I could do for the Navy was to advise it about the wisdom of importing, to America, such German research programs and/or their personnel. I wrote several reports, while I was in Germany, arguing against these importations, where the projects were ill-conceived, such as the one at Prien, and the personnel were technicians rather than scientists. I think my advice was not followed but that the record says that I was right!

Somewhere in Bavaria I encountered the Navy Captain who was head of the Power Branch back home. He and I disagreed on all these recommendations. He had a lot of ideas about where I ought to go in Germany and what I ought to do; talking to him I was aware how fortunate it was that I was over there as a civilian employee of Northrop--otherwise who knows when I would have been allowed to return to the U.S.A.?

Before leaving Europe I arranged to visit Britain and was able to visit the Royal Aircraft Establishment at Farnborough. I was also the houseguest of the Goldsteins in Cambridge. Sydney, who had spent the war years doing aerodynamic research at the National Physical Laboratory at Teddington, was back at Cambridge University. He told me that the beautiful big house that he and his family

lived in had been the home of Sir Horace Lamb, one of the great fluid-mechanicists and author of the most important book in the field--*Hydrodynamics*. Sydney remembered being interviewed by Sir Horace, in the room that was now his study, in connection with his studies for the Ph.D. Apparently Lamb was very shy, and Sydney, his student, sat before him awaiting instructions; they looked at each other in painful silence.

What Sydney was doing when I visited him was writing a recommendation for a young fellow who had worked for him at N.P.L. and now wanted to study at the University. Sydney handed me what he had written. I was amazed; the recommendation was glowing; I was aware of Sydney's high standards and could hardly believe that any young fellow could be that good! But he was (and is); he is James Lighthill, now Sir James, certainly one of the greatest of applied mathematicians and fluid mechanicians.

James was made a Fellow at Cambridge, and later held distinguished professorships at Manchester, University of London, and Cambridge. He was Director of the Royal Aeronautical Establishment at Farnborough. Always a colorful personality, he became famous for his dramatic style of lecturing.

One of my friends overheard the following remark, one applied-mathematician to another: "The trouble with British applied mathematics is that it is tyrannized by James Lighthill. And I didn't say 'dominated', I said 'tyrannized'!"

Once Mabel and I, on a visit to the U.K., were the Lighthills' houseguests--they rescued us when we arrived in Manchester, in pouring rain, and couldn't find a hotel room. When James and I went to the University by bus in the morning, I found that we were to jump off the bus, with our briefcases, at a corner where it didn't stop, although it did slow down a bit. A few weeks later, after returning home, we received a clipping from the *Manchester Guardian* reporting that Professor Lighthill had been arrested at a railway station: He had left his car at that station to make a trip to London, but the train back from London that evening didn't stop there. Professor Lighthill used the technique he'd perfected on the bus, but it was illegal at the railway; he hit the ground running--into the arms of a policeman.

I was in London when the newspapers announced, in very large

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headlines, that an "atom bomb" had been dropped over Japan. We had heard stories, during the war years, of a highly secret project to construct such a weapon, so that it was exciting to learn that it had achieved some kind of success. I recalled that H.-W. Liepmann had heard rumors, from some of his physicist friends, that people working on the project in Los Alamos spent their spare time arguing about the possibility that an atomic explosion would destroy the earth's atmosphere.

Back in Hawthorne I found that there was a widespread feeling that "the War Years" were drawing to their conclusion and that people were looking ahead, wondering about the future of the aircraft industry, and planning their careers. John Wild asked one of the young military officers--there were dozens of them who oversaw everything we did and every piece of paper we generated at Northrop--whether he planned to stay in the Army. The young man's answer was, "Oh, no. I don't think so. I'm not really interested in the Army. What I'm interested in is the home."

I guess he meant that he was interested in home economics or something like that, but John found his answer highly amusing. "How about you, Bill," he said. "Aren't you also interested in 'the home'? I certainly am!"

More seriously, John and I found that we were both attracted by the possibility of starting an aeronautical-engineering department in some university. We were aware that our profession had made tremendous progress during the war, and we had confidence in its future and its importance to the peacetime world. I learned that Cornell University was going to found such a department; the Dean of Engineering there, Solomon C. Hollister, had invited Dr. von Kármán to become its Director; Kármán had told him, "I don't want to be director of anything. You should get Bill Sears."

Dean Hollister proceeded to do just that, under the impression that von Kármán meant that he would go to Cornell, but not as director of a department, if only the Dean would get me to be Director. [I take this to be an example of how difficult it is to translate accurately from Hungarian (or at least from Kármánian) into English; it is a subject that I will return to.]

Mr. Northrop was very disappointed in me. He had great confidence in the future of his company and in the rôle I could play in

it. I was not nearly so confident--that company had never built a civilian airplane. Dr. von Kármán shared my skepticism; he and I had received part of our salaries in Northrop stock during the past year or two; we discussed whether we should liquidate those shares or hang onto them as investments, and decided on the former. I think now that we could have made more money by holding the stock, but I certainly did not think then that a person of my age and financial status, with a wife, two children, and a mortgage, had any business making such a speculative investment.

I am sure that Jack did not appreciate the attraction that the academic profession held for me and John. My few years at Caltech had been a great experience, and I was convinced by my observations at Northrop that there was a crying need for that kind of educational opportunity--namely graduate study, as at GALCIT--in aeronautical engineering.

## CHAPTER 6

## CORNELL ( I )

I first visited Ithaca and Cornell in the middle of the winter of 1945-46. Mabel and I were pretty ignorant about upper New York State and the Finger Lakes, although we remembered that when we had driven to Boston in 1938 we had been amazed and delighted by the lakes and the rolling hills. Like most westerners we had thought of New York in terms of New York City.

Our ignorance about New York State was only exceeded by our ignorance about Cornell University itself. We learned that it was part of the Ivy League, coeducational, an unusual mixture of public (State) and private ("endowed") colleges, moderate in size (probably about 12000 students in those days). We knew that it was located "far above Cayuga's waters", whatever that meant. I knew that it had a famous engineering college.

Mabel had some misgivings about moving to the effete East. She thought that at a university like Cornell and a town like Ithaca she would have to wear a hat--probably even gloves!--on social occasions, and wasn't looking forward to that sort of thing with pleasure. We didn't realize that, although Cornell is part of the Ivy League, it is more like the big midwestern universities in some ways, and there is little stuffiness or formality, especially in

social things. Cornell even has a College of Agriculture, and the Cornell Faculty is loaded with westerners and middle-westerners. She never wore hat or gloves indoors in our 28 years there.

[But she did weep on January 1, 1948, when she listened to the broadcast of the Tournament of Roses parade from Pasadena. We both had wonderful memories of Pasadena, as you must have gathered by now.]

One of the motivations for Cornell's establishing a Graduate School of Aeronautical Engineering was that the Curtiss-Wright Corporation had established a research laboratory in Buffalo, across the street from its Airplane Division, and had donated that laboratory to Cornell University at the end of 1945--acquiring, thereby, a considerable tax advantage. It now bore the name **Cornell Aeronautical Laboratory**.

It was not at all clear, at the beginning of 1946, how the Cornell Aero Lab would be related to the university in Ithaca. I think some people had very naïve ideas about this--perhaps even Dean Hollister--including the possibility that the G.S.A.E. might be located in Buffalo (110 air miles from Ithaca). My own convictions on this matter were clear and firm: the G.S.A.E. would be a bona fide department of the university, at the university; the Lab in Buffalo would be a non-profit (or not-for-profit) institution, serving government and industry and having warm, cooperative relationships with many individuals and departments of the university.

I made it clear that this was the only structure that would interest me. I think most of the several hundred professional personnel at the Lab were happy with this structure. (Although I am not sure the Lab's Director, Dr. Clifford C. Furnas, was.) The Board of Trustees of Cornell University appeared to agree, since it set up the Lab as a not-for-profit corporation governed by a Board of Directors that included, among others, Dean Hollister and me, and whose Chairman would be Cornell's Vice-president for Research, my friend Dr. Ted Wright, distinguished engineer, formerly of Curtiss-Wright but more recently America's first Civil Air Administrator.

The Lab's history, before it was given to Cornell, had been somewhat stormy. Dr. Furnas was a chemist, not an aeronautical specialist of any kind, and this rankled in the hearts of some of

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its top research personnel, including Dr. Norton B. Moore. Moore was one of the early GALCIT Ph.D.s (1934)--he was one of those who warned Mabel about the difficulties of working for Professor von Kármán. He organized a revolt in the Curtiss-Wright Research Lab, the rebels demanding that Furnas be replaced. The revolt was unsuccessful and it was Moore who was replaced.

I learned that when the news reached GALCIT that I had accepted the position at Cornell someone said to Clark Millikan, "Cornell! Oh dear, isn't that the place where they have that terrible guy, Furnas, who made so much trouble for Norton Moore?" [We'll overlook here the question of who caused so much trouble for whom; that's not the point of this story.]

I'm told that Clark replied, "Yes, that's the place, but I don't think we have to worry about Bill Sears, he's a lot tougher than Norton Moore." [I'm flattered by the story. There's another story, farther below (Chapter 7), that shows Clark worrying about me.]

Since I have introduced the subject of the Cornell Aero Lab at this point, let me jump ahead and tell you how it fared. In brief, it prospered; it was certainly one of the best of the independent, not-for-profit laboratories, as regards both its personnel and its equipment. Its work was supported by contracts with the armed services and, occasionally, the aircraft industry. Some of us rode to Buffalo on Friday afternoons and presented classes--three hours of an evening!--for the benefit of C.A.L. employees. Such courses earned Cornell credits for the students. The Lab also worked out a much more sensible arrangement for its personnel to take courses in the University of Buffalo (which later became "SUNY-Buffalo", a unit of the State University of New York).

My main point is that the Lab continued as an independent institution, having impressive wind-tunnels and other expensive equipment not particularly well-suited to students' research, and an outstanding flight-test operation. Much of its work was classified and available only to U.S. citizens. In Ithaca, in the G.S.A.E., we developed a high-class department for graduate (and some undergraduate) education, with close relationships with the Departments of Mathematics, Physics, Chemistry, and the various branches of Engineering and the "engineering sciences". There was some cooperation

between the two institutions but not enough to keep some of the Cornell trustees happy; they were disappointed that our students weren't in Buffalo, and were shocked by the idea that some Lab employees were working for degrees at the University of Buffalo!

Fifteen or twenty years later, some things had changed. By now most of the aircraft (and related) companies had research labs or departments and were competing with C.A.L. for government contracts; this resulted in complaints to the Board of Trustees. ("Why should the University be competing in this unfair way --not-for-profit!--with American Industry?") This was also the era of student unrest and militant pacifism, and there were loud protests from the students about Cornell's owning and sponsoring the big laboratory in Buffalo, where research and development were being done on military matters--even secret military matters!

It seemed to the Trustees that both problems could be solved, and with a multi-million dollar profit to the University, by simply selling the Cornell Aero Lab. Most of the Lab's Board of Directors, including Ted Wright and me, thought that both of these "problems" and their proposed solution involved fallacies, but the Trustees overruled us: the Lab was sold to the Arvin Corporation (who paid for it mostly in stock); it became, and still is a part of, the Calspan Division of Arvin.

I left the employ of the Northrop Aircraft Company on 15 April 1946 and went off, by airline, to Ithaca to found the Grad School of Aero Engineering, leaving Mabel, David, and Susan behind. In the small city of Ithaca I knew only Dean Hollister. He had arranged for me to live in a nice room in the mansion of Miss Eugenia Van Cleef on Stewart Avenue just West (downhill) of the Cornell campus. One of my biggest responsibilities, obviously, was to find us a house, and that was no easy task; no houses had been built in Ithaca (or in the U.S.A., roughly speaking) for about five years. I think there were about three houses in or near the city that were of the size we needed and were for sale in our price range.

[Incidentally, my salary as Director of the G.S.A.E. would be substantially less than at Northrop, and although we made a nice big profit (on paper) on the sale of our Inglewood house, it became clear that Ithaca's prices were at least as inflated as Inglewood's. And in those days the paper profit on the sale of a resi-

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dence was taxable!]

I was quite overwhelmed by the difficulty of that task, and was feeling sorry for myself--although I must say that Dean Hollister and his right-hand man, Associate Dean Walter Conwell, gave me assistance and good advice. I found a house--a new house, under construction; this meant that it was built of such materials as were available in 1946, but it offered the advantage that we could specify some details such as color of paint, etc. Mabel agreed when I described it over the telephone. It wasn't a very well-built house, but it served us and we liked the neighborhood so well that when we sold it eight years later we bought a very nice one just half a block away.

The deans gave me a temporary office in Sibley Dome and assigned to me a temporary secretary. What's more important, they gave me a budget for the first year of the G.S.A.E. I interviewed some applicants and hired, as the School's secretary, a young lady, Alice Anthony ("Toni"), just out of Keuka College--it was her first job.

I don't know how I managed to be so smart--or lucky--but Toni turned out to be a perfect choice: bright, industrious, completely devoted to Cornell, the G.S.A.E., and all of its past and present students and faculty. She is now the Administrative Associate in the Sibley School of Mechanical and Aerospace Engineering--thinking about retiring some day, and all of us are wondering how Cornell can ever get along without her.

I recommended John Wild for appointment as Associate Professor; he and his little family arrived in Ithaca on October 1 in a snow-storm. Dr. H. S. Tsien, enroute from Pasadena to Cambridge, MA, stopped off in Ithaca, accompanied by his friend and protégé, Dr. Yung-huai Kuo. He recommended Kuo for a junior staff position in my new School, and I agreed; Kuo was appointed Assistant Professor. My first Cornell graduate student also arrived; he was another Chinese, Hao-sung Tan, who arrived in Ithaca early in the summer and began his studies for the Doctor's degree.

I also had on my desk a small pile of applications from young men who hoped to be graduate students in the G.S.A.E.; all of them were war veterans who had been in uniform for one to five years and could continue their studies under the program of veterans' bene-

fits. It was my job to study their applications, the transcripts of their earlier college records, and to decide whether they could probably handle the kind of graduate program we proposed to offer.

One such applicant was the son of a distinguished and beloved Cornell professor of Classics, Professor "Bull" Durham. I decided not to accept him. Whereupon I received a visit from his distinguished father, who glared at me and said, "I am informed that you have elected to reject the application of my son for admission to your School."

I wondered, for a moment, what kind of an institution I had cast my lot with: If there was to be pressure from the senior faculty in academic matters outside their fields, what other kinds of pressure would I encounter? I looked back into the glaring, pale-blue eyes of the Professor, took a deep breath, and said, "Yes, Professor Durham, I have studied your son's academic record very carefully and find no evidence that he can handle the demanding kind of program that we are going to require here."

He looked back at me with no show of emotion and said, "You're absolutely right, Professor. Thank you for your courtesy."

There were lots of stories about Professor Bull Durham. It is said that he was a dinner guest, one evening, of one of the fraternities and drank a bit too much. When the boys drove him toward his home at the end of the evening, he felt the need of some fresh air and asked them to let him off a block or two away from his house. But it turned out that he was a bit lost--couldn't find his house--so he went to the front door of a house that had a light on, rang the bell, and asked the lady who came to the door, "Excuse me, Madam. Can you direct me to the home of Professor Durham?"

"Why," she replied, "You are Professor Durham!"

"Madam!" he protested. "You are not answering my question!"

One of the important attractions of my new job, of course was the wealth of distinguished, impressive scholars and scientists who became my colleagues and friends. One of the greatest was Norman Goodier, Professor of Mechanics in the College of Engineering. Of British background, Norman was a first-class engineer-scientist and applied-mathematician. He became my closest colleague and my advisor on all things about Cornell and the College that I needed to

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know. It was a severe blow to me when, in the Spring of 1947, he told me he was leaving Cornell to become the successor to his famous father-in-law, Professor Stephen Timoshenko, at Stanford.

At Cornell it was popular to attribute our loss of Norman Goodier to the fact that Professor Timoshenko lived in Palo Alto and Mrs. Goodier, Marina, wanted to live near him. I happen to know that the truth was something different: Marina loved her aging father but was not at all enthusiastic about living in the same town; the move was occasioned by Norman's professional considerations, in spite of the complication of living near the father-in-law.

Norman left Cornell, he told me, because he didn't want to be a department chairman and was, nevertheless, serving as chairman of two departments, Mechanics and Machine Design. I urged him to go to Dean Hollister and tell him that. Norman replied, "Bill, Hollie knows exactly how I feel." I think he was just too proud to go, complaining, to Hollister, so we lost him.

I am happy to have had one year with him. His father-in-law, Timoshenko, was especially famous for his popular textbooks on theory of structures. [The best edition is the one by Timoshenko and Goodier.] It is said that, in European academic circles, talent descends not from father to son but from father-in-law to son-in-law, and here is an example. It is also not unknown that the son-in-law might surpass the father-in-law.

Timoshenko and von Kármán were old friends. Apparently they once rode together in an airliner, and von Kármán called Timoshenko's attention to the wrinkles in the skin of the upper surface of the airplane's wing: "There, Timo, are the wrinkles caused by the shear transfer, according to your book!" The famous structural theorist looked at the wrinkles, had never seen them before, and was terrified! Von Kármán had to reassure him, with paper, pencil, and diagrams, that everything was all right.

[To my non-technical readers: You see here one of the small rewards you get for reading this book: When you are an airline passenger and see, by looking out of the window, that there are diagonal wrinkles in the top-surface skin of the wing, you will know that this is just as intended by the designers. If you wish, you may tell your seatmates that these are, "of course, evidence of the shear transfer". That ought to impress them.]

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Another of my distinguished new faculty colleagues was the Nobel Laureate Peter Debye, Chairman of the Department of Chemistry. At Dr. von Kármán's suggestion I went to his office and introduced myself; the two famous professors had been friends (and, in a way, rivals in research) many years earlier. Debye was most friendly--wanted to know "How is old Kármán?" After I had replied and had explained what I intended to do at Cornell, he asked, "And what do you want from me?"

"Nothing, thanks," I answered. "Maybe some moral support."  
This pleased him: "Oh well, we can provide a lot of that."

He didn't know--nor did I--that the relationship between the G.S.A.E. and his Department of Chemistry would be close in years to come, particularly in the person of Professor Simon Bauer, a leader in physical chemistry, who worked (and still works) with our experts in shock-waves and the very rapid processes they produce.

Not all my introductions to my distinguished new colleagues were as pleasant. I introduced myself to the new Director of Mechanical Engineering, W. Julian King, recently hired out of General Electric's Research Laboratories in Schenectady. I knew that he had worked in the field of combustion, and I asked him if he hoped to continue in this important field at Cornell. He answered, "Oh, no. I think my principal field of research is Engineering Ethics."

I was puzzled. I asked him how you do research on that subject. He explained: The basic science underlying Engineering Ethics was, clearly, Philosophy; now that he was a college professor he would maintain close relations with the Philosophy Department.

What resulted was that he attended the Philosophy Department's weekly Colloquium. [For my non-academic readers I might explain that a "colloquium" is an informal gathering of faculty and scholars in a certain field of study, assembled to hear a colleague or, more likely, a visiting specialist, deliver an hour's lecture. It is the same thing that was called a "seminar" at Caltech--although I suspect that a "seminar" is really supposed to mean a series of lectures on a certain subject, usually with participation by the audience--a kind of advanced class.]

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Anyway, Julian King attended the Philosophy Colloquia, didn't understand anything the specialists were talking about, and took it upon himself to protest: Why were they discussing such obscure and arcane matters (as, I suppose, what Kant or Wittgenstein or somebody did or did not mean when he wrote so-and-so)? He demanded that they discuss, instead, such "practical" (!) questions as Determinism Versus Free Will. The situation came to the point where a petition was presented to the Chairman of the Philosophy Department, by his faculty, asking him to ask the Director of Mechanical Engineering not to attend the Philosophy Colloquia.

I guess Julian complied; it must have been a first for Cornell. But he was unbowed. We invited Professor George Carrier of Harvard, a distinguished alumnus of Cornell, to speak to the Mechanical Engineering Colloquium. George complied and we entertained him at dinner, after his talk, at the famous Dutch Kitchen of the Ithaca Hotel. Present were Dean Hollister and I, and Professors Mark Kac (you'll read more about him), Will Feller, Goodier, and Dwight Gunder, who had been George's teachers. We had drinks and we were having a wonderful time discussing George's student days--he had played clarinet and saxophone in dance bands--when Julian rapped on his glass for silence.

He said, "When we have the honor of a visit of such a distinguished scholar as Professor Carrier, it seems to me that we must take the opportunity to pick his brains--to ask for his answers to the questions that are worrying us!"

The jovial conversation ended. Kac asked, "What questions are those, Julian?"

"Well," said Julian, "if nobody else has a suggestion, I'll suggest: What about the question of Determinism Versus Free Will?"

There was a painful silence. Then Feller, a bombastic Yugoslavian--one of the great men of mathematics--controlled his disgust long enough to spit out, "I must hope that we will not insult our visitor with such nonsense!"

After dinner we adjourned to Gunder's apartment, where the conversation continued. King told us that he wanted to be sure that a Mechanical Engineering graduate from Cornell would be "a recognizable product, with pants pressed, shoes shined, and hair trimmed."

Never mind the rest of the details of that evening. I remember

that Feller, at one point, suggested that King's Mechanical graduates might be recognizable morons--but he pronounced it "maroons". Among a small clique of us, the word "moron" has, ever since, been pronounced "maroon".

Dean Hollister didn't have very much to say during that evening; he listened. And not long after that evening he convinced Director King that he (King) had made a mistake to think that he could move directly from industry into the position of the head of a university department; that he really ought to go to another university as a professor for a few years, before trying to be a department head. Apparently he was pretty firm; at any rate Julian became a professor at a California university after just one year at Cornell.

Julian and his wife came to our house in Ithaca one Sunday afternoon, paying us a formal "call". Soon after our somewhat stiff conversation began in our living room, something came up about the recent war and related political matters. Julian was inspired to tell us that he couldn't take seriously all that emotional stuff about Nazis and anti-Nazis. We, in America, have our Republicans and Democrats, and in Germany they had Nazis and anti-Nazis, he pointed out. Mabel pinned him down: "Are you saying that the difference between Nazis and anti-Nazis in Hitler's Germany was no different from the difference between Republicans and Democrats in this country?"

Julian said Yes, that was what he meant. Whereupon his hostess left the room, not to return. Mrs. King said to me, "Oh dear. I'm afraid Julian has hurt Mrs. Sears's feelings." Soon they departed.

I must not forget at least one great favor that King did for me, unintentionally, before he left Cornell: One of my very best graduate students, George Morgan, began his graduate career as a candidate in Mechanical Engineering, but changed to Aeronautical Engineering after his first interview with the Director of Mechanical Engineering. Director King called him into his office and asked him what he planned to do for his dissertation. George replied that as a brand-new grad student he hadn't any fixed ideas about research topics, that he was confident his studies at Cornell would lead him to interesting areas, and that he was open to suggestions.

King then told him that he had a wonderful topic to suggest: "Vending machines! Nobody has really done a good job on vending

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machines!" George is still not clear what it meant to "do a good job on" vending machines--nor am I--but he knew that didn't sound like the sort of thing he had come to Cornell to do; he marched over to my office and changed his major subject to Aeronautical Engineering.

Another of my outstanding students, Frank Moore, was also a gift from Mechanical Engineering, but this had nothing to do with Director King. Moore had come to Cornell for graduate study, after military service; he remembered especially his undergraduate courses under Professor Goodier and came to Ithaca to study with him. He was as disappointed as I was when Goodier decided to leave Cornell. He asked Goodier what he should do, and Norman replied, without hesitation, that he should come to me and change his major subject to Aeronautical Engineering.

Both Moore and Morgan were brilliant students, as was Hao-sung Tan, my very first Cornell student, whom I have mentioned above. The Graduate School of Aeronautical Engineering was off to a very strong start!

When Morgan finished his Ph.D. he was in need of a job and, with strong recommendations from all of his professors, was looking for one in a university group. He was interviewed at New York University by the famous applied-mathematician and fluid-mechanicist Richard Courant. Courant listened politely to George's recital of interests and accomplishments--he had written a first-class dissertation, explaining and elucidating some long-standing mysteries of flow in rotating bodies of fluid. Courant was impressed--and offered George an assistantship. George supposed that the professor had misunderstood his status, and said, "Perhaps you misunderstood me, Professor Courant. I have completed my Ph.D. at Cornell, and am looking for a full-time job."

"Oh, yes," replied Courant, "I understood. But a Ph.D. at another institution only qualifies you for an assistantship here."

George was, of course, shocked, but remained courteous; he stood, thanked Courant for the interview, and started out of the office. Courant was not a bit fazed, but he remembered that he had forgotten something: "I say, Morgan, you don't by any chance play a musical instrument, do you?"

It happens that George did play a musical instrument; he was an

accomplished pianist--so talented, in fact, that he had been invited to join the weekly "reading sessions" of the Music Department during his residence at Cornell. He imparted this information to Courant, who was not much interested; he said something like "Oh, only piano. I was just curious."

Now George Morgan was getting his dander up. "Do I detect, Professor Courant, that I might qualify for something more than an assistantship if I played some instrument other than the piano?"

The professor's answer was, "Eh? Oh no, of course not. But we do need a second-violin for our string quartet."

When George returned to Ithaca (with a nice junior-faculty offer in the distinguished department at Brown University), he asked me to explain Courant's strange behavior. I told him about the string quartet of the Applied Math Department of N.Y.U.: it was really Mrs. Courant's hobby, but obviously her husband also took it very seriously.

George Morgan has had a very successful career at Brown. After some years of fine research and teaching in fluid mechanics he came to the conclusion that what he really wanted to teach and study was Philosophy of Science. Brown didn't have a department to accommodate George's background and interests, so they created one and put him in it.

Another Director, when I arrived at Cornell, was Professor Fred H. ("Dusty") Rhodes of Chemical Engineering. [Everyone named Rhodes is nicknamed "Dusty", it seems, except my dear Mabel--she was even called "Jay", for her middle initial, but not "Dusty". Her brother Meredith, mentioned earlier, was always called "Muddy", in Montana and at Caltech--which I always thought was a great name!--but after his career in the shipbuilding industry during the war even he was (and is) called "Dusty".]

Dusty Rhodes of Chemical Engineering was not one of Dean Hollister's outstanding appointments. He was not in any sense a leader in the self-study of the chemical-engineering profession during the couple of decades through which chemical engineers have wondered what their profession really consisted of, nor was he ever seen as one of us who argued for a more scientific, more mathematical definition of engineering. He was, to be sure, a tyrant when it came to the scholastic performance of his School's students; they

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had better get A's! But he was a well-known anti-Semite. I believe that he called into his office the Chemical Engineering students whose names seemed, to him, Jewish, and explained to them that the chemical industry didn't want them or have any places for them. (Fortunately, many of these students paid no attention to their Director's advice.)

[I remember discussing Rhodes's prejudices and behavior with Vice President Ted Wright. He was shocked and angry, and said there was no place in Cornell University for such a person. Fortunately Director Rhodes aged and retired. His successor, Charles Winding, was a much more principled man.]

The Director of Civil Engineering at Cornell when I arrived was Dr. Nephi Christensen, one of the graduate students at Caltech in the late '30s, and not, as far as I could remember, one of the best.

I looked at myself in the mirror. I said to myself. "Hollister appointed those guys, and he also appointed you, old boy!" It was a reflection not likely to cause a swelled head.

Let us return to my recollections of arriving at Cornell and Ithaca, where I became, at least temporarily, a lonesome bachelor again, didn't know anyone in the town, and had to find a house. I missed my little family very much, was depressed by the task of buying a house, and I'm afraid my reports back to Mabel were gloomy.

Suddenly I became aware that I did have an old friend in Ithaca, Dr. Joe LaSalle, who had been a fellow graduate student at Caltech, one of the brilliant members of that remarkable class on mathematical methods taught by Fred Lindvall. Joe was also a remarkably good tennis player--better than Clark Millikan or I--probably in the category of Louis Pipes. He had spent the years of World War II in Ithaca, working on a government-funded research project--something in the area of Physics.

I can't remember who called my attention to the fact that Joe was there, but we did renew our friendship and undertook to get back to playing tennis together. I found that tennis was a bit more difficult in Ithaca than in Southern California. The courts were clay and needed a couple of days without precipitation to dry out; it seemed that no such days ever occurred in Ithaca in the Spring.

I recall that once, when the courts were wet, Joe and I decided to watch a Cornell baseball game at Hoy Field instead; this was in June. The game got started but was soon "called"--not because it rained but because it snowed.

[I also remember that when the snow began we huddled with a few other fans under some sort of cover. We couldn't avoid overhearing a conversation between two electrical-engineering students: They were facing final exams and were lamenting the difficulty of their subject. One said, "I sure don't understand what they mean by this here 'impedance', do you?"

His buddy was not too sure either. He replied, "Well, I know it's something like resistance, but it isn't the same."

Joe looked at me solemnly and suggested, "Those boys must be about ready to graduate."]

But the greatest thing about finding Joe LaSalle in Ithaca was that he and his wife invited me to a party at his house. At that one occasion I met a dozen or so members of the Cornell scene who became my best friends and colleagues for the next decades! Some I've already mentioned: the Goodiers, the Kacs, the Fellers, the Bauers. I met the brightest young members of the Physics and Mathematics departments and some Engineering colleagues whose convictions about engineering education and research were like my own. When I called Mabel the next day I was able to tell her that the sun was shining in Ithaca, that I had met a whole roomful of attractive, impressive, new friends, and that the move to Cornell was going to be a big success.

The evening at LaSalles' party even put me in touch with recorder players--my recorders had been pretty quiet during the Northrop years. Mark Kac owned a tenor recorder and played it, not very often and not very well. (He was a great mathematician but not a very good arithmetician. His notes were the right ones and they came in the right sequence, but I can't say much more for them.) But he knew serious recorder players, notably Mike Abrams, brilliant young professor of English, and Conrad Rawski, who was Dean (though the same age as Mike and I) of the Music School at Ithaca College. I got together with them; they played with a somewhat eccentric graduate student in Music at Cornell, Albert Hess. Albert was an accomplished recordist and even the editor of a

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collection, *800 Years of Music For Recorders* (Associated Music Publishers, Inc., N.Y.).

Meyer H. ("Mike") Abrams, a product of Harvard, was pretty new to Cornell; he went on to become a most eminent scholar and critic and holder of a distinguished chair at Cornell. He and I played recorders together regularly during the next 28 years; Ruth Abrams and Mabel always joined us, but only for intelligent conversation--and to enjoy the music, I hope.

Conrad Rawski and his wife Lisbeth were natives of Vienna: colorful, bright, and sometimes controversial. Conrad became a fine-arts librarian in Cleveland and later dean of the library school at Case Western Reserve University.

There were other recordists in our little informal quartet, as time passed. One was Ted Lowi, Professor of Government, who was really a very good oboe player; the recorder was a kind of second choice for him. Ted became famous in his professional field.

And after Ted Lowi, Jerry Meinwald, then Assistant Professor, a specialist in organic chemistry. Besides being a world-famous star of that field--and recipient, recently, of a big international prize for his great work in organic chemistry--Jerry is a flautist and a talented musician.

So the recorder scene at Cornell was wonderful. Rawski has great stacks of music and professional knowledge of how it should be played; Meinwald is a player of professional quality. Most of our playing was private, the audience being our wives, but there were also occasions when we performed in public, such as the surprise concert the Music Department organized in honor of its distinguished musicologist, Donald Grout, J. L. Given Professor of Music.

I have belittled Mark Kac's recorder playing, but I must give credit to Katherine Kac's singing. She had a lovely, pure, soprano voice; I got her to sing with us the voice line of Purcell's "The Bashful Thames". Katherine was always called "Kitty"--she was Katherine Mayberry, a native of Ithaca. Since the Polish surname Kaç is pronounced, in America, as "Katz" (being simply the Polish transliteration of that honorable Hebrew name), her married name seemed to be "kitty-cats", which was quite appropriate, because both Mark and she were great cat lovers.

Rawski, like most musicologists, was very much at home at a

piano or harpsichord keyboard, but had never studied any instrument very seriously. On one occasion the famous British composer, Ralph Vaughn Williams paid an extended visit to Cornell, and Conrad sat next to him at a dinner. The conversation was about music, naturally, and the distinguished guest asked, "And Professor Rawski, what is your instrument?" Conrad answered modestly that he played around with piano and harpsichord, not very well, and that he guessed that his instrument was the recorder. "Oh, I say," said Vaughn Williams, "the recorder isn't really a musical instrument, is it?"

"Well," replied Conrad, "that's what some people would say, but you have written some lovely music for it."

The composer was delighted. "So! You know my 'Suite for Pipes' do you?" That is a very nice quartet for recorders that we had worked on for some time but had never truly mastered--not well enough to play in public, anyway.

Albert G. Hess, Ph.D. candidate in Music--musicology, as I remember--was a young man from Germany. Throughout World War II he had served the Allied cause, in the battle zones, as a translator. Captured German soldiers were brought to him for interrogation. When the war was over he had accumulated a great deal of front-line time, and a great deal of veterans' benefits, accordingly. Not only was he able to embark upon a Ph.D. career at Cornell, but also he bought a small airplane, an Ercoupe--a single-engine two-seater with fixed landing gear and a simplified control system.

[I must interrupt myself here to say a bit more about the Ercoupe (pronounced "air coupe"). It was designed by a great aeronautical engineer and airplane designer, Fred Weick, who wanted to produce a practical, safe airplane for the "common man". The Ercoupe has no rudders: its turns are made by ailerons alone; it does not have enough elevator power to pull it into a stall, so it cannot be put into either a stall or a spin.

The eminent aerodynamicist Robert T. Jones, whom you met in Chapter 5, is the owner of an Ercoupe. During his many years at Langley Field, Bob became a personal friend and admirer of Fred Weick. When he decided to learn to fly and to buy an airplane, Fred told him which model of Ercoupe to look for. Bob found one, bought it, and his flying career was born--no, not born but reborn!--he had soloed many years earlier! Between his "first solo" and his

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recent, successful acquisition of a pilot's license is a span of about 40 years! He and I believe that he may have set a (rather unenviable) world record!]

Back to Albert Hess. He owned an Ercoupe and was licensed to fly it. This was somewhat startling, because Albert couldn't really drive a car well. When he parked in front of the Abrams's house or the Rawskis', he ended up with one wheel up on the curb, etc.

I worried about his flying. One day, at the old Ithaca Airport--the one down at the end of the lake--I stood with the airport manager, H. M. "Pete" Peters, as we watched Albert taxi out in the Ercoupe. I exclaimed, "Pete, his tires are almost flat! Shouldn't we stop him?"

Pete replied, "He's a jerk." I thought it was not true, but also that it was irrelevant. Anyway, Albert made the takeoff and, later, the landing, successfully.

[Those of my readers who have had much experience with the fraternity of the "F.B.O.s"--the fixed-base operators (as distinguished from airline or air-terminal operators)--the persons who rent, refuel, and repair airplanes and, usually, also teach student pilots, will not be too surprised by Mr. Peters's unpleasant behavior; the F.B.O.s are mostly a difficult, crabby, highly opinionated lot. They may love airplanes, but they don't love human beings. That is because--let's face it--the people who fly airplanes are typically unreliable, not very smart (not very well trained), and poor. They want to rent the F.B.O.'s airplanes, while he carries the insurance. He has a hard time making any money. Mr. Peters was not destined to appreciate Albert.]

On another flight, crossing Ontario between Detroit and Buffalo, Albert ran into bad weather and made an unplanned landing at a Canadian Air Force Base that happily appeared through the mist. We can only imagine the confusion caused by the unscheduled landing of the unscheduled American airplane and its heavily-accented musicologist-pilot. We must hope that he had with him his naturalization papers.

He finished his studies at Cornell and became a staff member of one of the branches of the University of Minnesota.

I wanted to require my graduate students in Aeronautical Engineering to take a good, demanding, graduate-level course in mathe-

matics, one that would go beyond the modest courses in calculus, differential equations, and analytic geometry that (I hoped) they had studied as undergrads. To my delight I learned that the Chairman of the Physics Department, Lloyd P. Smith, had recognized the same need for his graduate students and had prepared such a course and taught it himself. He had also written a splendid textbook for the course. I was advised to look up Professor Smith--something I had definitely planned to do anyway, since meeting him at that famous party at the LaSalles'. We were obviously kindred souls--Lloyd owned a four-place Stinson and used it for much of his travel. But he informed me that, having agreed to be Chairman, he was cutting down on his classroom teaching; his course, Mathematical Methods in Physics, Physics 405, would be taught by a new, young Assistant Professor of Physics, one Richard P. Feynman. He told me to "Go around to Dick Feynman's office and see if Physics 405 isn't exactly what you want." He felt sure that it was.

I found Feynman in his office, introduced myself as the Director of a new program in Aeronautical Engineering, told him that I had learned of the course and that I proposed to require it for my graduate students. I'll never forget his reaction: He had been working at his desk while I talked to him; he swung around to face me, put one of his feet on the rungs of a chair near his desk, pushed the chair violently, so that it slid across the floor to crash into the wall, and barked, "For Christ's sake why?"

I was, of course, astounded and confused; his angry response completely surprised me. I replied that I wanted just such a course for my students and that Professor Smith had recommended Physics 405. He groaned and moaned, but the result was that my students did take the course from him.

It wasn't an easy relationship, from the beginning. I was visited, in late October, by a small committee of my students. Feynman had remarked that

$$\cos x = [\exp(ix) + \exp(-ix)]/2$$

and had angrily scolded those in the class who claimed they had never seen such a thing.

Of course they had. In the meantime they had fought in World War II and forgotten such esoterica. I asked the students, "What text-

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book did you use in Advanced Calculus or some such course?" One of them was bound to remember one of the standard textbooks that I had on my shelves. Of course the exponential formula was right there. They agreed to return to Feynman's course and to be open-minded.

Feynman also undertook to travel to Buffalo, on the Lehigh Valley R.R., on Fridays, to present Physics 405 to the employees of the Cornell Aero Lab, scores of them. That undertaking was a calamity; I gather that there was no communication between the brilliant physicist and his students. Somebody else took over the class.

What I have written will suggest to you how difficult was the (slight) relationship between me and the future Nobel Laureate. We met a few times while we were both teaching at Cornell, and our relationship was never any better than what I've described.

At Cornell there is a famous endowed lecture series, the Messenger Lectures, named after the donor of the endowment. It is a series of three or four lectures intended to present Science to a general audience. Such scientific giants as Theodore von Kármán, the psychologist Menninger, the astronomer Struve,... have been Messenger Lecturers, and, some years after he had left Cornell, Richard Feynman was invited. I was unhappy with his lectures--not the content, which was a brilliant review of modern physics and astrophysics--but with the manners and mannerisms of the lecturer. I had heard several previous Messenger Lecturers and had observed how hard they worked: how seriously they tried to make their esoteric subjects understandable to their intelligent, educated, but non-specialist audiences. [I'll write more, in a subsequent Chapter, about Dr. von Kármán's lectures.]

