The Flying Professor
a very rough draft

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Abstract

Professor R. M. Kiehn, B.Sc. 1950, Ph.D. 1953 (Physics, Course VIII, MIT), started his career working (during the summers) at the Argonne National Laboratory on the Navy's nuclear powered submarine project, and on special projects at MIT. Argonne was near his parents home in the then small suburban community known as Elmhurst, Illinois. At Argonne, Dr. Kiehn was given the opportunity to do nuclear experiments using Fermi's original reactor, CP1. The experience stimulated an interest in the development of nuclear energy. After receiving the Ph. D. degree as the Gulf Oil Fellow at MIT, Dr. Kiehn went to work at Los Alamos, with the goal of designing and building a plutonium powered fast breeder reactor, a reactor that would produce more fissionable fuel then it consumed. He was instrumental in the design and operation of LAMPRE, the Los Alamos Molten Plutonium Reactor Experiment. He also became involved with diagnostic experiments on nuclear explosions, both in Nevada on shot towers above ground, and in the Pacific from a flying laboratory built into a KC-135 jet tanker. He is one of the diminishing number of people still alive who have witnessed atmospheric nuclear explosions. Dr. Kiehn has written patents that range from AC ionization chambers, plutonium breeder reactor power plants, to dual polarized ring lasers and down-hole oil exploration instruments. He is active, at present, in creating new devices and processes, from the nanometer world to the macroscopic world, which utilize the features of Non-Equilibrium Systems and Irreversible Processes, from the perspective of Continuous Topological Evolution. Dr. Kiehn left Los Alamos in 1963 to become a professor of physics at the University of Houston. He lived about 100 miles from Houston on his Pecan Orchard - Charolais Cattle ranch on the banks of the San Marcos river near San Antonio. As a pilot, he would commute to Houston, and his classroom responsibilities, in his Cessna 172 aircraft. He was known as the "flying professor". He is now retired, as an "emeritus" professor of physics, and lives

in a small villa at the base of Mount Ventoux in the Provence region of southeastern France. He maintains an active scientific website at http://www.cartan.pair.com

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Dr. Kiehn left Los Alamos in 1963 to become a professor of physics at the University of Houston. He lived about 100 miles from Houston on his Pecan Orchard - Charolais Cattle ranch on the banks of the San Marcos river, a bit east of San Antonio, and a bit south of Austin, TX. As a pilot, he would commute to Houston, and his classroom responsibilities, in his Cessna 172 aircraft. He was known as the "flying professor". He is now retired (2000), as an "emeritus" professor of physics, and lived 2000-2010 in a small villa at the base of Mount Ventoux in the Provence region of southeastern France . Late in 2010 he moved back to Texas, about 30 miles north of Austin. This autobiographical sketch is presented as a series of anecdotes, written in chronological sequence, but in a form that does not have to be read in sequence. This is a very rough draft. Old topics will be re-written, and new topics added, as I find time.

Cessna 172 Preflight inspection

2 Early years in Chicago,1929-1938

Until the age of 8, I lived on the southside of Chicago, on Crandon avenue, between 81^{st} and 83^{rd} streets, just to the west of a heavy duty ethnic Polock - German neighborhood. The earliest I remember was when my family lived in the second story of a 3-story flat on the southwest corner of 81st street and Crandon. Above us lived the Clancys, and my Super Hero (because he was ~4 years older): Raymond Clancy. I tell this story, for there is no doubt that the events during that period influenced my decision to go directly to Los Alamos from MIT in 1953. Raymond had an famous uncle (a painter and an author, with the pen name of Holling Clancy Holling - more later).

I can remember (when I was 6 or 7) getting up early in the summers to ride in the horse-driven milk cart with the milkman - as he would make his rounds up and down the back alleys. The iron monger use to come by about once a week with his horse drawn wagon, shouting out "ragsali" (rags - old iron). I can remember jumping on the back end of the 79th street cars, and hitching a ride to the Avalon movie theater, where I would watch the movies twice. I can remember taking the street cars and elevated trains to the museum of science and industry, and the aquarium, without parental supervision!

I started grade school at Horace Mann school, and walked to school with my buddies, Don and Bruce Masterson, who lived on Luella avenue. Dr. Masterson had a huge radio set in his office, and the boys always bragged that he had been one of last people to stay in contact with Amelia Earhart.

2.1 The Book of Indians

As a birthday present, the Clancys gave me two books by Holling C. Holling: the "Book of Cowboys", and the "Book of Indians". Holling lived in the art community of Taos, New Mexico, and was steeped in the authentic features of life in the West. His books were written for children in terms of vignettes in the life of different Indian tribes, with each page containing many perfect sketches of scenes and features of times and culture. I learned how to confine a horse in the desert by digging a small hole and tying the Halter rope around a rock to be buried in the hole. I learned that he who catches a fish has to clean it. I also learned that if you pull the fangs from a rattlesnake, he will grow back a new set in a short time. I learned to build rabbit traps (very useful in metropolitan Chicago) and many other useful things of interest to small boys.

One of the chapters was about the Pueblo Indians and the area in the Los Alamos region. When the time came, there was no question in my mind that I should go to work at Los Alamos, for this part of the West had been drummed into my brain by "The Book of Indians". Thank you, Holling C. Holling.

2.2 Travel through the USA.

My mother always had a lust for travel, and by the time I entered high school I had been in most of the 48 states. Dad let the two of us go on such trips, with me acting as the chaperone. I learned to read from road signs, road maps, and Burma Shave signs, while standing upright on the right front seat of the car – no car seats in those days. The education of travel stuck with me.

3 Elmhurst 1938-1946

3.1 Peter Quill

I should start this part of the story in 1938. Dad had rented a small house on May street in Elmhurst, which in 1938 was an almost-rural suburb of Chicago. We lived in that house for about a year, while our new home was being constructed at 476 south Kenilworth Ave in Elmhurst. The street was lined with gorgeous Elm trees that formed an impressive closed arch.

Thanks to moving from the ethnic German-Polish south-side of Chicago to the Elmhurst school system, I was given a number of tests. Even though I was eight years old, they put me into 5th grade! I was about a foot shorter, and 10 to 20 pounds lighter than my classmates. Bummer!

The reason that I focus on this time period is that, in those days of no television, radio was the media choice of entertainment. At that time, station WGN in Chicago began to broadcast, as a radio program, the adventures of "*Peter Quill*" (on the radio introduction the name was dragged out as "Peter, Qui-ui-ii–lle-lle"). Peter Quill was a hunchback, mysterious, scientific genius, who invented, among other many other things, a death-ray. You can read about the program by google-ing "Peter Quill".

To the best of my memory, my interest in science, in effect, started with my thrills of listening to the episodes of Peter Quill on the radio. I went, one day, to my 5th grade class, hunched over and dragging my foot. When my 5th grade teacher asked what I was doing, and admonished my posture, I responded that I was "Peter, Qui-ui-ii–lle-lle". (She quickly informed me that I should stand up straight.)

We moved to our new home, and I entered 6th grade. During the 6th, 7th, and 8th grades, my mother insisted that I have some musical training, and I learned to play the clarinet under the tutelage of P. M. Kiest (sp) and performed in the 7th and 8th grade orchestras. However, when I entered high-school my interests turned to playing the saxophone in dance-bands of the Harry James, Benny Goodman, Glenn Miller, Duke Ellington variety. Dance music and ball-room dancing became a great interest for me early on. Moreover, Dad and Mom were always working for the War Effort. I entered high-school (9th-12th grade) at the age of 12 (1942), and still a foot shorter and 10-20 pounds lighter than my classmates.

3.2 John Drummond

During 7th and 8th grade summer vacations I worked at Mount Carmel cemetery, a huge catholic cemetery outside of Chicago, but a short bicycle ride from our new home on Kenilworth Avenue. I started out, cutting grass and watering grave flower plantings (circumventing whatever child labor laws existed at the time) , but that led up to digging graves as I grew older, and WWII took away the able bodied men! All jobs were attempts to add to the family income, and my possible education, but they did not add much to my scientific skills. One thing that was important to me was that I met John Drummond, while digging a grave next to the one he was digging. John was 60+ years old; a tough old Polish immigrant, who worked hard all of his life as a laborer. At 14-15 years of age, I decided that the only difference between the old Polock (John) and this young Polock (me) was that I had a chance to get a good education, that John never had. Later on, at MIT, when I became discouraged, I would think of old John Drummond, digging in that hole, at the age of 60 plus. Often I would think to myself that image of John Drummond could be me if it were not for the opportunity that I had to obtain a good education. That thought always motivated me to continue through the many tough times at MIT (thank you, John Drummond).