So I was badly put off by Feynman's behavior. In the first place, he had decided to make these lectures into a book, and demanded that they be video-taped; in those days that required that he speak into a hand-held microphone with its cable attached and that he be brightly illuminated by floodlights. The traditional dignity and decorum of the Messenger Lectures was absent. Moreover, he gave the lectures in a broad New-York-City accent, intended, I suppose, to charm the Cornell student body: I can still see him, in his first lecture, kicking the microphone cable away from his feet, pointing to slides showing galaxies, and calling them "dese big babies and dose big babies".

The next day I had my lunch, as usual, in the Rathskeller of the

Statler (Faculty) Club, and met my friends the youngish (Feynman's generation) professors of Physics; I had seen some of them at the lecture. I said, "Well, how did you-all like last night's Messenger Lecture?"

There was a pause before anyone answered; then someone said, "I enjoyed it. Then I went home and turned on the T.V. and got the real Danny Kaye. He was even better."

Nevertheless, Richard Feynman was a true genius. Mark Kac said (paraphrasing Sir Isaac Newton), of all of us who do research, that if we are successful it is because we stand on the shoulders of giants, but that Feynman had the unique ability to stand on his own shoulders. He was a physicist, but he invented his own mathematics when it was needed! He was a model of what an applied-mathematician like me believes in!

Another famous man during my years on the Cornell faculty--one whom I found to be a delightful colleague--was the Professor of Government, Clinton Rossiter. He wrote a wonderful, insightful book on the American Presidency and sent a copy of it to President John F. Kennedy, modestly and respectfully inscribed. He received, from the White House, a thank-you letter from President Kennedy. The President disagreed only with Clint's opening quotation. For this book, he suggested, the opening quotation should be from King Henry IV Part I:

GLENDOWER: I can call spirits from the  
vasty deep.

HOTSPUR: Why, so can I, or so can any man;  
But will they come when you do call for them?

[Yes, Dear Reader, that was the President of the United States!]

My family--Mabel, David, and Susan--joined me in Ithaca in July. They came East on the train; we found that there were Pullman cars that came from Los Angeles to Chicago on the Santa Fe and were shunted across the city of Chicago to become part of the New York Central. I met them in Albany.

[That represented a great improvement in rail travel. Always,

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before that, passengers had to get off the Santa Fe, with all their baggage and children, and be transported by taxicab across town to the New York Central. Suddenly there were full-page advertisements featuring a large hog, with the message, "He can go from coast to coast without changing trains and train stations in Chicago, but you can't!" The campaign succeeded, and my little family was able to ride in the same car from Pasadena to Albany.] Mabel says that David (age 3+) ran up and down the length of the car, charmed everybody, and had a wonderful time.

I had returned to Inglewood to help with selling the house and preparing for the move--and to witness the first flight of the XB-35! I drove the Plymouth (1940 four-door sedan) across the country with the dachshund, Braunie. My good friend Louis Pipes accompanied us and shared the driving. Louis was marvelously good company and a good driver; he did all of his driving in the mornings. After lunch he would check with me: "Am I through driving for the day?" The answer being affirmative, Louis would have a little drink from a pocket flask and drop off to sleep: a nip and a nap.

His knowledge of dogs was just about zero. Braunie had to be allowed to run around a little during our lunch stop, and I once asked Louis to round him up and put him into the car. When I was ready to get going I found Louis politely holding the car door open and saying to the little "dockel", who was avidly exploring across the street and paying no attention, "OK little dog. We're ready to go. We're ready to go."

Of course Braunie had to be in the car while we ate our lunches. Louis liked to order his lunch and then explain to the waiter that we would also need a hamburger for Grandma, who was in the car. Recovering from the novelty of a couple of guys who left their grandmother in the car while they had lunch, the waiter would ask about onions, pickles, catsup, etc. for Grandma, and Louis would explain that she wanted her hamburger very rare and with no trimmings at all.

The new house on Valley Road was not ready for us when we arrived; Miss Eugenia Van Cleef allowed us to continue as her tenants for a few weeks more.

Our next-door neighbors on Valley Road were Associate Professor and Mrs. Heinrich Schneider and their son. Professor Schneider's

field was German Literature; he represented a fine old tradition of that subject: his writings--he presented us with some gems--were, either in the original or translated into English, incredibly ponderous and abstruse--composed of long and involved sentences and expressing obscure Teutonic concepts. I learned that that style of literary criticism was very much out-of-style in the 1940s; the last bastion in this country, apparently, was Harvard, where a Professor Karl Vietor held sway. Schneider was an admirer of Vietor--I claimed that he made a little bow toward Massachusetts when he spoke the Great Man's name. He was unhappy with his lot at Cornell, and his ambition was to be invited to Harvard.

Professor Vietor, it seems, had a nephew who was an aeronautical engineer! The young man, an Austrian, had been released from a displaced-persons camp somewhere in Germany and had walked from there to Vienna. Now he needed a job, and aeronautical-engineering jobs were nonexistent in Austria and Germany. Schneider could hardly believe his good fortune: He had a new neighbor on Valley Road who was an aeronautical engineer! He promptly told me all about Vietor's nephew and pled for my assistance. I didn't have a job for the nephew at Cornell, but agreed to write to a few other schools of engineering and call their attention to the case.

Professor Schneider was overjoyed! He was so thrilled and so grateful to me that I thought he must have misunderstood me; I warned him that I really had no idea whether my letters would produce any results; I didn't know if there were jobs available, and couldn't even recommend the young man--had never heard of him before.

This warning elicited from Professor Schneider a remarkable bit of philosophy: "I understand, Professor Sears, but the important thing is not whether the nephew gets a job. The important thing is that Professor Vietor knows that we respond to his request and indeed write letters on his nephew's behalf."

That seems a bit cynical to me, but, *mirabile dictu*, as a result of my letter the nephew was offered a teaching position at the University of Illinois; he is Professor Helmut Korst, who has completed a long and distinguished career at that institution. Mabel and I were in Champagne a few years ago when I delivered a named lecture; we met Professor and Mrs. Korst and were entertained at their home. I asked him if he knew of the modest part I had

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played in his career; he did, and he enjoyed hearing my version of it.

We might have hired Helmut Korst ourselves in 1946 or '47; we did have an opening for a junior faculty member, but sometime before Professor Schneider made his appeal to me we filled the slot by hiring, as Instructor, Dr. Francois Frenkiel, an aeronautical engineer with a Polish background, recently liberated by American troops from the infamous concentration camp at Buchenwald. He came to us an extremely shy young man whose demeanor seemed to tell of the dreadful experiences of his preceding several years. He left Cornell in 1948 to take a civil-service position at the Naval Ordnance Lab in Washington. None of us felt that we had gotten to know him; we worried about him a bit, wondering if he knew that a contingent of German "scientists", imported by the Navy Department from laboratories of the Nazi government were working at the N.O.L. and whether he would find the atmosphere there congenial. When I asked him, he smiled shyly and assured me that he would do his work and not worry about the Germans.

Dr. Frenkiel did, in fact, do his work well at N.O.L., later moved to a position at the Taylor Model Basin (another Navy establishment), achieved some distinction in the field of fluid mechanics, and was made Editor-in-Chief of the Journal *Physics of Fluids*. His overly shy personality gradually transformed into one of charm and good humor. I went to N.O.L. once to give a seminar talk, at his invitation; my subject was "The Aerodynamics of Not-So-Slender Bodies". [I'll have to interrupt the narrative to explain this title: There is a well-known theory of the aerodynamics of slender bodies, such as projectiles, and my co-author (my student Dr. Mac C. Adams) and I constructed a theory for such bodies in the second approximation--it would apply to projectiles that are slender but need not be as slender as required by the first-order approximation: bodies that are slender but "not all that slender", as the British would say. "Not-So-Slender" seemed to us to be the right description of what we were talking about.]

But when I started my talk and asked for the first slide, a photograph of a lovely, buxom (not so slender), bathing beauty in a bikini appeared on the screen. It had been sneaked into my slides by the chairman, Dr. Frenkiel.

He also had an intriguing story about a seminar talk that he himself had given at N.O.L. He spoke about turbulent flow, and in his audience was the great, old, irascible fluid-mechanics pioneer, Dr. Max M. Munk. Frenkiel started his talk from the most elementary basis, pointing out that the randomness of turbulent flow is not the ever-present randomness of the molecules themselves. [It seems a bit pompous to say that to an audience of fluid-mechanicists.] Whereupon Dr. Munk commented, to all present, "Who speaks of molecules? There are no molecules. Proceed."

Frenkiel never knew just what Munk's comment was supposed to mean, and I am equally puzzled. Perhaps he was objecting to the pomposity and meant "Molecules have nothing to do with this subject".

Writing about Francois Frenkiel reminds me of the international meeting on magnetohydrodynamics in Williamsburg, Virginia, at the beginning of 1960. By then he had achieved a position and reputation of some distinction in his field and was appointed co-chairman of the meeting. His opposite number from Europe was the internationally famous Soviet Academician, Professor Leonid I. Sedov.

Sedov had a good sense of humor. He asked Frenkiel if he understood the basic difference between Capitalism and Communism. The answer, he said, was: "Capitalism is the exploitation of man by man; Communism is exactly the opposite."

I had given Frenkiel the names of some Soviet workers in the field of M.H.D., who I thought ought to be invited to the meeting in Williamsburg. One was V. N. Zhigulev; Sedov had never heard of him: "Who's that?" Francois replied that Zhigulev was someone who, Sears had said, was doing some very good work in M.H.D. Sedov shrugged. "OK. I guess he must be someone from outside of Moscow. Invite him."

Ed Resler and I, as well as our colleague Henry Booker, Professor of Electrical Engineering, attended the Williamsburg meeting; we planned to make the trip by "Sears Airline"---that is, to travel in my Bonanza---but those were the days before I had the qualifications for instrument flying, and the weather, when we prepared for the trip, was not suitable. We drove Ed's car.

Our local C.I.A. sleuth, having learned that we were going to an international meeting attended by real, live Russians, provided us

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with a list of questions he hoped we could get the answers to in our conversations with the Soviet attendees. As we three drove through Pennsylvania we read and discussed the questions. In the first place none of us felt quite right about trying to be amateur intelligence agents and to extract information for the C.I.A. from participants at a scientific meeting. We found that the questions were about where certain research was being done and where certain persons were working; they seemed, to us, questions very unlikely to come up in friendly conversation and most likely to sound like just what they were: questions given to us by the C.I.A. One by one, as we drove along, we tore the paper into small bits and dropped them out of the car's windows. Henry Booker pointed out mock-seriously that Soviet spies might be driving behind us gathering up the bits of paper and piecing them together, but we decided that that would be a good thing--it would surely keep some Soviet spies busy.

One of the C.I.A.'s questions was so ludicrous that I insisted on keeping it and trying it on our Soviet guests: Apparently some "scientist" in the Soviet Union believed that the ventilating systems in public buildings should be equipped with devices that would ionize air--that it would improve people's health to breathe ionized air. [What that had to do with M.H.D. was surely a mystery--probably only the word "ionize".]

Anyway, on the occasion of one of the cocktail hours at the Williamsburg Inn, I brought up the question: "Hey, I hear that you Soviets have finally found a practical use for M.H.D.!"--that the new Palace of the Soviets would have M.H.D. ionizers in the ventilating system. They were amazed that I had heard the story, and convulsed with laughter. They explained to us that the proponent of the system was an old fellow, an Academician with political clout, a continual nuisance because of his irrational conviction that people should breathe ionized air.

Ed Resler and I shared a room in the Williamsburg Inn. Before dawn the morning after our arrival we were awakened by the sound of footsteps on the porch roof outside the window of our room. Filled with thoughts of Russians, spies, and secrets, Ed got out of his bed, crept to the window, and peeked out. Sure enough, there was a man out there on the roof! I got out of bed and joined Ed at the

window; we watched to see what the shadowy, suspicious fellow would do.

What he did was to attach a carefully folded American flag to the hotel's flagpole and raise it. We returned to our beds for another hour of sleep.

"My" man Zhigulev came to the meeting. He turned out to be a big, handsome, blond young man--the stereotype of a Russian farm boy--making his first trip to America and eager to make friends. After the day's sessions on M.H.D. we all gathered in the hotel's bar (called a "club", membership in which was conferred upon every registered guest in the hotel). Zhigulev came down from his room with arms laden with bottles of Russian vodka, which we all sampled and which he imbibed generously. Besides us M.H.D. specialists there were some local residents of Williamsburg who were "members". One of these was an attractive housewife who asked who that blond young man was, who was having such a fine time. When she was told that he was a Soviet scientist, she was thrilled: "A real, honest-to-God Communist?"; and when we confirmed that, she decided that she just had to take him to her house, where she had friends coming in for drinks and dinner.

Zhigulev was delighted to have made such a hit with the American lady; he thought her intention to take him home with her represented a conquest on his part; he put his arm around her and they were on their way out when Academician Sedov noticed what was going on. He told Arky Kantrowitz, "I don't want Zhigulev go with American lady." Arky sized up the situation and informed his colleague Harry Petschek of Sedov's wishes; soon Arky was on one side of the young man, Harry was on the other, and the lady was gently pushed aside. She was angry, but Zhigulev was quietly reminded by Sedov that tomorrow was a work-day. The young man's urge to cement American-Soviet friendship was set aside.

I have not forgotten to tell you about my personal-flying career; I seem to have begun the story when I reported on the Civilian Pilot Training Program at Caltech and my learning to fly at Monrovia Airport, back in Chapter 4, and to have dropped the subject at that point. The fact is that, after that beginning, my own flying became, more and more as the years went on, such an impor-

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tant part of my life that I've decided to devote a whole Chapter to it. It will be Chapter 9.

But I didn't really do a lot of flying, as you will learn there, until some time after I moved to Ithaca and Cornell. For example, my trips to Buffalo to teach courses at the Cornell Aero Lab in 1946 and 1947 were made on the Lehigh Valley Railroad. I became, of necessity, somewhat of an expert in the subject of that road--so much so that I have some stories to tell about its idiosyncrasies.

I used to ride the L.V. to Buffalo in the morning, work as a consultant at the Lab during the day, teach--for three hours--after supper, and return to Ithaca on the late train. As I became expert in this procedure I learned that the late train could be boarded in Depew, a suburb east of Buffalo, where the L.V. connected up some coaches from Toronto, for the trip to New York. There was a little station at Depew, but it was not open in the middle of the night when these cars were connected. One stood near the station and watched a locomotive come in, drop the cars, and depart. At that point one could board the cars; passenger cars have doorbells, believe it or not, and the conductor will let you in. The rest of the train came out from Buffalo a few minutes later--so soon, in fact, that if one had driven into the city from the Lab after finishing one's class one would have missed the train at the Buffalo station.

Some of the cars from Toronto, including a Pullman (sleeping car), were destined for Philadelphia, rather than New York; they were uncoupled from the rest of the train in the rail yard at Bethlehem, sat there for a few hours, and were pulled to Philadelphia early in the morning. I learned that this offered a good way to get from Ithaca to Philadelphia, where I consulted with Westinghouse's gas-turbine department, or to Washington. You could get a pretty good night's sleep while that car sat in the yard at Bethlehem. There was an analogous return journey, to get back to Ithaca.

Inevitably, I once missed the connection in Philadelphia on the return journey. The ticket agent there explained that there was another train from Philly to Bethlehem that would get me there before the sleeper was picked up. I arrived in Bethlehem in the middle of the night. My sleeper was pointed out to me: "Over there, the dark one, See it?" I saw it, and with my baggage in hand I hiked out into the unlit rail yard, across a lot of tracks, climbed

the stairs of the dark Pullman car--the first step is a high one!--and rang the doorbell. A sleepy porter finally opened the door, took one look at me and my luggage, smiled, and said "You must be Lower Six. I been wondering where you was!"

You can see that I was getting to be quite an expert on the Lehigh Valley and its idiosyncrasies. But "pride goeth before destruction", the Bible says. One night, returning to Ithaca from the West by airline, I missed the last flight out of Buffalo to Ithaca. But I saw that I had time to catch the late L.V. train at Depew. I got a taxicab to take me to the Depew station, which was closed and dark, as I knew it would be. The driver was unhappy about leaving me there in the dark--no sight nor sound of life. I assured him that I knew what I was doing; he departed and I waited patiently for the Toronto cars to arrive. They finally did--at least a locomotive arrived, but it was not pulling any cars, and when its headlight was extinguished the engineer and fireman jumped down from the cab, bade each other goodnight, and went off into the dark with their lunch pails. Something was wrong!

What was wrong was I. I had forgotten to correct my watch for Daylight Time; the last L.V. train had come and gone more than an hour earlier. I finally realized it, picked up my bags, and wended my lonesome way through the dark, under a viaduct, and finally found an all-night bar in the suburb of Depew. From there to a hotel by taxicab, and home to Ithaca the next morning.

Still, I think there was more adventure in trying to learn and exploit the oddities of the railroad system than there is, today, in trying to cope with the airlines!

Here's another oddity: I found that (provided I came to the Depew station at the right time and not an hour late!) I could purchase my ticket--Depew to Ithaca--from the conductor after boarding. The ticket he sold me included a mysterious portion reading

PAID	---
REFUSED TO PAY	---
CLERGY	---

Now, you can understand that these weird words would trigger my curiosity, especially since there's not all that much to do on a

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two-hour ride to Ithaca so late at night. I asked the conductor to explain.

"Well," he began, "to begin with, there is a 25-cent penalty for not buying your ticket at the station."

"But the station at Depew is closed at night," said I.

"That's why I didn't charge you the penalty."

"I see, but what is 'REFUSED TO PAY'?"

"Why, we can't force you to pay, in any case, since the fare from Depew to Ithaca is set by the Interstate Commerce Commission, and we can't increase it by 25 cents."

I was getting confused. We seemed to be talking about a voluntary penalty!

The conductor continued the strange tale: "If I punch 'PAID', you can always turn in this ticket at any Lehigh Valley office and receive 25 cents."

"Well," I said, "what about 'CLERGY'?"

"That's simple," he explained. "Members of the clergy are exempt from the whole thing."

Which reminds me of another story about a voluntary penalty:

Nicholas Rott is a brilliant professor of fluid mechanics--and all other varieties of mechanics--who was one of my colleagues at Cornell. A Hungarian, he had his undergraduate and graduate education at the famous "E.T.H.", the Swiss Federal Institute of Technology in Zürich. I can hardly overestimate the importance of Nicholas in our School at Cornell, nor his impact upon my own career in engineering research.

His name (Niklaus, in Switzerland) is, of course, shortened to "Nick" in America, but since it was Miklós in Hungarian his good friends call him "Miki". Hungarian or not, Miki had been in Zürich so long that in some ways he seemed very Swiss. There grew up in those days the practice of the technical and scientific journals' charging authors a "publication fee" for the printing of their papers. [Yes, dear reader who is not familiar with the customs of the academic world, you might think that an author should get paid for his history-making paper. After all, his paper has had to pass the tests of "peer review" and all that. No, on the contrary, he (or, to be more precise, his institution) has to pay for the privilege of having it published.] To avoid hardship to a poverty-

stricken author who doesn't have an institution behind him, the journals permit him to turn the invoice over and sign the reverse side, where it says "Unable To Pay" or "No Funds Available"; then his paper gets published anyway.

I discovered, to my embarrassment, that Miki was regularly signing this statement when a paper of his was accepted for publication. I didn't like the idea that the world of technical journals was being told that Cornell University was destitute. When I scolded him he was incredulous. "But Bill," he protested, it's a voluntary charge! Surely nobody in his right mind pays a voluntary charge!"

Well, nobody who's Swiss and in his right mind, I guess.

Miki Rott is an incredibly kind and gentle man. We learned that when he was truly upset--even angry--his reaction was to sigh deeply and to say, quietly but firmly, "Excuse me!" The chairman of a technical meeting once told his audience, after Miki had delivered a lecture, that "Professor Rott's paper was interesting, but unfortunately violated the Second Law of Thermodynamics. We will now proceed to the next paper."

"Excuse me!" Miki interrupted. "We will certainly not proceed until that remark has been discussed!" It was discussed, and the chairman had to admit his error.

We learned that when Miki said "Excuse me" to us, or to an unfortunate Colloquium speaker, it meant, in plain American, "You idiot! What you've just said is completely wrong!"

[Sydney Goldstein's version of this message was also deceptively polite--on its surface--and one had to learn, by experience, what it meant: It began with "But, my dear boy, ..." The message was the same.]

Several of us of the G.S.A.E. Faculty indulged in vigorous exercise, usually at lunchtime. At one period our favorite exercise was swimming, and, as we started off toward Teagle Hall and its pools one day, we met Miki and I idly suggested that he join us.

He was visibly shocked by the suggestion; "Go swimming? If I am on a ship," he declared emphatically, "and the ship sinks, then I will go swimming!"

This gave me the impression that he was generally not interested in physical exercise, but I was not quite correct: When he was

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younger, he told me, he had been devoted to fencing. My colleagues and I had some trouble imagining our gentle friend in such an aggressive activity. But, upon reflection, I remember the courteous bows and "Voilà! Touché!" and all that ...

Nicholas Rott speaks and writes perfect English and German and was unhappy when his children, growing up in Ithaca, began to pick up the inevitable sloppiness and slang of their American peers. He came to me for advice; I asked for examples. He gave me three:

"He don't"

"Sump'n" (for "something")

"Ain't"

Knowing his interest in languages and his pride in his mastery of them, I was not surprised by this conversation. I undertook to relieve his mind: I didn't have much to say for "He don't", but the others, I told him, were just part of the youngsters' careless diction; after all, all kids speak two languages, one for their teachers and the other for their peers. You can consider yourself flattered if your own kids use their "peers" language when they speak to you.

The conversation was being heard by our good friend and temporary colleague, Professor George Batchelor, another of the great fluid-mechanicists, of Cambridge University. George couldn't accept what I'd just said. "I couldn't agree with you less!" he exclaimed. "If children don't learn habits of precise speech when they are children, when shall they learn them?"

[His delightful wife, Wilma Batchelor, M.B.E., has been known to refer to George as "my stuffy husband".]

Miki Rott's interest in languages extended into the realms of slang and profanity, despite his refined, gentle demeanor. I recall that he came to me one day asking for my assistance in what he termed a subject in which I was an expert. I was a bit flattered and wondered which one of several topics he was referring to, only to be a little deflated when he said that it was "vulgarity".

Once our mutual friend, a great aeronautical engineer, Professor

Antonio Ferri, accosted me with the declaration "You got a crazy guy in your department!" Miki had been invited to give a colloquium talk at Brooklyn Polytechnic Institute. His subject was an ingenious study, carried out by him and a student, of a problem in three-dimensional wing theory. Tony Ferri, it seems, came late to the meeting and stood in the back of the auditorium as Miki finished his talk. The chairman, Professor Nick Hoff, invited comments and questions from the audience. There were none until Tony spoke up from the back of the room: "The trouble with this calculation is that there's no way the Kutta-Joukowski condition can be satisfied."

Now that is a pretty serious accusation to make to Miki, since the "K-J condition" is the fundamental condition for the flow at the trailing edge--the condition that defines what is and is not a wing! So his response was vigorous: "What? You think we don't satisfy the K-J condition? I'll bet you a case of whiskey we do!" That response was what made Tony say that Miki was crazy. The audience loved it. I was amused by the story but said to Tony, "I don't think they violated the trailing-edge condition."

Tony said, "No, I know that. I only said what I did to make some discussion. Nobody else was making any comments."

In the Fall of 1946 I received an inquiry from Dr. Arthur R. Kantrowitz, an employee of N.A.C.A. at the Langley Field laboratory, concerning the possibility of a position in our faculty. Arthur had been a student in experimental physics at Columbia University under such giants as Edward Teller and I. I. Rabi. From Columbia he had gone directly to Langley Field and had made gasdynamics his specialty. I knew his name from his published reports and papers in that field--I also knew that H.-W. Liepmann evaluated his work highly, as did R. T. Jones and many others; he was obviously a prospect to be taken very seriously, and I invited him to visit Cornell.

We got along very well from the start. Arthur said he wanted to broaden the scope of his research. In particular he was fascinated by the discovery that vorticity, a kinematic property of a fluid-flow field--of the most difficult and important flow-fields, in fact--must actually be a quantum quantity: It must actually occur in discrete quanta, rather than continuously. And since we are

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often concerned with flows of small vorticity, he was excited about the possibility that its quantum nature might have significance in applications.

That didn't seem likely to me, since our practical definition of "small" must be very large relative to the quanta--just as our practical meaning of "rapid" would be very slow relative to atomic and molecular processes, etc. But I was attracted to a gasdynamicist who wanted to study such fundamental (but far-out) things. He asked me whether, if he came to teach at the G.S.A.E., he could study this kind of phenomenon, and I told him Yes, of course, that his background and experience qualified him to say what is important and worthy of study in gasdynamics.

He decided to join our group. Months later, having heard no more about quantum vorticity, I asked him whether he had changed his mind about that line of research; he confessed that he hadn't been quite serious about it. He had been testing me. He did not propose to give up his career with N.A.C.A. and become a college professor unless he could pursue research even in crazy, far-out areas.

The appointment of Arthur Kantrowitz to our Faculty was one of our smartest moves. His background in experimental physics was very different from mine, John Wild's, Miki Rott's; we were all products of the Prandtl/Kármán tradition. Many times during the ten years he worked with us it became vividly apparent that our approaches to aeronautical-engineering problems were different. Surely this offered tremendous advantages to our students.

And Arthur hugely enjoyed being a professor and being at Cornell--being a colleague of such giants as Debye and Hans Bethe, whose work he had long known and appreciated.

Arthur Kantrowitz is not a simple case. I have a plethora of anecdotes about him, not all of which I can relate here. He richly deserves a Biography of Arthur Kantrowitz. First, let us simplify his name: A.R.K. became "Arky"--I believe it was one of Bob Jones's children who coined it.

Arky and Bob (Robert T.) Jones not only knew one another during World War II, when they both worked for N.A.C.A. at Langley Field; they lived together. Bob and his children lived in a house where the housekeeper was a Miss (or Mrs.) Mahler, and young Dr. Kantrowitz rented a room. [Yes, Ms. Mahler was related to the famous

composer/conductor--a daughter-in-law, I think.]

I think Arky and Bob must have been, like me, Mabel, and our friends at Caltech and Northrop, "premature anti-Nazis"; they certainly shared some good liberal views. One night Bob failed to come home--Arky was pretty sure that something had happened to him, and, sure enough, upon checking with the police next morning he learned that Bob had spent the night in jail. Bob had watched two policemen rough-up two black soldiers, in uniform, who had apparently not stepped off the sidewalk--or not stepped quickly enough--to let the cops pass. So Bob had intervened, protested on behalf of the soldiers, and had been arrested and made to spend the night in jail.

He had also been mildly harassed on another occasion; his neighbors had reported that the Joneses and their friends were learning Russian (!)--they could hear the tapes being played! The fact was that these young folk had discovered the delights of English-language tapes played backward on a tape-player. I suspect that the real problem was that N.A.C.A.'s Langley Field lab had brought a sizable number of Northerners (damn Yankees!) to the Tidewater Virginia area, and some of the natives were unhappy about it.

Arky was the kind of teacher who convinced his students that everything was, after all the derivations and explanations, really pretty obvious. (The great Peter Debye had the same reputation.) Arky is not an applied-mathematician; he believes that mathematics is only a shorthand in which we write what we know--i.e. what the experimenters have discovered. [Remember what Robert Millikan said to Sydney Goldstein?] To me that is a terrible understatement; I believe, with most scientists, that there is a wonderful, mysterious affinity between mathematics and the physical world; such geniuses as Albert Einstein and Richard Feynman have exploited this affinity--have learned from the very form of the mathematical statements great, hidden truths about the world. [Another brilliant physicist, Eugene Wigner, has written a fascinating paper about this phenomenon--I think he called it the "unreasonable" relationship between mathematics and physics.] It is surely not just a shorthand!

One of Arky's brightest students, Shao-chi Lin, now Professor Emeritus at the University of California, San Diego, taught Arky's course in gasdynamics when Arky was on sabbatical leave. He told me

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that he found that many things that Arky had made "perfectly clear" when he (Arky) taught the course were not clear at all, especially at about 2:00 a.m. when he (Lin) was preparing to teach them at 8:00 a.m.

That he was a very talented teacher is attested to by the record of his students, a remarkable number of whom have become leaders in our field of engineering and science. He has been prone to--and rather well-known for--his bombast, exaggeration, oversimplification; among his students and admirers there is a useful adjective, "Kantrowitzian", that refers to these. During his years at Cornell he was absolutely convinced, and told us frequently, that the only good research was that done in the universities. But in 1956, caught up in the enthusiasm of his "consulting" for a division of the Avco Corporation, he told us with equal conviction that the only good research was that being done in industry. [I have put the quotation marks around "consulting" because his position and authority at Avco-Everett were far beyond those of any consultant.]

He definitely believed what he said, and he soon resigned from Cornell to assume the full-time position of Director of the Avco lab at Everett, Massachusetts. Dr. von Kármán was making one of his extended visits to Cornell (about which you will read more in a later Chapter). He asked, "Now, Arthur, why will you leave the university?"

To which Arky replied, very seriously, "To keep the United States from becoming a second-class power." That is Kantrowitzian!

Von Kármán looked at me: "You know, Bill, in my career I have been advisor to generals, admirals, and heads of industries and governments. But it never occurred to me that I could alter the course of history."

[Recently Arky has told me, "You were right, of course, Bill." I was naturally happy to know that I had been "right" about something, but was not sure what subject Arky was alluding to; I asked him. He replied, "You said, back in 1956, that I shouldn't leave Cornell. You were right." That is also Kantrowitzian. In the intervening thirty-or-so years he had achieved a spectacular career and become a world-renowned figure in industry and research.]

Arky's research, which was so imaginative, original, seminal, and therefore valuable, was based on the use of the shock-tube--a

rather simple device to produce a shock wave in a laboratory--to study very rapid physical/chemical processes in air and other gases.

In one expository paper he described what happens in a gas like air when a shock wave passes: "At first nothing happens. But after a few milliseconds ...."

Some reader must have sent this in to *The New Yorker*, whose editors reproduced it with the comment "Do you expect us to wait around?"

In the laboratory, as I have said, a shock wave is produced in a shock tube, which is, at least in principle, nothing but a metal tube divided into two sections by a diaphragm. The part of the tube on one side of the diaphragm is pressurized; the other part is pumped out to a low pressure; the diaphragm is broken, either by the pressure difference or, mechanically, by being punctured. The high-pressure gas is thereby abruptly released into the low-pressure side; a shock wave is formed and propagates through the tube. Understandably, such an experiment involves a lot of pressurization, pumping to low pressure, and preparation of elaborate instrumentation to measure the details of the phenomena--sometimes hours of work--followed by a loud "bang" when the diaphragm is broken, and a lot of instrument readings that are made, usually photographically, in "a few milliseconds", as Arky wrote, above.

Arthur's brother, Dr. Adrian Kantrowitz, is a well-known worker in cardiac medicine--a pioneer in development of artificial heart-pumps, etc. He came to Ithaca occasionally to construct such devices in our laboratories. On one such occasion he was sharing the lab with Arky's student Abe Hertzberg, who was making experiments ("runs") in a shock tube; Abe politely informed Adrian that he was going to "make a run", for which he would have to turn out the lights for a few minutes. Adrian, being a polite guest, said Yes, of course, thanks, no problem, and interrupted what he was doing while Abe turned off the lights.

What we know about what happened next was that Adrian, visibly shaken, arrived in Arky's office to say, "You've got a crazy man down there! He tells me he's going to 'make a run', if I wouldn't mind, then turns off all the lights and fires a 75-millimeter cannon under my nose!"

[Abe went from the G.S.A.E. to the Cornell Aero Lab, and thence

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to the University of Washington, where he is a professor and department head. Much of his distinguished career has involved shock tubes and wind tunnels driven by shock tubes, all of which have made noises like 75-millimeter cannons. I think it was Frank Moore who suggested that such loud, explosive noises should be measured in a new unit, the hertzberg; most explosive noises, like gunfire and automobile back-firing would be measured in micro-hertzbergs.]

Another Kantrowitz student who has had a particularly brilliant career and has played an important role in the history of the G.S.A.E. is Edwin L. Resler, Jr. He was one of our earliest students, coming to us, after a brief Navy tour of duty, from the University of Notre Dame, where he was a student of Joe LaSalle. Ed wrote his Ph.D. dissertation, under Arky's direction, on his research, mostly experimental, on these rapid phenomena in gasdynamics. He designed the complicated equipment and instrumentation for this study; it involved a lot of intricate glasswork, which the skilled glass-blowers in the glass shop at Cornell were, happily, able to construct. When the glasswork was finally completed, Ed set it up in our lab and undertook some late-night working hours, eager to get his experiments going.

But there are hazards involved in late-night work and, late one night, Ed knocked the whole ensemble off the table. With all the irreplaceable glasswork lying in shards on the floor, Ed came up the stairs to Arky's office, where he too was working late, and announced that he would be terminating his Ph.D. career. Apparently when Arky learned what had happened in the lab he glanced at his watch, shed his jacket, and remarked that they had better get back down there and to work. Ed pointed out that his life's schedule--he had a family to worry about--didn't allow for starting over on the glasswork, but Arky was not impressed, predicted that "we" would have its parts ready to go back to the glass shop by morning and that the glass-blowers would know exactly how to do the job much more quickly the second time. He started off toward the lab, and there was nothing for Ed to do but to go along. They worked all night, and Arky's predictions proved correct. I am sure that Ed will never forget what Arky did for him.

I have mentioned, above, how Cornell acquired the services of

Dr. Yung-huai Kuo in the summer of 1946. Kuo was a successful teacher of courses in fluid mechanics and theoretical aerodynamics--although his style of teaching was somewhat unconventional and required a little getting-used-to on the part of our students; they had to learn that the importance of anything he was teaching them was inversely proportional to the vigor with which he presented it. When, after an hour or so of mathematical derivation, he arrived at a really important equation, his voice would drop to pianissimo. I think this was done in deference and respect to both the equation and the students; he was sure that they were aware of the importance of the final result and had been looking forward eagerly to its emergence. After all, one does not shout and wave flags when one presents the Lord's Prayer.

During the cold winter of 1946/47 our offices were, temporarily, in a little old building ("Sibley Annex") that once had been a paint shop or something like that. It was heated by the university's steam system, but didn't have its own control valves; to heat it required that a whole row of the older buildings on the quadrangle be heated. In the interests of economy those buildings were not heated all night nor on weekends, and nobody seemed to be discomfited except Kuo. I got a portable electric heater for him, but found that he was still freezing. I asked Buildings & Grounds to correct the situation, but was told that heating the whole row just for the comfort of one person was unreasonable: Why couldn't he work somewhere else?

I reported the problem to Dean Hollister, who asked me if what Kuo was working on during those cold nights and days was really important to our program. When I answered in the affirmative he simply called the Director of Buildings & Grounds to say that it was necessary that Sibley Annex be heated--by whatever method was most acceptable to the Director. That was the end of the problem, and I was happy with Cornell's conduct.

Kuo's work progressed and he was promoted to Associate Professor. He became so Americanized as to subscribe to *The New Yorker*, but he had great difficulty understanding its cartoons. Every week he paid a visit to my office with the latest issue, place markers marking at least a half-dozen cartoons, to which he would open the magazine and await my explanations. Example: "Dr. Sears, I don't understand why these strange animals are carrying away all these

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baskets."

"Well, Dr. Kuo, those are trash baskets on the streets of downtown New York. It seems--I have read it in the newspapers--that they are disappearing mysteriously. This cartoonist is suggesting that interplanetary visitors from Mars or someplace are taking the baskets away in their space-ships. Heh, heh."

That didn't seem very funny to Dr. Kuo--nor to me either after I explained it. Kuo did not laugh, but proceeded to the next place marker.

There was an extremely bright and attractive young lady, Miss Pei Li, teaching the Chinese language in the Oriental Studies Department; it seems that her accent was perfect Mandarin, and she spoke near-perfect English as well. She and Kuo decided to marry and came to Mabel and me for advice about strange American customs and laws. We explained about the Wasserman test, marriage license, and wedding ceremony--to be accomplished in that order--and that the Mayor of Ithaca was empowered to perform marriages. All this was understood and agreeable; we made an appointment with His Honor the Mayor--but there were a couple more items that we hadn't mentioned and which Kuo and Pei took care of for themselves: one was housing and the other was a honeymoon. They found a charming little house, moved into it, and went off on a honeymoon--all before the wedding!

In the 1950s our social customs were a little more formal than today, but Mabel and I were quite sure that nobody would be upset by the Kuos' slightly unconventional schedule. We sent a bouquet to the house, and a corsage for the wedding. We were their witnesses. It was the Mayor's first wedding ceremony. He absolutely murdered the pronunciation of the Chinese names, whereupon Kuo, to avoid repeating them back, tried to get away with expressions of assent--"Yes, yes"--but His Honor would permit no shortcuts. Then both bride and groom were embarrassed by such intimate words as "To love and to cherish ..." and didn't want to repeat them; again, no shortcuts were permitted. Meanwhile our children, ages 4 and 7,--although we tried to convince them they were flower-girl and ring bearer!--were running noisily around the Mayor's office, finding the whole affair highly amusing.

After Hsue-shen Tsien went back to China in 1955 and founded the Institute of Mechanics in Beijing, he invited Kuo to join him

there. I don't think the decision was a very difficult one for the Kuos; they had maintained ties with family and friends there and had high hopes for the future of their native land. Kuo became one of Tsien's right-hand men and Pei became a highly respected and revered professor of languages at a university in Beijing. As Tsien became more and more involved with bigger responsibilities in the Chinese government, Kuo became the de facto director of the Institute. He died in a terrible airline accident; their daughter Jane, who was born in Ithaca in 1951, now lives in the U.S.

During the first ten years or so after their return to China political relations between the U.S. and the People's Republic were strained. We were not only surprised but incredulous when Professor Arthur Erdélyi, a famous mathematician and applied-mathematician at Caltech, returned to Pasadena from a trip to Hawaii to tell his colleagues that he had seen Kuo and spoken to him in the Honolulu Airport. Erdélyi had known Kuo (and Pei) rather well when he (Kuo) had spent a sabbatic leave at Caltech in the '50s.

Erdélyi himself was surprised to see Kuo in the U.S., and, upon his return to Pasadena, told his colleague Duncan Rannie about the meeting in detail. Kuo had said that he was going to spend the coming academic year at a university in the eastern U.S.--Arthur couldn't remember the name--and would drop in at Caltech in the Spring, on his way back home to China, to renew his friendship with all his old friends there.