My science exposure before WWII (and the adventures of Peter Quill) was through Popular Mechanics or Popular Geographics magazines that I devoured on visits to my uncle Ted's, or uncle Michael's, house. My dad had 8 brothers, and 3 sisters, all dead by the age of 54. My paternal grandfather lived across the street from the steel mills on Brandon avenue, south of 79th street in Chicago's south side.

3.3 WWII in Elmhurst 1941-1946

3.3.1 Membership in the Trade Unions.

During World War II, any body that would work was in demand. I started as a day laborer, pouring concrete on construction jobs. As I remember I had to join the Union (International Hod Carriers, Building, and Common Laborers) even though I was under 16. This Union in Chicago has always has bad connections with the Mob. Later on, playing in dance bands in Chicago required a Union card, at most locations. My musical skills almost made me miss MIT. I was offered a band-job on the winter circuit in Florida. I believe the salary was about $200 a month. Unbelievable for a 16 year old kid. Fortunately, John Drummond and my dominating mother saw to it that I went MIT.

3.3.2 Rosie the Riveter

My mother went to work using her seamstress skills to make parachutes. The only problem is that she made 3 times the number of parachutes that the other ladies could make per day. She was not popular, because of her skills. So she went to work at the Douglas Aircraft plant at what is now called O'Hare field. She got a C ration card to get enough gas to drive to work. This left no one at home to take care of my younger sister, who went to grade school, but was alone, except for me, when she came home. Everyone did his bit during the War. Dad left at 6 am and returned home at 6 p.m. My job was to do the best I could with my sister, and to start the oven and put in the prepared dinner (left by Ma before she went to work) so we all could eat one meal together.

3.4 Why I went to MIT

I developed an intense interest in aviation before and during the war, and constructed many intricate flying models. AS I mentioned above, my mother worked at the Douglas Plant building DC-4,s. She knew I was interested in model airplanes, and aviation in general (although she did not know that I used to sneak out to the local Elmhurst airport to take a few lessons in a Piper Cub with the Civil Air Patrol, whenever I made a few bucks in my band jobs to buy gas). She asked her boss at the Douglas plant of what he thought would be a good school where I could get training as an aeronautical engineer. The boss replied that he thought MIT would be a good choice. Ma came home and said "You are going to go to MIT when you graduate high school". I said "OK, Ma", before she could wop me on the back of my head to emphasize her edict.

Although I had recognized that God had given me a gift, my science training in high school was not inspiring. I can remember a science exam which had questions such as "How many buttons were on Ben Franklin's great coat?" Not much of a basis, I learned later on, to compare with the excellent preparation afforded to students that had gone to Chauncy Hall, specifically to gain entrance into MIT. I did manage to be Senior class president, Editor of the year book, and salutatorian of York Community HIgh School, Elmhurst, Illinois.

In the late summer of 1946, equipped with a cardboard suitcase and with instructions on how to send it home with my dirty laundry, I was packed on the train to Boston, at the age of sixteen (!), with instructions to attend MIT. However, although I had visited some MIT alumni interview events in Chicago, somehow, I had not taken the MIT entrance exam!!!

In the midst of all the confusion at the end of WW II, and with the large influx of returning GIs into the school systems of the USA, I managed to get enrolled at MIT in course XVI - Aeronautical Engineering (without the entrance exam!). I learned that the way to get things done at MIT was through the various departmental and registrar secretaries, who seemed to be overloaded, and who would always respond to appreciative bit of soft soap.

I also made use of my skills with alto-saxophone and clarinet playing in dance bands and jazz bands to work Friday and Saturday nights in Boston, which helped pay the bills. I learned these musical skills as a teenager during WW II, when most of the older musicians were off fighting the war. War or no war, people still wanted to have dance parties, and without the older musicians, they had to make use of any musicians that they could get – high school students!! I played many dance engagements, private parties, and even in a pit band for strippers in Cicero (the Mafia heartland)– all as a teenager.

My parents were good people, who worked hard, and helped me pay the way through school, but when I went into physics during my second year (transferring from Course XVI to course VIII (Physics), they were a bit lost, technically. In effect by the time I was 17 I had decided on my mission in life was to somehow get involved with Nuclear Power, and build a Nuclear Powered Rocket Ship. Except for visits home to Elmhurst, I was out on my own (at least from my perspective) but my parents always gave me moral (and financial) support when I needed it.

4 MIT 1946 - 1953

4.1 Undergraduate years

In the late summer of 1946, equipped with a cardboard suitcase and with instructions on how to send it home with my dirty laundry, I was packed on the train to Boston, at the age of sixteen (!), with instructions to attend MIT. However, although I had visited some MIT alumni interview events in Chicago, somehow, I had not taken the MIT entrance exam!!!

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GIs into the school systems of the USA, I managed to get enrolled at MIT in course XVI - Aeronautical Engineering (without the entrance exam!). I learned that the way to get things done at MIT was through the various departmental and registrar secretaries, who seemed to be overloaded, and who would always respond to appreciative bit of soft soap.

I soon learned that I could not have passed the entrance exam, but this caused me to sit down with a motivation during the first year to study harder.

4.1.1 Fraternity Life

I also made use of my skills with alto-saxophone and clarinet playing in dance bands and jazz bands to work Friday and Saturday nights in Boston, which helped pay the bills. I learned these musical skills as a teenager during WW II, when most of the older musicians were off fighting the war. War or no war, people still wanted to have dance parties, and without the older musicians, they had to make use of any musicians that they could get – high school students!! I played many dance engagements, private parties, and even in a pit band for strippers in Cicero (the Mafia heartland)– all as a teenager.

My parents were good people, who worked hard, and helped me pay the way through school, but when I went into physics during my second year (transferring from Course XVI(Aeronautical Engineering) to course VIII (Physics), they were a bit lost, technically. In effect by the time I was 17 I had decided on my mission in life was to use God's gift of intelligence and the MIT opportunity, to stand on the shoulders of the scientific giants and contribute something significant to the world. Big ideas for a 17 year old. I also knew that I should somehow get involved with Nuclear Power, and build a Nuclear Powered Rocket Ship.

Except for visits home to Elmhurst, I was out on my own (at least from my perspective) but my parents always gave me moral (and financial) support when I needed it.

4.1.2 Getting the most out of MIT

After the first year, I had learned the system, for all of the courses had problem assignments for the complete term handed out during the first week of classes. The trick was to read and absorb as much as you could, skipping over the hard parts, until you could ask questions during lectures, and try to work as many problems as you could for the whole term. I found out that I could do more than 70 % of the terms homework in the first 3 or 4 weeks. I got much more out of the lectures, then, for I could ask specific questions that would help my understanding. The method worked, and I made good enough grades to get MIT scholarship money, throughout my undergraduate years. Such money was based upon your grade point average for the term - a number between 0 and 5. Scholarship money was available for anything over 4.0 with larger amounts for scores over 4.5.

At MIT, I was perhaps super casual (super cool in the patoire of 2008). I attended class in GI khaki pants and a T-shirt, with a log-log duplex decitrig slide rule sticking out my back pocket (no case). I initially enrolled in Course XVI (aeronautical engineering), but quickly learned that the engineering courses were just physics with a change of notation, and Aeronautical Engineering included a lot of hours on the drafting board, hours that I felt were wasted. (I liked descriptive geometry). I figured that if I trained in Physics, I could become any kind of engineer if I decided to work in that particular field. Although I enrolled in Course VIII (physics) I found out that I could take courses in the EE department (or any other department at MIT) without incurring any additional expense. MIT required a fee per semester, not a fee for each course. So I used to sit in and/or enroll in all the courses in which I found interest. Brown and Campbell's course on servo mechanisms, McAdams' course on heat transfer, Dirk Struik's course on differential geometry were all extra courses that I could attend - for free. It made a full schedule, eight hours a day, including four hours on Saturday (MIT was a land grant school), but I was and always have been such a tight-wad, that I could not pass up getting all these extra courses for free. My entire senior year was dominated by graduate school courses in various topics, for I had completed all my undergraduate requirements by that time. OR so I thought.

4.1.3 German exam

At MIT you can take what are called advanced standing exams, which if you passed, you did not have to take the course, but would be given credit for the exam. So, without any formal course work in German, I took the advanced standing exams in 1st and 2nd year German, in order to get credit for the Physics dept. requirements, and to allow me to take other, more interesting, courses. I flunked the 1st year exam (too much grammar), but passed the second year exam. The physics department rules said I had to have a scientific reading knowledge of German, and told me that passing the second year exam demonstrated my proficiency. BUT a week before the end of the senior year and my graduation, I got a call from the registrars office saying that I did not have enough German credits (two years, not one) to graduate. Bummer.

So I arranged to take the third year course advanced standing exam on the very last day possible, after my final exams. I holed up for 2 days at the Fensgate hotel (where I had worked as a bellhop) doing nothing but studying German. Then the next day, Friday, went in and passed the third year exam (no grammar), and got my B.Sc. degree on schedule.

4.2 Professor Clarke Goodman

By senior year I had became fascinated by the idea of nuclear power plants, in particular with their application to Nuclear Powered Rocket Ships. Before the advent of a course in Nuclear Engineering at MIT, I learned that Professor Clark Goodman (physics department) had written a book entitled "The Science and Engineering of Nuclear Power" covering some classified work at MIT done in 1946. I went to him and said I wanted to be a Nuclear Engineer, and his interests (although in the physics department) as shown in his book, were what I wanted to learn about. He took me under his wing, and I did my undergraduate B. Sc. physics thesis under his guidance and help. I joined Professor Goodman's Nuclear Energy group and produces the article, "The detection of fast neutrons by means of threshold reactions", May 1950, MIT Laboratory for Nuclear Science and Engineering Technical Report No. 40".

4.2.1 Hyman Rickover

Professor Goodman had established a contract between MIT and the, then, Captain Rickover. So when he asked if I had considered graduate school, I said I did not have the money. So he said he would set up a meeting with Rickover and myself to see if the Navy would pay for my graduate education in trade for 6 years of Naval Service. As I waited outside Goodman's office for my interview, inside Rickover was questioning the officers who were taking grad courses at MIT prior to becoming officers on the Nautilus – Rickover's first nuclear powered submarine to be. I can remember that Rickover had these officers braced – full commanders being treated as if they were plebes at the Naval Academy. "Where did you stand in your class at Annapolis", Rickover shouted. The commander be interrogated replied "Sixth position, Sir". Rickover shouted back "Why did you not place higher". ... These preliminaries made me "quake in my boots".

However, when I entered, Rickover was quite cordial, the meeting initially went well. He was surprised that I had in effect given lectures in Nuclear Physics to his Officers, and had attended most of the available MIT courses that his Officers had taken.

THEN, he asked how old was I?

I responded "Nineteen, sir".

And I could see his facial expressions change. I was precisely the type of individual he wanted in his Navy as an officer to mix with his other submariner candidates; but you cannot become a Naval officer until you were in your mid twenties!

Rickover said he was sorry, " ...the meeting is over... would I like some lunch with him at one of the local Back Bay diners".

4.2.2 The Gulf Oil Fellowship

Professor Goodman was also becoming expert in neutron well logging, and had contacts with the Gulf Oil Corporation (and later on with Schlumberger. In the Spring of 1950, after the Rickover fiasco, he had me drop in his Office and asked if I would accept the Gulf Oil Fellowship. All tuition and expenses. What do you think I said. WOW. The only strings were that I should visit GOC after I got my Ph. D. to see what they had to offer as a job.

I enrolled as a Ph. D. candidate (skipping the master's degree) in the MIT Physics Department, and completed my degree requirements in 1953.

4.3 Argonne NAtional Laboratory

About the summer of 1949 I learned that they were designing the nuclear reactor for Rickover's submarine, the "Nautilus" at Argonne National Laboratory, just south of my parents home in Elmhurst, Illinois. That summer I drove out to Argonne and essentially sat for 3 days on the edge of Hoylande Youngs's desk begging for a chance to work on the submarine project at Argonne. I knew that I would need a clearance, as the work was classified by the AEC. She gave all sorts of excuses trying to convince me of the impossibility of getting the chance to work on the design of the Submarine reactor. About noon of the third day (I was persistent and determined) a man walked by (Dr. Joe Dietrich - the chief physicist for the Naval Reactor Division), and Hoylande yelled "Hey Joe, come over and talk to this kid. He wants to build a nuclear powered submarine."

After a bit of conversation with me, Dr. Dietrich turned to Hoylande Young and said "Hire him". I was on the way to my dreamland.

I was assigned to the electronics and reactor controls group, and because of my training in servomechanisms at MIT, I was able to contribute at least a little bit in my short (summer) stay. I remember doing experiments on CP-1-2, Fermi's original reactor that had been moved to Argonne from under the stands at Chicago University. I remember taking a sample out of the irradiation "rabbit" with a pair of tongs, and walking down the lab corridor, then running, after I set off the radiation alarms. I recall Dr. Alvin Weinberg (who became director of Oak Ridge National Laboratory) blanched as I went by, and tried to meld into (through the) wall of hallway.

Perhaps one important contribution that I made while at Argonne was the invention of an AC operated ionization chamber. [Kiehn, R. M. , U.S.A.E.C.U., 1630, Argonne (Aug. 1951).]

4.4 Graduate years at MIT 1950-1953

4.4.1 Nuclear Shielding Laboratory at MIT

After receiving the Gulf Oil Fellowship, instead of going home to Chicago during the summer, I would stay in Boston, living in the Sigma Chi fraternity house. I told Professor Goodman that I wanted to investigate neutron inelastic scattering cross sections in the few MEV range. Professor Goodman said I could use the Van de Graaff generator to produce proton or deuteron beams that could be used to generate neutrons from protons or neutrons on Li reactions. All I had to do was get the old Rockefeller Van de Graaff generator to run again. Previous people (not mentioning certain MIT professor by name) had essentially run the machine into the ground.

I told my self that I would spend 4 months trying to get some neutrons, and if that did not happen I would search for some other thesis.

All I had to do was to tear down this two story accelerator, and then put it back together again – by myself. Pete Demos, the director of the MIT radiation laboratory (the owners and original users of the machine, gave me (a snot nosed grad student who barely was old enough to by beer legally) Carte Blanche and dollar support, but no helpers! I should mention that a newer, bigger, Van de Graaff had been built in the back bay campus, and of course all the professors wanted to use the new one. The old machine became my responsibility.

4.4.2 Running the Rockefeller Van de Graaff generator

I took the Rockefeller Van de Graaff accelerator completely apart, cleaned everything, and put it back together again, essentially by myself, but also with Cambridge ragamuffins, and fraternity brothers who I could "con" into helping me.

A book by Elmore and Sands presented plans and instructions for most of the electronic (vacuum tube) devices constructed during the WWII Manhattan project. I use the facilities of the Van de Graaff to build as many of the different electronics devices as I could. I constructed my own multi-channel pulse height analyzer to be used with a photomultiplier coupled with a NaI crystal that I polished myself.

Servo Control of a BIG triode

Perhaps my best creation was based on my Servo Mechanism (extra) course that I took from Brown and Campbell in the EE department. I also knew that there was a large analog computer in the Aeronautics Department, a relic from WWII that was highly under used. I then considered the 2 story Van de Graaff generator could be modeled as if it were a large vacuum triode. The grid was the corona control arm, the plate the high voltage terminal, and the cathode the bottom terminal, which supplied electrons to the belt that carried the charges to the top terminal. I then took this model to the analog computer, and fiddled with the dials until I got the parameters adjusted for the feedback circuit to the grid. I thereby stabilized the plate voltage through feedback control, and enabled the old Rockefeller generator to operate reliably to 50% high voltages then were achieved in the generators prime.