He brought his wife Pei into the conversation--did Professor Erdélyi remember her? Of course he did. But where is your daughter? The answer was that "the children" would be cared for by their grandparents in China. Arthur had remembered only the one child, the daughter, but was not surprised that there were more by now.

Rannie was astounded by the news, having thought that travel back and forth between China and the U.S. had been cut off by both governments. He asked me if I were not surprised, and of course I was. One of my friends at Cornell told me that perhaps I should be discreet about spreading the story, since Kuo must be operating as an agent of the U.S.! That didn't seem likely to me, and I soon had a visit from our local F.B.I. agent, Pete, who said that his agency had heard of Kuo's return and wondered what I could tell him about it. I told him that I found the whole story fantastic, that I

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hadn't heard anything from Kuo--that I wouldn't believe any part of the tale if it weren't that Erdélyi was such a reliable witness. I told him that he would have to get the story first-hand from Arthur.

Soon I had a similar visit from our local C.I.A. sleuth, Perry, asking the same questions. I didn't want to repeat the whole (third-hand) story, so I told him that I had told Pete all I knew. But he insisted that I tell him everything too. He explained that the F.B.I. was a "taut ship", run by a tough boss, J. Edgar Hoover, that Hoover communicated with John Foster Dulles at the top but that Pete and Perry were not supposed to communicate down here at the bottom, in Ithaca, N.Y. So I told him what I'd told Pete.

So, you see, there was communication at the top, in Washington, and at the bottom, through me, in Ithaca. Both sleuths had assured me that they would keep me posted, and of course I promised to inform them both if (when) I heard from Kuo. But I didn't hear from him, and nobody knew at which university he was teaching. I had told my student and friend, H. S. Tan, the story, and one night, rather late, he called me to say that he thought he could "explain the Kuo story." I told him he'd better come over to my house.

He had just returned from Washington, where he had had a reunion with his old friend and former teacher (in China), C.B.Ling. Ling was now a professor in Taiwan, spending a sabbatical year at Virginia Polytechnic Institute. They had dinner together at the Ho-Ho Restaurant, and Tan told Ling the Kuo story. Ling snorted with derision; he did not believe that either the American or the Chinese government would permit Kuo to come back to the U.S. Tan said, "But Erdélyi saw him in Honolulu!"

"Nonsense," replied Ling. "I saw Arthur Erdélyi in the airport at Honolulu a few months ago, talked to him, introduced him to my wife Pei, told him I was going to V.P.I., would visit Caltech on the way home in the Spring. He asked me about the children, ..."

It was, of course, a classic case of mistaken identity. Ling and Kuo were both Chinese, both tall and gray-haired, both had wives named "Pei", and both had spent sabbatical years at Caltech, in GALCIT. Erdélyi had known both. When Rannie heard the denouement he had the unwelcome assignment of explaining it to Arthur. He asked, "Arthur, could that man you saw in Honolulu have been Chi-bing Ling?" Arthur replied, "Ling! Yes, of course, that's who it was!"

But I was left with the assignment of telling Pete and Perry the story. I did, and one of them said, "Well, I guess we can't win 'em all."

Before we leave the subject, let me tell you another story about the F.B.I., one that presents that organization in a better light.

One of my friends was accused, after World War II, of having been a courier, some years earlier, for a Soviet spy ring. He vigorously denied any such thing. The same local F.B.I. agent, Pete, came to me for an opinion. I said that I had no knowledge of the man's activities at the time, but I had confidence in his honesty.

One or two days later, Pete came to me again, to say, "I don't want you to think we are playing tricks on you. While I was questioning you, your friend confessed that the charges were correct: He had been the courier."

[I trust that the point of this story is clear: to question me about my friend when they already knew that he was guilty would have been a really underhanded trick!]

That case was never made public, but there was another one at Cornell--a real Hollywood-type tale of spies, mystery, and even sex--that we all read about. Once again I was asked a lot of questions by Pete.

In briefest outline: There was a physicist, Alfred Sarant, who was employed at Cornell on some project or other, who was believed to be involved with the famous Rosenberg case. There was also a very competent, pleasant young aeronautical engineer, who spent a summer at the G.S.A.E. working with us on a government-sponsored research project, who was also suspected of being involved with the Rosenbergs. Although I knew him rather well, and had also met Sarant and a man named Dayton and their wives at some political meetings, I was unaware of any relationship between Sarant and the aeronautical engineer--I suspect that there was none. But to the sleuths, of course, it looked like more than a coincidence that both were in Ithaca that summer; I was asked many, many questions that were intended to cast light on the suspected relationship.

The real excitement began when Mr. Sarant and Mrs. Dayton suddenly disappeared. Apparently the whole apparatus of counterespio-

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nage was brought into play to find them, but all that could be learned was that they had gone to Mexico together, using false papers.

That was in 1950. On 26 June 1984 the *New York Times* published a startling article recounting how a certain Dr. Mark M. Kuchment, "a science historian", had solved the mystery. Kuchment, says the article, emigrated from the Soviet Union to the U.S. in 1975; he was able to piece together all that he knew about a Soviet engineer-scientist, Phillip Georgievich Staros, and Alfred Sarant, and to prove that they were one and the same person. It appears that Staros was highly successful in the field of micro-electronics, was chief designer in a laboratory of the Soviet military, received high honors for his brilliant work, ran afoul of the Soviet bureaucracy, retired, and died in 1979.

Those of my readers who are not in the university world may not be aware that professors are besieged by investigators, civilian and military, who want to ask questions about our former students; they are more interested in their private lives and political views than in their professional abilities. One came to my office at Cornell, asked a lot of such questions, took copious notes in a little black notebook, and finally thanked me and departed. As I prepared to get back to more productive work I discovered that he had left the notebook on my desk. Without taking the time even to glance at its contents I ran down the stairs and out the door; he was just starting to drive down the road; I yelled and waved my arms; he stopped. When I handed him the little book he blushed and said (Yes, you guessed it) "You can't win 'em all. Heh, heh." [Maybe that's the motto of the so-called intelligence community.]

Throughout my career at Cornell I maintained a pleasant and profitable professional relationship with personnel of the Cornell Aero Lab in Buffalo. A good number of our G.S.A.E. graduates went to work for the Lab when they finished their studies; the relationship was close enough so that the Lab usually knew the students before graduation and had an inside track when it came to picking the best candidates. The Lab also contributed funds for a number of fellowships at the G.S.A.E.; these were of tremendous help to us at the university, where it is always difficult to support students

financially in the first years of their graduate studies.

In other words, the relationship between the G.S.A.E. and the Aero Lab was as close and as mutually valuable as I had hoped for--with some misgivings--when it all began back in 1946.

I think of two anecdotes concerning this relationship: One has to do with our student Mr. R, who went to work at the Lab. He was not one of our best students and apparently I wrote a rather unenthusiastic recommendation for him. They hired him anyway, since, as they told me, "We also need hewers of wood and drawers of water."

On one of my periodic visits to the Lab, my good friend Dr. Al Flax chided me gently about that: He told me that R was working for the Lab's esteemed Dr.G and that Dr.G had said that he was doing a fine job--that he thought our standards in writing recommendations for our students might be too strict. Flax said, "According to Dr.G this fellow does more than hew wood and draw water."

I thought that was fine. But just then Dr.G himself came by and Al told him what we were talking about.

"Yes," Dr.G agreed, "I gave him the job of interpreting our wind-tunnel data on ... (so-and-so), and he did it." I swelled a bit with pride and remarked that it was the sort of thing our graduates should be able to do.

But Al Flax was puzzled: "I thought you told me, Dr.G, that he made a mistake in that problem--that his solution was all wrong."

"Well, yes," Dr.G admitted, "He forgot to account for all the image vortices--the tunnel-wall effect--but except for that he did a good job."

At that point I couldn't resist being a bit mean: "Well, now I understand your standards. He worked the problem you gave him; so what if he did it wrong? At least he worked it."

I told them I was reminded of an old story about two Jewish fellows in Poland, each boasting about how smart the rabbi in his particular village was. Finally one fellow says, "Listen, in his service last Sabbath our rabbi closed his eyes and could see all the way to Warsaw! He could see that it was raining in Warsaw! Now, can your rabbi do that?"

The other fellow was unimpressed: "Wait a minute. I was in Warsaw last Sabbath; it didn't rain at all; the sun shone!"

Whereupon the first fellow said, "Never mind that. Can your rabbi even see to Warsaw?"

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The other story is more serious. The Lab had contracted with a big company to make a theoretical study of the maximum range of a certain category of guided missiles. The Lab's Dr. Joseph Foa carried out the study and came to the remarkable conclusion that there was an optimal value, less than 100 percent, for the efficiency of the entrance diffuser of such a missile. That means that if you had a perfect diffuser you could make the missile fly farther, with the same amount of fuel, just by beating up the diffuser a little--so it wouldn't work so well.

In those days the Lab had an editorial committee that perused such reports before they were published, and Al Flax was a member of that committee. He read Foa's report and refused to let it go out--argued that the Lab couldn't bill the customer for the work--until this weird result was understood. The Director, Dr. Furnas, faced a dilemma: His Dr. Flax and his Dr. Foa were in total disagreement and were both adamant. He asked me to settle the matter for him; I agreed and returned to Ithaca with Foa's report and began to check its every word and symbol.

Every word and symbol seemed to be correct, but the conclusion remained unbelievable. Arthur Kantrowitz came by my office and wondered what I was doing; I told him. He reacted with some heat: "I object to your wasting your time on such nonsense! The conclusion is patently wrong, and that's all we have to know!" I replied that I had to find the error--or how would I avoid making the same error myself some day?

When I did find it, it was on page 1 of the report, of course, in "Equation (1)". [For my technical readers: Foa had used a formula for what is called the "interior thrust", which accounts for all that happens to the airstream that goes through the inside of the missile, whereas the real thrust has to take into account what happens outside, too.] It gave me some satisfaction to solve the mystery.

But the story is not ended. I reported my success to Dr. Furnas, who now had an unequivocal answer to his dilemma. And what did he do? He approved Foa's wrong report and billed the customer!

From Cornell Aero Lab Furnas went to Washington as Under-Secretary of Defense. The record shows that he couldn't make difficult

decisions there either.

Our weekly Colloquium in the G.S.A.E. was a copy of the GALCIT Seminar and brought us some memorable speakers. When the illustrious Cambridge professor, Sir Geoffrey I. Taylor, agreed to visit us and give a lecture, I knew that others on the Cornell campus would want to see and hear him and that our customary seminar room might not be large enough for the occasion. I asked Lloyd Smith if we should make it a joint meeting of the Physics Colloquium and ours; he was sure it should be. What neither he nor I knew was that Taylor was a very poor public speaker; he spoke too softly, mumbled, his slides were self-made--apparently by writing with a fountain pen on tissue paper, and his mathematical notation--the meanings of his symbols--was different from slide to slide. In informal discussions over a table he was wonderful; his ability as a lecturer seemed to be inversely related to the size of his audience. In any case the content of his talk would be great, but his hearers might have to struggle to get it.

I went to Lloyd Smith, after Sir Geoffrey departed, and apologized for imposing such a poor speaker upon the Physics Colloquium. Lloyd dismissed my apology, remarking that it was educational for their graduate students to learn that even such a great man could be such a poor speaker.

Professor Hans-Georg Küssner, about whom I have told you earlier, was another story. He reported to our Colloquium on some numerical calculations he had made concerning the aerodynamics of wings. For those of you who are familiar with such things: His technique was a kind of collocation; he assumed various properties of the distribution of lift over the wing and chose their magnitudes to fit the boundary conditions at an array of points. One of the unknowns was the exponent of the leading-edge singularity. (The leading edge was assumed to be sharp.)

His numerical results gave a value of 0.499 for the singularity, and he seemed to think that was important. Someone politely asked him if that wasn't just a good confirmation of the value  $\frac{1}{2}$  that one would have assumed. He replied, "Oh no! My computations are correct to the fourth decimal." Al Flax, who had come over from Buffalo for the meeting, pointed out that there is a family of wings for which

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the exact solution of the problem is known, namely elliptical flat plates, and for which the exponent has exactly the value  $\frac{1}{2}$ . Küssner smiled proudly and stated, "Ja, exactly, which makes my result even more important!"

At another meeting of our Colloquium the speaker was a Professor Luigi Broglio, from Rome. He spoke on numerical calculations of mixed, sub- and supersonic transonic flow over wings. This is a subject of major importance and, at that time, a new one. After I introduced him he showed us slides of his results, which were indeed impressive. But after showing these, with no explanations and with only half his allotted time expired, he said that that was all he had to say, and he sat down. I protested that he hadn't explained how he had obtained these results; he replied that unfortunately, "for reasons of priorities", he was unable to tell us more. We didn't learn any more about his work that day, and have never heard a another word about it in the subsequent thirty-or-so years.

On still another occasion our speaker was a young man who had just finished his dissertation at a major American university; we had a vacancy in our faculty and were considering him as a candidate. His work concerned a theoretical problem in unsteady magnetohydrodynamic ("MHD") flow. After being introduced he wrote the initial and boundary conditions of the problem on the blackboard. Our brilliant student, Harry Petschek, sitting in the front row, interrupted the speaker to point out that one of the initial conditions seemed wrong: "Shouldn't that be  $\delta B/\delta x = 0$  instead of  $B = 0$ ?" he asked. The other young man undertook to explain, but Miki Rott exclaimed, "Yes, Harry is right! It should be  $\delta B/\delta x$ !"

By this time everyone seemed to agree that the wrong condition had been used. The speaker was very pale. It was my responsibility, as chairman, to behave like Prof.-Dr. Pauli (See Chapter 4) and terminate the meeting, or to think of a better solution. I could not be so cruel to the young man; he was faced with the fact--at least the possibility--that his dissertation was all wrong. I think I earned my salary that day: I said, looking at my watch, "It is clear that there is disagreement about that initial condition, but we are running out of time. I suggest that the speaker proceed to

present his solution, and at the end of his talk we will return to the question and see how we can re-interpret his results in the light of the initial and boundary conditions imposed."

Of course we did not have time, at the end of the talk, to re-interpret. I believe the young man had to go home and do a good deal of work, but I suppose it wasn't too onerous, since he was already expert in the field and the techniques.

I have one more Colloquium story--a short one. In those days of MHD research we were continually concerned with the geometries of so-called "vector products": three mutually perpendicular vectors whose spatial arrangement is remembered with the aid of the thumb and two fingers of your right hand--since we always use right-handed coordinate systems. Nobody tries to give a lecture in MHD without frequently exhibiting the triad of fingers of his right hand. But one of our speakers suffered an accident to his right hand just a few days before coming to Ithaca; it was swathed in a shapeless bandage. Early in his talk he mentioned one of these triads of vectors and used his left hand. "Oh, I forgot to say," he explained, "on account of my little accident I'll have to ask that all coordinate systems be left-handed today."

What is surprising is that he was able to carry that through consistently, whereas, I am sure, most of his audience had signs confused most of the time. I think he had practiced!

## CHAPTER 7

## CORNELL (II)

Our decision to accept Cornell's invitation and to move to Ithaca in 1946 was of overwhelming significance in shaping our lives, and turned out to be a very wise one. I enjoyed tremendously being a Cornell Faculty member--I learned a great deal about university faculties, boards of trustees, alumni, traditions, funding, etc. I had been brought up first in one of the big state universities, then had gone to one of the most elite of the elite institutes of technology--where I had come into contact with the traditions of European higher education--and now I was part of the Ivy League. Ithaca was a wonderful place to live, work, and raise a family: a city of about 20,000, plus college students, in beautiful upstate New York. Mabel and I became Easterners rather quickly--citizens of a most progressive, enlightened state.

By a remarkable coincidence we were soon reunited with my mother and my sister Helen. After a few years of teaching in the public school systems of Chaska and Minneapolis, Helen decided that she wanted to devote herself to teaching and research in Spanish language and literature. With admirable courage and determination she resigned her tenured position and undertook graduate study, first at the University of Minnesota and then at the University of Cali-

fornia at Los Angeles, where the Spanish program was unusually strong. Mother and Helen sold the family home on 10th Avenue South and moved to L.A. in 1944; so they and my little family were neighbors--at a distance of about ten miles--for two happy years. They enjoyed L.A., and UCLA's Department of Spanish came fully up to Helen's expectations. We became a close, loving, extended family, so far as gasoline-rationing permitted: David had a grandmother and an aunt, and Mabel had a mother for the first time since she was three years old! Mother and Helen were with us during the exciting process of acquiring Susan in 1945. Parting from them was one of the difficult aspects of moving to Ithaca.

Helen got her Ph.D. in 1949; her dissertation is entitled *The Concepts of Fortune and Fate in the Comedia of Lope de Vega*. Her first post-doctoral job was an assistant-professorship at the Texas State College for Women in Denton, TX. [It has, since then, become coeducational and had its name changed.] In 1951 she received a grant for research and spent the academic year at Cornell. And instead of returning to Texas at the end of that year she accepted an invitation to join the faculty of Wells College, in Aurora, N.Y., north of Ithaca on Cayuga Lake! Her choosing Cornell as the locale for her research in '51/'52 was not a coincidence, of course, but the fact that the offer in '52 came from a college only 27 miles from Ithaca seems to have been entirely unplanned.

So we were reunited; Mother was able to watch her grandchildren grow up; for nine years there was not a happier family!

Mother died in 1960 at the age of 78, while Mabel, David, and I were in Europe. Susan moved in with Toni Anthony for the period of our absence; she hadn't accompanied us to Europe because, as a member of the Ithaca High School Band (alto and baritone saxophones), she had to be at "band camp"!

Helen's career at Wells College was brilliant: After several years of teaching, which she enjoyed, it occurred that the college was facing a vacancy in the position of Head Librarian. Helen was offered the opportunity to obtain a Master's degree at the prestigious Library School of Syracuse University and to move into that position. Her friends at Cornell were convinced that the path from teacher/scholar to professional librarian was the right one and advised her to take it. She took evening courses at Syracuse, commuting, and was in residence there for a semester.

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She loved her work as chief librarian of Wells and carried it out efficiently and devotedly until her death in 1965.

One thing that made Cornell such a great place to work was that, rather than being a stuffy, tradition-bound Ivy League university, it was a place of innovation and flexibility. One of the administrators told me that "at Cornell anything can be done; the rules are only there to be broken". I'll tell you later, for example, about a proposal for a department to be located in two colleges. (Horrors!)

Here is another example: In the G.S.A.E. we had a student, Jerry Brainerd, a Ph.D. candidate who had serious difficulties with his requirements in Mathematics. Jerry's situation became so bad that his Committee flunked him! He was demoted to candidacy for the Master's Degree. His Committee, consisting of Professors Resler, Booker (of Electrical Engineering), and Kac (of Mathematics), was reduced to Resler and Booker, constituting a Committee for a Master's candidate. This committee of two told Brainerd to write up what he had already done, concerning shock waves in magnetohydrodynamic flow, and they would accept it as a Master's thesis.

Jerry, it turned out, had done a lot of good work on the shock waves: For ordinary, garden-variety, gas flows the shock wave, if there is one, is determined by two parameters, the Mach number ahead of the shock and the angle through which the flow direction is turned by the shock. But when the gas is electrically conductive there are more parameters, including the strength and direction of the magnetic field, and Jerry's project, which was to identify all the possible cases, turned out to be a big one. He got it all organized in families of graphs and presented it formally to his two-man committee; it was his Final Oral Exam for the M.S.

The result was that the two eminent Professors marched from the exam, down the hall to my office, came in, closed the door. Professor Booker, a dignified Britisher from Cambridge, announced to me, "Professor Sears. What we have just heard from your student, Mr. Brainerd, was not a thesis for the Master's degree. It was a first-class Ph.D. dissertation. What do we do now?"

I picked up the telephone, called my friend Don Cooke, Dean of the Graduate School, and put the question to him. His answer was, "Hell, Bill, at Cornell we can always handle a nice problem like

that. We will simply return Brainerd to Ph.D. candidacy and put Mark Kac back on his committee. Of course Brainerd will have to satisfy Mark's requirements for Mathematics, some way or other."

That took some doing, but Mark was infinitely helpful, and Dr. Brainerd became one of our distinguished alumni.

Mark Kac was always infinitely helpful. The Kacs were our dearest personal friends in Ithaca, and it was indeed a blow to us when Mark accepted a chair in Rockefeller University in New York City and they moved to Scarsdale. Mark was so exuberant, extroverted, and colorful that there are innumerable stories about him. There is even one about his daughter, Deborah:

The name Kac is mostly mispronounced; Kitty herself mispronounces it for the benefit of people to whom she talks on the phone. [In our modern society it seems to be most important to have our names spelled correctly, even if that causes them to be mispronounced.] Deborah was accustomed to having her surname murdered; in the universities today the professor receives a printed class-list from a computer before he ever meets the class, and learns how to pronounce the names by reading the list to the class at its first meeting. Until Debbie went to a certain math class at New York University she had never heard a professor pronounce "Kac" correctly, but this young man, after some hesitation, did so.

Debbie responded, "Here."

The young mathematician looked at her. "Any relation?"

"He's my father," replied Debbie. Whereupon the young man groaned, "Oh migawd!"

When she tells the story she adds, "He needn't have worried; mathematics is my father's field, but it certainly isn't mine." I suspect that it was the only mathematics course she ever took.

Her father spoke only a little English when he immigrated to the United States in the 1940s. His first American job was at Johns Hopkins University, in Baltimore; in that city he found a little diner where the prices fitted his very modest income, and there he discovered a sandwich that he especially liked: tuna on toast. He was not at all clear what "on toast" meant; the counterman regularly asked him, "On toast?" and when he answered, "On toast!" he re-

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ceived what he enjoyed. It seemed to be a ritual, and when Mark looked up "toast" in the dictionary he found, sure enough, that one of its meanings was a salutation, as preceding a meal or a drink.

But, as time passed, he saluted his American friends with the same words and the misunderstanding was explained to him; he was much amused and enjoyed telling the story on himself--to his new friends at Cornell when he became a professor there a few years later.

Still later, having attained a brilliant reputation in Mathematics, he found himself in Oxford University, in England--an honored visitor. He was introduced to a distinguished academic community at "high table", after which the begowned dons escorted their guest to partake of port, I suppose, with their Master. As the party walked toward their destination, the man who had sat silently beside Mark at the table asked him, "Are you not from Ithaca, New York?" Mark confirmed that he was.

Slyly, the man grinned at Mark: "On toast, I believe."

I think the story must have crossed the Atlantic when our distinguished colleague, Professor David Daiches left Cornell for a chair at Oxford a few years earlier.

Mark was invited to lecture on Probability as part of a high-powered, rapid-fire program of advanced mathematical courses given in the summer at one of the western ski resorts; it was one of these programs that consists of a few overcrowded days of high-pressure lectures by teachers who presume--or hope--that the students have heard it all before. Mark looked out upon his audience and said, "This course will move along rapidly. When you come to class please bring two pencils, not just one. Otherwise, if you happen to drop your pencil, by the time you pick it up you may have missed Differential Equations or something like that."

Cornell had a kind of crisis when its president, Edmund Ezra Day, retired in 1949. The Provost, an eminent historian, Cornelis de Kiewiet, was made acting president, and many people, including members of the Board of Trustees, thought he would be chosen President. But there were objections from a lot of faculty members; the Search Committee--made up of six professors and seven trustees, as I recall--undertook a survey, involving a secret ballot, to ascer-

tain how serious the opposition was.

There was also a rather weird business at about this time--I am not quite sure of the chronology--when the Board decided that its own Chairman, an important Wall Street attorney, Mr. Arthur H. Dean, would make a good president for Cornell. According to *The Ithaca Journal*, he agreed but needed time to arrange his business affairs in New York before the appointment could be announced. The story was leaked to the *Journal*, however, and published, whereupon Mr. Dean withdrew. We were deprived of the opportunity to see how a Wall Street lawyer would function as president of a major university.

Shortly after this fiasco I was made a member of the Search Committee, replacing a professor who had left Cornell. At the first meeting that I attended, the results of the faculty survey on de Kiewiet were presented. Several of the trustee-members of the committee were shocked and incredulous when they heard them and realized that the Faculty's near-unanimous opposition spelled the end of that candidacy. De Kiewiet had stepped on too many toes as Dean and as Provost, and he had also acquired a reputation of being vindictive.

[All this did not keep Dr. Cornelis de Kiewiet from being chosen President of the University of Rochester. There were many wisecracks about this on the Cornell campus, of course. One of the cutest was that of Robert J. Walker, a rather staid bachelor professor of Mathematics, not noted for clever remarks: After de Kiewiet accepted the position at Rochester but before he reported for duty there, he made a trip to Africa and engaged in some big-game hunting. A headline in *The Ithaca Journal* announced, "De Kiewiet Charged by Angry Buffalo!" Bob Walker dryly commented, "Yes, but just wait a few months and we'll read 'De Kiewiet Charged by Angry Rochester!'" ]

The Search Committee had done months of hard work and was not close to a successful result. There was one trustee-member who thought he had the solution. He was William Carey, an attorney from Kansas City, Cornell graduate and Rhodes Scholar (as I recall), now one of the more influential members of the Board. Mr. Carey brought us the candidacy of Deane W. Malott, President of the University of Kansas.

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Carey had done his homework: He had come to our meeting, which was held in the Cornell Club in downtown New York, with a big pile of documents to show that President Malott was loved and admired by the faculty, student body, alumni, and Regents of the University of Kansas, by the legislators in Topeka, and by the business world of the Midwest. Mr. Carey presented all this to the committee. Someone said, "This fellow certainly sounds like a promising candidate. I suggest that we proceed to learn more about him."

This was not what Carey wanted to hear. He said, "I've presented all the facts. Dr. Malott is upstairs, waiting to be invited to join us here right now. You must make up your minds: Shall I tell him to come down or not?" The committee decided Yes, and we had a pleasant meeting with Malott. Within a few weeks he became our choice and the choice of the Board; Cornell had a new President.

He got off to a poor start when he came to Ithaca to assume his duties and to deliver an inaugural address; some paragraphs of his address were plagiarized--undoubtedly due to incompetent staff work but still the responsibility of the speaker himself. [It was *The New Yorker*, again (!), that caught the boo-boo and published it in their "Department of Funny Coincidences".]

His ten-year administration at Cornell is generally seen in retrospect to have been a good one. [Cynics say that only means that Cornell's endowment funds increased during his watch.] He was surely not a deep thinker, nor one who took Cornell's problems so seriously as to ponder on them or "do homework." His style, faced with a problem, was to tell his Provost to bring together all the parties involved--typically at 8:30 Saturday morning. That meant that a half-dozen or dozen people came to his office and took seats around a large, bare, conference table; Malott greeted them jovially with a wisecrack about the grass that wasn't being mowed and said, "All right, now what is this all about?" [Some people tell me that that's the way all problems are attacked at the Harvard Business School.]

All this brings me to a story that relates to President Malott but is not really about him--it is about the above-mentioned William Carey and the responsibilities of the trustees of universities.

It occurred near the end of Malott's long tenure as Cornell's

President. I was enroute to Evanston to give a seminar talk at Northwestern University and landed at an airport called "PalWaukee"--that's a combination of the place-names "Palatine", a suburb of Chicago, and "Milwaukee". I asked the airport manager how I might get transportation to Evanston; he said that "those fellows" who had just landed in a handsome business twin were going to Evanston in a limousine, and suggested that I ask whether I could ride with them. I did and was introduced to their boss, "Mr. Carey", who was agreeable; we started off for Evanston. I recognized the William Carey I had known ten years earlier, and I said, "We've met before, Mr. Carey. I'm Bill Sears from Cornell; we served together on an important committee for Cornell."

"Really?", he replied. "What committee was that?"

"The Search Committee that recommended Deane Waldo Malott for the Presidency of Cornell," I explained.

His response was almost incredible to me: "Is that a fact! Tell me, how did that fellow make out at Cornell?"

When Malott became President, Lloyd Smith thought he ought to be made Provost. Instead Malott chose another professor, one who, in my opinion, was not nearly as well qualified as Lloyd--but probably a much better choice for Malott, since he and Lloyd wouldn't have worked very well together. At Cornell in those days the Provost was Assistant President; today, in some universities, he is the academic top-man while the President is concerned with other things.

At any rate, Lloyd became disillusioned with Cornell. He and Clark Millikan, who were old friends, dined together somewhere and Lloyd apparently poured out an alarming story of Cornell's new administration--so alarming, it seems, that Clark, when he got back to Pasadena, told his colleagues that he was worried about Bill Sears and thought they might have to find me a new job. When I learned that--probably from H.- W. Liepmann--I was rather touched but was able to reassure my Caltech friends.

Cornell's undergraduate students, like those of other universities, loved to indulge in big, highly organized, practical jokes. Once, just a year or so after Malott became President, a team of them took over the broadcasting facilities of WVBR ("The Voice of the Big Red"--the students' radio station). It was a low-power, AM

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facility whose signal was heard only in Ithaca and its environs; it played popular records and was listened to by a sizable fraction of the student body. The pranksters waited until Exam Week, when WVR's audience was probably greatest, and occupied the studios by force!--or at least the threat of force.

Their message to WVR's listeners, late one night, was, "We interrupt this program to bring you a news bulletin. Unidentified aircraft have been detected flying toward the U.S., over the North Pole. Stay tuned to this frequency for additional bulletins." And, half an hour later: "Air Force officials have identified the aircraft flying over the North Pole as U.S.S.R. military airplanes. Stay tuned ... etc."

By this time it was past midnight. The next message to the harried students trying to study for exams was "from the Commandant of Cornell's R.O.T.C. units" and instructed all members of those units to report immediately, in uniform, to Barton Hall. It also promised more bulletins: "Stay tuned ..."

The next bulletin was this: "We have a statement from President Deane W. Malott. President Malott states--and these are his words, addressed to the university community: 'We have nothing to fear but fear itself.'"

In view of Mr. Malott's problem with plagiarism, recounted above, that was a bit cruel on the part of the students, wasn't it?

On another occasion, inspired by an upcoming football game against Syracuse University, Cornell students managed to hijack all copies of that Saturday's *Syracuse Daily Orange* as they were being trucked from the printer to the paper-boys and newsstands. They substituted a bogus version that announced, among other calumnies, the firing of Syracuse's head football coach for being caught in *flagrante delicto* with somebody's wife.

As in other universities there was some sentiment in Cornell's fine Mathematics Department in favor of recognizing the distinction between pure and applied mathematics. I think it arose from the fact that some of the mathematicians who studied mathematical applications and mathematical techniques useful in applications were not given the same degree of recognition as were their colleagues whose interests were in mathematics *per se*. It was--and

is--a somewhat sensitive subject, since there are quite a few mathematicians who are clearly members of both categories; and, as Mark Kac pointed out, there are only two kinds of math: good and not-good.

The argument for some kind of separation or bifurcation of the two categories usually arose at institutions where applied (or applicable) math was thought, by people in the sciences, to be neglected and badly taught. This was clearly not the case at Cornell in those days; nevertheless the argument arose. A proposal was made to form a Department of Applied Mathematics, with considerable overlap in personnel with the Department of Mathematics. A delicate question remained: What College should the new Department be a part of? Some ingenious person suggested that it be a part of both the College of Arts & Sciences and the College of Engineering; such a thing had never been seen before, but the deans of both colleges were happy to try it, and assured us that they could make it work. Professor Barkley Rosser, Chairman of the Math Department and well known for his work in both pure and applied math, would be the Chairman.

But Rosser was a somewhat controversial figure in the Department of Mathematics, and before all the details of the new structure were worked out two distinguished members of that Department's Faculty went to the Provost to announce that they were going to quit Cornell unless Rosser was removed from the chairmanship of Math. Their complaint had something to do with the plan for Applied Math--I think they claimed that he had not been frank in keeping his Math colleagues informed of the scheme.

Now the only answer to give someone who comes to you and says "Either ... or I shall quit," is "We are going to miss you." But the Provost didn't seem to know that, and, instead, called Rosser into his office to say to him, "You will have to be replaced. After all, you wouldn't want to continue as Chairman under these circumstances, would you?"

It happened that Rosser and I were involved in the meeting of a government committee in Washington the next day. Rosser joined me in our hotel, quite shaken. He said, "I wanted to tell the Provost that Yes, I would want to continue under those circumstances. But I had to say No."

The whole Applied Math plan collapsed. A substitute plan was

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made up, wherein Applied Math would be the responsibility of a Center, not a Department; that means that participating faculty would all be regular professors in some university departments and would contribute their time, effort, and graduate students to the Center as they wished to. [That is the kind of organization used in a number of other universities.] I was asked to be the Director of the Center, and I accepted. I had been Director of the G.S.A.E. for seventeen years; my successor in that job was a very able colleague and former student of mine, Professor Edwin L. Resler, Jr.

Barkley Rosser went from Cornell to the University of Wisconsin; there, a few years later, he became the Director of a big Applied Math project sponsored by the military services, which, I believe, was (and is) considered to be a very high-quality operation.

Cornell's Center for Applied Math still operates under the same structure and has, I think, been a success; it has attracted some distinguished faculty members and graduate students, and has strengthened the program of instruction in its important subject area.

Let me describe one fault (two cases) where I found the Center structure to be inadequate: First I must explain that I was given authority to recruit new faculty members--applied-mathematicians who would come to Cornell as members of regular departments; the funding ("line items") of my budget would then be transferred to the permanent salary budgets of those departments. Gifts out of the blue skies for those departments, wouldn't you think?

But in two cases, although the individuals being recruited were eminent scholars, the departments involved declined the heaven-sent gifts! In one case, "He works in an area where we don't; there would be nobody for him to talk to." In the other case, "There are other areas that we want to strengthen; we're afraid that the administration won't give us funds for those areas if we already have this new person added to our faculty."

What we are seeing here is something like the "N.I.H." factor, where "N.I.H." stands for "not invented here". They refused to believe that a faculty colleague recruited by someone else could be as valuable as one they would choose for themselves, even if he came at no cost to them.

Of course, they have a point. A department in a university should carry the responsibility for teaching and research in its

field; the field should be defined so broadly as to meet the needs of the students and of society, but narrowly enough so that its faculty is competent to master it, to evaluate it, to predict its future. If Applied Mathematics is a bona fide field, it should have a department empowered to make its own judgments, its own successes, and its own mistakes, and the same goes for the departments that refused to accept my "gifts from out of the blue".

The Center for Applied Math had its offices in Olin Hall, the building that housed the School of Chemical Engineering, so I had two offices--one there and one in Grumman Hall, the home of the G.S.A.E. One of the pleasant chores I acquired was to find and hire a secretary. The job was advertised and I interviewed some applicants. One was a beautiful, bright, young lady, Janice Moulton, who had just completed her undergraduate career at Cornell in the humanities and had gone to a local business college to learn secretarial skills so that she could get a job and help finance the Senior year of her friend George Robinson ("Robin"). Jan and Robin were deeply devoted to each other and, well ahead of the times, were unabashedly sharing an apartment without benefit of marriage. Jan was, like Toni Anthony 17 years earlier, completely inexperienced in secretarial work but, like Toni, gave every impression of intelligence, character, and seriousness of purpose, so I hired her. Again my good judgment paid off; she was the one and only secretary of the Applied Math Center for several years. After that Jan and Robin did graduate study at the University of Chicago; she earned her Ph.D. in Philosophy, he in Psychology; she is now called one of the leading feminist philosophers. They write together and sometimes present joint colloquium talks; they recently spent an academic year in residence at a college in Wuhan, China. They are so kind as to consider Mabel and me some kind of editors of their books (those outside their professional fields, of course), so that we have read with great pleasure about their year in China and fictional accounts inspired by it.

Life with Jan as my secretary was never dull. On a trip to Italy--an aeronautical meeting in Naples--I wanted to buy a little present for her, and decided upon a very brief bikini. (I'm sure that the prospect of seeing Jan in that little garment was in my subconscious mind, at least.) She seemed pleased by the present. I

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went into my office, which adjoined hers, to attack the stack of mail piled-up in my absence. Suddenly my door opened, and there she was in the bikini! Wow! But our offices were at the end of a corridor of Olin Hall and the front of her office was all glass. I didn't even want to think about her changing into the bikini there! [I'll bet that she really enjoyed my flustered reaction to her little style show.]

Jan and Robin were enthusiastic liberals and took part in most of the many demonstrations that were so prevalent on the campus in the '60s. Occasionally they were arrested for sitting-in and things like that. Mabel and I worried for fear that the police or the newspapers would make trouble for them--for their having the same home address and not being married, or something of the sort. [Believe me, Dear Reader, it was a different era from today; the "authorities", in those days, did not believe that such things were none of their business.]

I asked my friend John Summerskill, Vice President of Cornell for Student Affairs for advice--asked him whether I should urge them to get married. His answer was unequivocal: He thought that sooner or later their unmarried status was bound to cause them trouble. I had told him that they were sincerely devoted to one another--a permanent commitment--so they should marry. Jan and Robin deferred to our pleas, although I'm sure it was a surrender of their principles. A local judge performed the ceremony; Mabel and I were, once again, the witnesses. We suspect that they have never really forgiven us for talking them into such a silly and antiquated custom. They are still married, of course, but they are still "Janice Moulton and George Robinson", even in their letter-heads, return address, etc.

One of our colleagues in Applied Math was a delightful fellow, a splendid applied-mathematician, David Block. In the process of his research Dave invented and constructed a learning machine, a computer against which you played a game. If you played the game enough times, the machine always learned to beat you. It always "learned" to beat you because Dave had programmed it to eliminate from its software the moves that had not been profitable to it in playing against you.

It was quite impressive. Dave wrote-up the research in a report

published in *American Scientist* (pp 59-79, March 1965). The paper was livened up a bit by inclusion of some rather innocuous photographs of the machine being played against by Jan and Robin--they were good friends of Dave and were certainly photogenic. One photo carries the caption "Miss Moulton vs. Machine G-I." Farther along there is another photo with the caption "Mr. and Mrs. G. M. Robinson vs. G-2-P. Match arranged by Prof. W. R. Sears".

Surely that sets some kind of record in the field of in-jokes!

I believe that Dave Block had a lot to do with the elimination of some antiquated rules that were being enforced at Cornell's "Statler Club". I must set the stage by explaining what that Club was: Cornell, as my readers may already know, is a leading university in the field of hotel management. [It is probably not difficult to become a leader in that field, since there are so few universities that undertake to instruct in it.] It is also somewhat of a surprise to find that this distinguished Ivy League institution includes a School of Hotel Administration. It is housed in Statler Hall, and one of the features of that building is the faculty club of Cornell University, The Statler Club.

One of the features of the Statler Club, of course, was a rather elegant dining room--after all, you can't have a faculty club without dining facilities, and Heavens! you can't teach hotel administration without them, either. In those days the Statler Club featured, in addition to the dining room, a cafeteria called "The Rathskeller".