I had to make all of these automated control devices, as I used to operate the machine by myself. The day I left MIT I filled the liquid air tanks on the vacuum system, took the machine up to 3 MEV and left it to run all day long with no one at the control console. It was because of these successes that Pete Demos made me the "boss" of the old Rockefeller generator. If a professor wanted to use the machine, he had to get permission from a graduate student, me!

Radar interference - a Battleship at 50 yards

The Radiation Laboratory for Electronics had been experimenting with new large radars, to be installed on the "Dew Line", and other locations. They had huge antennas sitting on top of the Radiation Lab, about 100 yards from "my" Van de Graaff. I had built most of my electronic equipment and radiation detectors, but one day after a long run of data taking, my detectors suddenly went berserk. About every 3 to 4 seconds, they would yield a burst of electrical pulses - which I interpreted as a breakdown discharge. So I took the detectors apart, and rebuilt everything. I starting taking data again.

After another long run, the bursting discharge started again, ruining a large amount of work. So I reconstructed the radiation detectors again. This continued for several days. I remember going out in the sunshine and sitting on the ground with my back to the Van der Graaff building, trying to reason out what could be the cause of the burst discharge, then 3 to 4 seconds of no burst discharge, then another burst, anther 3 to 4 seconds of no burst ... Then it came to me. I noticed that the big rotating Radar antennas would sweep around every 3 to 4 seconds. The strong radar signals were saturation my sensitive counters.

I called up the Radar group and discussed my problem, and its solution if they would let know when they were not operating the radar so that I could do my experiments. An agreement was made. However, the next day the agreement was broken, and I lost a days work again. More telephone calls, but with resolution of the problem.

So then I built a small transmitter on the Radar frequencies, and beamed directly into the large antenna which was less than 100 yards away. The Radar then would see the equivalent of a battleship at 100 yards when I turned on my transmitter, and screw up their measurements. The agreement was reinstated, so that both experiments would not interfere with one another.

4.4.3 Plutonium Fast Reactor Design with Manson Benedict

Mason Benedict, who was to become the chairman of MIT's Nuclear Engineering Department, had received a request to design new reactors to produce tritium. He formed a small group of heavyweight professors to come up with ideas, and a small group of graduate student grunts to carry out the calculations and analysis of various reactor designs. I was chosen to look into the possibility of Fast Reactor designs, which ultimately led to my interest in Plutonium breeder reactors. The idea of designing reactors, that would produce more fissionable material then they consumed seemed to be a no brainer idea.

[NUCLEAR PROBLEMS OF NON-AQUEOUS FLUID-FUEL REACTORS MIT-5000; Oct 1952]

4.5 Job Interviews

4.5.1 Crawford Greenwalt experience,

I think I had more 2 or 3 dozen job interviews, but the most notable, save for Los Alamos, was that conducted at the Dupont Research laboratory.

more later

4.5.2 The Book of Indians influence, and the Pueblo Indians of New Mexico

Chose to start work at Los Alamos fall 1953 (the only place that had Plutonium)

more later

5 Los Alamos 1953 to 1962+

By the end of Fall in 1953 I had completed the requirements for a Ph.D degree in Physics, and my first few publications in the Physics Journals. Although I had many job interviews, I knew that I wanted to be involved with a Plutonium Fast Neutron Breeder Reactor design. Los Alamos was the only place that had plutonium. So armed with that fact, and with the dreams of New Mexico Pueblo Indians in my head (thanks to the "Book of Indians"), I accepted the position as a staff member at Los Alamos, at a salary 60% less than a job offer in Washington, D.C. I did not own a car, had very few belongings, but the climate of northern New Mexico, and the thought of doing science among the Ponderosa Pines on the side of a mountain, in the heart of the Pueblo Indian country was an extraordinary draw.

The train ride on the Atchison, Topeka, and Santa Fe from Boston to Lamy, was a bit gloomy in that I was not only leaving Boston and its culture, so different from Elmhurst, but

I also was leaving behind a main squeeze who did not want to follow–bummer. Be aware, the Sante Fe railroad does NOT go to Sante Fe, NM, but it does stop at the Whistle Stop town of Lamy, NM.

The town of Lamy consists, essentially, of an old time swinging door Saloon, a Water tower, and a train station – right out of a John Wayne western movie. A government Khaki colored 49 vintage car picked me up and took me to Sante Fe, where I went through a security check. They then drove up to Los Alamos, through the Guard Gate and into the town. I was deposited in my assigned efficiency apartment, with no furniture, no automobile, no nothing. I slept on the floor. I was close to the LASL motor pool so got a lift to K site which was a few miles away from the town site of Los Alamos. To get around, to the Indian Pueblos, to Sante Fe, to Jaurez, Mexico, I used to hitchhike. I waited at the guard gate until someone would pick me up. Getting back to LASL was another story. One day, on a trip to Juarez, I was picked up by Vic Swiatek, a draftsman at the LAB. Vic asked if I needed a ride to Santa Fe, and I said yes. Then during the trip to Santa Fe he found out that I was going to El Paso, and he decided to drive me the whole way!. Vic lived across the street in what were called the Concrete apartments. We became close friends. He introduced me to Santa Fe society, where he (and I) were always in demand as dancers, for those ladies whose husbands were not dancers.

5.1 K division

I joined K Division (the reactor division) at Los Alamos, which at the time was constructing a Phosphoric Acid Uranium thermal reactor. The reactor was intended to be a high power version of the Los Alamos Water Boiler, but retaining all of the built in safety features (automatic shutdown do to fluid thermal expansion) in case of excursions. Bottom line, it would not blow up. L.D.P. King, the designer of the original water boiler reactor, was the K-1 group leader. When I first arrive I decided to help out with the instrumentation and control room design, for I had gained some experience with these problems when I was at Argonne. It wasn't long, however, that I focused on a Plutonium breeder reactor design.

5.2 T- Division – Main Frame Computers

While working for M. Benedict, I used to do 8 - 10 multi-energy group Boltzmann neutron transport calculations by hand, using a Marchant (pull the lever) for numeric computations. Ugh. Remember – in 1953 this was way before the PC – even before the TI hand calculators. Harvey Brookes -one of the heavyweights on Manson Benedict's team, from Harvard, had contacts with the GE people who were considering Fast Reactor designs which were computed on some form of elementary digital machine. The IBM main frames had not arrived. When Professor Brooks told me about computers, I was hooked.

5.2.1 Learning the IBM 701 and 704 computers

When I got to Los Alamos, T Division had recently (1953) acquired two of the first mainframe computers, the IBM 701. The 701 was a vacuum tube computer with a cathode ray tube (CRT) memory of 2k 36 bit words. IN May of 1954 IBM 704 was announced as a redesigned 701. It had floating point arithmetic and 4k words of magnetic core storage, later increased to 32k. The first models were scooped up by Los Alamos, which IBM used as a test bed. The 701 did not have an assembler. Programs had to be written and modified in Machine Language. The man responsible for formulating the Multi-group Boltzmann neutron transport code was a mathematician by the name of Bengt Carlson. Through Bengt I learned how to operate and modify multi-group cross sections in his "Sn" code, to suit my goals of having an automatic critical mass computer for reactor research.

[Nuclear science and engineering, v.4, no.2, p.166-179 (August 1958)]

5.2.2 Computer programing Skills,

What I did was to use my knowledge of neutron cross-section measurement techniques to develop a list of cross-sections for uranium and plutonium, as well as coolant and structural materials, that could be used in Bengt's codes. Then I would check the computations against the critical mass data of various configurations experimentally verified by LASL. Then I would adjust the multi-group cross section numbers until I got a best overall fit. I used to run programs on the two 701s from midnight to dawn. This was before the PC, but I in effect treated the two 701,s as if there were PC,s. The Transport codes were iterative things: make a guess for the neutron fluxes and critical mass then iterate the guess until some degree of convergence was achieved. I use to set at the console, and watch the convergent rate. I would stop the main frame and feed in new data by hand and restart the program. Just as you can do on a PC. But nowadays an individual cannot mess with a mainframe.