Believe it or not, The Rathskeller was for male faculty members only!--of course that is no longer the case--and males could enter the dining room only if they wore jackets and neckties. Professor Block wanted to lunch with a female colleague but was not wearing jacket or tie. As he undertook to enter the dining room he was intercepted by an undergraduate student of hotel administration whose job it was to enforce the rules. Dave explained that he was, in fact, wearing both jacket and tie, which he always wore under his shirt. When the "hotelly" was unconvinced, Dave asked, "Does the Statler Club really propose to have the members of the Cornell faculty undressed by undergraduate students?"

Apparently Dave's (irrefutable?) logic was enough to open a small crack in the wall of the Statler Club's fortress of antique

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etiquette; the jacket-and-necktie rule soon disappeared, along with the exclusion of women from the Rathskeller.

During my long stay at Cornell I was active as an engineering consultant to industrial firms and governmental agencies; I was also a panel member of "AGARD", the Advisory Group for Aeronautical Research and Development, which was a part of the NATO organization and was founded and chaired by Dr. von Kármán. Consequently I had a lot of traveling to do. All my domestic travel I did in my own airplane(s)--there was a series of them, increasing in cost monotonically over the years--as I will describe more fully in a subsequent chapter. For AGARD, however, I usually traveled abroad and had to fly as an airline passenger. Then the "airline" was MATS, the Military Air Transport Service, and that was a source of many adventures and anecdotes.

MATS's airplanes were a good deal more comfortable than those we were flown in back in 1945. In fact, as members of AGARD we qualified to ride on what the military called "The Blue-Plate Special", which was the DC-6 used by the commercial airlines, with comfortable airline seating--albeit sometimes with the seats facing aft (a safety measure). The B. P. Special was intended for the use of high-ranking military officers and us civilians who were deemed to have equivalent "rank".

I learned that when the B. P. Special flew across the Atlantic it was followed, about an hour behind, by an equivalent airplane, but with denser seating, carrying lower-ranking military personnel including the wives and children of the B.P.'s passengers! On one trip I discovered the military logic of this. Our DC-6 landed at the Azores with engine trouble. (Of its eight magnetos, providing ignition for its four engines, three were not operating.) Without hesitation we and our luggage were off-loaded. When the back-up flight with the women and children aboard landed, they and their baggage were promptly off-loaded, we took it over, it was refueled, and off we went toward Washington, feeling self-conscious and somewhat guilty.

Another story about MATS that I like concerns a great German aerodynamicist, Dr. Adolf Busemann, and a younger colleague and traveling companion, also German, Dr. F. Ringleb. Busemann had made the transatlantic trip on MATS several times but it was a new

experience for Ringleb. As the flight proceeded eastward Busemann told his companion what to expect: In the middle of the night we will land at the Azores; there will be funny little soldiers in 19th-century uniforms, speaking an incomprehensible language; we will be met by a bus carrying the logo "U.S. Air Force"; the bus will carry us up to the top of a mountain where we will be served a steak dinner; if you wish to, you can purchase cigarettes and bottles of whiskey and gin for ridiculously low prices; and then we'll come back to the airplane, go back to sleep, and be off to Paris.

Everything happened exactly as Busemann predicted, and after landing in Frankfurt Ringleb told a friend how interesting he found the Azores to be. It was some days later when someone corrected him: their flight hadn't landed in the Azores but in Iceland!--but every detail fitted: the soldiers, the Air Force bus, the steak dinner, ...

On one of my trips I found myself in New York City with time on my hands, but I knew how to make good use of it: I needed a haircut. I was near Rockefeller Center and found that on one of its lower floors there was a big barbershop--there must have been 15 or 20 barbers. All of them, I gathered, were Italian-Americans and talkative. Soon I was in one of the chairs and engaged in a conversation about baseball.

Something came up about the Minnesota Twins and I mentioned that I was from Minneapolis. My barber was a bit confused: "You are from Minneapolis or Minnesota?" I explained to him that Minneapolis was a city, Minnesota a state, and that the baseball team was named after the state. He interrupted the hair-cutting to come around to the front of me; he was obviously skeptical.

To bolster my story I volunteered that The California Angels and the Texas Rangers were also named after states. By now hair-cutting had pretty much stopped in the whole shop. I was surrounded by Italian-Americans holding scissors and asking "What's about Baltimore Orioles?--named after state? Chicago Cubs? Philadelphia?--is city or state? ..." I'm sure that I transmitted a good deal of geographical information on that occasion, but I began to wonder about my expensive New York haircut and those of about a dozen other customers.

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And while I'm on baseball I'll tell you another tale, this one about baseball and the Japanese language. There was an interesting former major-league player, catcher for the Boston Red Sox, named Moe Berg, well-known to baseball fans and sports writers as "The Professor", not because he ever taught but because he was a university graduate--from Princeton. There weren't as many college grads in professional baseball in those days as there are now. I would like to call Moe a "star", but I did so once in his hearing and he scolded me for it.

Moe Berg was a most interesting, intelligent fellow. He was a great admirer of Dr. von Kármán. He is remembered for carrying out some modest espionage just before World War II; he was in Japan to play baseball and, it is said, acquired some information and/or photos that were of use to the famous Jimmy Doolittle flight over Japan during the war.

I met him at Cornell on the occasion of one of Dr. von Kármán's extended visits to the G.S.A.E., about which I propose to tell you in a later Chapter. I remember that he stood with me on the front steps of our building, Grumman Hall, and looked across the road to Hoy Field, Cornell's baseball field; he recalled having played there more than once, for Princeton, against Cornell. The fact that von Kármán's knowledge of baseball was rudimentary, at best, did not hamper their friendship and mutual admiration. [I remember the great aerodynamicist lecturing once about the forces on spinning projectiles and reminding his audience that "a pitcher directs the ball in such a fashion as to be of greatest advantage to his team." (Can't argue with that!)]

But this is a story about language. In my office Moe wrote on the blackboard some Japanese characters and asked my (temporary) colleague, Yasujiro Kobashi, "Did you ever hear of this person?"

Kobashi-san replied, "Oh, yes! Famous manager of Philadelphia Athletics, Kohny Mock."

"O.K.," continued Moe, "and did you ever hear of this person?" and wrote another array of Japanese characters. Kobashi frowned and read, slowly, "Connie Mack. No. Who is that?"

Berg's demonstration had worked exactly as he had planned: He

was making the point that a great part of Japanese mispronunciation of English is due to faulty transliteration of the English words into phonetic Japanese.

For some years I was a member of an Advisory Committee to the Air Force Systems Command. It was a small committee and a remarkably good one--probably only six or seven members, chaired by the famous optical-physicist, Professor Brian O'Brien. We reported to the Commanding General of the Systems Command, Gen. Bernard ("Benny") Schriever, his deputy, Col. Barney Marschner, and their Chief Scientist, Dr. Bernhardt Goethert; in other words we reported to the formidable triumvirate of Benny, Barney, and Bernie.

We met at the various research-and-development establishments of the Air Force: Vandenberg, Eglin, Hanscom, etc. I had to have prior approval to fly my own airplane into some of these--and had to carry at least one million dollars in liability insurance!--I was never convinced that I could cause a million dollars' worth of damage, but, remembering how the Air Force inflates the cost of toilet seats, ... For years I included Raimy A.F.B. (Puerto Rico) when I asked for such prior approval, hoping that the O'Brien committee would hold one of its meetings there, but it never did.

Once, when we held our meeting at Eglin A.F.B., Bernie Goethert also flew his own airplane, a Beech Bonanza, and landed it at Eglin after a relatively short hop from Tullahoma, Tennessee. The problem for us do-it-yourself pilots was to buy gasoline at an Air Base. I had learned to solve it by making another landing at some nearby civilian field for fuel--a nuisance, but better than coping with Air Force red tape.

Bernie, however, had arranged to get his fuel at the Air Force Base. They brought out one of the biggest tank trucks you ever saw and started to refuel the Bonanza from a hose at least three inches in inside diameter. An airman took the filler cap off the Bonanza's tank, opened the nozzle-valve, and waved to his helper to start the truck's pump; gasoline poured out, all over the Bonanza's wing and the airman's shoes, before the valve was closed. The helper had not even turned the pump-motor on!

Our meeting was in session, but a message was forwarded to Dr. Goethert to come down to the flight line right away; I accompanied him. The airmen said, "Didn't you tell us to fill the starboard

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tank, Doctor? We tried to, but it seems to be full."

"Oh no!" replied Bernie, "I used only the starboard tank coming down from Tullahoma! There is some mistake!"

I realized what was wrong. The Bonanza had used only ten gallons or so coming down from Tullahoma. There was much more gasoline than that in the Air Force's big hose. As soon as the nozzle-valve was opened, the starboard tank was filled to overflowing, without the pump's even being turned on.

OK, no harm done, except perhaps for the airman's shoes. But Eglin Field's paperwork was going to require some straightening-out. They had a requisition for aviation gasoline, signed by Dr. Goethert, but the initial and final readings of the tank-truck's gauges were identical. Heavens!

At another meeting Bernie proudly showed me a new book written by one of his old professors in Germany. Its subject was The Future, namely estimates of markets, production, demand, and supply of just about everything in the coming decades: petroleum, electric power, water, metals, wheat, ... and people. I was actually not terribly impressed, because I gathered from the book's jacket that all these astronomical numbers were the results of rather naïve extrapolations. But that is not the story I wish to tell. The story arises from the author's rather unusual name: He was Prof.-Dr. **Fucks**. I understand that in German that is nothing but "Fox", which is a common Anglo-Saxon name, and, in fact, it is fairly common in Germany, too, but usually spelled "Fuchs".

I was, at the time, a member of a select Advisory Panel for Douglas Aircraft, Inc. I learned that Douglas engineers were preparing to present a briefing to Benny, Barney, and Bernie on the future of aviation; I told them that they would be wise to become familiar with Prof. Fucks's book, since Bernie would surely bring it up in their meeting. The Douglas engineer responsible for the briefing agreed heartily and wanted the title and author's name immediately so that he could tell the Douglas Company's librarians to procure it. I obliged, but I pronounced the Professor's name in German style ("Fooks"). He wrote "Fuchs" and I corrected him.

He was dismayed: "Fucks? No way! No way am I going to tell our library ladies that I want a book by Fucks! No sirree! I resign!"

I thought his reaction was exaggerated and rather unsophisticat-

ed. But I knew the solution: I simply wrote a note to Bernie Goethert asking if I could borrow the book for a few days. He complied, the Douglas engineers borrowed it from me, and everyone was prepared for the briefing.

Now, here is a little anecdote about Barney--Dr. Marschner--and it also has to do with the German language, but it is not so vulgar.

In Göttingen, in connection with an AGARD meeting, I found the modest restaurant that was Dr. von Kármán's favorite when he lived and studied there: "Der Schwarz Bär". Mabel and I went there for dinner; we were seated in a booth in which there was a somewhat strange little collection of things in a glass case on the wall: a stovepipe hat, a gavel, and a tuning fork.

Later in the week we met Dr. Marschner and his wife at dinner time; they wanted to go to the Schwarz Bär in honor of Dr. von Kármán, so we four proceeded there, and by chance were seated in the same booth. But the glass case was empty. In my bad German I asked the waitress where the things were that had been in the case: "...der Hut, der Hammer, der -----?" (I had no idea how to say "tuning fork".) But Barney, who didn't speak any German, provided the word immediately: "die Stimmgabel". [With a name like "Marschner", one is likely to have a German grandmother, and so it was with Barney, and "Stimmgabel" popped out of his childhood memories.]

So where were the things? It was the evening of the monthly meeting of a Göttingen Singverein in a room in the restaurant. The President wore the hat, pounded the table with the gavel, and gave the Verein an "A" when they were ready to sing.

Miki Rott told a cute story about accents: After World War II the U.S. Navy sent some of its young officers--perhaps a half-dozen of them--to the E.T.H., the Swiss Federal Technical University in Zürich, for graduate study. The members of the Aerodynamics department there were not usually party-minded, but on one occasion did come together on a social occasion.

Miki remembers joining three of his faculty colleagues who were standing together and discussing the accent (in English) of the wife of one of the Navy officers--her husband had brought her

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across the room to meet the three young faculty members. It seems that she had a strong Southern accent: "Ah'm mighty pleased to meet y'all .... (and so forth)". When Miki joined the trio he found that one of the young Swiss colleagues had had his feelings hurt: "She was making fun of us, talking in babytalk like that!"

One of his comrades was more kindly disposed: "I don't think she was ridiculing us; I think the unfortunate lady has an impediment of speech." The third Swiss had an even sadder theory: he thought the lady was mentally retarded.

Miki was more cosmopolitan, and explained that it was none of these things; the lady was simply speaking to them in what is called "a Southern (American) accent". As he recalls, his explanation was thought to be the least credible of the four.

In carrying out my duties as a Panel Chairman of AGARD I had much to do with the "simultaneous translators" assigned to us by NATO for our technical meetings. I always found their skills amazing: the speakers were giving technical papers--usually ad lib and therefore full of starts and stops, not to mention "er"s and "ah"s and mumbles. Nevertheless what came through the headsets in French was remarkably good. We were asked to give them a list of technical terms beforehand and to urge the speakers to go slowly, but all that was often forgotten. The speakers they had the most trouble with were those who spoke broken English and--you guessed it--those with deep-Southern accents. Unfortunately there are many of both these categories in the fraternity of American aeronautical specialists.

Back to the Cornell campus in Ithaca. I served a term as a member of the faculty committee that had the responsibility for the distinguished scholars and public figures who came to Cornell as lecturers, and another term as a member of the faculty committee that planned the visits of musicians and musical groups. These assignments often led to wonderful opportunities to meet such famous persons.

Once Cornell was visited by the great Soviet violinist, David Oistrakh, who played a concert in Bailey Hall. Our committee lunched with him and, of course, his Russian traveling companion, whom we'll call his "escort". Oistrakh was a jovial luncheon guest.

Someone asked him if he was buying anything in the U.S. His answer was that he was buying all the phonograph records and ball-point pens that "he"--indicating the "escort"--"will let me."

Mme. Oistrakh was also making the tour with him, but she did not speak English. The committee found a seat in Bailey Hall for her beside Mabel and me, for her husband's performance. In spite of the language mismatch we tried, of course, to be hospitable. At the intermission she was joined by the "escort"; after the intermission she did not reappear.

On another occasion my committee status allowed me to have lunch with Harry S. Truman, whom we found to be a man of charm and humor; he told us how his daughter Margaret teased him while the Trumans stood in a receiving line. She saw Senator Bricker coming toward the line and whispered to her father that she intended to trip him "so he would fall on his fat face on the marble floor." Bess Truman, he said, put a stop to such misbehavior with a sharp word to Margaret.

One of us professors asked him, "Mr. President, what was the most difficult decision you had to make as President?" I think all of us thought of the decision to drop the atomic bombs on Japan. But Mr. Truman's answer was not that; he said without hesitation that it was the decision to send troops to Korea. He said, "I was not at all sure that I wasn't starting World War III."

We also lunched with a famous scholar of Oxford University, Professor F. R. Leavis, and his wife, Queenie D. Leavis, who is also a scholar. Since F. R. Leavis was the outspoken critic of C. P. Snow--remember the "Two Cultures" debate?--, I went to the luncheon prepared to disagree with him on almost anything. But I learned that Q. D. Leavis was a world-renowned expert on Mark Twain, and I was so bold as to mention the Rees Family History (which I have alluded to in my Introduction) and the remarkable relationship between Samuel Clemens and the Reeses, which is recounted there. My boldness was well-received; she didn't know the stories and wanted all the details.

The story is this: Samuel Clemens, when he was a young man, came to Keokuk, Iowa, to work for his brother, Orion, who was a printer. Samuel and Orion became friends of the Rees boys, whose father, William, had purchased and was publisher of *The Keokuk Daily Post*. One of the Rees boys, George, found Samuel "so droll in his manner

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and speech" that he ought to try writing; he agreed to pay Samuel \$5.00 for each piece; Samuel agreed and that is what was paid for the first one. Samuel asked for and received \$7.50 for the second, and wanted an incredible ten dollars for the third. The Reeses didn't think that would be justified "as a matter of business", and the series ended.

I would like to give you a small sample of Samuel's writing style. Here is the second paragraph of the first piece written for the Post, which is supposed to be a letter to the Editor from one "Jonathan Snodgrass", from "Cincinnati, Nov.22d, 1856":

You know arter going down there to St. Louis and seein so many wonderful things, I wanted to see more, so I took a notion to go a-travlin, so as to see the world, and then write a book about it (a kind o' daily journal like) and have all in gold on the back of it, "Snodgrass Diorrea" or something of that kind, like other authors that visits forren parts.

Thomas Rees, the author of the Rees Family History, thought that this was the first writing that Samuel Clemens was ever paid for--which would, of course, be rather exciting--but it seems that Samuel wrote a first "Snodgrass Letter" earlier, when he was in St. Louis.

Anyway, when Samuel Clemens became famous my grandfather and his brothers resurrected the Keokuk letters, which were in the public domain, and republished them. Samuel, then known as "Mark Twain", was furious. He wrote, at considerable length, about how impossible it was that he could have written such trash, and concluded that there are decent people in the world and then there are the Reeses!

His diatribe against "the Reeses" is included in the book *Mark Twain in Eruption*, B. de Voto, editor (Grosset & Dunlap, N.Y.). There is also an Editor's footnote telling us that Mark was dissembling--that he did, of course, write the Snodgrass Letters!

I didn't know all this when I bragged to Mrs. Leavis about my relationship to Mark Twain, but another colleague at the luncheon was Professor Robert Elias of Cornell's English Department; what I told her reminded him of things he had heard before, and he directed all of us to the de Voto book.

We also enjoyed a visit of Peter L. Kapitza, the eminent Russian-British physicist. He had been at Cambridge for some years but was back in Moscow and serving as an officer--perhaps President--of the Soviet Academy of Sciences. I remember the following from our visit with him:

As an officer of the Academy he had a direct telephone line to the Kremlin. But the Kapitzas' housekeeper was the wife of a civil servant who, she thought, was not paid enough. So she elected to use Professor Kapitza's direct telephone line to register a vigorous protest. As a result, the direct line of the Professor/Academician to the Kremlin was removed. He seemed to think this was hilarious.

We asked his opinions on a number of subjects, one of them being nuclear power. He thought fission was a terrible way to generate power, but he wasn't much worried about it because it was only a stopgap--a way to provide power while we work out the details of fusion power generation.

He hadn't said anything about medical research and progress; someone at the luncheon asked him about that, and about medical progress in the U.S.S.R. His answer was that he couldn't get worried about medicine and the health of a species that is, already, rapidly overpopulating the planet!

In the Physics Department of Cornell there was a husband-and-wife team, the Doctors Cocconi, whose research involved cosmic rays. They had the idea of placing instrumentation in the abandoned salt mines beneath Seneca Lake--that is the Finger Lake west of Cayuga Lake. I remember that Mrs. Cocconi was not permitted to go down into the mine because the miners believed in some superstition about women bringing disaster to a mine (as well as to ships at sea?)

The Cocconi, as we liked to call them, gave a party at their home one evening and invited a young fellow countryman who was an assistant professor, or the equivalent, in the University of Padua. Somebody at the party, to make conversation with the young man, asked, "How is life for a junior faculty person in a provincial university in Italy these days?"

The young man answered: "Ah, life is grim. If it weren't for writing the theses I wouldn't have enough income to live on."

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His interrogator was puzzled: "I don't understand. Why do you 'write theses'?"

"The student needs a thesis; I need the money," replied the young man. But the conversation was overheard by his host, Professor Cocconi, who entered the conversation: "I'm afraid you must explain; you seem to be saying that you write the thesis for the candidate and receive payment from him for doing so!"

The young professor became sarcastic: "Come, Doctor Cocconi, You know it's the same in universities everywhere. Don't pretend that it's any different at Cornell University!"

Cocconi looked at the fellow for some seconds, then turned on his heel, marched to the coat closet, and returned to hand him his coat and hat: "Here, sir, are your coat and hat. I now ask you to leave my house."

I imagine that there are and always have been situations where a university degree can be "bought"; I have recounted in Chapter 4, above, the story of a candidate who tried to "buy" a Caltech degree. I have heard, recently, of cases--not at Caltech!--where such "purchases" have been made; they are cases where there were complicated relationships between universities and government funding agencies: professors who were also employees of firms that wanted government funds; students who were also employees of government agencies--altogether too many people "wearing two hats".

In the 1960s American universities were embroiled in riots, protests, takeovers of campus buildings, ... Cornell was no exception. I was engaged at that particular time with research on aerodynamic noise--the noise that is caused by propellers, wings, compressors, turbines, jets, ... I had some students doing thesis work in this field; one even explained, for the first time correctly, the noise ("click") that is made by two billiard balls colliding on a billiard table: it is purely an aerodynamic phenomenon, and is not caused by the vibration or deformation of the ball! In a more useful vein we were struggling with the complex case of the helicopter rotor--a maker of dreadful noise, as we all know.

I think I must have been rather smug in my certitude that I was "doing good" for the world as I endeavored to reduce helicopter noise, but one day I found my office in Grumman Hall surrounded by a noisy, belligerent crowd of militant students protesting against

me and my research! It was, to be sure, supported by U.S. Army funds. The protesters proclaimed loudly that my students and I were developing quiet helicopters that would be able to sneak up silently on barefoot Vietnamese and kill them more easily.

I wanted to speak to them and argue that noise reduction is always a noble endeavor, and that every such noble endeavor can, unfortunately, be perverted to war-making purposes, but some young men representing the Cornell administration escorted me away.

There was also an occasion when the militants decided to attend one of our weekly seminars, when our speaker, although his topic was something esoteric about boundary layers, was himself an employee of an aircraft company. When I learned of the probable disruption of our seminar I offered the speaker the option of canceling it; he declined that option and told me that he would ask for "lights off" and "the first slide, please", as soon as he was introduced, and would keep the lights off all through his talk. It worked perfectly; the students who wanted to disrupt were completely frustrated.

Colonel "Barney" Marschner, whom I have mentioned above, went to Colorado State University as a Vice President when he retired from the Air Force. There he was given the responsibility of confronting their militant students. He did it with strong-arm tactics--"Now I want you to come out of there in single file and give your names and I.D. numbers to the policeman at the door. And if you don't, the police are coming in there to bring you out." He seems to have been successful.

On the contrary the President of Cornell, James A. Perkins, who I thought was eminently qualified for the job, undertook to treat Cornell's militants, when they forcibly occupied Willard Straight Hall, armed with rifles and shotguns, as fellow members of an intellectual community; he was rudely rebuffed. And so was John Summerskill, who had left Cornell to be President of San Francisco State University, when he tried to reason with that institution's militants.

On the evening that the students took over Willard Straight Hall, a couple dozen of us, concerned professors, came together at Mike Abrams's house. The bone of contention was that the student self-government organization--the competent authority--had imposed certain punishments and penalties on some militant students for

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destruction of property in Willard Straight Hall in the process of one of their protests. We, the professors gathered in the Abramses' house, were convinced that the university had to back up the student government: that the punishments and penalties had to be carried out. We thought, and declaimed, that to do otherwise would spell the end of orderly, civilized life in our intellectual community, and thereby the end of the scholarly pursuits that the university represented. I remember that there were a few prophets of doom who predicted that any such attempt to enforce the rules would lead to the destruction of Willard Straight and other buildings.

History shows that we were quite wrong. The Administration capitulated to the militant students, who agreed to evacuate Willard Straight **with their guns**. A vice president was allowed to enter the building, carrying the Administration's written agreement, and he was escorted from the building by an armed squad of victorious students; a photograph of his ignominious exit was published world-wide. Nevertheless, the university survived. Student bodies have short memories; campus militantism was never again as exciting as it was in those turbulent years. Students in later years knew that "all that had already been done."

There was a period, during the years I spent at Cornell, when my colleagues and I in the G.S.A.E. became fascinated by the area of fluid mechanics called *magneto-hydrodynamics*, or "MHD"--it concerns the motions of fluids--liquids or gases--that are conductors of electricity, such as mercury or ionized gases, in the presence of electric and magnetic fields. The astrophysicists got us interested in this somewhat esoteric subject: they believed that some of their unexplained phenomena in the skies might be attributed to MHD-effects.

So we were studying our equations of fluid dynamics with electric and magnetic effects included. We needed to know more about moving electromagnetic fields, induced electric currents, and the like. We convinced Cornell to bring to Ithaca a famous astronomer/astrophysicist, Thomas Gold, who could teach us these arcane matters. Tom stayed at Cornell, ultimately, and became Chairman of the Department of Astronomy. He is surely one of the most original and imaginative physical-scientists in the world: The remarkable Continuous-Creation model of the universe, mentioned in

Chapter 4, attributed to Gold, Fred Hoyle, and Hermann Bondi, although now rejected by the physics community (including its authors), has been described as one of the most ingenious products of human science; I am told that its basic ideas were Gold's.

Tom lived near Cayuga Lake and was bothered by the raucous, rasping, whining noise of motorboats. He learned that the worst of them were those towing water skiers, and he came to me with a plea that I should join him in the invention of low-drag water skis: If their resistance was small enough they could be towed by sailboats (!), and if not, by smaller, quieter motorboats.

I had some ideas on the subject and we had a water ski made accordingly. I can still see Tom carrying out his idea of a test of my ski: It was winter in Ithaca; Tom had donned a diver's wetsuit against the cold and was holding the ski--then, for comparison, a conventional one--in the water while riding in a boat with outboard motor. "Yes!", he cried, "Eureka! The drag is much less!" (Not even a grocery-store spring scale in evidence.)

He was so convinced--the drag seemed so much less--that we proceeded to have the second ski rebuilt according to my invention, and now Tom, again in the wetsuit, undertook to ski behind a boat. Unfortunately he found it impossible to stand up; he said that he seemed to be skiing on stilts! Apparently my design reduced the drag but ruined the lateral stability of the skier.

We never got back to that project again. Tom had another idea, anyway: He wanted me to join him in inventing silent plumbing for homes. I do agree that silent plumbing would be a great boon to mankind--more important than water-skiis!--but I am happy to leave the problem to somebody else to solve.

At one point during my tenure at Cornell, that distinguished university was in a financial crisis. [That is supposed to be a joke, my friends, because all colleges and universities are always in financial crises.] Anyway, in response to the moans and groans about finances I wondered how the university could rescue itself from bankruptcy, and I invented the "Ruml-Sears Plan". Beardsley Ruml, it seems, was the man who invented the "pay-as-you-go" income-tax plan. It works because the government is always with us--for eternity, we suppose--and therefore it can ask us to pay our taxes in advance; it never has to pay us back.

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I reasoned that the university, too, will always be with us. We should ask the public to pay its children's tuition fees in advance--say 17 years in advance. We must only convince the public that tuition costs will increase faster than inflation--fast enough to make the investment a profitable one. Cornell had about 15000 students in those days; I estimated that it could sell enough advanced-tuition vouchers to make a once-in-history windfall of at least a million dollars. Like Ruml's scheme, it would be based on the conviction that the university would always be there and would sell the required number of advanced tuitions every year.

After pondering over the idea for weeks I took it to the Treasurer of Cornell. He missed the point completely: His response was, "Bill! You're promising to let these kids study at Cornell seventeen years from now at today's costs? We'd go broke!" I tried to explain that I would have sold the advance-tuition vouchers every year of the seventeen and would be selling them, at their inflated values, to a new crop of parents, when the 17-year-olds came to Ithaca.

I despaired of making him understand the logic; I went to Dean Andy Schultz of the College of Engineering. He not only understood the plan immediately but pointed out that it amounted to the university's borrowing a million dollars from a lender (the public) who didn't require any payments on the principal. It is as if there were a bank that would--or would be allowed to--lend Cornell a million dollars in return for our only paying interest annually, which we would do using the income from current sales of vouchers. Andy also despaired--in colorful words--of explaining all this to the Treasurer and his staff.

At this time I was a member of a Visiting Committee at Princeton; I made an appointment with their Provost on the occasion of the Committee's next meeting, and I presented my plan to him. He understood it and he liked Andy's analog. I asked him if he thought any bank would be allowed to make such a loan; he thought not. He also doubted that a university's charter would permit it to make such a loan from a bank.

Nothing came of my idea. Everyone recognizes that it has a flaw: What if the child whose daddy bought our vouchers seventeen years ago now finds that he can't meet Cornell's entrance standards--or doesn't want to go to Cornell? The answer is that the tuition

vouchers have to be accepted at any of a consortium of universities and colleges.

Today, as you may have recognized already, what I have called the "Ruml-Sears Plan" exists in several states--but with no credit given (or due to) me. The "consortium" consists of the colleges and universities in a state. I am left with the reassuring conviction that there are other people as smart as I.

Here is another story about the Treasurer's Office: Toni Anthony was preparing the paperwork for paying some of our faculty members and graduate students for summer employment on sponsored-research projects and needed Ed Resler's signature. Ed objected to the way Toni had calculated their pay; Toni told him that she agreed: "It's wrong, but it's what the Treasurer's Office tells us to do."

Namely, each person was paid, every 14 days, an amount equal to 14 times his "daily salary", which was  $1/365$  of his annual salary. This is wrong, of course, because he received 26 paychecks and 26 times 14 is 364, not 365; every Cornell employee was being cheated out of one day's pay per year. Ed walked over to Day Hall to protest. The Treasurer's staff laughed at him: "Come on, Ed, are you really going to quibble about the difference between 364/365 and 1?"

It happened that Cornell had just recently raised parking fees for the Faculty; Ed's daily parking was costing him over \$100 per year, so he replied, "No, I won't quibble if you will just credit my one day's pay against my parking fee." He also estimated the amount represented by  $1/365$  of Cornell's total payroll and asked whether the Treasurer really wanted that big number reported in public! He didn't, and Cornell's method of calculating paychecks was soon corrected.

It must have been in 1964 that I encountered a dental problem that seemed to require the assistance of my friend from the '30s and '40s, Dr. Eugene Dyer. The problem concerned my "bite", and I knew that Gene had become deeply interested in problems of that kind. I wrote to him about it, saying that I would like to have him look at my teeth on the occasion of a trip West.

I must have also made some remark about the political campaigns and candidates for president that year, because when he

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replied--saying that he'd be happy to have me as a patient again--he also said that in his opinion my politics were obviously as badly in need of correction as my jaw! And when I did go to his office I found it decorated with at least a dozen banners, portraits, and posters of the Goldwater campaign. I realized that the display was intended for me personally.

So we argued politics, without, of course, changing either's convictions. His dental services were more helpful; he explained how teeth always grow and therefore cause trouble like mine. He pointed out that a "Grand Old Man" of orthodontia was still active and practicing in New York City: a Dr. Eby. And, being an old airplane pilot himself, he pointed out that weekly (say) visits to Dr. Eby, from Ithaca, would be quick, convenient, and a lot of fun.

I followed his advice. Eby turned out to be a delightful octogenarian; we became good friends immediately. He constructed a delicate wire-and-plastic "bite plate" that I was to wear day and night for some months--thereafter at night only--that allowed my back teeth to grow. I still, 28 years later, wear it at night. I remember how happy Dr. Eby was to have a patient so mature that his bite plate would probably not be stepped on, or flushed down the toilet.

I took a sabbatic leave--my first--in 1965/66; we went first, in September 1965, to Palm Springs, where I began the preparation of a "course of lectures" on magnetohydrodynamics ("MHD") for the University of London. At the end of October we traveled to London. The "course of lectures" consisted of four, two at Queen Mary College and two at Imperial College.

In Palm Springs we lived in a small "apartment complex"--which is California terminology for a group of eight or ten small individual apartments around a swimming pool. It was delightful, sitting at the side of that pool, preparing those lectures. I had deposited the Comanche in a hangar at PSP on one of my trips west and we drove to Palm Springs in our new (1964) Corvair "Spyder" coupe--the turbo-supercharged model--with a lot of baggage strapped to its top.

[That's a story in itself. As we drove farther West, up the mountain roads over the Continental Divide, the cylinder-temperature gauge read 400--then 450--degrees. I had asked Chevrolet dealers in Indianapolis and points West what the permissible maxi-

mum was; none of them knew the answer; they assumed that if the "idiot light" on the dash didn't go on everything must be OK. Well, I am a professor of aeronautical engineering and a flyer of airplanes with air-cooled engines, so that answer didn't reassure me; I knew that 450 degrees was pretty hot for an air-cooled cylinder. We drove more and more slowly as we progressed West; we even had to stop a couple of times to let the cylinders cool off. We got to Palm Springs and asked the Chevrolet dealer there about our problem. There was no problem; the cylinder temperature was OK even up to 500 degrees. When we drove back eastward in midsummer we found that it never did get that hot, or even close to it, no matter how fast we drove.]

When I finished my preparations for the London lectures I needed to have slides made. I supposed that there were photographic studios in Palm Springs that could make the slides, but when I realized that there was also a college in the town--The College of the Desert, or C.O.D. for short--I naturally assumed that its Photo Science or Visual Aids people would be more familiar with the task; you can't have a college, I reasoned, without professors who want endless numbers of "lantern slides" to illustrate their lectures. But I learned otherwise: Their people told me they "didn't seem to have any requests for that kind of thing" at C.O.D., so I had to go elsewhere for my slides.

At the first of the lectures in London, in a large hall at Queen Mary College, I found about a dozen of the world's most distinguished fluid-mechanicists sitting in the front row--they included several who had written highly important papers in MHD. There was nothing to do but to resort to a famous von Kármán anecdote:

The story is that Dr. von Kármán encountered the same situation once when he went to Ann Arbor to lecture at the University of Michigan. He was introduced to the audience, rose, and said, "Good afternoon, Ladies and Gentlemen. I was invited here to deliver a lecture to your graduate-students' Seminar. Now I am here and I see, in the front row, not students but the eminent members of the Faculty. It is too late for me to alter my prepared lecture. The only thing to do is to deliver what I prepared for the graduate students; I will speak slowly and distinctly and hope that the Faculty will also understand."

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In the following March we moved from Palm Springs to La Jolla, where I taught a regular class in MHD to grad students of the University of California at San Diego. (UCSD's academic year was divided into quarters.) Then back to Ithaca.

From both Palm Springs and La Jolla I commuted, in the Comanche, semi-weekly, to Douglas Aircraft where I was a consultant.

In the late '60s I took up Scuba Diving. A young man named Jack Marshall advertised that he was forming Scuba classes and would hold them in the swimming pool of the Ithaca YMCA; I joined, and took my "final exam" in the icy waters of Cayuga Lake the following winter. Mabel took a look at the crowded little pool at the "Y" and decided to forego the course, but Jack took that as a challenge; he held private sessions with her, and soon she too was a licensed diver. We Scuba-dived in Lake George (Mabel's "final exam"), twice in the Gulf of Aqaba (at Eilat, Israel), twice in Pennecamp State Underwater Park (Florida), at Santa Catalina Island (California), at Cabo San Lucas and San Carlos (Baja California). Some of these dives were easy and some difficult. I had a nice underwater camera (Japanese) and on a couple of occasions I got some terrific color pictures of tropical fish and coral. On one occasion, diving in the giant-kelp beds off Catalina, both Mabel and I got tangled in the kelp and were awfully glad that Irv Ashkenas, a veteran diver, was with us to untangle us.

In the spring of 1973 I had the honor of being chosen the 16th Lanchester Lecturer of the Royal Aeronautical Society; my lecture had to do with "self-correcting" wind tunnels--later we called them "adaptable" or "adaptable-wall" wind tunnels. Because Mabel accompanied me, and because both the president of the Society and our host, Professor Lewis Crabtree, agreed, the Society discarded its hoary tradition and permitted ladies to attend the formal dinner at the Society's home in London. History was made!

It was a great occasion; not only did Mabel and several other wives attend, but so did the two Lanchester ladies--the widow and the sister of Frederick W. Lanchester, the great British aeronautical scientist, after whom the Lecture is named. In the past, not even the Lanchester ladies had been invited to the dinners in honor of the Lanchester Lecturers!

One of the guests was a famous, German-born, British aerodynamicist, Dr. Dietrich Küchemann. After we had properly toasted "The Queen", Dietrich rose to propose a toast to Mabel in the form of a tri-lingual verse:

Mabel!  
 Ich hab nummal ein faible  
 Für Damen die fashionable  
 Und reizend sind wie Sie!

[Of course "faible" and "fashionable" have to be mispronounced a little to rhyme with "Mabel". My translation would be

Mabel!  
 I have a weakness--can't help it--  
 For ladies who are as fashionable  
 And charming as you!]

By the early 1970s Mabel and I became aware that retirement (at age 65 in those days) was around the corner. We weren't sure that Ithaca, which we had enjoyed so much for all these years, was necessarily the place we wanted to live after retirement. I was eligible for another sabbatic leave in 1973, and by dropping a hint to the right person I got an invitation from the University of Arizona to spend it there. We enjoyed the half-year (January - June, 1973), and when we prepared to return to Ithaca our colleagues here (Tucson) suggested that we make the move permanent. The suggestion fell on fertile ground; we could visualize a life in Tucson after retirement. I must confess that I also had a feeling that I might have been long enough in one place; there were signs that I was growing into the same category as some old trees on the Cornell campus--they'd been there as long as anyone could remember, and it was taken for granted that they would always be there.

One of my young colleagues asked me whether my decision to leave Cornell and Ithaca was due to the attractions of Tucson and the University of Arizona or dissatisfaction with Ithaca and Cornell. I replied that when a charged particle is placed in an electric field you don't ask whether its motion is due to the attraction of the

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positive pole or the repulsion of the negative.

Shortly after I arrived in Tucson I renewed my friendship with an old acquaintance from Cornell, Professor Jean-Jacques Demorest, whose specialty is French literature. He showed me an amusing clipping from the *Boston Globe*, telling about an alarming "brain drain" from the Northeastern States. It was a long article describing (without names) a number of university professors who had recently departed from the Northeast to the West; Demorest was identifiable in the account; we decided that so was I. In the case that we thought described me the article reported that this professor had probably been seduced to Arizona by the better flying weather--he being an enthusiastic airplane pilot and owner of a light twin.

Well, I can't deny the enthusiasm, but the writer's knowledge of aviation may be elementary. As I will explain in a later chapter, my flying has involved a lot of flying "on instruments" and dependence upon the "IFR (Instrument Flight Rules) System" and my capability to operate in it. And that is a good deal more challenging in the mountainous West than in the Northeast. In some respects the Northeast holds attractions for a do-it-yourself airplane pilot.

## CHAPTER 8

## VON KÁRMÁN

Professor von Kármán has already been an important character in these memoirs. Such a colorful person, with whom Mabel and I were associated so long, has left us a legacy of memories and anecdotes that is almost inexhaustible. Let us break out of the (roughly) chronological organization of the account to focus upon the personality and character of this remarkable man.

There is an invaluable "autobiography": *The Wind and Beyond: Theodore von Kármán by him "with Lee Edson"* (Little, Brown and Co., 1967), which was written by Edson from conversations and extensive tape-recordings. It is a wonderful source of factual information about the Professor, including many anecdotes and reminiscences. It suffers only from the usual fault of autobiographies: It tells us only what the author remembers and wants us to know. A real biography, written by a skilled biographer/science writer, would be welcome but seems unlikely to appear at this late date. Dr. von Kármán died in 1963, and the population of those of us who knew him well--already a small population--has shrunk.

In 1979 Mabel and I wrote a chapter entitled "The Kármán Years at GALCIT" in *Annual Reviews of Fluid Mechanics* Vol.11, pp.1-10, which included a short bibliography (5 entries). And in 1984 I gave

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a talk to the Fluid Dynamics Division of the American Physical Society, entitled "Von Kármán: Fluid Dynamics and Other Things", which was published in *Physics Today*, January 1986 (7 pages). That paper is accompanied by a number of delightful photographs of the subject and his mother and sister. In the present collection of reminiscences and anecdotes I don't wish to repeat the stories already written in these earlier efforts--or, at most, only a few of the "most important" ones.

I have already written, in earlier chapters of these memoirs, some of the factual data: He was born in 1881, in Budapest, to a distinguished Hungarian-Jewish family--his father, Maurice, a professor of education, held an important position in the Austro-Hungarian government; I have been told that he was the father of the secondary-school system of that Empire, and I suppose that the honorific "von" of the family's name relates to honors bestowed upon him by a grateful government. [At any rate it is a translation from a Hungarian original into German; Theodore's sister Josephine ("Pipö") preferred the French form, de Kármán.]

Theodore (Tódor, in Hungarian) went to school and to college in Budapest, then to graduate study under the great Ludwig Prandtl in Göttingen. He was in many ways a nineteenth-century man; he had a classical early education and many of his attitudes toward scholarship, science, government, and family were carried over from the Central Europe of the nineteenth century. He was "very Hungarian", in the sense that he loved the Hungarian language, people, and traditions; he had a very soft spot in his heart for the Austro-Hungarian Empire.

When they came to Pasadena, the Kármáns became members of a most interesting community of Hungarian artists and intellectuals of Los Angeles County--most of them associated with Hollywood and the film industry: actors, actresses, writers, directors. Both Pipö and Tódor spoke English with pretty strong Hungarian accents, although their mother's English was considerably better. She complained to Mabel, "My children will never learn to speak good English."

Pipö's was mixed with French. She criticized Maj Klein once in a sentence that I have never forgotten: "I do not say for Paris, Budapest, you know, but he is an I don't know what." The translation is "I do not argue for Paris or Budapest (or other European

cities), but he is ... je ne sais pas." It seems that when Maj finished his studies his family wanted to give him, as a present, a trip to Europe, but he said there was nothing over there that interested him; thus her comment.

[Pipö, to be sure, was "an original". Mabel once invited her to have lunch with us at our apartment on South Sierra Bonita Avenue. At the appointed hour she had not shown up; we waited another half-hour, wondering if she had forgotten, then I walked out to the front door of the apartment house, as one might do, wondering if she didn't understand the doorbells and speakers. When I got there I heard a persistent honking of a car's horn somewhere in the neighborhood. I ran toward the sound; Pipö was driving her little Plymouth coupe back and forth on the nearby cross-street (Blanche), blowing its horn. She couldn't find our building and hoped someone would find her if she made enough noise. It worked.]

Her brother's accent was typical of Hungarian English, characterized by a heavy accent on the first syllable of every word and name. He went to a ticket window once to buy a train ticket to "Schenectady", and returned to tell me, "The fellow thought I wanted a ticket to Kansas City."

I had a date to meet him, one day, in the Mayflower Hotel in Washington. I got his room number from the desk and called him from the lobby on the "house phone". Apparently he was napping and was not wearing his hearing aid. I said, "Hello, Dr. von Kármán, it's Bill. I'm in the lobby." He said "Hello! Hello! Who's there?" "Bill. Bill Sears," I replied. "Im in the lobby." He still wasn't hearing me, though I shouted so that the whole hotel could hear me.

In desperation I decided to try Hungarian, of which I knew at most six words: "Jó napot, Tanár Úr." ("Good day, honorable Professor".) The response was immediate: "Oh, Bill, hello. Where are you?"

Some of his "mispronunciations" in English were not that at all; they were deliberate corrections of our sloppy ways with words derived from Greek or Latin.

He had asked his secretary, Miss Mabel Rhodes, to correct his English when he dictated. So when he suddenly seemed to be talking, in his dictation, about "cows", she stopped him: "What are you saying about cows?" He took her pencil and wrote "chaos". "Don't

you know this word?" he asked.

"Yes," she answered, "But we pronounce that `chaos'." He was disgusted: "But Ma Belle, it's a Greek word. You can't mispronounce it like that!"

And again, one day in 1940 when he and I were working at his home, in his study, his almost-spherical Hungarian cook interrupted us to say that a Mr. Carmody was on the phone from Washington to speak to the Professor. I guessed that it was the head of the Federal Works Agency. It was shortly after the Tacoma Narrows Bridge had failed in a gale. Dr. von Kármán went to the phone; I could hear him saying--then shouting--"I think I could contribute to the aerodynamics." But he was pronouncing the word as a combination of "aeros" and "dynamos", both properly enunciated in Greek, but with a strong Hungarian accent on the first syllable. Ultimately Carmody seemed to understand, and the Professor returned to the study.

"You were right, Bill. It was that Carmody; he's forming a committee to study the Tacoma Narrows disaster. I told him I think I can contribute to aerodynamics"--here the proper Greek vowels again, with his Hungarian accent. "He calls it `aerodynamics'!" --and here he said the word as we Americans always do.

Dr. von Kármán told me that when the committee held its first meeting he brought up the matter of the fees that the committee members would charge for their professional services; he thought it would be appropriate for the members' fees to be comparable. He stated his usual fee for consulting on government projects; another member--a younger professor of aeronautical engineering--volunteered modestly that his daily fee would be somewhat less. But another committee member, the famous civil engineer and recognized expert on suspension bridges, Professor O. H. Ammann, spoke up:

He confessed that he would have to bow to the erudition, in matters of aerodynamics, of the two professors of aeronautics, but it was clear to him that they would have to listen to a civil engineer when it came to fees for professional services. Dr. von Kármán asked, "Oh? Do you civil engineers require larger fees?"

Whereupon Professor Ammann explained that, for a case like this one, a civil engineer would expect to be paid a few percent of the

cost of the project--which was something like six million dollars, for the Tacoma Narrows Bridge.

Another famous designer of suspension bridges, the civil engineer D. B. Steinman, published a long paper in *American Scientist* on the aerodynamics of suspension bridges (pp.397 - 438, 460, July 1954) in which he disagreed with the committee's conclusions. It included, as usual, a list of references--19 of them. I was interested to note that all the references, except one, were papers by the author, Steinman, himself. I thought the single exception must be a reference to a most important book or paper; indeed it was: its title was "On Steinman's Theory ..."

Professor von Kármán was a great teacher, as I have already told you in Chapter 3. His lectures were carefully prepared and well organized. The diagrams and equations that he wrote on the blackboard were beautiful: the lines were straight, properly aligned, and legible. He understood very well that students learn by progressing from the specific to the general and not the reverse, as so many textbook writers seem to believe. When he taught us the theory of compressible-fluid flows it was by developing all the phenomena first in one-dimensional flow--as in channels, nozzles, diffusers. Then, in three dimensions, the equations became partial-differential and all the phenomena reappeared in generalized form.

To him, insight into a physical problem was best expressed by differential relationships: those involving the physical quantities and their derivatives. He was the recognized master of the art of obtaining useful approximate solutions to complicated problems, and these were typically obtained from such differential formulas. He had little confidence in, or patience with, approximations made by generalizations from integrated results for "typical" cases. He insisted that the approximations made in solving the differential equations be clearly stated.

I think he believed that any engineering problem, no matter how complex, must profit by having its intimate differential relationships written down and studied. He was also a grand master of the art of detecting the "asymptotic" behaviors of complex systems from their differential equations. Such matters as these are not (or were not, in von Kármán's day) usually included in the training of

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engineers.

He had a powerful effect on the engineering profession and on engineering education. Shortly after he moved to America he attended a national technical meeting, heard a paper delivered by a young man from an American university, and criticized it in the ensuing discussion. After the session, the young man's department chairman sought out Dr. von Kármán and apologized profusely: He had no idea, he said, that the work was so poor! Von Kármán had to explain that it wasn't poor work; it needed extension and strengthening. In those days--the early 1930s--it wasn't customary, at engineering meetings in America, to offer even constructive criticism, but only platitudes.

His standards for engineering education and practice were high. In one of his published papers on the subject ("Atomic Engineering?") he directs a barb at Caltech: "... at the institution where I teach ... the theory of a beam on three supports is considered a difficult problem."

His research was always pursued in a spirit of joy. Nothing was more fun for him than to tackle a difficult problem and to achieve an elegant, useful, understandable result. I think that if the result was an approximate one, but still elegant, useful, and understandable, it was even more fun! I'll never forget when, with the Clauser twins, he attacked the problem of mixed, subsonic/supersonic flow and found that the result came out in terms of some esoteric analytic functions: "... confluent hypergeometric functions á la Whitaker-Watson!", he exclaimed. I can still hear him: "hypergeometric" pronounced as on the Continent, "Whitaker" and "Watson" both beginning with "V"s, and the whole stated with the happy incredulity that any such thing could happen.

His attitude toward other researchers and their work was totally different from what we have all learned from *The Double Helix* (Crick and Watson)--how their research was characterized by jealousies, secrecy, and intrigue--how they endeavored to keep Linus Pauling ignorant of their work. Much of von Kármán's research was directed toward "the problem of fluid turbulence", on which G. I. Taylor, Johannes Burgers, Hugh Dryden, et al. were also working. There was complete cooperation among all these men and their colleagues.

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He was always tolerant of honest error, but not at all of sham, posturing, pomposity, or what he called "pseudoscience". An example was a paper, given at a national aeronautical meeting, in which the speaker presented what was supposed to be a generalization of the Kármán-Tsien theory for the transonic flow regime. It contained a lot of flowery mathematics, but it was really only a suggestion that a constant (the slope of the density curve in the K.-T. approximation) be replaced by a different number dependent, empirically, upon what wing-shape (profile) family was being used. Kármán was asked to comment, and said that the paper reminded him of his childhood in Hungary: "Every year," he said, "we were visited by Gypsy magicians, who entertained us with their magic. The difference between them and this speaker is that the Gypsies only pretended to violate the laws of nature."

He was also intolerant of those who asked for special favors. One unfortunate graduate student, who came to Caltech from a Central European country, encountered some scholastic difficulties in GALCIT and was so unwise as to go to the Director and ask for special privileges "because we Central Europeans ought to stick together". Von Kármán told me, "I threw him out of my office."

Some people assumed that Dr. von Kármán was not a skillful administrator, probably because his style of administration was, indeed, somewhat unusual--people liked to think of him as fitting the stereotype of an absent-minded genius with his head in the clouds. Actually he was an ideal university department chairman--and I speak from long years of experience and observation of department chairmen! He was not only a master of the department's field of study, but a chairman who understood, with amazing insight, his department's faculty members and the university's administrators, as is confirmed by some of the anecdotes that I have recounted here.

He was happy to delegate to younger colleagues the routine responsibilities of GALCIT's operations; they handled efficiently the scheduling of classes and examinations, and they kept track of the rules of the Graduate School and knew who had keys to the building. We students assumed that the relationship between him and

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Clark Millikan, for example, was like that between some other famous scientists at Caltech, who carried the title of Chairman or Director, and their younger colleagues who really took care of the administration. I learned otherwise. Often Mabel or I heard conversations between von Kármán and the other professors concerning important matters, such as invitations to foreign experts and similar commitments of departmental funds. Von Kármán made decisions on such matters, often in the corridor on the second floor, after a few questions and a few moments' consideration. And when the inevitable next question was asked: "Where will we get the funds?", he had the answer. It seemed to us that the status of GALCIT's finances was right there in the Director's memory.

He was, nevertheless, not dogmatic; such important matters were definitely open for discussion with the members of his small Faculty.

An exception was the time when the scholastic requirements for "Del" Knoblock were being debated. Del was an experienced aeronautical engineer who came to GALCIT in the '30s to work for a Ph.D. in experimental turbulence research after having had a career as one of the designers of the all-metal Hamilton monoplane transport airplane. Our requirements for the Ph.D. included a course in Airplane Design; Del thought he had fulfilled that requirement in his industrial career; the GALCIT Faculty, which I think numbered five without the Director, considered the matter and voted unanimously that he had not.

The Director was informed of the vote. "The vote is unanimous," he told us with what I thought was a mischievous smile, "but I am Director, so Knoblock's petition is approved."

None of the Faculty was upset by being overruled. I can't explain, at this late date, why we (the Faculty) were so stuffy. In retrospect Del's request seems quite reasonable.

The recollection reminds me of a much worse story about our--no, my--stuffiness: As the junior member of the Faculty I was given the thankless job of writing replies to prospective graduate students whose letters of inquiry did not impress us very much.

One such inquiry came from a prospective student who had done his undergraduate work at New York University, which we did not consider to be an especially outstanding institution in our field

and who, nevertheless, wanted to be excused from some of our most important courses because he had taken courses with the same titles at N.Y.U. [You may recall that I had repeated a number of courses that I had taken at Minnesota. (Chapter 3)] I had no hesitation in writing the young man that he would have to take our courses and that he would profit thereby. The result was that he gave up the idea of doing graduate work at GALCIT.

The embarrassing aspect of this story is that, years later, I learned that the young man I turned down so glibly was Alexander H. Flax, whom I have written about in Chapter 6. Had Al come to GALCIT he would have been a star student--without repeating any of his N.Y.U. courses!--and his brilliant career would have cast honor upon Caltech.

There were occasions when von Kármán's younger colleagues were happy to turn over certain difficult or unpleasant administrative tasks to their distinguished boss. I have recounted (Chapter 3) that Georgianna had been hired to replace Mabel as secretary but had to be fired after a month or so; I promised to tell you the details of that.

I was in Clark Millikan's office; Clark seemed to be fussing about the subject of Georgianna--twiddling his moustache, talking about her shortcomings in the job, suggesting that she would have to be let go. Finally Dr. von Kármán interrupted: "Look, Clark, I agree she has to be fired. What is it you want? You want me to fire her?"

Clark answered, "Yes, I guess so. Would you do it?"

"Sure," said the boss. "I'll fire her." He walked past me, toward the door of Clark's office. As he passed me he shrugged his shoulders in a typical Continental way and said to me, "Will be unpleasant, but will last only five minutes."

Actually, he was a very compassionate man. I was told that he and Pipö were in Europe, and so were Clark and Helen Millikan, when the Millikans' infant child died in a crib accident, and that the Kármáns travelled "across Europe" to be with the bereaved parents.

On another occasion, years later, Dr. von Kármán returned to Pasadena from a trip abroad; he asked his secretary--one of Mabel's successors--to separate his accumulated mail into important and

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less-important categories. When he finally got around to the latter he found there a letter from a prisoner--a "lifer"--who was studying the Kármán-Biot book, *Mathematical Methods in Engineering*, and trying to work its problems, many of which are difficult! He had written to the senior author to ask for help with them. Dr. von Kármán couldn't believe that the secretary had considered that letter unimportant. He told me that she said, "But he's only a convict in a prison." She didn't understand her boss, he told me.

He was in demand as a consultant, both to industry and to governmental agencies. His fame as professor, lecturer, author and research engineer/scientist preceded him, and he charmed managers and officers by being obviously interested in their problems and eager to help solve them. The Hungarian accent and little eccentricities, such as his beret, were part of the charm, especially when it became clear that he was a warm, friendly, outgoing person who wanted to be helpful and who also had a store of anecdotes and a liking for slivovitz.

He was never patronizing; he told me on several occasions, "I always assume that the other fellow is exactly as smart as I,"--which, I must confess, I found a little unlikely. But I did learn from him, as we often worked together as consultants, not to be fooled by engineers who "spoke a different language". Time and again we found that practicing engineers used terminology different from ours, referred to different books and papers, but were really talking about the same things as we.

[According to the memoirs of Stanislaus Ulam, the famous physicist and "father of the hydrogen bomb", John von Neumann credited von Kármán with "inventing consulting". Von Neumann and von Kármán, fellow Hungarians, were good friends. The paragraph in Ulam's book has Ulam asking, at a cocktail party, "Who is that gray-haired guy?" and von Neumann giving that answer--"He invented consulting." Unfortunately Ulam goes on to say that von Kármán was one of the earliest European airplane pilots--which he surely was not. Ulam also quotes Kármán's clever definition of a practical engineer--which I've told you in Chapter 5--but, alas! omits the word "practical"! Dr. von Kármán must be turning in his grave!]

Sometimes our consulting activities were very interesting and amusing. We (including Homerjoe Stewart and, especially, Duncan

Rannie) were responsible for the aerodynamics of the "Smith-Putnam Wind Turbine", a gigantic windmill for generation of electric power. Palmer Putnam was a scion of the famous publishers' family; he was a colorful New Englander who, with the support of the S. Morgan Smith Company, turbine manufacturers, organized the design, analysis, and construction of the big windmill on a mountain called "Grandpa's Knob", near Montpelier, Vermont, in the late 1930s. Von Kármán was amused and fascinated by Putnam, whom he called "Poot". Putnam was a great believer in the modern methods of communication, and used to send von Kármán ten- and twenty-page telegrams concerning the project. I remember sitting at lunch with the Professor and seeing him sign for (and tip for) fat Western Union envelopes, which he then stuffed under his luncheon plate. I said, "Aren't you going to read Putnam's telegram? It's marked 'URGENT'"

"Bill," he replied, "'URGENT' only means it's urgent for the sender, not for me."

It turned out that the telegram included sentences like "Erection on Grandpa's Knob giving great satisfaction." (This was when the windmill was actually being constructed on the mountain.)

[Especially for the benefit of my technically sophisticated readers, I should say some more about the Smith-Putnam project. The design process included an elaborate optimization that was supposed to produce the best windmill, in dollars per dollar invested, given the facts of aerodynamics, structure, interest rates, and the meteorology at Grandpa's Knob and other sites. It was a remarkably ambitious undertaking, in those days before the advent of high-speed computers! The procedure led to a two-blade windmill with "coning" blades. Rannie detected a problem of two coning blades operating in the earth's boundary layer, but it was too late to redo the whole design. Dr. von Kármán said that this was like the Wright brothers refusing to fly until they had designed the DC-4; he wanted Smith and Putnam to build a (quarter-scale?) model of their optimal wind turbine and run tests of it on Grandpa's Knob or some comparable mountain.

Instead, they built the full-scale article. It worked beautifully, except for the two-per-rev oscillation--two blades rotating and coning in the earth's boundary layer--that Rannie had predicted. This became a serious structural problem in the gale winds of Grandpa's Knob. The wind turbine had to be stopped.]

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But what really happened to the Smith-Putnam project was that World War II began. There were no funds, materials, or personnel for civilian enterprises like the wind turbine. Palmer Putnam turned his talents toward military needs; he was the inventive genius behind such projects as a "tri-phantous jeep".

In the process of our studies of big windmills we were, of course, concerned with the problem of the vulnerability of such machines in high winds--gales and storms. The procedure decided upon by the Smith-Putnam team was to make the windmill face into the wind and to operate its blade-angle mechanism so as to limit its r.p.m. It was decided that this was more practical than stopping the rotation and designing the stopped machine to withstand gale winds. It occurred to Dr. von Kármán that this would be facilitated by having trailing-edge flaps on the blades. There seemed to be no earlier suggestion of the use of wing-flaps on windmill blades, and we prepared a patent application covering the idea. I don't know whether a patent was ever granted.

Dr. von Kármán made another ingenious invention that was never patented or exploited: Sometimes the weather got uncomfortably warm in Pasadena, inspiring him to think about schemes for keeping cool; air-conditioning was not at all common there in those days. He imagined a "negative fireplace"--a device to cool a person or persons by radiation of heat. This would involve radiation of one's body heat to a low-temperature "sink". He sketched it for me: a person working at his desk and radiating his heat (at 98.6 degrees F) to a semicircular, refrigerated receiver (at 30 degrees F, say) that half surrounds him. I pointed out that the person being cooled would have to be naked (a word he never learned to pronounce), but he didn't seem to think that was important.

As the war progressed the world's ideas about armaments became more and more sophisticated: radar, rockets, jet aircraft, and ultimately even nuclear weapons. All of us in fields of engineering and applied sciences were called upon, by the planners in Washington, to serve on advisory committees, to carry out studies of future weapon systems, and to write reports. Dr. von Kármán was

more in demand than ever. General H. H. ("Hap") Arnold asked him to organize a major program to study the future of warfare; he assured von Kármán that he could enlist the talents of the best people in all of the relevant disciplines.

The general provided von Kármán with an office and assigned a colonel to assist him. Von Kármán came to Washington with his list of experts and started to go down the list with the colonel. All went well until the Professor came to the name of Sir William Hawthorne, a leader in jet-engine technology. The colonel was perplexed: "You mean he's an Englishman?" Kármán replied that Sir Will was indeed an Englishman, that he was clearly needed for this project, and that General Arnold had said "... the best people ..."

The colonel needed a minute to think about this. It hadn't occurred to him that there would be any foreigners in the study, but the British were our allies, after all. He added Hawthorne's name to the list.

But the next name on von Kármán's list was that of William Bollay, an officer of the U.S. Navy. The colonel put his pencil down; it seemed that this was going too far. "But, Colonel," von Kármán pointed out, "the Navy are surely our allies, too?"

This also required a moment's reflection on the part of the colonel. "Yes, yes, of course they are," was his conclusion, "not as close as the British, but a damn sight closer than the Russians!"

The study commissioned by Gen. Arnold was carried out; its conclusions were presented to the Air Force in a multi-volume report under the title "Toward New Horizons".

Dr. von Kármán apparently became Gen. Arnold's right-hand man for science and advanced technology. They became good friends. One of the General's staff officers was a handsome young colonel, Robert Rees, whose job was to look after the general's public relations. I don't remember whether it was von Kármán or Bob Rees--or perhaps I--who discovered that Bob and I were cousins! When he learned that in civilian life Bob was a newspaper reporter in Chicago, Dr. von Kármán asked him "not on that newspaper, I hope?" Cousin Bob had to admit that Yes, he worked for the Tribune.

[When he returned to California, Dr von Kármán told me, "You did not tell me you had such a nice cousin!"--almost the same words he

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used about my mother one morning after meeting her at some social occasion the evening before--"You did not tell me you had such a wonderful mother!" As if any man is going to tell his friends (and boss) that he has a wonderful mother!]

The top brass of the Air Force became convinced, about that time, that they ought to form, as a permanent feature, an advisory panel, and Dr. von Kármán agreed; The Air Force Scientific Advisory Group was formed. The first step was that a number of us were asked to serve and to pay visits to some Air Force installations whose missions were related to research and development. I remember making a trip with Drs. von Kármán, Hugh Dryden, and Vladimir Zworykin. We visited Eglin Field, in Florida, and the nearby missile range on the Gulf shore. The S.A.G. became the Scientific Advisory Board and began its official existence in 1946; again the chairman was Professor von Kármán.

When NATO (The North Atlantic Treaty Organization) was formed in 1949, Dr. von Kármán and his friends in the military services conceived the idea of an international advisory group, patterned after the S.A.B., to advise the NATO nations in the areas of aeronautical sciences. The nations greeted the idea enthusiastically, and, naturally, everyone agreed that von Kármán was the only possible choice to be its chairman: His reputation as an aeronautical scientist and as an advisor to military services was unique, and he had scores of friends, former students, and colleagues in all the countries. I think his identification with AGARD--the Advisory Group for Aeronautical Research and Development--guaranteed its success.

He and Hugh Dryden had the brilliant--and somewhat startling--idea of making AGARD an unclassified organization. One of the advantages of this enlightened policy was that specialists from countries outside NATO could be invited to AGARD's technical sessions. Dr. von Kármán says in *The Wind and Beyond*, "I envisioned a return of the vagantes, or wandering scholars of the Middle Ages, who would, I felt, act as ambassadors to lay the foundations of international good will."

AGARD still exists as I write this; its name has been changed a bit: the second "A" now stands for "Aerospace." It has an admirable record of technical meetings, papers, and books. To me it has

always seemed to be an international "in-group" for aeronautical specialists--an international fraternity of aerospace engineers with common interests, especially in areas of military applications.

When his beloved sister died in 1951, Dr. von Kármán established a charitable trust in her name, The Josephine de Kármán Trust, for the purpose of granting scholarships and fellowships to especially worthy college students. The Trust was administered by two committees, one for finance and one charged with the responsibility of choosing the awardees. The students may be studying in any field and in any American college or university. The original committee members were appointed by Dr. von Kármán, and subsequently have been chosen by the members themselves. Each of the two committees had four members, with the Professor serving on both--thus seven persons operate the trust in cooperation with the Trustee, a bank in Pasadena, in accordance with a charter drawn up by Dr. von Kármán in 1951. Following a tradition established by the original committee members, some preference is given to students in the Humanities--because Josephine (Pipö) herself was a student in that broad field.

In his will, the Professor left the biggest part of his modest estate to the Trust. As I recall, that provided the Trust with a capital corpus of about \$650,000--not bad for a college professor in 1963. [You can read in *The Wind and Beyond* how he thought he was cheated out of another twelve million dollars that he had hoped to leave to the Trust!]

Nevertheless, the Finance Committee and the Trustee have done their work well over the years--they were required to disburse the earnings each year in order to maintain tax-free status, but they managed to cope also with inflation. In contrast, our colleges and universities have allowed many of their scholarships and endowments to suffer the attrition of inflation, so that distinguished chairs and similar grants no longer serve their purposes.

I had the honor of being named by Dr. von Kármán as his successor on both committees, so I have served in those capacities since 1963. The duties of the Fellowship Committee are rather demanding in January and February of each year, when we receive hundreds of applications and have to choose from them about a dozen de Kármán

Fellows. Fortunately I have the assistance of Mabel. There are also some very gratifying aspects in these chores: For one thing, I am annually amazed by the high quality of our applicants; in these days, when we are bombarded by stories of the disintegration of our schools and our society, it is surprising and reassuring to read these impressive applications. The other great reward is the pleasure of meeting with the two committees.

One of the original committee members was Professor René Bellé, Chairman of the French Department at the University of Southern California--a department, incidentally, with which Pipö had warm relations. Bellé had immigrated to Los Angeles some thirty years earlier, but was still, in the 1960s, a typically French scholar in dress, demeanor, and accent. [His successor on the Fellowship Committee was Professor Arthur Knodel, another scholar in French language and literature, who has an incredible talent for mimicry--at least, mimicry of Professor Bellé:

(Meeting a class at the beginning of a term at U.S.C.) "For many years I 'ave 'ad a great love for your language, English. Unfortunately ziss love 'as not been requited ..."]

But on one occasion, when the Committees met in California, I overheard Professor Bellé saying to Mr. Eddie Beehan, one of the founders of Aerojet and an original member of the Finance Committee, "But Eddie, zey cannot use ziss name. It is very, very bad!"

"I know, René, I told them that, but they like the name and want to use it."

I sensed something interesting. I sidled up to Bellé, as we walked toward lunch after the Committees' deliberations. "What name is that, René?"

"Aetron," he replied. "Aerojet has a wholly-owned subsidiary called 'Aetron', which means Architectural and Construction Engineering. But zey cannot use ziss name! It is very bad!"

I had to admit ignorance.

"Do you know French?" he asked. "Well, un peu", I replied. "Do you know ze French word merde?" "Yes, I know that much."

"Well," he said, "aetron is ze vulgar word for merde!" [That's going pretty far.] Sure enough; I was visiting the Malinas in

Paris, and I told Frank and Margie this story. The response was a dull thud. Frank said, "Your friend Bellé is pulling your leg." I replied that Professor René Bellé had never pulled anyone's leg. Margie went to their bookshelves and returned with an old, well-used copy of Larousse, the dictionary. I was vindicated; the dictionary defined aetron (or ætron or étron) just as René had done.

Nevertheless, near the Charles de Gaulle Airport there were signs on the rooftops of factories reading "AETRON" in letters ten feet high.

On a later occasion, when we, the Committees, were having lunch after our annual meeting in Pasadena, I asked Eddie Beehan, "How is Aetron doing?"

He replied, "Aetron? Haven't you heard? Aetron has gone down the drain."

Our luncheon companions overheard this exchange and laughed appreciatively at Eddie's clever pun. But Eddie only looked around the table, puzzled by the laughter, until it dawned upon him what a witty thing he had said; then he too laughed--"Yes, ha ha! Down the drain!"

Once when Dr. von Kármán and his sister were visiting Paris, they heard Hungarian being spoken in some public place--perhaps a post-office--and discovered a community of young Hungarian intellectuals. Of course Tódor introduced himself and Pipö and began a conversation. He wondered why all these young people were in Paris; he told Pipö he doubted that they all spoke French. Sure enough, by speaking French to them he found a young man who spoke only Hungarian.

He asked the young man why he was in Paris. "Because I am a lyric poet," replied the boy in Hungarian. "There is going to be a renaissance of lyric poetry, and I want to be here when it comes."

Dr. von Kármán told me, "I asked this fellow, 'You are convinced there will be a renaissance of lyric poetry and that it will happen in Paris, but can you also be sure that it will be announced on the bulletin boards?--and in Hungarian?'"

I became convinced over the years that the Hungarian word for "Yes", which is *egin*, must actually mean "Yes, but"--in other words a kind of "No"--but that Dr. von Kármán translated that as just

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"Yes". For example, when Dean Hollister invited him to come to Cornell and join the new School of Aeronautical Engineering, as I have described at the end of Chapter 5, I am sure the answer was supposed to be a kind of "No", but the Dean thought it was "Yes".

Duncan Rannie was part of a "traveling seminar", headed by Dr. von Kármán, that visited all the Western European countries after World War II, giving technical talks on wartime progress in aeronautical engineering. Rannie said that at every stop von Kármán was presented with the key to the city by the Mayor and was invited by the Rector of the local university to become a professor and director of a laboratory.

"As far as I could make out," Duncan told me, "he accepted both, every time."

Later, on a trip to Washington and New York, I met Clark Millikan in Washington, and he told me, with considerable satisfaction, that von Kármán had met, the day before, with Dr. Lee DuBridge, President of Caltech, and had agreed to return to Pasadena and become active again in GALCIT. Clark said that DuBridge had put the question bluntly: "We have to know whether you are coming back to Caltech or not," and that von Kármán had said that he was.

Clark and I agreed that that was good news for everyone--that our old Boss was too old to be living out of a suitcase in Washington and Paris.

I had a date with von Kármán in New York the next day. Before I had a chance to bring up the subject he told me that he had met with DuBridge in Washington: "He wanted to know am I coming back to Caltech. I told him 'No'."

Dr. von Kármán had a reputation as a "ladies' man", which I believe is a rather old-fashioned term for a man who enjoys the company of attractive women and is appreciative of their charms. In my opinion it was a reputation that he fostered; I think that he wanted to be seen as a romantic Hungarian bachelor. Some ladies, such as the wives of some of our students who were military officers, made a great deal of his romantic behavior--sometimes, I am sure, with a bit of exaggeration. Some husbands were quite flattered by the attention paid to their wives by the famous professor.

I watched him turn on his Continental charm on several occasions. An example: at a party of our aerodynamics specialists and

their wives he danced with one of the young wives--who was indeed beautiful--and at the end of that "set" escorted her back to her husband, Jack, who was standing near me. He accosted Jack: "Young man, you are supposed to be a scientist, and scientists are dedicated to the Truth. But your wife tells me that you do not tell her that she is beautiful!"

The rest of us, who heard this declaration were amused, but the young lady herself was quite overwhelmed, blushed beautifully, and whispered to me, "Isn't he wonderful!"

On several occasions he came to me and began conversations with the disclaimer, "Now, Bill, as a lonesome bachelor I, of course, do not understand these matters, but you must tell me ..." And what followed would be some question, observation, or opinion concerning married life and the relationships between husbands and wives. There was, actually, little that he didn't understand.

One such occasion was particularly bizarre: He had been visited, in his Caltech office, by the father-in-law of one of his former students, who stated that "Your student abuses my daughter!"

He reported that the former student, when he came home at three a.m. from a committee meeting, demanded his "rightful sexual privileges", and, when the lady demurred, removed the belt from his trousers and beat her. I don't know what Dr. von Kármán said to the distraught, aggrieved parent, but what he said to me was, "At three a.m., Bill! Is that not unreasonable?"

What I will never understand is why the father-in-law decided to make his complaint to the son-in-law's former professor.

Von Kármán surely had an impressive mathematical talent, which manifested itself during his childhood, as you can read in *The Wind and Beyond*; according to that record he also had prodigious arithmetical abilities--he could carry out rapid numerical computations in his head--which is not the same thing at all. A remarkable thing about his mathematical skills is that they were mostly intuitive, rather than skills learned in formal classes.

In 1936 I was taking the splendid, demanding course in Mathematical Analysis taught by Professor Morgan Ward, as mentioned in Chapter 3. It was a course characterized by difficult homework problems, and we were allowed--even encouraged--to work on them in teams; Bill Bollay and I often worked together. Dr. von Kármán came

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into the office where we were covering blackboards with calculations, and, characteristically, wanted to know what we were doing. We explained that we had to prove that a certain function was integrable over an infinite interval. "How will you do that?", he wanted to know; we replied that we thought a certain theorem would apply--all we had to do was show that our function satisfied the postulates of that theorem.

He was fascinated. He said, "I never learned theorems like that! Let's look at that function." He soon had a sketch of the integrand on the blackboard; it had infinitely many singularities in the infinite interval. After a couple of tries he was able to find another function, nice and integrable, whose integral, he could show, was smaller, in spite of the singularities. This proved what was to be proved, without recourse to any theorem.

Bill and I wrote up von Kármán's proof and handed it in to Ward. We felt a bit guilty when Ward called the class's attention to "an ingenious solution submitted by Mr. Bollay and Mr. Sears" and had us present it to our fellow students.

On another occasion he and I were writing up a joint paper, late at night at his house. He found that I had evaluated a certain integral beyond the limits of its validity and had appealed to "analytic continuation" for justification. [That is the process of extending a function beyond its ostensible limits on the principle that it remains analytic.] He didn't like analytic continuation --thought it was sloppy mathematical technique. He worked and worked, looking for a nicer technique, but I was getting sleepy; I muttered that other authors appealed to analytic continuation, but that didn't impress him. Just as I was dozing off he slapped his pencil down on the desk and exclaimed, "You know, I am born a mathematician!" [He meant "a born ..." but always misquoted that American cliché a little.] He had found a neat way to render my integral analytic in a larger realm before identifying it; he didn't need analytic continuation. That other authors had resorted to it only made him more self-satisfied.

He used the same, slightly misquoted, cliché on another occasion: I gave a failing grade to a GALCIT student whose father was a kind of scholar. The father came to von Kármán to complain that I

had had the effrontery to do such a thing. [I assure you, Dear Reader, that the failing grade was well deserved.] I learned about the father's protest only when Dr. von Kármán came to my office grinning mischievously. I said, "You look like the cat that ate the canary." What he said was, "You know, I am born a diplomat!"

I replied that I didn't know any such thing. "Yes," he insisted, "Dr. ----- came to me to complain that you flunked his son. I told him 'Ah, Doctor, you must remember you are not the first savant whose son did not follow in his father's footsteps.'

"And the old fool went away happy!"

In 1956 his *Collected Works* were published. An effort was made by us, his colleagues, to edit the papers--five volumes--to remove typographical errors or other imperfections. Von Kármán was delighted to learn from us that the famous, seminal paper he co-authored with Max Born, on the theory of specific heats, had more typos than any other. He remembered that Born had told him, "I learned a lot from working with you, Kármán, but happily I never learned your *akademische Schlamperie*" [which means "academic sloppiness"]. And it was Born, not he, who did the proofreading of their joint paper back in 1913!

GALCIT was an institution singularly free from quarrels, intrigue, and the kind of intra-departmental politics that are so common in colleges and universities and so popular with writers of modern novels. Mabel and I knew of only one such conflict: It involved Professors von Kármán and "Maj" Klein, and the subject was the former's beloved little sister--obviously a delicate issue.

Pipö accused a hairdresser of stealing jewelry from her handbag while she was under the dryer; it resulted in a court trial and was reported in detail in the local newspaper. The accused lady vehemently declared her innocence, and Maj, unfortunately, told his colleagues that he was convinced that Pipö was lying. His remark was quoted to Dr. von Kármán. I was told that the Director then announced to Maj that communication between them would henceforth be limited to what was required to carry out their duties in the Institute of Technology.

Dr. von Kármán was in residence with us at Cornell several times

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during the years between 1946 and his death in 1963. One of these periods was the occasion of his Messenger Lectures (1953).

The events did not begin auspiciously. Just a couple of weeks before the lectures were to begin I had a call from him from Washington; he was in the Mayflower Hotel, ill, suffering from congestive heart failure. He wanted me to send him one of our bright graduate students who could stay in Washington and assist him with the preparation of the lectures.

One of our brightest students was Nelson Kemp. But Nelson, besides being bright, was brash. He was the type who would say to me, "That's a new necktie you're wearing, isn't it? Much nicer than the ones you usually wear." And when he met Professor and Mrs. Rott one night, coming out of a cinema, he said, "Oh hello, Dr. Rott. Taking your own wife to the movie tonight, eh?" But I had a fatherly talk with Nelson, explaining what I wanted him to do for our distinguished, old, sick friend in Washington. "I understand," he replied, "You want me to go to Washington and throw Dr. von Kármán's weight around at the Library of Congress and all."

This was not too encouraging to me. I explained that I wanted him to be relaxed and easy-going--to sit and talk to the old man--not to be a nagger or a nuisance. Nelson now understood me perfectly: He said, "I understand; you don't want me to behave with Dr. von Kármán the way I usually do with you!" I agreed.

When I told Miki Rott what I had decided, he said I must be crazy. But I was right: Nelson handled the assignment perfectly. He and von Kármán became good friends; they found that they not only had the same given name ("Nelson", for Nathan, short for Jonathan, "Gift of God", and "Theodore", meaning the same) but also, according to family lore, were both descendants of the legendary rabbi of Prague who built the golem.

Dr. von Kármán called Nelson his "aide de Kemp".

They came to Ithaca and we housed our distinguished guest in the Research Wing of the Cornell Infirmary. [He was supposed to occupy one of the three rooms there, but it was just prior to Income Tax time, and Dr. von Kármán needed a place to spread out his receipts and records, so he distributed them on the bed in a second room. When Nelson and others of us came to help him with the lectures, he also needed a place for us to sit, so he more-or-less took over the third room as well.