5.2.3 Help the French (build bombs !)

It became quite easy for me to accept rough designs for reactor systems, stuff the material mixtures and concentrations into the program, and let it run, until an particular enrichment went critical. I had foreign visitors (I remember one guy was Finnish, and flew Me109s during WWII – I was impressed) come and visit, and then request my codes and cross sections for the "design of nuclear systems for peaceful purposes". But to me it was obvious what they were after. Particularly, the French. I objected to handing over this (then) classified material, but orders from higher levels arranged the transfers. It was not too many years later that the French exploded bombs in Algeria

5.2.4 Help the British Dounreay Fast Breeder Design

One of the requests was for an estimate of the critical mass of the British Dounreay Fast Breeder design. Art Coffinberry, Hans Bethe and I had been sent to England for some meeting at Harnwell, and while there the British invited me up to Dounreay, in appreciation for the help in estimating the reactor critical mass. They dressed me in a Naval Officers foul weather coat, (that I wanted to take home with me), put me into a DC-3 and flew me to Dounreay, on the northern tip of Scotland. I toured the plant and then they returned me to the small Hotel, where a group of us (me and about 20 Brits) were to have dinner. What an honor for a kid in his twenties.

5.2.5 Becoming a Pilot at the Sante Fe Airport.

Before coming back to Los Alamos from Dounreay, I decided to take a vacation and see some of Europe. It was winter time when I went to Amstel, Holland from London. I got a message from the USA that I had to return immediately – my 3 week vacation was ended. It turned out that I could not get a flight back for several days, so I left Amstel by train to spend 1 day in the Swiss Alps. I was not, and still am not a skier. While sitting alone in the deserted bar (almost everyone else was on the slopes, skiing) of a the hotel at the base of Jungfraujoke, I met an extraordinary young girl – from the USA. She was in Switzerland with her father to clear up her grand mother's estate. We had dinner, and I mentioned that I had been called back to the USA (cutting short my intended vacation) and had to catch the train to Geneva, and the next day a plane to Paris, and then wait for a couple of days in Paris unit I got a seat on the next-available Constellation (prop-driven) flight back to the USA. We parted company and the next day I went on to Paris.

On the early flight back to Paris from Geneva, my seat mate was a young French soldier, just discharged from Algeria. He wanted to get into the tourist business, and was eager to speak English. I mentioned that I was going to see if I could find where Claude Luter (a jazz clarinetist) was playing that night in Paris. We parted company at the airport, and I went on to my hotel. Early that evening I was sitting at a west bank bar, waiting for Claude Luter to play, but, all in all, feeling a bit lonely. Then in through the bar front door came my discharged French soldier friend, with two young ladies. They had been searching for me and said they were out to have a good time in Paris – with me.

My French friends took me a club. I was told not to speak (English). We walked down a stairway in the old section of town, and my ex-soldier friend knocked on the door. A small speak-easy door opened and after some conversation, the door was opened and we were admitted. I was told I was the first American to enter Whiskey y Go-Go. Inside the dimly lit rooms that evidently were part of an old wine-cellar, there was an old Wurlitzer juke-box playing records, and group of well-dressed, obviously elite, French young adults, conversing, smoking and drinking warm scotch. I do not remember getting back to my hotel.

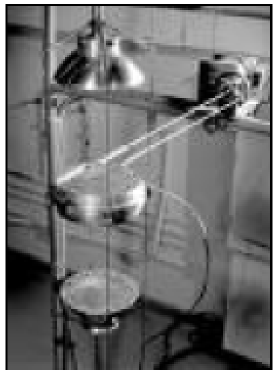
After my big (and first) night in Paris, an early morning telephone call awoke me. Unexpectedly, it as from the girl I met in Jungfraujoke! She had come to Paris to meet me and ride home to the USA on the same flight that I was taking in a couple of days. The young lady lived in San Antonio, TX, but was going to school in New Orleans. By this time I had managed to buy a car, a 1956 Chevrolet convertible. One 3 day weekend I decided to drive to New Orleans to see the young lady. I left on a Friday evening after work, first down to Albuquerque, to meet some friends and have a few drinks. I woke up the next morning in a Clines Corner motel with a hangover, and no memory of how I got there. I start to compute the time it would take to get to New Orleans and then back to Los Alamos. I would be able to spend 10 minutes saying hello and then I would have to start the long drive back. There must be a better way; I abandoned the trip.

The next day I drove down to Sante Fe Airport and told them I wanted to learn to fly. I figured that with an airplane would make the 3 day trip reasonable. I ultimately became a private pilot, bought and flew a used Cessna 170 B, and used Cessna 172, and finally in 1962, a brand new Cessna 172. As you will see I flew this airplane from my pecan-orchard, cattle ranch, on the San Marcos river, back and forth the University of Houston, where I became a Professor of Physics. I never did see the girl from Switzerland again.

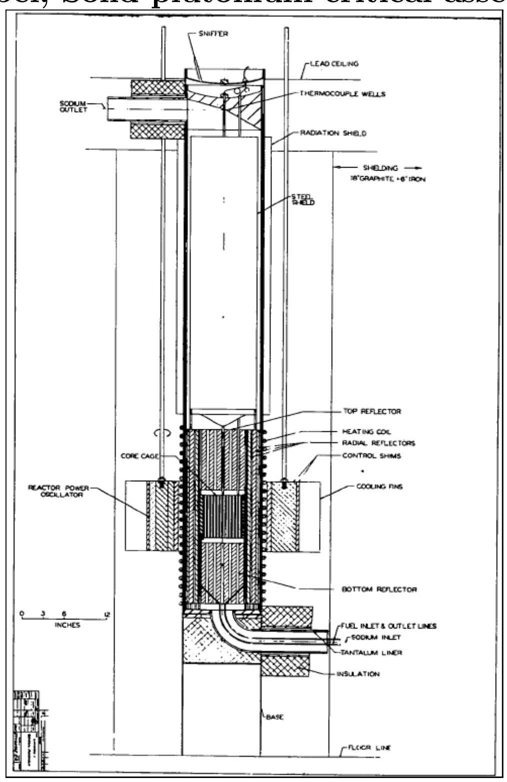
5.3 Lampre LA 2112 1955-1957

However, my agenda was always directed to Plutonium Fast Breeder Reactors.

Designing LAMPRE, The Los Alamos Molten Plutonium Experiment.

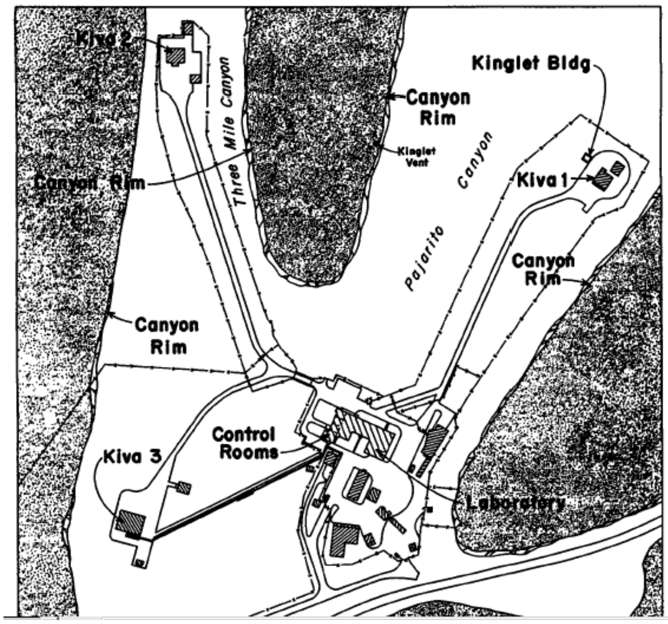


Jezebel, Solid plutonium critical assembly



5.4 Critical Experiments in Pajarito Canyon

After I got OK to proceed with the Lampre concept, the first thing that I had to do was to get some experimental data on critical mass and size of the molten plutonium core. This was done by designing a mock-up critical assembly to be installed in one of the Kivas – a large laboratory shed each in an isolated canyon, and a suitable distance (1/4 -1/3 mile) from the control center.