It is not surprising that Dr. Norman Moore, Director of the Infirmary, was a bit unhappy about his distinguished guest and patient. Actually the relationship was not too bad until Dr. von Kármán asked Dr. Moore, "Do you believe, Doctor, that Americans have more heart trouble than other nationalities because they live a faster life?" The physician agreed that Yes, that was well known.

"Ja, it is well known, but it is not correct. Americans have more heart ailments because their doctors are no good."

[What he believed was that American doctors prescribe too many medicines: drugs that conflict with one another.] Years later, when I'd meet Norm Moore at lunch at the Faculty Club, he'd ask me "How's that old friend of yours?" To which the answer was "He's fine. Still going strong." Moore was nonplussed: "Well, he'd better learn to have more confidence in his doctors, or he won't go on much longer."

The subject of the Messenger Lectures in 1953 was "Aerodynamics: Selected Topics from a Historical Perspective"; as I have indicated earlier, the task was to make it all interesting and understandable to an unusually intelligent, educated audience of non-specialists. My faculty colleagues and I, as well as the "aide de Kemp", worked diligently to help prepare the lectures, especially with diagrams, slides, and photographs. Dr. von Kármán brought drafts of the text to us, to try them out on us. On one occasion he read some proposed sentences to Professors Rott, Kuo, Itiro Tani (visiting from Japan), Carlo Riparbelli, and me.

All of these colleagues nodded solemnly; they liked what they heard. But I didn't. I said, "No. I don't think you can say that."

He looked at them and at me. "You know, Bill, here you are, surrounded by one Hungarian, one Chinese, one Japanese, and one Italian. Nobody will walk through a doorway in front of you, and nobody will give you an honest opinion!"

At another point, writing about Ludwig Prandtl, who was the son-in-law of another famous Professor, August Föppl, von Kármán quoted an old saying to the effect that "In German academic circles talent descends not from father to son but from father-in-law to son-in-law." Suddenly he said "Oh Gott! I can't say that, because Hans and Rose Bethe will be in the audience." [Rose Bethe is the daughter of Professor Paul Ewald.]

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I assured him that Hans and Rose both had well-developed senses of humor and would enjoy the joke--and enjoy it even more if he would remind the audience that it fit the local scene.

He began the first lecture with slides of photos of Newton, d'Alembert, Helmholtz, Rayleigh--the giants whose work laid the foundation for aerodynamics. He was standing so close to the screen that he could't really see them. When the picture of Helmholtz came up, Dr. von Kármán said "This is supposed to be Helmholtz--that's not Helmholtz, is it?" Sitting in the front row, I assured him that it was, and he proceeded.

At the end of the lecture Professor Peter Debye came down the steps of the lecture hall, toward the podium and the speaker. They had not seen each other for some thirty years, but von Kármán recognized his old friend immediately: "Peter Debye!" he exclaimed.

Debye stopped on the steps in mock amazement: "What? You don't know Helmholtz but you know me?"

I think von Kármán charmed his audience completely; it was clear how hard he was trying to make his subject accessible and entertaining to his audience. The lectures were the basis for the admirable, delightful little book *Aerodynamics: Selected Topics in the Light of Their Historical Development*, (Cornell University Press, 1954). That book has been translated into Japanese, Spanish, Italian, German, and perhaps other languages. It has been required reading for my students since its publication.

He also charmed Pope John XXIII when they met at the Vatican. The occasion was a meeting of AGARD personnel in Rome. I was not there, but was told that the Pontiff and the Professor looked very much alike--short, stout, and gray-haired--as they stood together on a rostrum. The Pope, obviously having had the words of the acronym "AGARD" explained to him, delivered a short speech in which he thanked the members "for their advice". The Professor responded: He thanked the Pope for his kind words and said to the AGARD members, "I am delighted that the Holy Father, although he has direct communication with Heaven, as we all know, thanks us for advice."

This personal charm was certainly one of the qualities that made it possible for him to be a successful advisor to military officers and industrialists. He had all the professional qualifications and

had received wide recognition in science and in the academic world, but it was important for him to communicate with people outside that world who might be frightened by his scientific stature and put off by his Central European manners and accent. His wonderful sense of humor was a great advantage.

In meetings of committees, boards, and panels he often found that he could best make his point by quoting Sam Goldwyn:

"Listen, I have a vunderful idea! But I don't think much of it."

"OK, OK, I hear what you say, but you can include me out!"

"My answer I can give you in two simple words: im possible!"

When he (or someone else) got a name wrong, he liked to tell the story of Sam Goldwyn and the well-known author, Louis Bromfield: Goldwyn had hired Bromfield, brought him to Hollywood, and provided him with a palatial apartment and office. But he didn't give him any work to do, and Bromfield became unhappy. He came to Goldwyn and announced that he was quitting and going back to New York.

"What's the matter?" asked the tycoon, "don't I pay you enough? You don't like the apartment?--the office?"

Bromfield tried to explain: "All that is perfect, Mr. Goldwyn, but you don't give me anything to write."

"Aha! I see the problem now," said Goldwyn. "You don't understand. We didn't hire you to write. We hired you for your famous name, Mr. Bromberg."

Dr. von Kármán often used humor to make a point. GALCIT had contracted with N.A.C.A. to carry out research on the effects of curvature on the turbulent boundary layer. The studies taught us a great deal about the turbulent layer and the uses of hot-wire anemometers, but when the time came to renew the contract the effects of curvature were still a mystery. Dr. George Lewis, N.A.C.A. boss, pointed this out: GALCIT had not really achieved the objectives of the first contract.

Von Kármán told Lewis, "I'm sure you remember, Doctor, that Christopher Columbus made a contract with Queen Isabella to find a

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water route to the Orient. He didn't fulfill the contract, but it is generally agreed that the results of his effort were worthwhile!"

Sometimes, I'm afraid, he used a **Big Falsehood** technique. One time, when I was heading the computing staff of the ten-foot wind tunnel, he appeared at the balance room on the third floor of GALCIT with an elderly couple in tow; they were wealthy prospective donors to Caltech and Chairman Robert Millikan had asked him to show them around GALCIT.

I opened the necessary doors for the trio and escorted them to the working section of the tunnel, one floor down. Dr. von Kármán pointed out the model mounted in the working section for test and explained the rigging system that held it in place. [In those days the rigging system was made of wires, which had to be in tension, and the model was mounted upside-down in the tunnel, so that "lift" was a downward force. There was a wire running down from the model to a weight below the tunnel to assure that the rigging would be in tension even at "zero lift" and modest negative (upward) values.]

He skipped rather lightly over that particular little idiosyncrasy, and the elderly visitor was puzzled; he asked why the model was upside-down. My distinguished Boss replied with an amazing lot of nonsense: something about lifting the weight that they could see below the tunnel. It didn't make any sense at all, and undoubtedly my face showed my bewilderment. The visitors seemed satisfied; I was thanked, and I returned to my desk on the third floor.

Fifteen minutes later Dr. von Kármán was back there too. He said, "I saw in your face that you did not approve of my explanation of the rigging system, Mr. Sears." I tried to protest.

"No, you must not apologize. But I must teach you an important rule. When you are talking to technically illiterate people you must resort to the **Plausible Falsehood** instead of the **Difficult Truth**."

And he proceeded to tell me the story of an earlier encounter with the **Difficult Truth**--one that I must confess I can hardly believe: He said that he was a junior officer in the "Austrian-Hungarian" Army in World War I. [You can read about that in *The Wind and Beyond*.] His unit had the honor of a visit of the Crown Prince himself, and Leutnant Kármán was asked to show the royal

visitor an engine: the first six-cylinder engine the Army had received!

The Crown Prince was impressed, and he asked the Leutnant a technical question, one that suggested that he was surprisingly knowledgeable: "But, Leutnant, is this not a four-stroke engine?"

However, upon being assured that it was, indeed, a four-stroke engine, he was baffled: he couldn't understand how a four-stroke engine could have six cylinders.

Now, what Dr. von Kármán told me and I hardly believe, is that he answered, "I am very sorry, Your Royal Highness. I failed to make clear that one cylinder on each end of this engine is a spare." And the Prince went away happy.

Sometimes his famous humor was simply silly. I don't think it will diminish your respect for the Grand Old Man--perhaps it will endear him to you--if I recount here some of the silly jokes that he enjoyed at least enough to tell them to us:

On that automobile trip to western Florida with Dryden and Zworykin in 1945 we were given a demonstration, at one of the Air Force establishments, of the use of television to steer unmanned weapons. It was the first time that I--and perhaps some others of our party--had ever seen television, but Dr. Zworykin was the world's expert in the subject: the inventor of the picture tube. So, as a prank, Dr. von Kármán did not introduce us to the young officer who demonstrated the system to us; instead he suggested that he explain television to us in some detail. He introduced us, including Zworykin, only when the demonstration and the explanation-in-some-detail were finished. The young lieutenant recognized the name, of course, and was suitably embarrassed, but I think he realized that it was all in a spirit of fun--some big shots being silly.

On another occasion I myself had the experience of being a victim of his teasing. Professor Richard Courant, the famous applied-mathematician, visited him at GALCIT. I learned later that von Kármán was not altogether an admirer of Courant--always referred to him as "kleine Courant"; I guess Courant, although an applied mathematician, was a bit too "pure" for von Kármán's taste. I was near the stairwell on the second floor of GALCIT when von

Kármán and his guest came up the stairs. Von Kármán introduced us: "Professor Courant, I want to introduce Dr. Sears. Dr. Sears took a course that used your book (*Methoden der Mathematische Physik, Band I*) as a textbook, and he tells me he didn't like it."

The result was, of course, that I protested: "No, no. Professor von Kármán misunderstood me. It is a splendid book! (etc.)", while Courant was insisting, "I am so sorry about the book. You must tell me all about its shortcomings, Dr. Sears!" And von Kármán watched us, grinning, obviously enjoying the whole thing hugely.

At a meeting of the Scientific Advisory Board we received a request (via a congressman) that we explain certain mysterious green lights in the western skies. Our distinguished Chairman responded by appointing a two-man subcommittee to look into the subject: One member would be Professor Joe Kaplan, who had discovered green lines in the spectrum of nitrogen. With a mischievous smile the Chairman appointed the second member, a priest, professor of geology at Fordham. "I suspect that the green lights in the sky may be in one, if not the other, of your fields of expertise, Father."

At another meeting of the same advisory board he himself was the victim of his own love for jokes and riddles--but an innocent victim, as I'm sure you will agree. Some of the committee members, especially Professor George Gamow, a brilliant, ebullient physicist and astrophysicist, loved mathematical puzzles and riddles. Especially at lunchtime they challenged us with their latest and most difficult. We lunched with the high-ranking Air Force officers, and I'm afraid that these clever puzzles were often well above the heads of some of these gentlemen. For example, Gamow counted heads--there were about 20 or 25 of us at lunch--and quickly wagered that two of us must have the same birthday. [He was right. Suppose there are 20 of us; given any one birth-date there is only a probability of  $19/365$  that someone else has the same date, but we are not asking for a certain date--only the coincidence of any two of the 20 dates, so the probability is much greater.]

This calculation of probabilities was flying back and forth over the lunch tables, and it was clear that some of the officers, including General "Tooey" Spaatz, were being left out. Dr. von Kármán, the "born diplomat", wanted to get the General back into

the conversation, so he said, "Here is a cute one, General," and posed the one about the guy who has socks of two colors in a drawer and, in the dark, wants to take out the smallest number of socks that will assure that he has a matching pair. The answer, of course, is three socks, regardless of the total number in the drawer. But General Spaatz, after pondering the question, gave the wrong answer. There was a protracted silence during which, I suspect, we were all thinking that von Kármán had gotten himself into this and now could get himself out. He did; not especially gracefully, but by changing the subject to something other than puzzles.

He liked the story of the millionaire who proposed to buy a Rolls Royce--he wanted the best car available--he didn't want one that would not be perfectly dependable. But, after writing a check for the purchase he noticed that there was a hole below the radiator; "What's that for?" The salesman explained that it was the place to insert a crank if the engine needed to be hand-cranked. The customer tore up the check. "I told you," he said, "that I wanted a car that would be perfectly dependable, not one that might have to be hand-cranked!"

He walked out of the salesroom. The salesman called him back: "Sir, when you get up in the morning and stand before the mirror, do you not see two brown spots on your chest, here and here?" The gentleman admitted that he did, but he didn't see the relevance.

"Those brown spots are there," said the salesman, "for the case that you might have a baby. The hole below the radiator of the Rolls is for the case that you might have to crank the car."

The man rewrote the check.

(One of the silliest:) Dr. von Kármán was born in 1881, which is a date that reads the same if viewed up-side-down. So, he told me, he was convinced that he would die in 1961, which was the next date with that rather trivial property. [And the next one will be 5009!]

He asked Mabel, "Do you know what is the Perfect Life for a man?"

She didn't. He defined the Perfect Life: "To have Japanese wife, Chinese cook, and French mistress."

She was insulted and took the bait--hook, line, and sinker: "Is

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that so! What about us American girls? "

"Oh, I forgot to say:--and American kitchen equipment."

So that is my brief sketch of my great friend and teacher. Did he have any faults? In my *Physics Today* paper I asked that question and answered that at least in one respect he was quite undependable: the recommendations he gave for us, his students and junior colleagues, were far too kind to be of any use.

A fault that he did not have was false modesty. He knew well that he was unusually gifted--but he did not expect the world to grant him special privileges for that. When the son of one of his former students visited him in Pasadena, he told Dr. von Kármán, "I am going to be an engineer, and I will be happy if I can be half as great as you, Professor."

The Professor asked me, the next day, with that mischievous twinkle in his eye, "What do you think, Bill, is fifty percent a modest wish?"

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[Note added in proof: A new biography of von Kármán has recently appeared: M. H. Gorn, "The Universal Man. Theodore von Kármán's Life in Aeronautics". (Smithsonian Press, 1992) It will be found to provide remarkably detailed data and documentation concerning its colorful subject.

Unfortunately, when the biographer tries to tell this anecdote (which I told him) he ruins it--or allows a typographical error to ruin it: Instead of "fifty percent", the book says ".05". (page 156)]

## CHAPTER 9

## FLYING

Back in Chapter 4 I told you about the beginnings of my private-flying career--a few landings and takeoffs in the Naval Reserve's N3N biplanes at Long Beach, but mostly in the 65-horsepower Porterfields at Monrovia Airport, when I went through the "CPTP" program. My log book ("Volume 1") shows that I had my first lesson in that program on 21 September 1940; my instructor was Al Blackburn; we did "shallow & steep turns" in a flight of 30 minutes. On 13 October, when I had accumulated three hours and 50 minutes, we did takeoffs and landings and my instructor noted "Doing good", but, two days later, only "Fair". On 30 October I made my first solo flight.

By the middle of March I was ready for dual "cross-country" (Monrovia, Ontario, Fullerton, Vail, El Monte, Monrovia), and on 20 March--our fifth wedding anniversary!--I flew solo from Monrovia to Ontario, Pomona, Fullerton, and back to Monrovia. And on the next day passed my flight test and became a licensed pilot.

All that was accomplished just before we moved from Pasadena and I took up my duties at Northrop. I found that there was a Northrop Flying Club, which operated a Piper "Super Cruiser" from Western Airport, an airstrip down on Western Avenue about halfway to Long

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Beach. I joined it and got a few more hours in my log book before the U.S. was at war and private flying was pretty severely restricted. When I moved to Ithaca in 1946 I had logged only 124 hours, and a lot of that was acquired as copilot on flights of Northrop's utility airplanes--a single-engine seven-place Bellanca, a Cessna twin (UC-78) called the "bamboo bomber"--and even the P-61s. (Our test pilot, after takeoff with me in the front gunner's seat, would exchange places with me--a bit difficult in that narrow fuselage--and let me do the flying when mostly straight-and-level flight was required.) I had the honor (?) of being the first to test, in flight, the P-61's pilot's relief tube, which I did over Hearst's castle at San Simeon. (It worked satisfactorily.)

In 1942 it was decreed by the Civil Aeronautics Administration that only American citizens would be allowed to pilot airplanes in the U.S. All of us who wanted the privilege had to present evidence of citizenship, so I hustled over to the C.A.A.'s office at Mines Field with a photocopy of my birth certificate--that heart-rending one I've told you about, which shows that the poor child had to wait 26 years for his mother to give him a name. But the young official handed it back to me; it seemed that the regulation, which I'd never read, said that a photocopy wouldn't do, unless it was a certified true copy. Frustrated, I was turning to leave his office when he said, "Hey! It says here that you were born in the Northwestern Hospital in Minneapolis. That's where I was born! Well, I can't reject this birth certificate." Whereupon he turned it over and wrote "Compared with original and found to be exact duplicate. L.D. Pudney, C.A.A. Inspector, Mines Field, Inglewood, Calif. 5/9/42"

But it wasn't long before they required flight plans for all flights in the Los Angeles region and introduced other restrictions that made it impractical to do much private flying there. Some of us amateur pilots organized a week-end trip to Owens Valley, to an airport (Olancho) outside this restricted region, where we could practice landings and takeoffs, etc. When we arrived there we found that the trainers they had for rent were two-place Taylorcrafts with side-by-side seating. Unfortunately the Porterfields I'd learned to fly in, as well as the Piper Super Cruiser--and even the P-61!--had the pilot sitting in the middle and operating the throttle(s) with his left hand and the control stick (wheel) with his

right hand. When I tried to check out in the Taylorcraft, with the throttle on my right and my left hand on the wheel, my landings were pretty rough; it looked as if I'd spend the whole day (and all my money) checking out. Actually it took an hour, and it was a good lesson for a smart-aleck pilot with a Ph.D. and a whopping 62 hours total time.

In the Spring of 1943 we had to move our experimental flights of the little flying wing (NLM) from Muroc Dry Lake, which was flooded, to Silver Lake near Baker, CA. I found that I had lots of time to kill in the afternoons there, and there was a small airport at Baker with Pipers and Porterfields to rent, so I checked out and got a little flying time. I remember discovering, while taxiing out in one of their airplanes, that the airspeed indicator was completely inoperative. I was about to taxi back to the line and change to another airplane when it occurred to me (a) that I ought to be able to fly without an airspeed indicator and that (b) this was the place to do it, with perfect weather, a runway about five times as long as I'd need, no air traffic nor air-traffic control. And I had booted students out of Caltech's CPTP program if they couldn't at least fly successfully around the field with the airspeed indicator covered up! So I went flying, and had no trouble.

On another occasion, my student, colleague, and fellow Naval Reserve officer, Fink Fischer, and I arranged to schedule a flight in one of the SBC4 dive bombers, from Long Beach, to meet the Northrop people at Silver Lake; Fischer wanted to see the NLM and watch the flight tests; I wanted to fly the SBC4. [And you, Dear Reader, should be aware that none of us who fly around in (unscheduled) airplanes can resist the temptation to arrange wonderful, complicated plans that require us to fly here and there, meet this and that person, pick up somebody's car there or here, drop somebody or something someplace, and so on ...]

So here we-all were on the dry lake, Silver Lake. It was one of the occasions when Dr. von Kármán, accompanied by Frank Malina, had been driven out from Pasadena, before dawn, by Mr. Northrop. As usual there was a delay in the flight testing, and we-all basked in the desert sunshine while the Northrop crew, headed by hard-boiled Tom Ruble (who was once a Navy Chief Petty Officer), made some modification or other to the NLM. Fischer was still a degree candi-

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date at Caltech and wanted, I am sure, to impress the Director of GALCIT. So he invited the Director to climb into the SBC4, and he proceeded to point out to him all the controls and indicators in the cockpit of the dive bomber.

Dr. von Kármán was not paying careful attention to the lecture. He was idly operating some of the switches, when suddenly there was a little "bang", the sounds of small aluminum panels falling off the SBC4's wings and of gas flowing through small tubing: the flotation gear--big yellow bags between the upper and lower wings of the biplane--was being deployed and filled with carbon dioxide. Frank Malina leaped off the wing and placed a safe distance between himself and the dive bomber. [Later the Professor claimed that the lever he operated was marked "To be operated only after landing on lake or ocean" ... or something like that. And we were on a (dry) lake, after all. But the truth is that he was bored by Fischer's lecture and was just killing time by operating switches.]

There was no way to get the carbon dioxide out of the bags so that they could be stuffed back into the little compartments of the wing whence they had emerged. We climbed up onto the lower wing and explored how the bags were connected to the airplane. It looked invulnerable. Fischer and I stood on the lower wing and hugged one of the yellow bags between us, hoping to deflate it. No go! All we accomplished was to make Tom Ruble and his crew laugh with joy. (Fischer and I were wearing the uniforms of officers of the U.S.Navy.)

There seemed to be no way that the SBC4 was going to be flown off Silver Lake. Lt. Fischer and Lt.(j.g.) Sears would probably be court-martialed. Jack Northrop climbed up on the wing, put his hand through the little opening that the bag had come out of, and ascertained that the cable attached to the bag was fastened to the wing spar by a nut and bolt. He borrowed a small wrench from Ruble and undertook to disconnect the cable, working "blind" with one hand, sweat running down his face in the desert heat. With great patience and perseverance he succeeded. We wanted to relieve him from going to the other side of the airplane to remove the other bag, but he said that now he knew how to do the job, so he would do the other side too.

Fischer and I flew back to Long Beach with the flotation bags stuffed into the cockpit, and successfully negotiated a secret

agreement with Tom Ruble's counterpart, another hard-boiled Chief, who agreed that it wasn't necessary to report the incident.

Colonel Donald L. Putt, who had been one of our graduate students at GALCIT, was made Project Officer of the XB-35 project, with his headquarters at Wright Field near Dayton, Ohio. On one of his trips West he was flying a well-seasoned B-18--a bomber related to the Douglas DC-2, DC-3 family--one of the major differences being that the forward part of the fuselage leaked badly in rainy weather. He invited me to fly with him from Northrop Field in Hawthorne to the Army Air Force's test station on Muroc Dry Lake, where we would join the Northrop flight-test people. So I got a bit of B-18 time in my log book ("Time actually flown by co-pilot is logged," the book says.)

The Air Force's operation at Muroc was primitive: They had some temporary, tent-like structures on the east side of the dry lake; the officers and men actually wore those large, broad-brimmed hats--"campaign hats?"--that soldiers wore in World War I. Don had been there before and assumed that that was where we would park the B-18, meet the Northrop crew, and stay overnight. What he didn't know was that modern facilities were being built on the west side, including a control tower. We were in communication with the tower and carried out our landing on the lake bed near the tents on the east side. On the ground, we asked for taxiing and parking instructions and were told to taxi toward the control tower--which we didn't see. Don asked "What direction from our present position?" and was given a compass heading. We started out in that direction but still didn't see the tower. He asked "How far to the tower from our present position?" and was told ten miles. He then asked if it would be permissible to take off and fly to the tower, and the permission was granted. I thought I heard the controller giggle.

The facilities being constructed on the west side were actually the beginnings of the elaborate facilities now known as Edwards Air Force Base.

[The incident reminds me a little of an experience of Bob Stanley, who has been mentioned earlier in a couple of places. Bob made a successful, record-breaking soaring flight from Harris Hill, near Elmira, N.Y., to Idlewild (now John F. Kennedy) Airport on Long Island. Actually, Idlewild was just being built--wasn't yet open to

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any aircraft operations, and the glider, after landing, was quickly approached by a police car, whose occupants informed Bob of that fact and that he would have to depart immediately. He explained: "But this is a sailplane! I've just set a record by flying it here from Elmira!"

The police were unimpressed. "Doesn't matter what kind of an airplane it is. You flew it in here and now you've got to fly it out of here, with no more delay." Colonel Putt and I got a much more friendly reception at Muroc.]

Donald Putt was promoted to Brigadier General not long after this. After the war he played an important rôle in the Air Force, responsible for research and development. One of his duties--which he enjoyed hugely--after more promotions, was to work together with Dr. von Kármán and the Scientific Advisory Board.

In 1944 the government offered the public the opportunity to purchase some of its airplanes, including such primary trainers as the Waco UPF biplane. My friend and fellow Northrop employee, test pilot Harry Crosby, quickly made a bid on one of these and bought it. I got a phone call from him asking me to take a day off from Northrop business and to go with him to Phoenix to acquire it and fly it to Glendale. There was, obviously, no way that I could pass up such a flying adventure! He had managed to get the UC-78, the Cessna "bamboo bomber", assigned to himself for "tomorrow"; we would fly it to PHX, check me out in the Waco, and fly in formation back to the L.A. area. If you don't understand how incredibly exciting this prospect was to me, it is because I have failed completely to explain the lure of flying to us amateur aviators! I would even be flying in an open cockpit and wearing helmet, goggles, and (at least in spirit) a white silk scarf!

We flew to PHX, he signed papers and became the owner of the Waco, and we undertook--in somewhat of a hurry--to check me out in the Waco. It was a beautiful biplane, with lots of power, cockpits open to the breezes, control stick in right hand and throttle in the left. I donned the helmet and goggles and proceeded to make a few takeoffs and landings, there at Phoenix, under his supervision.

It wasn't quite that easy; the Waco was equipped with a tail-wheel lock, controlled from the cockpit. This was something new to me. You had to unlock the tail wheel to taxi and lock it before

takeoff, but I was unaware of this. When I opened the throttle for my first takeoff, the biplane made an abrupt left turn. I tried twice, with unsatisfactory results: the Waco made the same abrupt left turn! Crosby came out to the runway and explained about the tail-wheel lock. It would have to be OFF while I taxied, but ON before I opened the throttle for takeoff.

Having solved this problem, we successfully departed Phoenix and flew the two airplanes to Glendale's Grand Central Air Terminal. It was a great adventure!

Harry began the practice of commuting to Northrop in the Waco, but for some reason, just before Christmas of 1944, he was in Glendale and the Waco was at Northrop. He phoned, asking me to bring the Waco to him instead of having lunch. I looked out the window and saw typical Southern-California weather: clear but with limited visibility in smog. I agreed, cranked up the Waco, and took off.

Visibility was really not very good. I found it necessary to fly northbound at an altitude low enough to see the streets and identify Crenshaw Boulevard. When I got to Wilshire I turned right, eastbound, then northbound again, into the Valley. About the time that I saw Griffith Park on my left, above me! I asked myself what I was doing there. But Grand Central should be just a bit farther--why didn't I see it? Because it had been elaborately camouflaged, to fool the Germans or the Japanese--and me. On the runways, taxiways, and aprons had been painted streets, sidewalks, lawns, houses, and garages.

The illusion was not really awfully good, at least from my modest cruising altitude; the houses and garages were pretty two-dimensional. I identified the runway, entered a pattern, and landed. As I taxied in--tail wheel unlocked--I saw Harry Crosby impatiently waving me toward parking. His greeting was, "Where the hell have you been? This weather is deteriorating." He dashed to a telephone to file a flight plan, but the fellow on the other end interrupted: "What flight plan? This airport's been closed for half an hour." It cleared up later that day and we were able to depart Glendale for Northrop.

One of the first things I did when I went to Ithaca, without my little family, in the Spring of 1946, was to visit the Ithaca

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Municipal Airport--the strip down at the end of Cayuga Lake--to get acquainted with the personnel and the airplanes. I met Mr. H. M. Peters, the irascible manager of the airport (mentioned earlier herein), his charming wife, Dot, and the Bulls--Gifford, who was one of our most talented graduate students at the G.S.A.E., and his wife, Gracie--both dedicated, skillful, professional, airplane pilots. I remember Gracie swinging the propeller of some little airplane I wanted to check-out in--my log book says it was a 65-horsepower Piper Cub, on the 24th of May, 1946. I was sure that I, as a gentleman, ought to be out there swinging the prop for that cute little blonde girl.

I was active as a consultant to the Cornell Aeronautical Laboratory in Buffalo, and discovered that a light airplane was a very practical method of transportation when you live in a small city like Ithaca, which has only limited airline service. The distance to Buffalo Airport was about 110 nautical miles, so it took me just over an hour to fly there, and the Lab was located right adjacent to the airport. Of course Ithaca didn't have any kind of instrument-landing facility in those days--and, for that matter, neither did I have an instrument rating--so it wasn't a completely reliable transportation system. With all the flying I did in the Northeast in those days--to Philadelphia, Washington, and the New York area, as well as Buffalo, I became highly proficient in the art of flying in marginal weather, at low altitude, across the hilly terrain. [After I became instrument-rated and could legally penetrate the clouds (usually to fly above them), I looked back on those days of "valley flying" and "scud running" with some horror.]

But, as I've said, I developed some proficiency. I often had passengers. I learned, over the years, that some passengers' tales of flying with me became more and more exciting as time went by: flying into snowstorms, "couldn't see a thing", etc. I took Professor Dale Corson from Ithaca to Teterboro Airport in New Jersey once, and he has used an exaggerated story of that flight--how we flew down the Hudson River at low altitude, etc.--to tease me on at least three public occasions.

I do remember one flight from Ithaca to Buffalo in the Super Cruiser that really was harrowing. I was flying with Dr. Ted Wright, who, as I've written, was a Cornell Vice President and

President of the Cornell Aero Lab Corporation, to a meeting of that corporation's Board of Directors. We decided to make the trip in the Super Cruiser, and on the basis of age (not flight experience) and courtesy I offered to ride in the back seat and let him be pilot. Our weather deteriorated as we flew west; I leaned forward and suggested that he tune to a low-frequency beacon at Dansville, about 35 miles east of Buffalo, which, I knew, gave current weather at Buffalo, Rochester, etc., continuously. He fussed with the radio a bit but didn't get any weather broadcast. Our weather continued to get worse, and I repeated my suggestion. It became evident that he didn't know how to use the radio, and we were flying at minimum altitude, trying to maintain visual contact with the ground. I was scared. I was in the process of telling him that he should reverse course when suddenly we saw the hangars and runways of Buffalo Airport below us; he quickly descended, carried out an approach, and landed.

On the ground, he turned to me and said, "Well, we made it!" I replied that we wouldn't have if I'd been flying--that I certainly would have reversed course 30 or 40 miles back there and landed at Rochester or someplace, to wait-out the weather. His reply to this was a classic: "I've learned that reversing course is very dangerous. It's a good way to get lost."

At the Aero Lab our friends had called the Buffalo control tower some time earlier, asking whether they had any word from us. They were told, "No, and they can't get in here now, it's strictly I.F.R. (Instrument Flight Rules) here... Oh, wait a minute! I think they are coming in right now!"

My mother and Helen moved to Denton, Texas, in 1949, after Helen received her Ph.D. at UCLA and became a professor at Texas State College for Women. In the summer of 1950 I flew Pete's Super Cruiser to Knoxville where Mother met me and we proceeded to Ithaca. She enjoyed riding in the light airplane and was fascinated by the air-traffic control system.

Some years later, when we spent a summer at Woods Hole, Massachusetts, where I was Deputy (to Dr. von Kármán) Director of an Air Force study, Mother flew with me in the Tri-Pacer. I made the practice (as many pilots do) of running one fuel tank dry before switching to another--to maximize range. I said, "Mother, don't be

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alarmed if the engine quits; I'm going to run this tank dry."

My mother retorted, "That, Bill, is a stupid thing for you to say. If the engine quits I am certainly going to be alarmed!"

In connection with one of my many flights to Buffalo I stopped off at Buffalo Air Park, a smaller airport nearby, to look at a "Mooney Mite" that was being offered for sale. I shall have to describe the Mite without getting carried away and using too many pages.

The Mooney 18C was a single-engine (65 horsepower), single-seat monoplane with retractable landing gear. It was tiny, constructed mostly of plywood formed into smooth, graceful, aerodynamically clean shapes--no external struts or wires. It cruised at 120 to 125 m.p.h. and had a maximum range of about 300 miles.

Of course its shortcomings were that you had to fly alone--no family or friends, and not much baggage--and with only 65 horsepower and very limited space and weight, you had to get along without a lot of electrical and/or radio equipment.

At Buffalo Air Park, on 1 May 1952, I fell in love with the Mite at first sight. I asked the seller--the local F.B.O.--how, since I was interested, he intended to check me out. He replied that he would give me a "cockpit check-out" and turn me loose: "You'll not have any trouble flying it," he predicted, looking at my log book, which showed, by then, 361 total hours. That first flight in the Mooney was surely the most thrilling of my career. It was a delightful airplane to fly! I purchased it for \$1700, and proceeded to figure out a way to get both it and Mr. Peters's Super Cruiser back to Ithaca. It was a joy to fly for the next 5½ years--until Friday, 13 September 1957, when it (and I) were run into by another airplane, on the ground at Washington National Airport--but that's a story I'll write about later. During those 5½ years I put over 500 hours on the Mooney, including two round trips from Ithaca to the West Coast.

Its maximum range was, to be sure, a bit less than I would have liked. It was enough to make the Ithaca-Washington leg safely unless a gale was blowing!--but Ithaca-Cleveland was a bit too long for safety; when I flew the Mooney to the West Coast there were often westerly winds and I had to make a lot of landings. I didn't really mind that; I got acquainted with a lot of airports and a lot

of interesting people.

My Mooney Mite was painted a bright yellow; it was so small that it inspired numerous clever remarks on the part of tower personnel, airline pilots, and FBOs. More than once when I inquired about hangar space for overnight the answer was something like "If I can't find hangar space for that little thing I ought to be fired." Once I taxied out, at some airport, at the same time as a bright yellow DC-3. The ground controller couldn't resist asking whether the DC-3 had given birth overnight. The DC-3 pilot wasn't sure about that, but pointed out, with a sigh, that my cruising speed would exceed his. (I'm afraid he wasn't quite right.)

On the occasion of one of the Mooney's trips to the West Coast I was to meet Dr. von Kármán at the Odlum Ranch, near Indio, California. Mrs. Odlum was the famous aviatrix, Jacqueline Cochran; she and Dr. von Kármán were friends and mutual admirers. When he introduced me to my hostess he told her that I had flown out from Ithaca in a Mooney Mite. She was much impressed: "That must have taken 30 hours of flying!" she exclaimed, and I answered, "Certainly not! Only 25."

I have said it was painted yellow; actually, when I bought the Mite it was bright yellow except that on both sides of the fuselage, in the largest red letters possible, it carried the message "Ralph Flanagan!!!" Flanagan was a dance-band leader who also was a pilot. His big band traveled by bus, but their leader went ahead in the Mooney, and, of course, liked any publicity he could get by flying around in the tiny yellow airplane. It took several weeks for me and Pete Peters to get the fuselage repainted, and in the meantime it was I who got whatever notoriety accrued: control-tower personnel liked to address me on their frequencies as "Ralph" and ask me questions about the band's schedule.

At one airport, when I used the men's room after flight, the fellow using the next urinal asked me, "Been dropping any leaflets over Atlantic City lately?" It seems that Ralph Flanagan had gotten in trouble with the authorities, sometime earlier, for doing just that--dropping advertising leaflets from the Mooney when his band was playing in Atlantic City. I explained that I wasn't Ralph, but the fellow didn't seem to believe me.

A Mooney Mite anecdote that's not a happy one: After landing at Mansfield, Ohio, I was tying the Mite down for overnight and was

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approached by a courteous young man who asked, "I guess that little airplane of yours must be pretty good for aerobatics, isn't it?" I said No, and explained to him that it was agile and would surely be perfect for aerobatics, but that it was not designed to the load factors that would be required. It was designed by the Mooney brothers, Al and Art, for transportation, not for stunts, and that that accounted, to some extent, for its economy and its success. I was not sure that he, probably not an engineer, would understand me. He did. He admitted that he was testing me; his best friend had bought a Mite, had loved to perform aerobatics in it, and had been killed when he overloaded the structure.

The demise of my Mooney had nothing to do with aerobatics, aerodynamics, or aerodynamic loads. It was parked at Washington National Airport and I was ready to fly it back to Ithaca on a Friday afternoon after a series of technical-committee meetings. I called Ground Control and asked for taxi instructions; I was cleared to Runway 15 and proceeded in that direction. As I taxied I heard Ground tell another airplane, who also wanted to depart, to hold his position to allow a military airplane, a TBM torpedo-bomber, to precede him; he explained that the TBM pilot had no visibility forward, because of his big radial, air-cooled engine.

Arriving at Runway 15, I changed to Tower frequency but, for some reason, didn't get an answer. So I went back to Ground frequency and reported my problem; I was told to stand by--that meant to stay on Ground-Control frequency while he looked into the matter for me.

As flying students we are taught that our heads should be on swivels--we should always be looking around--and it's a good thing I did that: The TBM had been directed to the same runway and, I saw, was taxiing toward me from my left rear! I decided to get out of his way--knowing that he couldn't see me--and simply taxied forward toward the grass adjoining the taxiway.

But not soon enough. His big propeller blades were chopping the Mooney's tail surfaces, and yellow pieces were flying around me. When I got over to the grass I reviewed my situation and decided that the thing to do was to park there, shut down my engine, and get out to see how bad the damage was. I remembered that one should shut off ignition and electric power in case of an accident, and I

realized how ironical that was, with the little Mite, now tailless, taxiing happily on its tricycle landing gear across the grass to an out-of-the-way place. [I even thought of the years I'd spent designing tailless airplanes!]

When I climbed out of the Mooney I discovered that the situation was--or had been--a bit more serious than I had realized. The big propeller blades had chopped up not only the tail surfaces but also the whole rear part of the beautiful monocoque fuselage, and had cut great gashes in the plywood within about a foot of my neck, before I drove the Mooney out of its range!

The pilot of the TEM had only learned of my presence when the yellow parts started to fly, whereupon he had jammed on his brakes so violently that the big torpedo-bomber had nosed over and damaged its propeller. He climbed down from his lofty cockpit, trembling. I said, "Calm down. I'm the guy you hit, and I'm OK!" His response was "Jesus Christ! I thought you'd gone by now!" [So, you see, he did know that another airplane was around there someplace.]

About then a jeep came roaring up to Runway 15, its occupants being the personnel of the control tower. Their message to me and the TEM pilot was "When we tell a pilot that he is cleared to take off on a given runway, that means that the runway is clear, not necessarily the area between his present position and the runway."

Well, that was obviously intended to clear tower personnel of responsibility, but it is not the way things are done at airports all over the country. Routinely they will say, "You are cleared for takeoff if you can get past that airplane ahead of you,"... or the equivalent.

[And within a year of my accident, a new official warning sign appeared near the run-up area of Runway 15: "Warning! This area not visible from the control tower!"]

The TEM was a torpedo-bomber, as I have said, but it had been transferred to the Department of Agriculture and converted for spraying of forest fires. It was based at Beltsville MD, and needed to be flown to National Airport just to be weighed! With its new weight-and-balance, it was on its way back to Beltsville when it ran into me. What bad luck for him--and for me! The Department of Defense was permitted to settle claims out of court for accidents several times greater than could the Department of Agriculture.

I had a colleague and neighbor back in Ithaca, a professor of

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Law, who knew all about such things, having worked for the Government for some years before coming to Cornell. With his help I sued the Government under the provisions of the Claims Act of nineteen thirty-something. I had been run into on a government airport by a government airplane flown by a government pilot and under the control of government tower personnel. Uncle Sam settled with me out-of-court for about \$2400, which was more than I had paid for the Mooney in 1952 but less than its market value in 1957. My colleague/lawyer explained to me that the Claims Act limited attorneys' fees to 25 percent, and announced that he would be willing to handle the case for me--I thought he was going to say "for less than that", but he said "for that amount". (How magnanimous!)

After the accident, on the afternoon of 13 September, I needed a place to sleep and a place to be cheered-up a bit; I called my old friend General Don Putt, who lived with his lovely wife, Margot, at Bolling Field. I told him briefly what had happened to the Mooney and he told me to get a taxicab and come over. When I arrived at their quarters he opened the door with a dry martini (for me) in his left hand, saying, "So they chopped your tail feathers off, did they?" and roared with laughter.

My Mooney Mite was bought by someone who had "ground-looped" in one; his had both of its wings damaged. The two Mites were combined into one and went on to fly again.

From then until the Spring of 1961 I didn't own an airplane but continued to travel around the country in airplanes rented from Ithaca Flying Service--i.e. from Pete Peters. Mostly these were Piper Tri-Pacers. The Tri-Pacer is a rugged, dependable, four-place single-engine machine with a fixed, tricycle landing gear. It is not at all handsome and offers rather meager performance, but it does have room for three passengers and their luggage. And so Mabel was able to accompany me when I flew out to Seattle, where I was doing some consulting for Boeing. We took the opportunity to visit her hometown and birthplace in Montana.

A young man had shown up at our front door, asking her, "Are you Mabel Rhodes?"--to which she naturally answered Yes. He was D.C. Hodges, a Cornell student, the son of one of her school teachers in Sheridan MT (not Wyoming), population about 700. When he learned that we were going out that way he urged us to land at Sheridan,

which, he said, had a landing strip. He thought Mabel ought to remember the strip and the rancher, Ray Elser, who was permitted to graze his cattle there. If Ray's cattle were on the strip when we got there, he said, we only needed to make a low pass and Ray would remove them.

But when we got there I didn't like the look of the strip--couldn't really tell whether there were cattle or sagebrush on it--so we went on, about 10 or 12 miles farther, to Twin Bridges, which is about the same size as Sheridan--and is Mabel's birthplace! Twin Bridges's airport was actually on the chart, so I felt much better about landing there. After landing we taxied up to the one hangar and got out to look around.

There was nobody around. There wasn't a sound until the hangar door said "clank" as a little breeze passed. Mabel said, "Come on. Let's go to Seattle," but I persisted. We carried an emergency tie-down kit, but there was no way I could screw it into the hard earth. I pulled the Tri-Pacer around to where I could tie the tail to the hangar door and one wing to a fence, and we put a note in the windshield saying "Gone to Sheridan, c/o Hodges".

Mabel said "What do you propose to do now? Let's go to Seattle."

But on the far side of the field there was a green, park-like area and even a car. I said, "We'll walk over there and get transportation." So off we trudged with our luggage; the green area turned out to be a cemetery and the car was a caretaker's. He was surprised to see us, went to his car, put his false teeth in, and offered us a ride into Twin Bridges. He even remembered Will Rhodes, Mabel's father. He took us to an automobile dealership, where the proprietor told us he was also the airport manager and that if we had circled the field he would have come out to fetch us.

We had a delightful two-day visit to Sheridan, then proceeded to Seattle. My work for Boeing was with their Boeing Scientific Research Laboratory, but we also made contact with my former Cornell student, Jack Waddell, and his wonderful wife, Audrey. (Audrey is originally from Fiji, where she met Naval Aviator Lt. Waddell during the war.) Jack was a Boeing experimental test-pilot--made the initial flights of the 747 and set numerous aeronautical records when he delivered the first 747s to faraway lands.

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Jack used to call me at B.S.R.L. and solemnly announce that they needed my services at Experimental Flight-Test for a couple of hours. What Jack really wanted was for me to accompany him and his crew on a flight, and that meant I was to sit in the copilot's seat and get some experience in flying big, jet-powered airplanes. It was, needless to say, a thrilling and informative experience.

It is well-known among aeronautical engineers that the 707 has an annoying sort of motion at low speeds; it tends to "wallow" from side to side in a rolling, yawing oscillation [as did the Northrop N9M, as I have described in Chapter 5]. In the approach to landing in bumpy weather it can be quite unpleasant. So, in one of these flights, Jack put our 707 into the "wallow" and asked me to correct it. I knew that what was needed was damping in roll, so I operated the ailerons to oppose the rolling motion--not to raise the low wing and lower the high wing. My trick cancelled the "wallow" like magic. Jack said to his copilot, "That must have been just beginner's luck. Let's put the professor in a big one this time."

So they got the 707 rolling with an amplitude of about plus-and-minus 90 degrees and said, "Now, Professor, let's see you stop this one." I did; my scheme worked like magic again. I couldn't resist telling Jack, "I tried to teach you something about damping-in-roll at Cornell, Waddell."

Soon it was time to land, and my friends insisted that I do it. I protested (but not too vigorously); Jack and his copilot both announced that they had no intention to make the landing--that I had to do it. It was an amazing experience; unlike propeller-driven airplanes the 707's airspeed is not affected by the throttles. The control column controls the airspeed and the throttles control the vertical speed--rate of climb or descent. The pilot can set the airspeed, using the longitudinal-trim button, and then simply adjust the throttles to give him the desired rate of descent.

On another flight with Jack and his crew I flew the beautiful 707 that became Air Force One, and again I carried out the landing.

I think it was during that visit to Boeing that Jack convinced me--didn't have to twist my arm--that I ought to look for a nice, used Bonanza for my business-and-pleasure flying. I found a well-seasoned Model A35 and bought it. The Beech Bonanza is a single-engine, four-place airplane with retractable landing gear and a

well-deserved good reputation. I bought it in March 1960 and traded it in on a newer airplane of the same general category, a Piper Comanche, four years later.

But I am getting somewhat ahead of my recollections of the years of Tri-Pacer flying. One of Dr. von Kármán's visits to Ithaca occurred during those years, and I had the pleasure of flying him from Ithaca to Schenectady; he enjoyed the flight a great deal, patted the side of the homely Tri-Pacer and said it reminded him of airplanes he had flown in in his youth.

But his friend and protégé, Tony Ferri, when he learned of our flight to Schenectady, was furious; he didn't think I should have endangered the great professor's life and limb as I had done. Dr. von Kármán himself was amused by Tony's attitude.

Tony was consistent: There was an international meeting of combustion specialists--I suppose it was an AGARD function--and someone in Washington was so clever as to obtain transatlantic transportation for the whole American delegation in a military transport airplane. Again Tony, my somewhat excitable Italian friend, was furious. He sent an urgent message to Dr. von Kármán, asking him to prevent this flight: "If that airplane should go down, the United States would lose its entire competence in combustion engineering!"

Dr. von Kármán turned to me with that pixy-ish grin. "You know, Bill, times change," and he told me a story from his younger years. One of the early Congresses of Applied Mechanics was held in Stockholm, and the entire German delegation traveled to the meeting on one steamship. He remembered that, as the ship sailed off toward Sweden, the younger academics, who were perpetually plagued by the lack of jobs in the universities, watched it and remarked, "Boy, if that ship sinks there'll be some vacancies!"

Because that story had to do with military transportation and combustion engineers, I am reminded of another one: There was a national meeting of those same specialists, somewhere in the Western United States, and again someone arranged to convey a considerable delegation in a big military airplane, in this case a Lockheed "Constitution"--that was the big brother of the familiar "Constellation"--only a few were built. It was a somewhat underpowered airplane and, especially for flight into the high country of the

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West, it was equipped with Jato rockets. More precisely, the Constitution was equipped with fittings for Jato (jet-assisted take-off) rockets, should they be needed.

Before the flight, with its cargo of combustion experts aboard, left Patuxent River, a truck drove up to the airplane and unloaded Jato rockets. They were unloaded by being dropped from the truck to the ground. Some of the experts aboard the Constitution were alarmed: They knew that the rockets should not be dropped like that--8 or 10 feet--and they complained to their pilots. Sure enough, the rockets carried on their cases a decal saying "... must not be dropped ..." The pilots were properly impressed, but assured the engineers that they had "no intention" of using the Jato rockets.

The big airplane took off as advertised, without any Jato. It took the combustion engineers to Colorado Springs or some other high-altitude stopping place for overnight. It became very cold overnight, and, at the airport next morning, one of the experts pointed out that the decal on the rocket cases also warned that they were not to be used after being exposed to sub-freezing temperatures. Again their pilots were appealed to, and again the experts were assured that the pilots had no intention to use the Jatos.

Everyone got aboard, the Constitution taxied out, the takeoff was initiated, and all the Jatos were fired. As you may have anticipated, they performed flawlessly and the airplane, with its precious cargo of combustion specialists, proceeded to its destination.

Back to the Bonanza A-35: It was certainly a great airplane; aeronautical history has recognized the greatness of the Beech Bonanzas for more than 40 years. But my A-35, 185-horsepower, was getting to be pretty old, and it had its crochets: Its system for selecting the fuel tanks was combined with the manually operated fuel pump used in starting the engine and was famous for confusing pilots to whom it was unfamiliar--including me! It had an electric generator, rather than an alternator. It had an electrically operated constant-speed propeller. Also, its radios were pretty old-fashioned, but that shouldn't be blamed on the airplane or its manufacturer.

I thought I knew all that I had to know about the Bonanza when I offered to transport Professor Theodore Lowi from Ithaca to New York (via Teterboro). But the fuel-tank-selection matter just referred to proved me wrong. Half-way to Teterboro I wanted to switch tanks. I had deliberately taken off from Ithaca with tanks only partly full, knowing that I had plenty of fuel to get to TEB. But when I tried to switch tanks I found that I couldn't. I concluded that something was broken in the selection linkage. I didn't panic. I informed Ted Lowi that we would have to make an emergency landing and asked him to look for a promising meadow or grain field in which to carry out such a landing.

Not long thereafter he said, "Here's a green field down here with a little airplane on it." I couldn't believe our luck, but there it was, in the middle of Pennsylvania: a farmer had a little landing strip, and his yellow Cub was parked near it. I circled, inspected the strip, and landed on it without incident. On the ground, we communicated with Scranton Airport, and they sent a pickup truck with a gasoline tank and pump. To be very conservative I sent Ted to Scranton with the truck and I made a nervous takeoff, solo. From Scranton Ted and I proceeded, with full tanks, to TEB. There I told the flight-line personnel that I would need some mechanical work done on the Bonanza, since the fuel-tank-selector system was obviously busted. Their mechanic looked at me with skepticism; he knew the reputation of the Bonanza's fuel-selector system. Sure enough, there was nothing wrong with the airplane (except the nut that held the control wheel!) You simply had to know how to re-engage the selector after it had been used as a manual fuel pump.

Actually the electric propeller was a good idea in several respects: One turned it on before takeoff, so that it would govern--maintain constant r.p.m.--during takeoff and climb, but then one could switch it off for cruising. But mine sometimes didn't operate--didn't govern--when the switch was moved to "ON"--until I delivered a modest kick to the junction box down below the instrument panel. After a meeting of Princeton's Advisory Committee, of which General Don Putt and I were members, I offered the distinguished general a free ride in the Bonanza to his destination, Washington. We took off from the little airstrip at the

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Forrestal Center; I had the propeller switch "ON", but this was one of the occasions when the propeller didn't govern and was therefore stuck in full low pitch. As we accelerated along the airstrip, throttle wide open, the propeller r.p.m. went to about 1000 above maximum-permissible, and the Bonanza fairly leapt into the air. As I gave the junction box the required kick, Don Putt, unaware of the details, said, admiringly, "Boy, this little machine sure gets up and goes, doesn't it?" I saw no reason not to receive the compliment gracefully.

The single-engine Comanche, the Bonanza's immediate successor, in 1964, was a newer airplane of the same size and, with 250 horsepower, somewhat better performance. [You should understand, Dear Reader, that the ritual of "trading up", for an airplane owner, is basically the same as is practiced by so many automobile owners. The airplane is bought by making a sizeable down-payment, followed by regular installments. When the vehicle is almost paid-for, there is nothing to do (it seems) but trade it in as the down-payment on a newer and more expensive one, etc. Thus one progresses from a Mooney Mite to an old Bonanza, to a much newer Comanche, and finally, as you will see, to the luxury of a brand-new Twin Comanche. If there was only more time! one could carry the process on and even arrive at something turbine-driven and pressurized!]

The Comanche was a joy to fly and to travel in. I decided that its performance and its equipment--electrical and electronic--were so advanced that I was foolish not to complete my instrument-flying instruction and move into the modern world of professional flying. I did that, and, of course, the utility of my flying was increased enormously--even though the Comanche did not provide the relative security of anti-icing systems or two engines.

It was the single-engine Comanche that we used to make our sabbatic leave possible in 1965. Cornell agreed to the leave, and both Santa Monica and Long Beach branches of Douglas Aircraft were agreeable to a consulting agreement that would put me at each plant one day per week. Mabel and I thought we might live in Lancaster or Palmdale, from which I could commute to both plants. We visited both of those towns on the "high desert", and were sorely disappointed: We were brought to realize that that area gets cold in the

winter; when we asked landlords about swimming pools we were told, "Well, we drain the pool for the winter; you know, it gets pretty cold here." Somebody straightened us out: We should go to the low desert--Palm Springs--and I could commute by air to Santa Monica and Long Beach from there.

We did just that; we took an apartment in one of the many little complexes grouped around their little swimming pools. I commuted to Douglas (Long Beach) on Tuesdays and to Douglas (Santa Monica) on Thursdays in the Comanche, and it worked out beautifully. The rest of my time I spent preparing my "Course of Lectures" for the University of London, which I have told you about in Chapter 7, and enjoying the beautiful desert, its weather, and the swimming pool.

I took off from Palm Springs on Tuesdays and Thursdays before dawn, and never needed an instrument clearance--that was convenient because the minimum instrument altitude leaving PSP westbound is 13000 feet. I did, however, always need a clearance to get into either Long Beach and Santa Monica. I departed from there at the end of the working day--about dusk, almost always under VFR (visual flight rules), and landed at PSP after dark, sometimes on a clearance in rain and clouds. I got into the habit of taking a dip in the pool before dinner, regardless of the weather; the pool water was warm--about 80 degrees F--and I found that if I swam vigorously I was warm enough to go from the pool to our apartment comfortably--even when it was raining. Some of the other tenants thought I was crazy; they came to the pool, in sweaters and raincoats, to watch me, and told one another, "Boy, he sure is going to freeze when he gets out!"

In early March of 1966 we moved to San Diego and I took up temporary duties at the University of California at San Diego, which is in La Jolla. I continued my Tuesday-and-Thursday schedule at the Douglas plants. The controllers at San Diego's Lindbergh Field got so used to my commuting--I needed an instrument clearance every morning--that they would give it to me ("the usual") as I taxied out. Although Lindbergh is located right on the seacoast, I needed an instrument clearance only once for returning there in the late afternoon.

[Perhaps I should explain to my readers that when I didn't require an instrument clearance it doesn't mean that I flew in the busy Southern California airspace without radio contact with the

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controllers! The difference between flying "on a clearance", and not, is that the instrument procedures are more rigid and sometimes more complicated. The controllers have to take full responsibility for fitting you into the IFR (Instrument Flight Rules) system, and they are often overloaded. When the weather is suitable, life can be made somewhat simpler for both pilot and controller.]

The Twin Comanche is essentially the same airplane as the Comanche but with two 160-horsepower engines and the ability to fly--although not very well--on either one. Each engine carries a 100-ampere alternator, providing considerably greater dependability of electrical and electronic equipment. The engines have fuel injection--direct injection into each cylinder--which essentially eliminates the problem of carburetor icing. We bought our Twin at the beginning of 1967 and had wing, tail, and propeller anti-icing systems installed later that year. Since Mabel and I flew it for 24 years, we have many happy memories and lots of stories relating to that airplane.

Many of my recollections from my fifty years of do-it-yourself aviation have to do with my passengers and their idiosyncrasies.

Once, in February of 1963, I had to attend a meeting at M.I.T., and my friend Barkley Rosser had some kind of business to do there on the same day, so I volunteered to provide round-trip transportation for him in the Bonanza. We would make the Boston-Ithaca flight after the working day, and I was going to leave Ithaca bright and early the next day for Washington: I had the honor of being invited to attend the ceremony at which President Kennedy would award the first U.S. Medal of Science to Dr. von Kármán at the White House!

But Rosser was late to meet me at the airport (Norwood, Mass.) for the flight to Ithaca--unconscionably late; we arrived in Ithaca long after dark, not having had any supper, of course. When I finally arrived home Mabel said, "Oh, you poor guy. You have to be off before dawn tomorrow for the big doings in Washington!" I told her I wasn't going to Washington: "It would be great to be there, but I don't have to be there--I'll surely see Dr. von K. someplace, to congratulate him, and, with luck, I'll get to meet Kennedy someplace, sometime."

I was wrong; I never saw Dr. von Kármán again and never did meet

President Kennedy. Before the end of 1963 both were dead.

I have never forgiven myself for missing that occasion at the White House. I have had some first-hand reports from those who were there. Tony Ferri was mortified when he heard von Kármán say to the President, "What? You don't know Tony Ferri, Mr. President? Here, I'll introduce you," and Kennedy, amused, reply, "I'm sure he's someone I should have met."

[In defense of Dr. von Kármán's manners I must say that I suppose he was thinking of Tony's vigorous and highly visible public statements about the proposed supersonic transport airplane, which he had argued for in the offices of Senators and before Congressional Committees, etc. He probably really did think that Tony must have come to the attention of the President.]

Passengers can almost be counted on to be late to their appointments for departure. Some of them also become airsick enroute, and, even if not ill, they require unscheduled landings because light airplanes, until you get into the category of seven or eight passengers, don't have toilets. I have come to the conclusion, over the years, that the urge to carry passengers is an ailment of novice pilots. If you know amateur pilots who dearly want you to be their passengers, you should suspect that they are inexperienced.

My good friend Professor Dave Block, who appeared earlier, in Chapter 7, was honored by the Iowa Academy of Sciences for his outstanding work in applied mathematics. I congratulated him and asked about his trip to Iowa to receive the honor; he said the Academy wanted him to deliver lectures on his work, but that he obviously couldn't accept the invitation because he didn't travel in airplanes. He could travel by rail, via Syracuse and Chicago, but that would require too many days away from his Cornell classes. I told him he should certainly go to Iowa--surely his faculty colleagues would help by teaching his classes for him.

I didn't see Dave for several weeks, and when I did he told me, proudly, that he had indeed followed my advice: He had traveled by rail to Chicago, a young man at Ames (Iowa State University), who was a pilot, had flown to Chicago, flown Dave back to Ames where he had lectured, flown him from there to Iowa City (University of Iowa) for another lecture, and thence back to Chicago and the railroad.

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I was perplexed: "But, Dave, you don't travel in airplanes."

"Oh, that's big airplanes," he explained. "I like to ride in the little ones."

That attitude is rather unusual, but I decided I would accept it. I asked, "How much experience does this fellow have? Does he own the airplane? Does he have an instrument rating and instrument equipment in the airplane? Does he have a record of any accidents?"

Dave was silent, and then answered, "I see. I should have known the answers to all those questions, eh?" Yes, he should have.

Mabel and I were on one of our frequent trips from Tucson to the East in the Twin Comanche--and all courses to the East went through Indianapolis, since that is where daughter Susan lives. As usual we landed for lunch at Wiley Post Airport, near Oklahoma City. The lunchroom manager knew us from earlier trips and knew we went from there to Indianapolis. He came to our table and asked whether we would be agreeable to carry one of their local pilots to Vincennes, Indiana, where his airplane was undergoing an "annual"-- the exhaustive annual inspection required by the Federal Aviation Agency. We replied that we would, of course, transport the fellow, but only if he was ready to go when we were. He was: He had dashed home to get his toothbrush, and off we went, with him in the back seat.

But we had only progressed a few hundred miles before he announced, from the back seat, that he had to ask for a "comfort stop". We had a date to meet Susan, and a landing at Vincennes was already going to make us late; I handed him my plastic jug--empty Clorox bottle-- and assured him we would pay no attention. He tried, and told me he was very sorry but the process didn't work. He was afraid we would really have to make an unscheduled landing.

I told him I understood his predicament. I had an inspiration, of a sort, and told him about my friend Marvin Martin, who found himself in the same situation on a flight from San Jose to Tucson with a lady passenger, a neighbor of the Martins. He, too, had neglected to use the facilities before takeoff. He asked his passenger to climb into the back seat; she would, of course, but couldn't understand why. He explained, and the lady worked her way into the back seat. But still he couldn't get the process to work, and he had to tell her that they were carrying out an unscheduled landing at Lancaster (General Fox Airport). They taxied in, Marv

cut the engine and jumped out, making a bee-line for the Men's Room.

Our passenger in the back seat giggled and suggested that I hand him the Clorox bottle again; this time he had better success, and we didn't have to make a landing before Vincennes.

We performed another little mitzvah, of a different kind, for our friend Billie Kozolchyk. Her husband, Boris, is a world-famous professor of International Law and travels far and often in connection with his work, but Billie, for some years, was inhibited from accompanying him because there were three children at home to be looked after--and also because airplane cabins made her claustrophobic. When the kids matured, she wanted to travel but still had the claustrophobia. Following professional advice, Mabel and I took her for a couple of rides in the Twin Comanche, first just taxiing around on the airport, observing traffic and listening to two-way radio communications, then sight-seeing flights over Arizona. I think the treatment was successful and that she now accompanies Boris on the airlines.

While we still lived in Ithaca, I once had as my passenger in the Twin Comanche a world-famous engineering-scientist, Professor Jakob Ackeret, Director of the distinguished Aerodynamics Laboratory of the Swiss Federal Technical Institute of Zürich, a man whose reputation and accomplishments were comparable to von Kármán's. I invited him to visit Cornell and deliver a lecture; we were both attending a technical meeting in New York, and I offered him a seat in the Twin from Teterboro to Ithaca; he accepted with pleasure.

But there is a phenomenon called the "V.I.P. Effect", which says that the probability of trouble is directly proportional to the rank of the personnel you are trying to impress or influence. When Professor Ackeret and I got ourselves and our baggage into the airplane and started to taxi out of its parking slot on our way to takeoff, my right-hand propeller hit a light fixture. It was one of the blue lights demarking the parking area, and it was not visible to me because my right engine and nacelle hid it from my view as I started to taxi. [That, of course, is no excuse; it is the pilot's responsibility to be sure that the area around his airplane and where he intends to taxi is clear of obstacles.] The tip of one propeller blade was nicked, and I knew that I had to get a profes-

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sional opinion before flying with it--the impact with the light fixture, even though it was a flimsy little structure, could conceivably damage the blade.

I was, of course, mortified and chagrined to have this unforgivable blunder occur just when I had a most distinguished passenger in my care. The Federal Aviation Administration has a big office at Teterboro, so I requested the services of one of their experts to decide whether the airplane was flyable or whether Dr. Ackeret and I would have to change our plans completely. The young man from the F.A.A. inspected the blade, pursed his lips, nodded his head, and assured me that all the blade needed was a little smoothing, at its tip, with a file. This done, I thanked "Whatever gods may be", and we flew to Ithaca.

While Ackeret and I took care of other things the next day, my good friend and awfully bright mechanic (and pilot), Allen Hayes, inspected the propeller at the Tompkins County (Ithaca) Airport. Mabel and I were scheduled to fly out on another trip the next day. Allen was suspicious of the nicked blade and of the F.A.A. man's advice; he had another airplane that used the same-model propeller, so he put one of its props on my Twin and flew my propeller down to the propeller shop in Lancaster, PA.

Can you guess the outcome of this complicated tale? The prop-shop in Lancaster found that my blade was structurally damaged (in its hub) and had to be replaced, while Mabel and I flew out on our trip with Allen's propeller.

That kind of personal, responsible service was typical of my friend Allen Hayes, and was, as you can imagine, one of the most important things that made my personal-flying career possible. I must confess that Allen's policy of borrowing equipment from one airplane to facilitate the flight of another sometimes became a little complicated. I bought a suitable red-paint-pen and made some little red dots on all the radios and other expensive electronic components in the Twin Comanche, so that I could tell when he had solved somebody's emergency by borrowing something of mine.

Some weeks after we moved to Tucson in 1974 I got a call from him to tell me that the transponder in the Twin had been taken out of another airplane--one of his--in one of these emergencies, and now he had sold that airplane to a dentist in New York City. He suggested that I have the instrument removed and ship it to him; he

would ship mine to Tucson. [A transponder is an expensive radio--those two words are redundant!--that automatically replies to the controllers' radar signals, in flight.] I had a better idea: I was making a trip to Europe (for AGARD), by airline, via New York (Kennedy), a couple of weeks later; Allen should fly down from Ithaca, meet me at JFK, and we would exchange transponders.

He liked that idea but improved upon it in typical Hayes fashion: He would have the dentist meet me at JFK to exchange transponders. His plan worked perfectly up to a point: The dentist and his lady-friend took me to lunch. My idea was to stash my transponder in a baggage locker at JFK while I went to Europe, but I discovered that there were no lockers at JFK because of the bomb threats! I was certainly not going to take it to Europe with me. I went to the manager of the hotel where I was staying overnight, near the airport, and asked him to keep the transponder for me; I had a reservation for overnight at the same hotel on my return journey. He said Sure, he would keep it for me, but when I asked for a receipt he demurred: "You say it's a transponder, but I don't know a transponder from a hole in the ground. Just ask for me when you come back; I'll have it for you."

Everything finally worked out; I've already told you that people who fly light airplanes love to concoct complicated schemes. [Actually this particular scheme got even a little more complicated, because my flight across the Atlantic was delayed and my hotel reservation was cancelled. The hotel was full: no rooms. I took their bus from the airport, just to reclaim the transponder, and when I told my friend the manager that I needed a room, he magically found one for me.]

But we who fly our own airplanes do sometimes make complications for others. I was a member of a team of inspectors--four of us, I think--who surveyed the engineering-degree program at Wright-Patterson Air Force Base, for accreditation. I parked the Twin at Dayton Airport, and when the inspection was finished, late one afternoon, all four of us were heading for that airport in an Air Force car; the others had airline reservations. Now the FBO where I had parked and been refueled was on the east side of the airport and the passenger terminal was on the west side. The Air Force Base is east of the airport. I had gotten a weather briefing and had

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been told that there would be thunderstorms in the Ithaca area about the time of my arrival; as a result I was edgy and eager to get started. So I was unhappy when the chairman of the inspection team told our driver to take them to the west side first (because they had an airline flight to meet) and then bring me back to the east side (since I didn't have a schedule to meet). I remonstrated, but he argued for the logic of his plan. In uncharacteristic (I hope) pique I said he could go to hell and instructed the driver (poor fellow) to drop me off at the FBO. There was a long and tense silence in the car as the driver did that; polite college professors don't speak to one another that way.

And when I got to Ithaca there wasn't a cloud in the sky; the moon and all the stars were shining brightly; and I was a bit ashamed of myself.

But the story doesn't end there. Shortly after our move to Tucson our Aerospace & Mechanical Engineering Department at the University of Arizona was scheduled for an accreditation inspection, and guess who would be the chairman of the team of inspectors. Yes, the same fellow! I thought that my innocent U. of A. colleagues would never know what a handicap they were acquiring when I joined them.

When the team arrived I greeted them all; then decided to face the music, reminded the chairman that we had once performed an inspection at Wright-Patterson, and asked him whether he had, indeed, missed his flight out of Dayton that night. At first he couldn't remember the incident at all, but then recalled: "Oh no! I remember now. Our flight was hours late; we even had dinner before leaving Dayton!"

On the other hand, the presence of one or more committee (or board or panel) members who are independent of airline schedules is sometimes convenient for the organisers: Duncan Rannie and I were fellow members, for a lot of years, of an advisory committee at the Allison Division of General Motors in Indianapolis. Duncan, who lived in California, liked to visit his parents in Ontario on the way home from the committee's meetings, so we developed the habit of flying, in my Twin, from Indianapolis to Buffalo after the committee's meeting; I would drop him off there and proceed to Ithaca. The officers of the committee quickly learned that, whereas

all the other members had firm airline reservations, Rannie and Sears didn't, and could be prevailed upon to stay over an hour or two to prepare a summary report on the committee's deliberations.

Well, it was a small price to pay for the joys of flying our own airplane!

Whenever one of these committees, boards, or panels meets, there seem to be members who make the airline reservations, for their journeys back home, for such an early hour that they have to miss the last hour or two--or three--of the meeting. I think the trick is intended to avoid getting assignments to subcommittees or other duties, but it can backfire: If you're not there it's easy for the chairman to assign you.

[Mark Kac explained to me once how to avoid getting appointed to committees, etc.: Usually it is sufficient to demonstrate static incompetence--that is, simply to be careful not to do anything. But if that doesn't do the job--if they persist in re-appointing you--you have to resort to dynamic incompetence, which means to do lots of things and do them very badly--really screw-up everything!]

And while we are thinking about the joys of owning an airplane, let me return briefly to the era during which I owned the Mooney Mite--which surely was a joy!--but had only one seat. There was an eclipse of the sun on 30 June 1954, visible in its totality in parts of Canada. One of my Cornell colleagues in those days was the brilliant theoretical physicist Philip Morrison, a man of fantastic, rapid-fire intellect, interested in just about everything, including airplanes and aviation. Phil thought he and I should make a flight from Ithaca to Kapuskasing, Ontario, a town about halfway between Lake Superior and James Bay, where the eclipse would be total. I thought it was a great idea, and somehow I got Ray Janney interested enough to provide transportation for us: Ray is a former Cornell student of mine who, at that time, had a small engineering company in Wilmington, Delaware--and the company owned a Tri-Pacer. Ray's company's pilot, a bright, personable young man, would bring the Tri-Pacer up to Ithaca from Wilmington and would accompany Phil and me on the junket. I had a good idea of how to fill the Tri-Pacer's fourth seat: my son David was delighted to make the trip. [About 6 years later, when he was a Cornell student, he took a

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course from Professor Morrison--a course in Physics for students in non-scientific fields (David's field was Government)--which he truly enjoyed and in which he did well.]

When we reached Kapuskasing we landed just ahead of a Martin 404 airliner, which, we were told, had been chartered by the Du Pont family (of Wilmington, Delaware) for the purpose of viewing the eclipse. The 404 could carry about 45 passengers, but the number of Du Ponts on this trip was equal to the number of windows, so that each Du Pont could have a window.

We were so naïve that we hadn't made hotel reservations--we thought Kapuskasing was so far away and boasted of enough hotels that reservations wouldn't be necessary. The FBO at the local airport there was less optimistic: "If you boys can't find a place to stay tonight, come back here. Me and the old lady will put you up."

We went boldly to the big hotel in Kapuskasing--the one in which Princess Elizabeth and the Duke had stayed when they made a trip to Canada a few years earlier, before she became Queen--and requested two rooms. The clerk informed us that there were no vacancies--and hadn't been for months. I undertook to plead but was gently nudged aside by our young pilot. He told the clerk, "We were assured that there would be rooms for the party from Wilmington." The clerk's manner changed: "You mean that you are part of the party who flew up in the airplane?" Of course we were, and, especially when he registered with a Wilmington address, there was no trouble and--except that we got only one room for the four of us--no hardship.

But at dawn the next morning Kapuskasing was covered by a low, unbroken layer of cloud. As we proceeded to the airport we found that the whole area was littered with telescopes, cameras, interferometers, and astronomers sitting around their expensive instruments looking unhappy. Just after we got to the field we saw the Martin 404 disappear into the overcast. I asked the FBO whether he had radio contact with the Martin; he did, and I soon ascertained, from its pilot, that the cloud layer was not more than 1000 feet thick, with "CAVU (ceiling and visibility unlimited)" above. It was, strictly, against Ray Janney's company's rules to file an instrument flight plan and fly up through the overcast in the Tri-Pacer, with its limited equipment, but the eclipse was about to

begin on schedule, and we did just that, with no hesitation. We went to about 6000 feet.

The eclipse was terrific! We saw everything: the sudden darkening from daylight to near-darkness, the "diamonds" around the moon's disk. We even had below us an unbroken white plane--the top of the overcast; Phil told us to watch for interference bands of dark and light on this white surface, and there they were!

When the show was over we began to think about returning to land. There was no instrument-landing system at Kapuskasing; we had to find a suitable airport--it turned out to be North Bay, Ontario, nearly 250 miles southeast. Oh why had we not been wise enough to bring our luggage--which wasn't voluminous--with us? We had left it in the hotel! So we had breakfast in North Bay, ascertained that the overcast at Kapuskasing was dissipating, and flew back there. I'm afraid we were guilty of gloating a little as we told the frustrated astronomers, packing up their expensive toys, how beautiful the eclipse had been.

Phil Morrison is the same man who discovered why the Hungarians are so much smarter than the rest of us, why their language is so strange and resembles no other on Earth, why Hungarian women (the Gabor sisters et al.) are so beautiful, etc. He pondered while working with Dr. Edward Teller, and realized that Hungarians are simply not Earthlings like the rest of us, but members of a colony from some other planet--possibly Mars. As I have recounted elsewhere (my *Physics Today* paper), I had the pleasure of introducing Phil to von Kármán and hearing Phil tell that Martian of his discovery. [His response was, chuckling, "Funniest thing I have heard! Mind you, I do not deny."]

Phil loves airplanes and flying, and one of its attractions to him is the variety of optical phenomena one sees in flight. As we flew over an undercast we saw the little black shadow of our airplane, below us in the direction opposite to the sun, encircled by rings of color. That, of course, is nothing but a rainbow. But as we continued over the clouds we saw a bright spot, moving with us and located in the same (azimuthal) direction as the sun but down below us, in the cloud layer. He asked us if we knew what that phenomenon was, and none of us did. Do you? It should be called "the Morrison Light" or "Morrison Shine". He knew immediately what

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it was, and I will tell you what it is a few lines below, after you have a chance to figure it out for yourself.

He and his wife have written the book reviews in the *Scientific American* for years--first Philip and Emily, then Philip and Phyllis--perhaps it is a requirement of marriage to him--in any case they are great reviews and should be read regularly by anyone interested in science. He has also been a television personality: I well remember his explanations of how an African termite colony is programmed (by evolution) to build those amazing structures and how an athlete--his example was an M.I.T. student pedalling a human-powered airplane--is, after all, a heat engine, whose work output is dependent upon its rejected heat being carried away--e.g. by a fan.

Now are you ready for the explanation of the Morrison Shine? We were flying above the cloud layer, and below it, there in Ontario, were lakes. The shine in the clouds was from the reflection of the sun by the water surface. The cloud layer was thin enough to let the sun's light through, but dense enough to make the ground and the lakes invisible to us and to diffuse the reflected light. Do you feel cheated?--the phenomenon is surely too simple to deserve an explanation, much less a name! But I can assure you that it was puzzling when we saw it.

Back to the subject of the Twin Comanche and our 24 years (5400 flying-hours) of experience with it.

We actually did have a failure of one of the engines shortly after takeoff from TUS on one flight. It was a gentle one; we were about 200 feet--maybe 300--above the ground, just beyond the end of the runway, with the landing gear already retracted, when the right-hand engine suddenly lost most of its power. I decided that the thing to do was to feather the right-hand propeller, open the left-hand throttle fully, and see how we were doing; the answer was that we were maintaining altitude with no trouble. I told the tower what our situation was and that I wished to make a left-hand approach pattern back to a landing on the runway we had just left. He cleared us for that and said he would send the emergency vehicles down to meet us.

After a big, gentle 180-degree left turn I found that we had gained about a thousand feet of altitude, and I decided to restart

the right-hand engine. It started and seemed still to be putting out some power--but not much, so I feathered it again to reduce chances of damage to it. After a normal landing, as we taxied in, the men in the emergency vehicles waved their caps at us and gave us the thumbs-up sign; I was a bit embarrassed, since the whole "emergency" had been so easy. The problem was found to be in the fuel-metering unit--what would likely be called a carburetor if the Twin hadn't had fuel-injection nozzles.

We had another kind of emergency on a flight in the Twin from Buffalo to Indianapolis. On the flight to Buffalo one of our vacuum pumps quit--they are the engine-driven pumps that provide the suction that drives several instruments required for instrument flying. I have mentioned, earlier in this chapter, that a twin provides this kind of redundancy. We were planning to be in Buffalo two days, so I decided to have the pump replaced there. And while the airplane was in the FBO's shop, would they please check the air in all three tires, which looked soft to me.

Yes, they would do those jobs--but apparently they gave the jobs to a novice, and when we were ready to depart on Friday afternoon he was still doing the vacuum-pump job. (He had removed the magnet--which you don't have to do!--and had trouble putting it back on the engine and resetting the spark-timing. We had to postpone our departure and stay over Friday night. Not surprisingly, my shop bill was much larger than expected.

But we did get away Saturday morning, and all went well until I tried to put the landing gear down for landing at IND, the Indianapolis Municipal Airport: It wouldn't come down.

I announced to Mabel that we would have to exercise the manual, emergency, gear-extension system, which involves fitting a big lever into the gear mechanism and pushing it forward to deploy and lock the gear. I had practiced the procedure when the airplane was up on jacks, but we had never had to use it in real life. We could not make it work. The gear would go part-way down, and from there Mabel and I, together, couldn't move it any farther. After several tries--and we even put Mabel in the back seat so she could push the big lever with her feet--we gave up; we decided we would have to make a wheels-up landing.

I explained our predicament to Indianapolis Approach Control and

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asked whether I could make the landing on grass at IND. He said I could, but there was no suitable grass area at IND--all their grassy areas were crossed by ditches. He suggested some things we should try in the cockpit, to get the gear down, but they were all things we had already tried (such as "Have your passenger push the lever with her feet").

About this time a new voice was heard on Approach Control's frequency. A pilot 50 miles away, near Terre Haute, had heard us conversing and wanted to know "Could I get into the act, too?" I was certainly happy to get more help, and Approach was eager to avoid a wheels-up landing at IND, so he gave us an exclusive frequency and some airspace near the airport, to let us try to work out a better solution.

The pilot from Terre Haute soon arrived, flying a Piper Cherokee. He flew in formation with us, below us, while we tried once again to push the big lever; he couldn't see anything blocking the gear's way, and suddenly he reported that he thought it was actually moving a bit farther on each try! With that hint I realized that the big lever was, indeed, moving a bit farther on each try. We persisted in our strong-arm (and -leg) efforts, and finally the gear was down and locked!

We landed, but at that point the pessimistic fuss-budgets took over and made us land on a short, out-of-the-way runway, insisted that we not taxi but must be towed, etc., etc. The young pilot from Terre Haute landed too, so we were able to meet him and express our gratitude.