Pajarito Canyon Kiva critical Assembly areas

The Critical Assembly was a remotely controlled device that assembled two non-critical components initially into a subcritical single component configuration. The top component would be fixed, while the bottom component containing plutonium in a small coffee sized can would be raised and be inserted into the top component. The top component consisted of various blanket-reflector materials that would change the reactivity of the "bare" can of plutonium. Gravity drop out was used as a fail safe mechanism. From a remote control center, the Plutonium can would be raised and inserted into the fixed reflector component. A measurement of the assembled reactivity would be made in terms of the neutron flux detected by BF3 counters. Then the assembly would be separated, a team of 2 or more scientists would leave the control center, get into a jeep, go through a manned guard post, and drive out to the Kiva. In the Kiva a bit of additional fissionable material (thin Nickel coated plutonium discs) would be added to "coffee can". The two Kiva scientists would return to the control center, and the remote assembly procedure repeated. A new level of neutron counting rate would be determined. The reciprocal of this counting rate was plotted on log graph paper versus total plutonium, and the data extrapolated to determine what amount of plutonium was predicted where the counting rate would go to infinity. The assembly would then be "critical" and the critical mass would be determined. The Approach to Criticality was done in many delicate steps, repeating the neutron measurements, disassembly, out to the Kiva, add a small increment of plutonium, return to the control center, make a new assembly, recompute the critical mass from the intercept of the inverse of the counting rate, etc.

5.4.1 The Blue Glow

Slotin's accident

The remote assembly criteria was mandated by an accident in which Louis Slotin was killed. A subcritical plutonium spherical mass was embedded in a Beryllium reflector. The upper hemisphere of Be was separated from the lower reflector by a screw driver wedge. Slotin would slide the screw driver in and then out causing the neutron counters to respond to the higher or lower degrees of subcriticality. For some reason, during a demonstration for Al Graves, who stood behind Slotin (looking over his shoulder) Slotin's hand slipped and the two reflector hemispheres came together causing the assembly to become supercritical. The pulse of neutron radiation caused ionization of the air, generating the name "the Blue Glow". Slotin died in a few days, suffering more than 2,100 rem. Al Graves survived.

Slotin's assembly, Plutonium core hidden by Be reflector shells.

The Plutonium core was a 6.2 kg spherical subcritical mass of plutonium that accidentally went critical on two separate instances at the Los Alamos laboratory, in 1945 and 1946. Each incident resulted in the acute radiation poisoning and subsequent death of a scientist. After these incidents, the core was referred to as the Demon core.

Lampre Experiments

During one sequence of experiments where the "coffee can" of plutonium was inserted into an annular cylinder of H20, the safety officer and I drove out to Kiva to add a bit more material in the standard approach to criticality. As the two of us walked through the large door into the Kiva, I told the safety officer to stop. "Something was wrong" I noticed that there was a small bit of water on the floor at the base of the assembly mechanism, but that is not what "freaked" me out. I had 3 pairs of BF3 neutron counters delivering neutron flux measurements back to the Kiva. One pair of counters was inserted with the BF3 tubes immersed into the top of the water blanket (for greater sensitivity). I had rigged these counters such that the audio neutron counting rate was broadcast over a speaker into the Kiva. Tick, tick ..tick ...tick, tick.

What had caused my alarm was that there was no noise in the Kiva. I yelled in over the intercom to the control room where the 3rd member of the critical assembly team was standing by. This third man was Kent Hansen, a fraternity brother from MIT, who had entered the Nuclear Engineering program at MIT, and who helped me run the Rockefeller Van de Graaff. Kent was a visiting summer student at Los Alamos. He later went on to become Chairman of the Nuclear Engineering Department at MIT.

I yelled: *Kent, What is the counting rate on high sensitivity BF3 counters?*

He should have heard the registers going click ..click at some slow rate.

He yelled back: *There is no counting rate.*

I yelled: *What about the lights on the binary counter electronics?*

Kent responded: *All of the lights are jumbled – all on at the same time.*

Now Kent did not have the experience, but what he described was as if the counting rate had gotten so high that it saturated and jumbled the binary counting apparatus. Had a prompt burst (THE BLUE GLOW) taken place in our Kiva?

I told the safety member of the crew *Let's get the Hell out of here. I do not want to go back in there unless we have some portable radiation and neutron counters.*

We jumped into the Jeep and drove back to the control room. As we approached the door way a number of other Lab scientists came rushing out asking *Did you have a prompt burst?*

I asked why, and they said that the Fission Product Ionization chambers had signaled an alarm.

That was enough to scare the crap out you.

I took my coins out of pocket, rushed over to the health monitor and told him to measure the coins for neutron activation.

I then went into the control room, and there was Kent, calmly sitting with his feet on the desk. I checked the Fission Product monitor in our Kiva, and indeed it was recording a trace from high off scale followed by an exponential decay of about the right time constant to agree with the idea that it was measuring Fission Product decays. I was really nervous by this time.

Then I checked the counting rate meters. Indeed the sensitive BF3 pair looked like the electronics had jammed. However, the other BF3 counters were clicking away at their normal BKG rates, as if nothing had happened. Kent had not reported this information to me when we out at the Kiva. The health monitor came back and said my coins were not radioactive. There had not been a BLUE GLOW EVENT. I sat down and gathered my frazzled wits together.

What had happened was an incredible collection of coincidental events. About the time of the BF3 jamming, the LASL power plant had a Voltage Spike. That triggered the Ion

Chamber Fission Product monitors all over the lab. The water level in the blanket water had risen a small amount, but just adequate to get into the electronic preamplifiers on the end of the BF3 tubes. The preamps shorted out causing the jamming of the electronic counters.

After that, I gained the reputation of being super cautious; if I went in, everything was OK. This became useful when I had to get my scientist crew to board a KC - 135 and to fly off to French Frigate Schoals. If I got on board, the rest would follow.

5.5 J- Division – Atom Bomb experiments with J-10 in Nevada 1957

In 1957 I got permission to work with J-10 doing diagnostic experiments on above ground nuclear explosions in Nevada. After some preliminary work with Herman Hoerlin's group I got on board the LASL Beechcraft Bonanza operated by CARCO to carry staff members to and from the Nevada test site (outside of Las Vegas). I was transported to Camp Mercury and lived in military style barracks during my stay. J-10's mission was to measure the compression of the atom bomb by means of a gamma ray pin hole camera. The bomb was mounted on top of a 500 foot tower, such that the fireball would not touch the ground, and radioactive fallout would be minimized. The "pin hole" in a large thick lead block was below the bomb. At the base of the tower was a large array of plastic blocks that would glow when stimulated with gamma rays. A few hundred feet away was a semi buried concrete bunker and a slit window filled with photomultipliers facing the array of plastic scintillators. Each photomultiplier output was fed into high-speed oscilloscopes. If everything worked properly, the gamma ray image would create a movie (of a couple of microseconds duration) of the compression and/or expansion of the dense materials in the explosion that generated neutron induced gamma rays. Clever – but the timing and triggering had to be perfect. This was achieved by a photomultiplier pointed at the shot tower that would give a light signal that could be used as a fiducial time mark of the start of the explosion. The gamma rays that went through the "pinhole" would travel down the tower at the speed of light, excite the scintillator light signals that would travel to the bunker with the speed c. As it takes longer for light to travel the two legs of a right triangle, it was an easy computation to program in enough electronic delay to start to sweep on the oscilloscopes just before the scintillator signals arrive.

5.5.1 Dry Runs in Nevada

All of this technology was doped out by H. Hoerlin and D. Westerfeld of J-10. I was just another grunt whose job was to see that it all got together without mistakes. This meant practice, practice, practice, such that all team members did their jobs diligently. Such practice sessions were called dry runs. When I helped out J-10 again in 1962, the necessity of dry runs proved again to be a necessary part of field experimentation.

We had twelve fast Oscilloscopes with attached cameras to record the amplitude and time history of the light signals generated by the Gamma rays. I always ordered 13 film attachment holders to be brought to the dry runs, and then during the check list phase, Johnny Gallegos would install the fresh film attachment holders on each of the 12 the camera backs. The film attachment holders were always loaded in the J-10 trailer before coming into the bunker. Johnny could not understand why he had to load 13 camera attachment blanks when we only had 12 cameras. Then the day of shot arrived. We went through the check list in the bunker. I asked Johnny for one of the camera attachment holders. He was a bit surprised, but when he saw that I opened the camera attachment holder just to make sure that there actually was film inside, his grin convinced me that he understood the method in my madness. The shot went off perfectly.

5.5.2 Me and the Bomb atop a 500 foot Shot Tower

Many of the shots in Nevada were conducted late at night, without a moon if possible. I remember one dark night being on top of the shot tower with my faced pressed up to the casing of the bomb, while fiddling with some alignment problem. Boy it was dark, but the coyotes were howling, and I could feel the heat on my face generated from the natural radioactivity of the materials that made up the bomb interior. Scary.

5.5.3 5 4 3 2 1 ...... Nothing !!!! (Bomb needs defusing !!).

It was usual on shot night for most of the scientists to gather outside the command bunker, several miles from the shot towers. Everyone would stand around with the ND9 goggles, waiting for the broadcast from the Command Post.

T is minus 30 seconds, put on you goggles or turn away. Then ... *5 4 3 2 1 ....*

The Bomb would go off. You could feel the heat radiation on your face; the sage brush ignited far away from ground zero. But there was no noise. A very aery feeling. Then you saw dust and sand disturbed from the desert floor, coming at you (at the speed of sound). It would take several seconds to arrive and there was not much you could do but brace yourself for the shock wave, and the big bang.

One night, we were in the usual positions outside the CP, when the command center broadcast the usual

... T is minus 30 seconds, put on you goggles or turn away. Then ... *5 4 3 2 1 ....*

Nothing happened !!!!!

The bomb did not detonate.

The two bomb armorers were called in by the command post, and sent out to deactivate the bomb. One of my Los Alamos neighbors was one of the armorers, Chuck Bottom. Can you image driving out in the dark night several miles to the shot tower, contemplating that you would have to ride up the elevator to the top of the 500 foot TV tower, and examine how to deactivate an atom bomb, which for all purposes was energized ready to explode? Had all the electronics been activated? Could it be that a slight jarring would cause some stuck relay to close. What had caused the malfunction?

Those two guys had guts.

It turned out that a safety relay on the elevator designed to prevent using the elevator after the bomb had been armed was the cause of the non-detonation.

5.6 Lampre Construction

More to follow:

5.7 J Division – Pacific Bomb Tests 1962

5.7.1 1962 Outfitting a KC-135 as a flying nuclear explosion diagnostic laboratory.

The Russians had shutdown atmospheric nuclear explosions on a (more or less) unilateral basis since 1958. The US followed suit. Then in 1961 the Russians started atmospheric testing again, and detonated the 50-57 Megaton hydrogen bomb on 0ct 30 1961.

President Kennedy called Los Alamos and in effect said go out and test as fast as you can; test any designs that you want to consider. The Russians will continue their tests, and when done, they will call for a world-wide moratorium on atmospheric testing. Los Alamos was given 32 days to get geared up and be on station.

I volunteered to go help J-10, as the entire lab went on a 24/7 mode to get the job done. This was typical of the morale and spirit of the staff at Los Alamos during the period. IMO, that morale declined about 1980+ and the lab lost its luster.

Many detonations were scheduled near Equatorial Christmas Island. Another set of tests was to detonate bombs in outer space. Bombs would be sent aloft from Johnstone island on rockets to altitudes of 400 kilometers, and J-10's assignment was to make diagnostic measurements of the explosions.

Three observation locations, one on Johnstone island, the second of top of Halieakala on Maui, and the third in an airborne laboratory flying at 44,000 feet, above the tropopause. I took on the assignment to convert a KC-135 jet tanker (a 707) into a flying diagnostic laboratory, assemble a crew of scientists, and be on station in Honolulu in 32 days.

J-10 scientists gathered together the information sensors that would be needed. We would have several spectrometers, numerous high-speed oscilloscopes with scintillor radiation detectors, a streak camera, and many other cameras including an all sky camera, and a camera with a telescopic lens that had a 1 degree field of view. All of this was to be controlled from a small console, that would energize the necessary parts, sweeps, and triggers in proper sequence, such that everything would be synchronized to the detonation signal.

5.7.2 "This Aircraft is a DOG"

I was told a New KC -135 would arrive at the Convair factory in Fort Worth-Dallas area in 10 days. This US Air Force facility was similar to the Skunk Works at Lockheed. It was a place where the US Air Force sent aircraft to be outfitted or modified for special projects. There were many problems that had to be resolved. Windows in the KC-135 had to be built and fitted with infrared glass. Most of the electronics (oscilloscopes) were designed to operate on 60 cycle, while the power plant on board the jet was 400 cycle. Suppliers of these devices could not retrofit or produce new units within 30 days. The answer: I scrounged the entire supply of 400 to 60 cycle converters that could be found in the US (mostly at SAC bases).

Placement of components, mounts for the instruments, facilities for my scientific crew had to be constructed. All most all of this was accomplished with hand sketches – there was not enough time for blueprints.

I went to Convair to meet the Airforce delivery of what was to become the LASL flying diagnostic laboratory.

My KC135

The plane arrived, and when the entry door opened, there was a note pasted inside by the previous crew chief, to the new crew chief. The message read: *This aircraft is a DOG.*

I went for a test ride with what was to be my SAC crew. As we came in for the landing, the aileron controls Locked up!!! in the approach!! The SAC pilot skidded the plane to a landing. The project was almost over before it got started. I was not only scared out of my wits, but also furious. The people at Convair had found two bushel basket fulls of Tech Orders that had not been completed (and did not tell me) – including a crack in the main wing spar. As I sat in the cockpit (on the ground) with my Convair coordinator explaining my concern with this aircraft, I happened to wiggle the speed brake lever. It felt loose, so I pulled on it and it came out of its socket!! The Convair engineer said *Not to worry.* HA-HA, who me worry.

The next test flight included a water boosted takeoff to simulate a heavyweight takeoff. It was almost another disaster. The water boost dumps water into the jet engines at take off, to give higher density and more thrust. There is a pump for each pair of engines on each side of the aircraft. On the takeoff everything seemed to work OK until the nose was raised for lift off. Suddenly the pump on one side blew its circuit breaker, giving asymmetric thrust at perhaps the most critical time. Again the SAC captain saved the day (a miracle), and I went ballistic with the Convair engineers. They agreed that all tech orders would be fixed before the next flight. I must admit they did a heroic job to get these things done, as well as to reconfigure the aircraft as a flying laboratory.

The Water booster pump failure must have been a recurrent problem for this old KC-135 (that had been substituted for the promised new one). It was ultimately discovered that the three phase wires had been inserted in to the pump housings, but one of the wires had not been skinned back so it did not make electrical contact. The pump would run single phase for a number of seconds and the overheat blowing the breaker. This was a problem that occurred in the assembly at Boeing in 56, and was never figured out until 62. No wonder the previous crew chief had said: *This aircraft is a dog.*

5.7.3 Training the Crew

My experience with J-10 in the Nevada Test series in 1957 made me force my crew to practice many dry runs. On one of the first excursions from Hickam Air Force base we went in daylight out to French Frigate shoals which was our designated loiter point. In the bright sunlight you could see the atoll from its bright coral reefs that extended over several miles. Only on one small piece of land was an old WWII runway that had been taken over by the coast guard. So far so good. Then, on our first night flight we went out and our radar could not pinpoint out position on the atoll; the coral reefs could not be seen! I had to send one of my crew out with the Navy to plant a radar corner reflector so that we could tell precisely where we were – in the dark. We got the new military satellite systems to get us precise (now called GPS) positions of our radar reflector on French Frigate shoals and Johnstone Island, so that we could find the astronomical data required for the proper orientation and elevation of our instruments. Remember, bomb was to be detonated at an altitude of 400 kilometers.