What caused all that trouble and--except for his help and some good luck--expensive damage? Answer: the person or persons in Buffalo who put air in my tires. On the Twin Comanche there are flat, circular, aluminum plates on the wheels, whose purpose is to reduce aerodynamic drag when the wheels are in retracted position in the wing's bottom surface. Whoever checked the tires had to remove those plates, each held to the wheel by three screws--and he forgot to replace one properly. It jammed when I tried to lower the gear at IND. We finally got the gear down by tearing that aluminum plate in half--no wonder that it took hard work!

The postscript to that story is this: I wrote to the FBO in Buffalo, told him what had happened, and asked for some monetary redress. That was rather naïve of me, of course, since he certainly

wasn't going to admit his mechanic's blunder. What he did, though, was to look up my bill and see that I had been charged a ridiculous price for the vacuum-pump job. He admitted that any mechanic should be able to replace that pump on that engine in a fraction of the time charged, and sent me a reasonable rebate. He also mentioned that that mechanic was no longer working for him.

We flew the Twin to Cabo San Lucas, at the southern tip of Baja California several times. The luxurious hotel of the same name had its own airstrip--one of the funniest we have seen: It was built on such a steep hillside, behind the hotel, that airplanes were always landed up-hill and taken off down-hill, regardless of the wind direction. Taking off was rather thrilling, since there was a levelling-off spot about halfway down the hill; with any luck you had flying speed at that point, and felt that you were being catapulted into the air at the end of the flat spot. Every pilot was reminded of a ski-jump.

The first time we landed there and taxied to the tie-down area we were met by a uniformed policeman, who wanted to see our passports, etc. When he saw "Ithaca, New York" he grinned and produced his handgun--manufactured by the Ithaca Gun Company. That was really amusing to us, since I was a member of that company's Board of Directors at that time. I had no idea that "we" had ever made handguns--I am so ignorant about guns that I can't tell you for sure, but I think it was an automatic pistol. It looked to me to be nickel-plated.

We also made trips to San Carlos (for Scuba diving), Chihuahua, Puerto Peñasco, Guaymas, Mazatlán, Guadalajara, Cozumel, and Mexico City (for a meeting of the Mexican Academy of Engineering). The trip to Mexico City was interesting in several respects. Between Mazatlán and Mexico City we encountered summer thundershowers and I undertook, with some misgivings, to file an instrument flight plan while en route. It turned out to be remarkably easy--the controller's English was certainly a lot better than my Spanish--except that several way-points that defined our route had multisyllabic Indian names, which I couldn't catch well enough to read back. He gave me three-letter identifiers for these, which I not only read back but also located on the charts.

We continued on IFR after we got past the thundershowers and

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high terrain; the weather in Mexico City was typical: sunshine and free of clouds but with such thick smog as to require flight on instruments.

We made our trip to Cozumel after attending a committee meeting in Tullahoma, Tennessee. We stopped at New Orleans Waterfront Airport to rent a life raft and life jackets; the jovial fellow who provided them asked me, "You don't intend to get these wet, do you?" I assured him that we had no such intentions. Next morning we headed out over the Gulf of Mexico, on a direct line to Cozumel.

The only unusual feature of this flight was that, after 150 miles or so, we were out of the range of both Stateside navigational radio signals and their Mexican counterparts on the other side of the Gulf, so that we were navigating by clock and compass only. There was no reason for this to be difficult, but we had to assume that there were winds aloft and that we would end up east or west of our direct course. If we drifted westward we would simply arrive over the broad Peninsula of Yucatán, but if we drifted eastward ... Cuba! Naturally, being somewhat unsure of Sr. Castro's hospitality, I was tempted to steer a little bit westward, and, sure enough, our landfall was on the Peninsula.

We had a good time on Cozumel; made a side trip one day, over to Chichén Itzá, where there is an airstrip. As is common in Mexico, a taxicab driver saw us circling the strip before landing and met us when we taxied in. He also found a very knowledgeable guide for us, who supervised our visit to the spectacular pyramid and other ruins. It was a most successful day, and we were back at our hotel on Cozumel for dinner.

Our return trip to New Orleans, however, did not go so smoothly. I succeeded in notifying U.S. Customs & Immigration at New Orleans, by telephone, that we would arrive at Lakefront, etc.--but only after first making one call to the wrong number (about \$20 wasted). We got a taxicab to the terminal, filed our flight plan with the Airport Commandant, paid for our fuel, loaded our baggage, and prepared to taxi out. Our transmitter was inoperative! I did not know whether I would be allowed to start off across the Gulf without radio communication; I did know that I didn't want to.

I checked and rechecked everything in the Twin Comanche's radio and electrical systems that I could think of, without success. There

was another twin, a Beech Baron with an American license number, parked nearby; I knew its pilot was preparing to depart for New Orleans because I had overheard him talking to the Commandant up in the terminal, and in desperation I asked him if we could fly together--a flight of two airplanes--to New Orleans. I apologized for the fact that he would have to fly about ten knots below his usual cruising speed, but he said not to worry about that. He asked me if he could check a few things in the Comanche's cockpit; I agreed, of course, and he went over the same things that I had.

We went together back to the terminal and explained our plan to "El Jefe", the Commandant, who was also most cooperative. He instructed us to have the Baron take off first; then, after a green light from the tower, the Comanche.

We complied, and when we got off the ground I found that our transmitter was working perfectly! I called the tower, thanked him for his help, and then called the Baron's pilot to thank him and release him from the ten-knot speed reduction. We learned that he was enroute, via New Orleans, to Gulfport, Mississippi, his home base, and that he was a friend and student of my good friend and former student, Gifford Bull. Gifford had been his instructor when he worked for his instrument rating!

[Regarding the transmitter that decided to work: There is a relay in the circuit that switches from receiver to transmitter when you push-to-talk into the microphone; it must have gotten stuck, and then freed itself during my takeoff run.]

Besides our numerous flights to destinations in Mexico, we made several flights to the Bahamas and, of course, to places in Canada. One of our most ambitious and enjoyable trips was one from Tucson to Alaska in 1977. We flew up there by way of Seattle, where we laid over a day with the Waddells. From there we intended to fly nonstop to Juneau but encountered some rather high clouds with icing in them and landed, instead, at Campbell River, B.C., to get more fuel and more information about the weather. Apparently that route, along the coast of British Columbia, frequently has that kind of weather. We decided to file an instrument plan and try again at a higher altitude, hoping to be "on top", and that is how it worked out.

Another way of making that leg, from Seattle to Juneau, would

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have been to file for a low altitude--below the freezing level. There was an airway that I could have used for that, which is over water and used the old low-frequency navigation system. It seemed to me that all I would have to do, enroute, would be to maintain enough altitude to clear the superstructures of any ships that might be out there. But, as I've indicated, we were "on top" at 13000. All the clouds below us disappeared at the Alaskan border.

A rather disturbing thing happened to us at Campbell River before we left there--something that had not previously occurred in all of my years of flying. I requested that the Twin be refueled, and I stayed around while the refueling was being done--which "they say" is a good practice. The fuel was dispensed from two pumps unusually far away from the line where airplanes were parked, by means of long hoses, one for gasoline and one for jet-fuel. The lineman who was taking care of my refueling was not quite sure about the Twin Comanche: It has four fuel tanks in the wing; thus four filler caps. It also has two alcohol tanks in the nacelles, with their filler caps; it is not unusual for linemen to ask about these--which ones are for fuel?--so I was not surprised when this fellow asked that. I answered him and, luckily, ran my eye back along the long hose whose nozzle he was carrying. It was the hose connected to the jet-fuel pump! "But not that fuel!!" I exclaimed. He apologized, pulled the hose back to the pumps, and brought the right hose out to the airplane.

The Twin's engines might have run long enough to put us into the air before the jet-fuel got to them. The Campbell River airport is surrounded by pine forest.

After Campbell River we landed in Juneau, in sunlight at about 8:00 p.m., went through American customs--that cost us \$25 because it was so late--and proceeded to a little airport at Gustavus, near Glacier Bay. The folks from the Lodge at Glacier Bay came to Gustavus to meet us. As we checked in we were asked whether we wanted to attend a "briefing" that evening in which some of the guides would show slides and explain what we would see the next day on our boat trip on the Bay; we definitely did.

There was, however, a large party of tourists from Japan staying in the Lodge, who would be on the boat too, and so the slide-show briefing would be narrated in Japanese. The Lodge people apologized when we told them we did not understand Japanese, but we did enjoy

the slides, and we were greatly amused by our Japanese fellow-tourists: The ladies of their party seemed to be quite un-Japanese in deportment--teasing the gentlemen and making what were obviously brash and clever wisecracks about them. For example:

TOUR GUIDE, making an announcement to the group: "So-and-so San (will be in charge of ... or will be responsible for) (something)".

JAPANESE LADY, in loud voice: "So-and-so San is (presumably something derogatory)!"

(Laughter, especially among all the ladies, covering their mouths discreetly in Japanese fashion, but clearly convulsed by their own cleverness and naughtiness.)

The boat trip the next day was a delight. We saw glaciers, icebergs, innumerable shore birds, whales that surfaced right next to the boat and dived, their sweptback tails saluting us and splashing us! We even got to see the fall of a great iceberg from a glacier.

In Alaska we were guests of our friend Nancy Knuutila, who was a classmate of daughter Susan in Ithaca High School and has been a special friend of the whole family ever since then. Nancy planned our Alaskan adventure for us, including a trip in the Twin from Merrill Field in Anchorage to the airstrip in McKinley Park, on the slope of Mount McKinley. Nancy planned our campfire dinner and our overnight in her little tent in the Park. She saw me, in her house in Anchorage, packing a little bag for the overnight and asked me why I was bringing my flashlight. I told her I might have to look for a toilet during the night. "Yes, she replied, "but you won't need a flashlight."

She was quite right, of course; it did not get dark at McKinley Park. Our only problems were our landing and our search for firewood--both minor problems. When we arrived at the McKinley airstrip, from the south, there were broken clouds all around and a rainstorm just north of the field. There was no instrument facility or procedure there. I told my passengers that if we didn't get onto the strip on the first try I would have to pull up into the rain-

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storm, climb to an altitude above the broken clouds and return to Anchorage; I did not intend to try to descend through the cloud layer and find the airstrip--in that mountainous terrain--without any radio guidance. But that wasn't necessary: The strip was clearly visible from a few miles south; we landed, and after decelerating made a U-turn on the runway and taxied back southward to the parking area. There, walking across the runway about where we had just touched down, were a mother moose and her calf. If they had decided to cross just when we were landing I would have had to pull up into the rainstorm and go back to Anchorage.

The other minor problem--that there was a severe shortage of firewood at the camping site--was solved by diligent searching and by being content with pretty small branches and twigs. We finally acquired a fire, little but adequate for Nancy to cook two big salmon, wrapped in foil, for our dinner; I remember that dinner as one of the best I've ever eaten.

Before we left Alaska we and Nancy made unforgettable flights over its mountains, glaciers, active volcanos, rivers and deep gorges; we saw grizzlies, Dall sheep, reindeer, and many more animals and birds. The flying itself was thrilling; we tried, of course, to keep track of all the landing strips on our charts as we flew over the beautiful mountainous territory. Sometimes, when we did find the little landing strip in a narrow valley far below us, we agreed that it might be a nerve-wracking trip for a person inclined to worry about the "what-ifs"--especially if he was flying in a single-engine airplane.

We flew back to "the lower forty-eight" by following the airways that lie approximately along the Alcan Highway. It is probably a better route to take than the coastal one, and in the northernmost sections of it, I did get to fly, once again, the ancient, low-frequency, airway.

We sold our beloved Twin Comanche in the latter part of 1990; at that point it had flown a total of 5330 hours--all but a dozen hours or so flown by me--I had 7826 solo hours including 1010 on instruments. My log book (Volume 10) shows only a total of 423 hours of night flying, which is rather surprising, since neither Mabel nor I has ever felt nervous about flying after dark. Actually there are advantages in flying at night: The airways and their

controllers are much less busy, and atmospheric turbulence--the bumps--is typically attenuated after the sun goes down.

There was a cartoon in one of the magazines some years ago--probably in *The New Yorker* but I haven't been able to find it--that showed a big Texan and his wife riding on an airliner. She must have been complaining, because he is saying to her, "Now Honey, you got to remember that on this trip we're not flying our own lil' ol' airplane." That's us!

But Susan carries on the tradition. She was a student, with a major in Sociology, at Butler University in Indianapolis from 1963 to 1967. When she came home to Ithaca in the summer of '66 she announced that she wanted to learn to fly. [I pretend that I can't understand where she would pick up such a crazy idea!] We told her to get herself a summer job and she could use her earnings to finance flying lessons. She promptly went out to the Ithaca airport and got an office job working for Allen Hayes; she was paid in flying lessons.

Before the summer was over there came an evening when she was unusually late coming home for dinner; Mabel finally called the airport to ask her "Where are you?"--Allen answered the phone: "Well, Susan's up in the air on her first solo flight. I guess she'll be home as soon as she figures out how to land the machine." Her mother ran to tell me the news and began to weep. In a few minutes she phoned; she was a very happy girl--her altitude above the floor was pretty high.

She is devoted to aviation and airplanes. For the next year and a half she continued flying lessons at Indianapolis Municipal, getting up on the cold winter mornings so early that she could get out to the airport--by bus!--have a lesson, and get to her morning classes, often at 8:00 a.m. She received her Private license in '67 and, in '69, bought one of the airplanes in which she had trained, a Beech "Musketeer". She now has about 2200 hours, an Instrument rating, and has flown the Beech from Indianapolis to Tucson and Los Angeles, to Cape Cod, and, of course, to many destinations closer to home. At the suburban field where she hangars her Beech she is the only female airplane-owner; for all I know she may be the only underpaid social worker who owns her own airplane.

Susan has her own stock of private-flying stories; I shall recount only one of my favorites here: She encountered bad weather

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one dark night, enroute to Ithaca, and landed at Oneonta, N.Y. She found the field unattended; there was a public telephone booth; she didn't have any change. She decided she would have to sleep in the Musketeer, but Lo!--automobile headlights approached. From the driver, another airplane pilot, she got three dimes and phoned Flo's Taxi. But Flo, when she arrived, reported that there wasn't a hotel room available anywhere nearby, because there were big construction projects. "Well," said Sue, "I can sleep in the Musketeer." Flo wouldn't hear of it; she said she would worry about Sue all night, and she took Sue home with her and gave her her daughter's room and bed for the night.

Susan's personal flying is obviously one of the most important features of her life. It has given her self-confidence and self-respect; it has given her relaxation and relief from her sometimes stressful professional responsibilities; her best friends are fellow aviators. She is now the Chair of the Indianapolis Chapter of the "99s", the national organization of women pilots, founded during World War II.

I don't know what the future of private flying is. Some people think that it is dim because the airspace and airways are becoming so crowded. But the record suggests that private flyers and their airplanes will keep up with aeronautical technology and that most of them will cope with increasing traffic density and associated complexity of procedures.

The biggest handicap faced by personal aviation is the cost of the airplanes and their equipment. We are told that one reason for the inflation of these costs is that the manufacturers have to purchase very expensive insurance policies to protect themselves against liability lawsuits and huge awards made by courts and juries--a common complaint in today's business world. It is my opinion that personal aviation, like so many activities in our civilization, does and always will involve risk, and that such risk should be accepted by those who participate, excepting, of course, the case where the manufacturer or seller has misrepresented the facts.

## CHAPTER 10

## ARIZONA

Having made the decision to accept the invitation of the University of Arizona--having resigned from Cornell, sold our house in Ithaca, purchased one in Tucson--we undertook to move everything out here. This undertaking was more like our two moves westward in 1965 and 1973, for our sabbatic leaves, than our move eastward in 1946; after all, no small children or dog were involved, and two cars and an airplane were involved. It was one of those tense moves in which you supervise the packing of the moving van and then are obligated to be present when it arrives at the new house three or four days later; the difficulty is caused by the fact that the moving van travels day and night, at 70 m.p.h. We accomplished the feat without actually having to drive all night; we went by way of Buffalo and left the Twin Comanche there, having seduced Gifford and Gracie Bull into flying it from there to Tucson for us, a few days later.

Mabel and I drove one of the cars, a Capri, to Tucson, and I found, without much difficulty, a pleasant young man, a Cornell student, to drive the other car--my beloved 1964 Corvair Spyder. He was enroute to Southern California at the end of the '73/'74 school year. I checked him out rather carefully, ascertaining that he had

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a good driving record, was not a drinker, etc. Therefore I was a bit disconcerted when, just before his departure, he asked me if it would be all right for him to take another student with him and to share the driving; he had advertised and had found a student who also wanted transportation westward. I answered that he would have to take the responsibility for that student's driving ability, reliability, and sobriety--there wasn't enough time available for me to make the same sort of check-out as I had made of him. He agreed; he said he was confident that she was perfectly well-qualified in every way.

We had the Corvair pretty well loaded with suitable boxes of our stuff, but when the students were ready to start driving they added a small tent. The young man explained that they would be pitching the tent for their nights enroute, not staying in hotels or motels, to save money.

Off they went. I was curious--thought I would ask Susan, who was, after all, closer to their age, about the significance of this program: "Does it mean that there will be an intimate relationship, during the trip, between these two students? It's a very small tent."

"I don't know," she replied. "Nobody knows and nobody needs to know."

Of course she was absolutely right, and I felt chastised for my prurient curiosity.

The Corvair arrived on schedule, we arrived in the Capri on a Monday and accomplished the "closing" of the house purchase before the moving van arrived on Tuesday, and the Bulls arrived in the Twin Comanche a few days later. I had business in the East shortly thereafter, so the Bulls and the Searses left Tucson together in the Twin, for a leisurely trip back to Buffalo.

The F.A.A. has a rule that requires every licensed pilot to have a "Biennial Flight Review", administered by a qualified instructor. Giff Bull was qualified to give such a review, and I was approaching the end of the two-year validity of my last one, so we had the bright idea of combining the two-day trip from Tucson to Buffalo with the required B.F.R. I wonder if there ever was another review that accomplished so well the goals of the B.F.R.: Giff was able to observe and criticize my flying techniques and my practices in

visual and instrument conditions, at small fields and large air terminals. Incidentally, I passed.

I have told you, at the end of Chapter 7, how I was suspected of quitting Cornell and going to teach at the University of Arizona because there would be better flying weather in Arizona. If the writer of that calumny had only known it, there really was an attraction to the U. of A., related to my do-it-yourself flying: The Department of Aerospace and Mechanical Engineering, which I joined, possessed a rather sophisticated twin-engine flight simulator that was available for my use. It is called a "Flightmatic 400", and has (simulated) almost exactly the same equipment as my Twin Comanche (which some of my readers will recognize: dual omni receivers, glide-slope, DME, ADF, marker-beacon receiver, autopilot, transponder, wing-flaps, retractable landing gear, electric fuel pumps. It costs about \$20 per hour to "fly" such a simulator, if you can find one, at your local flying school. It was available to me without cost, in return for the responsibility of keeping track of its keys and making sure that only qualified personnel used it.

For an amateur, part-time pilot like me this was true luxury. I made the practice of flying it two or three times every week, shooting instrument approaches--with and without engine failures.

I am told that the simulator was purchased by the A.M.E. Department back in the 1960s when my predecessors offered a ground-school course--probably non-credit--for U. of A. students. One of the important features of the machine during the time that I used it was that Professor Steve Kukolich was available. He is a Professor of Chemistry, an airplane pilot, and the owner of a twin, a Cessna 310. He had the uncanny ability to keep the simulator working--seemed to be able to fix anything that went wrong with it--and his services cost us nothing but its availability to him whenever he wanted some simulated instrument-flight time. Unfortunately it is out-of-commission as I write this, and without my needing it and providing for the cost of some worn-out parts, I am not sure it is going to be fixed.

I taught aerodynamics and fluid-mechanics courses at the U. of A. until I retired from the employ of the State of Arizona in 1978.

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I supervised the studies and the research of graduate students both before and after retirement, and did my share of committee work; we have succeeded in building up a remarkably strong faculty in the field of fluid mechanics, within the Department of A.M.E. and with the cooperation of the program in Applied Mathematics.

The U. of A. was so kind as to award an honorary Sc.D. degree to me in 1987. The Commencement ceremony at which this honor was bestowed was honored by the presence of Governor Ev Meacham, the highly controversial, far-right-wing politician who, shortly thereafter, was impeached by the State Legislature and removed from office.

Since "Gov Ev" and I were apparently the most important persons in the Commencement, we led the academic procession from the Library to the fieldhouse, McKale Center, where the ceremony was held. I thought it was incumbent upon me to make some small talk with the Governor--whose political views and actions were, of course, anathema to me. So, as we started our Grand March I tried to think of something notable but non-controversial to say to him. I hit upon a most ingenious subject: I said, "Beautiful day, isn't it, Governor?"--which it was.

The Governor, faced with the challenge to make a reply as profound as my question, showed himself equal to the task: "Yup," he said, "it's the kind of weather that makes Arizona's population grow!" At about that point the line of marchers led by him and the line led by me diverged and our sparkling conversation ended.

During my sabbatic semester at the University of Arizona (the Spring Semester of 1973) I became a member of its "Collegium Musicum", a musical organization devoted to old music, and was one of its recorder players until just recently. During the school year we rehearsed weekly and presented one or two public concerts each semester. As at Cornell, I found a great deal of pleasure in participating in this kind of musical group; the student musicians--especially the vocalists--are remarkably talented. I suppose the Collegium's public concerts are typical of amateur efforts: some of the pieces come off much better than others.

At one of the concerts, a few years ago, our recorder consort presented a version of "Taunder Naken" written by King Henry VIII

himself. It was, already in his day, a traditional piece, and Henry's version of it, written for a trio of recorders (or other instruments), is rather elaborate, tuneful, fast-moving, and syncopated--the top line a kind of virtuoso piece for alto recorder. [Henry, although unquestionably a villain of history, was a talented musician.]

I suppose the virtuoso top line was written to be played by the King himself, but in his absence that honor fell to me. We played from the stage of the large auditorium, Crowder Hall, in the music building. When my big moment arrived, the four members of our recorder quartet were on the stage; on my right was Rick Day, an astronomer and talented recordist, who was not playing in "Taunder Naken". All went well until, in the middle of page 2 (of the three-page piece), I realized that I hadn't put up page 3! It was on my music rack but covered by page 2!!! I nudged Rick vigorously with my knee without interrupting my playing; he moved his knee; I nudged again; he grasped that I wanted something and whispered, "What's the matter?" I skipped one note to say, "Page Three!!" He understood, uncovered page 3, and we finished "Taunder Naken" without further incident.

Even the young Assistant Professor who directed the Collegium, sitting in the front row of the audience, didn't notice our little near-catastrophe, but Professor Woods, Director of the School of Music, did. When the concert was over he came up on the stage and complimented us players. With a twinkle in his eye he said to me, "You play best when you have your music open in front of you, don't you?" The concert was reviewed in the Tucson Daily Star, including praise for "Taunder Naken".

Once we were invited to give a concert at Arizona State University. The A.S.U. campus is located in Tempe, a suburb of Phoenix, about a hundred miles north of Tucson. It happened that Giff Bull dropped in to visit us at just that time. He and a co-pilot/mechanic were flying in Calspan's variable-stability B-26 airplane. The B-26 is the attack-bomber manufactured by Douglas Aircraft in large numbers during World War II; it was called the "A-26", and there was a "B-26", a twin-engine bomber built by the Martin Company. For some reason the Air Force brass, after the war, decided to change the designation of the Douglas airplane to "B-26"--I suppose there

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were no Martin B-26s in service at that time.

Anyway, the Douglas airplane, which I will, perversely, to prove I am an old-timer, continue to call the "A-26", is such a useful machine, well-regarded by its crews, that it is--or at least has been--used for many peaceful purposes. One of these was to provide an airplane whose flying qualities could be changed to make the airplane simulate, in flight, other existing or proposed airplanes. This program was originated by the Cornell Aero Lab (later called Calspan Inc.) under Air Force sponsorship, and the modified A-26, whose stability-and-control characteristics could be varied over a substantial range of parameter-values is the one that came to visit Tucson on the occasion I am describing. It was a somewhat strange-looking airplane, having had aerodynamic features added to its wings and a second pilots' compartment built in its nose. Giff offered me a ride to Phoenix; I went to the Tucson Airport with my case of recorders and sheet-music and dressed in my tuxedo for the concert--but with black tie in my pocket--, climbed up the little ladder in the bomb bay, and off we went to Phoenix.

I confess that I am amused by this joining of 20th-century technology and 16th- or 17th-century culture--with the tuxedo as comic trimming. If you put your mind to it, Dear Reader, you can probably find in that flight a metaphor for my career--for my "Twentieth-Century Life".

But let's not, shall we?

At one point I was asked to serve on an interesting committee called together by the President's Science Advisor, namely one to answer the question "Why should mission oriented units of the government have the authority and funds to carry out basic research? This is a pertinent question, since "basic research" is supposed to mean research of which the goal is understanding and the practical applications are unknown. But all of us who work in the "applied sciences" are convinced that a good applied-science laboratory must do some of its research in the basic category. It must be sure that basic research is pursued in areas that are likely to impinge on its mission. And it must have personnel within its own walls who do pursue--and understand--such research.

So I accepted the assignment, and I found that the committee was made up of some first-rate people, including the Chairman, Dr. John

Galt. One of the members was Prof. D., head of a department at MIT. I was dismayed when, at one of the committee's meetings, Prof. D. presented a Table, which he thought ought to be included in our report. It listed, across the top, various categories of laboratories: "Governmental, Industrial, . . . , University", and, vertically, such qualities as "Originality, Training of Personnel, . . . , Reduction to Production, . . ." And each element of this matrix was given a grade, from A to F.

Some of the members protested, including me; they could think of lots of exceptions. Prof. D. answered that he would include a footnote saying that these were only typical grades, and that there would be exceptions. One of the committee suggested, tongue-in-cheek, that we include a footnote saying "This Table is not to be taken seriously."

There was a silence. I felt that I had to speak up: "I'm afraid I haven't made myself clear. If this Table is included, I will not sign the report."

There was a silence. Then a vote was taken, the Table was roundly rejected, and we proceeded to the next item. At this point Prof. D., who was sitting two seats to my right, reached around the person between us to say to me, "I hope you're not angry with me." I've never been quite sure why he thought that he ought to apologize to me. His Table was really a stupid idea; I can easily imagine someone using it as a quick and easy way to evaluate research proposals: Just give 4 points for every A, 3 for every B, etc., and the proposal with the highest total score gets the funds!

Tucson is often described as a cultural oasis in the desert. That is a bit ambiguous, isn't it; does it mean a cultural oasis in a geographical desert or a cultural oasis in a cultural desert? In either case, I guess, the University of Arizona should get much of the credit.

At any rate, the University has attracted some distinguished artists and scholars. One of these is the multi-talented Yulla Lipchitz, widow of the sculptor, Jacob Lipchitz. The campus is graced by several of his works, donated by her.

Yulla is a most creative and original woman. She is, herself, an accomplished sculptor; since her famous husband's death she has finished several of his major projects and has also established

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herself as a leading artistic photographer. Meanwhile, she has gotten interested in airplane flying--I tried to assist by putting her in touch with the right local flying schools--and in horseback riding. The picture we are expected to visualize is a lady trained in Tucson in a western saddle riding in Central Park of New York...

Anyway, she rode out of one of the local riding establishments into the Santa Catalina Mountains north of Tucson, and, once, she got lost. All the valleys and gorges she had come through looked the same as she tried to find her way back.

But, happily, she saw a couple on the mountainside above her. She shouted, "Hello! Hello! I am lost! Can you help me?"

There seemed to be conversation between the people up there on the mountain. Finally one of them answered her, shouting "Bitte! Können Sie Deutsch sprechen?"

She could, of course, and replied, "Ja, natürlich!"; the problem was soon solved.

I had not been living in Tucson very long when I encountered a medical problem--a new experience for me, for I had never been overnight in a hospital. I needed a hernia repair. We were members of a health-maintenance organization, sponsored by the university; the HMO staff included a surgeon, Dr. Felix Jabczenski, and I was assigned to him.

By remarkable coincidence, my faculty colleague, Professor E. K. (Ted) Parks, whose office was (and is) next to mine in the building housing the Aerospace & Mechanical Engineering Department, was also encountering an abdominal hernia and was scheduled to have it repaired by the same doctor, in the same hospital, at the same time. Of course the coincidence was noted by our colleagues and became the topic of clever jokes: It was suggested, first of all, that Dr. Jabczensky must have been the lowest bidder on the hernia job--also that the university got an especially low price for the two operations on the two aerospace professors as a package.

And, inevitably, the surgeon's Polish name made the whole affair seem more comical to some people. I asked Dr. Tony Forte, who was one of the heads of the HMO as well as an enthusiastic aviator--and, incidentally, the official flight surgeon and examiner of Tucson's airplane pilots--if there were any good jokes about Polish surgeons. He knew one, and was convinced that "Doctor Jabby"

would enjoy it: *The Polish Academy of Medicine* proudly announces the world's first successful hernia transplant!

At that time, Shirley (Miller) Ashkenas, the wife of my old friend and coworker, Irv, was working as a secretary to a surgeon, Irving L. Lichtenstein, who had written a book called *Hernia Repair Without Disability* (Mosby, St. Louis, 1970); when she heard that I was going to have the repair, she sent me her copy. I found it very interesting, but not entirely reassuring. For example, there were diagrams showing, with arrows, the several layers that make up the abdominal wall, with notations warning that the surgeon must not confuse Layer G (say) with Layer F [when he rebuilds the wall], since one of them has good structural strength while the other "has little intrinsic strength and is virtually worthless as a supporting layer for the repair."

I took the book to Ted Parks, intending to tease him with the idea that our surgeon might not have the book handy during the operation and might confuse the layers. Ted surely did find the volume interesting, and asked to borrow it for study. But he came back to my office with it almost immediately, looking very pale. He explained that "pictures like these"--they were only black-and-white pen-and-ink sketches of the anatomy--always made him ill. He would have to forego the engineering details of the operation.

While I was recovering from the repair--with, I'm afraid, a good deal of "disability"--Ted, recovering from the same procedure, undertook to drive, alone, from Tucson to Southern California where he had agreed to work for NASA through the summer. Just a few miles out of Tucson, in the desert heat, his car developed some kind of trouble that required him to raise its hood and work on it. He got it running successfully and made the journey.

In 1979 Mabel and I were invited to visit China. Our hosts were the Institute of Mechanics, in Beijing, which has already been mentioned several times in this book. This institution is one of a number of Institutes established and administered by the Chinese Academy of Sciences. In 1979 its Director was, officially, its founder, our old friend Dr. Hsue-shen Tsien. Actually, Dr. Tsien was busy with matters more difficult and more pressing than applied mechanics--he has been described as the highest ranking civilian scientist in the Chinese government--and had apparently visited his

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Institute infrequently for some years. His deputy had been Dr. Yung-huai Kuo, until his death in an airplane accident; in 1979 the organization was directed by a small committee of deputies, which included Dr. Hao-sung Tan, my former student and good friend.

I was asked to confer with research personnel of the Institute and to deliver a series of lectures on aerodynamics. Three more of my former students, Drs. Shou-hua Tsien, Liang-ju Pan, and Tien-fun Sun, and their families, helped to entertain us during our five-week stay in Beijing and its environs; together with Tan they constituted our "Gang of Four". Dr. Sun was, at that time, Professor of Mechanics in Beijing (Peking) University; the others were staff members of the Institute.

We had maintained contact with Kuo's widow, Pei Li, over the years, and were delighted to find that she was hostess to us in Beijing. In 1979 she was a professor at a graduate university administered by the Academy of Sciences. She, the "Gang of Four", and many of the Institute's staff met us at the airport upon our arrival; in fact, they seemed to take over the Beijing Airport.

Our visit to China was indeed a delight. We renewed old friendships and made wonderful new ones. We saw the members of the H. S. Tsien family on several occasions--we even had an opportunity to discuss politics with Tsien. He agreed with us on the importance of China's having a system of laws and courts to safeguard the rights of its people. He thought that the tragedy of America's war in Vietnam was that it distracted us from our real enemy, the U.S.S.R.

I lectured in English to classes of several hundred; I was assigned a bright young man, Yun-min Chen, who translated my lectures sentence by sentence. He did a splendid job of it; he is, himself, a fluid-mechanicist and understood the material very well. Occasionally he became so interested in what I was saying that he began to discuss it enthusiastically with me; I had to remind him that he was supposed to tell it in Chinese to the audience! When this happened it brought peals of laughter from them, many of whom obviously understood English.

Chen later went to Cornell as a graduate student, made a very good scholastic record, and acquired the Ph.D.

Our brief visit to China took us to some beautiful and famous

places--Shanghai, Canton, Hangchow, and (on the way home) Hong Kong, but it was not primarily a sightseeing trip. In Beijing we stayed in the Friendship Hotel. As we rode the hotel's elevator one morning with Dr. Tan, we were joined by a tall, blond, Anglo-Saxon who, when he looked at Tan, said "I say! You're Bert Fung!" Now "Bert" is Y. C. Fung, a distinguished professor of the University of California San Diego; he and Tan do, in fact, resemble one another, I think.

I spoke up. "No," I said, "this is Dr. Hao-sung Tan, and you are Ian Axford, and I am your old Cornell colleague, Bill Sears." So all that was cleared up, and we had breakfast together.

Mabel and I met Axford again, a few days later, in Shanghai. He had, indeed, been a faculty colleague at Cornell, where he was in Electrical Engineering and we both worked in magnetohydrodynamics. He is a native of New Zealand, I think, and is a first-class applied-mathematician. He told us that he was now a staff member of one of the Max Planck Institutes in Germany, and he offered the advice that Germany was now "the only place to do research".

Just one more little story from our trip to China: The Friendship Hotel was where most of us foreigners, brought to China by the Academy of Sciences, were housed, so we became acquainted with some interesting scientists from several countries. They included a psychologist, Professor Over, from Australia. [Could he be the inventor of the the coat, the shoes, etc., that bear his famous name?] [I apologize.] Over was lecturing to big classes of Chinese students, as I was, and was having trouble getting them to take part--to speak up in class. At one point he required every student to come to class prepared to report on his or her family's attitude about left-handedness. The result of the survey was that (a) left-handedness is not considered very serious, socially, but (b) it's a bit unfortunate for a girl, especially if (c) she uses her chopsticks with her left hand!

We got to talking about including jokes in one's lectures, and at this point an American professor of Chinese background spoke up: "Oh, you must never try to do that in a class of Chinese students!" he declaimed. But he was quite wrong. I included jokes and funny anecdotes in all my lectures, and they were obviously understood and enjoyed. They brought laughter, first from the front row, where

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a dozen or so professors were sitting; they understood English. And when Chen repeated them in Chinese the whole big audience laughed!

[Now, if you are quite skeptical, Dear Reader, you may suspect that Chen, in translating, said "This is a joke, and the American professor will expect laughter. So please laugh now." But you will have to explain why the front row laughed before he translated.]

We traveled back from Hong Kong to the U.S. in a McDonald Douglas DC-10. We were impressed by a bearded young man, obviously proficient in Chinese and English, who voluntarily assisted dozens of Chinese passengers in the intricacies of their paperwork for entrance into the U.S.

It happened that the terrible DC-10 accident in Chicago had occurred just then, and the DC-10s were temporarily grounded while we-all were out over the Pacific. When we made our scheduled landing at Anchorage we learned that there would be a layover while the airline brought other airplanes up from the "lower 48" to transport us, and that an emergency cafeteria line would be set up at the Anchorage airport to feed us.

As we stood in line at the emergency cafeteria we were amused to hear a small passenger ask his mother if he could have guacamole for lunch. His mother laughed and told him, "I don't think they will have guacamole here, dear, but you can have it as soon as we get to Tucson."

I couldn't resist going over to their table, when they were seated, to introduce myself--especially when I saw that the daddy of the small passenger was the bearded young man who had been so helpful on the flight from Hong Kong. He recognized my name! He had been a successful applicant, a few years back, for a Josephine de Kármán Fellowship, and had seen the names of our committee members in the announcement. He was now an Assistant Professor at the University of Arizona, in the Department of Oriental Studies. He had spent a year in Taiwan and was returning, with his little family, to Tucson, where they have guacamole. I remember that his surname was Priest.

One of my bright former students at Cornell, Dr. Dick Seebass, who had become a professor there--and even Associate Dean of the Cornell College of Engineering--came to Tucson in 1975 as Professor

in our Department of Aerospace and Mechanical Engineering. With him came one of his distinguished former students, Assistant Professor Kee-ying Fung, and two promising graduate students, Messrs. John Celeste and John Parker; clearly a Cornell Contingent was making a major contribution to the A.M.E. Department.

Mr. Parker had already, at Cornell, completed a substantial part of his course work for the Ph.D., but it was the decision of his Committee that he repeat one or two of these courses, including one of mine, to qualify for an Arizona Ph.D. I was very surprised, disappointed, and puzzled when he did badly in my course. I called him into my office and asked him what seemed to be the trouble: "You've seen all this stuff before. Why can't you work the problems and pass the exams?"

"Because I've seen all this stuff before!" he explained, obviously exasperated by my stupidity. "How can I study it and work out problems in it when I've seen it all before?"

I had to admit that I was still baffled. He tried, patiently, to explain: "Dr. Sears! Do you think that if you went back to a fifth-grade or sixth-grade classroom you'd be able to get an A in every test?" I told him I certainly hoped so!

We have named this the Parker Principle, in his honor; we think it takes its place with the Peter and the Parkinson Principles. Simply stated, it says that you can't work problems in a subject, or pass exams in it, if you know the subject.

None of us understands it, but we've had occasion to refer to it as the years have passed.

Meanwhile, Dick Seebass has gone on to become the Dean of Engineering at the University of Colorado and a leader in the community of educators in engineering in the United States.

I've written only briefly about our children, David and Susan, but nothing about our grandchildren, David's kids, Colin David and Shelby Ann, now 26 and 24 years old, respectively. Colin follows in his grandfather's footsteps--he's a drummer. He has now graduated from the University of Massachusetts, in History, with honors, and his sister seems likely to graduate from San Francisco State in June 1993. Colin plans to exploit his musical career (recordings, international travel, royalties, and all) for some time before

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going to graduate school. He is obviously very talented in his metier, which is rock music.

All books must end, and this one will end here. Writing it has been fun for Mabel and me; it has reminded us of all these wonderful people, places, and experiences.

For some years I have been somewhat startled by news of the retirements of my former students! But I shouldn't be surprised; I taught lots of students who were almost as old as I, and a few who were older than I.

What is sadder, of course, is that some of the principal characters of these memoirs have died, sometimes--as in the case of Arnie Sevareid--just as I was writing about them.