Map from Hickam to French Frigate Shoals

On our first flight where all elements of the research project were activated, we took off from Hickam Air Force base near Honolulu in the late afternoon, with jet fuel bringing us up to a takeoff weight of 200,000 pounds – the standard loaded weight for the KC 135. We flew out to French Frigate Schoals (about 550 miles) from Honolulu, and assumed our orbit position. We orbited and orbited and orbited while the scientists at Johnston Island and other places tried to get their acts together. The SAC captain had to fly the plane manually in order to preserve the precise orientation, and I know he was tired when we stalled out the big jet on one orbit. Just another thrill aboard the KC 135. The Pacific command center finally called off the exercise, and we spent the next hour or more returning to Hickam – everybody dead tired. The military crew went directly to bed in the officers quarters, but my scientific crew had apartments rented in downtown Honolulu. So we had to drive into town, and crash the rest of the day. Then it started. The Pacific Command Center said, get your crew together, we are doing it again. Only this time, they wanted us to go out heavyweight, so they could make us loiter for a few more hours, if they needed it.

Now military heavyweight meant 340,000 pounds, full water injection at takeoff, and hope that there were no engine glitches. Prevailing wind conditions meant takeoff was in the direction of the Pali mountains and downtown Honolulu. I remember that afternoon we took off super heavy weight and went through downtown Honolulu with the jet engines screaming with the water boost. I mean we went through downtown Honolulu at an altitude below the tops of the major high rises. No banks or turns allowed for maximum lift. In fact we had to go through the pass between Diamond Head and the Pali range. I got on the phone to Pacific Command and told them there would be no more military heavy weight takeoffs.

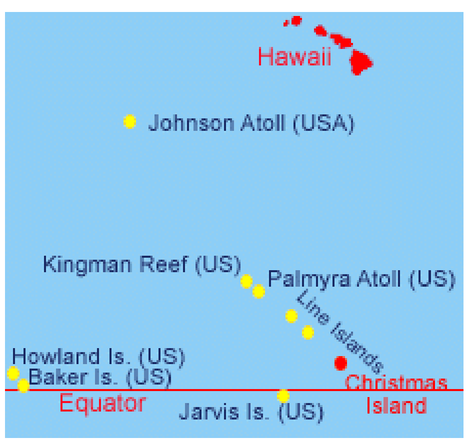
We usually went out with a normal (not super) heavy weight so that when got to our loiter point at French Frigate shoals, we could orbit for many hours waiting for the shot from Johnston Island to proceed. Time and time again we went out to arrive on station in the dark hoping the shot would go as scheduled. Time and time again, the boys on Johnston Island would cancel the operation, after we had been out over the ocean for 6 hours or more. We then had to fly back to Honolulu arriving in early morning, get debriefed, and then find our beds.

Remember the KC 135 is a tanker with 19 hours of range if it consumed all of its fuel stores. Our mission was to fly out to French Frigate Schoals, orbit in position until the hydrogen bomb carrying rocket was fired from Johnston Island, some 700 mile away. We were positioned such that we could observe the predicted impact point of the ionized debris as it spiraled around the earth's field lines until it impinged on the "atmosphere" causing an auroral like glow. We used a 1 degree field of view telescopic lens on the camera that was assigned this measurement. The US AIR FORCE research pilots at ABQ said that keeping the telescopic camera on target was impossible. However, as I now was a pilot with about a 1000 hours of experience, I knew that I could hold my attitude within a degree on a desired position. Just put a piece of chewing gum on the windshield, and line it up with a point of horizon. This translated to a piece of chewing gum on the windshield of the KC135, lined up with a selected star, all computed such that the one degree camera would be on target at shot time. Before leaving ABQ-Los Alamos for Honolulu, I arranged for a flight of the KC135 one night with my SAC crew and showed them how to hold the heading using chewing gum and a star. In addition I took all sky camera photos of the stars with a long time exposure to show just what sort of pointing wobble was could be expected.

Bottom line: The method worked. On the Starfish shot, the debris spiraled along the earth's magnetic field lines and collided with the denser atmosphere. The image was recorded in the upper right hand quadrant of the photo taken by the 1 degree field of view telescopic camera. Yeah.

5.7.4 Several months in Honolulu.

After many night after night flights that turned into dry runs, the Pacific Command would gave us a few days off. They would cancel the flights out to French Frigate Schoals, while they tried to fix whatever were the problems with the Rocket on Johnston Island. I would authorize a flight down to Christmas Island where we could test our diagnostic equipment, and my virgin crew could see a Nuclear Explosion. Most of them had never seen a nuclear explosion, for they did not have any Nevada testing experience. The trip was good as a training exercised, and even better for morale.



Map Hickman to Christmas Island

The Christmas Island flight took about 3 hours as Christmas Island was about 1400 miles to the south of Honolulu. On one night flight south we climbed to our orbiting position at 44,000 feet, waiting for the B52 to get into position and drop the bomb. We were about to start our countdown sequence in the darkened interior of the KC 135, when Bang, the thee foot diameter plastic astrodome on top of the fuselage blew off. We had an explosive decompression at 44,000 feet.

Air rushed out of the interior through the massive hole and raised such a dust cloud that turned to thick fog that it was hard to see more than a foot or two in the cabin interior. When you go above 40000 feet it is a rule that either the copilot or pilot are on helmet (and have their oxygen masks operating). The procedure is to throttle back, through out the landing gear, deploy the speed brakes and descend to 10,000 feet, where the air is dense enough to support life without additional oxygen. The only trouble is that if you are not strapped into your seat, you are in free fall and will float around the cabin.

Only two members of my crew and sit down stations and could buckle up it they wanted. But it was normal for none of them to be on helmet. We all were connected via intercom, and I warned every body to get on helmet. We all had been trained in the high altitude chamber at Roswell Air Force base, and new the decompression drill. My problem (besides trying to control the effects of free fall) was that I knew that we had two VIP hitchhikers on board, sitting in back on the seats scrounged from Eisenhower's airplane, but without helmet or oxygen mask outlets.

I called back on the intercom to one of my crew in the back, and told him to break out one of the small green oxygen bottles, and squirt oxygen into the mouths of the two hitchhikers during our jet penetration descent to 10,000 feet.

When we got to 10000 feet and level off (the noise through the hole was deafening) it was determined that one of my crew had followed procedures and turn on his oxygen outlet to 100% forced feed (as he was trained to do) but did not manage to connect the helmet hose into the correct socket. This caused the drain of the entire liquid oxygen supply. Bummer.

We could not go back up to high altitude and fly back to Honolulu using about 10,000 lbs of fuel per hour, we had to stay at low altitude and fly back using about 25,000 pounds of fuel per hour. We made it back to Hickam, made a straight in emergency landing, and had a tug come out to pull us off the end of the runway.

Some of my crew deplaned and kissed the runway. All of us were a bit flustered, to say the least. However, the KC-135 could not fly, until the astrodome was repaired, and the whole crew was released for a week of R and R.

5.7.5 Starfish: Megaton explosion at 400 kilometers.

Finally, the shot got off. more to follow

5.8 Ideas from Starfish

5.8.1 *More efficient Bunker Busters*

more to follow

5.8.2 Is it possible to *Maximize EMP?*

more to follow

5.9 1962 Sabbatical Leave, Los Alamos to Un. Houston

Worn out from the Pacific test series which lasted several months past schedule, Los Alamos gave me a sabbatical leave 1962-1963. A Los Alamos colleague John Allred, had left the LAB to become a Vice President of the University of Houston; he wanted me to come and start a graduate school in physics. I told him I would spend 1 academic year after the Test Series was over. Then almost simultaneously, Professor Clark Goodman had joined Schlumberger. Well Surveying corporation in Houston, and wanted me to come down and work with him. I later learned that he wanted me to consider a job of director of Schlumberger's prestigious research center in Ridgefield, Connecticut. I had no intention of staying in Houston for the next 37 years.

By this time I had been married for 5 years and had a new born son (which went to Hawaii with us). As a pilot, my wife and I flew down to visit her family in Luling, Texas. We would spend several weeks around Christmas time, and in the small town of Luling (population about 2100) I got bored. So I bought a 80 tract on the San Marcos River, and started to plant a Pecan Orchard. *More LAter*

6 University of Houston 1962-1999

6.1 Rad Detector Patent.

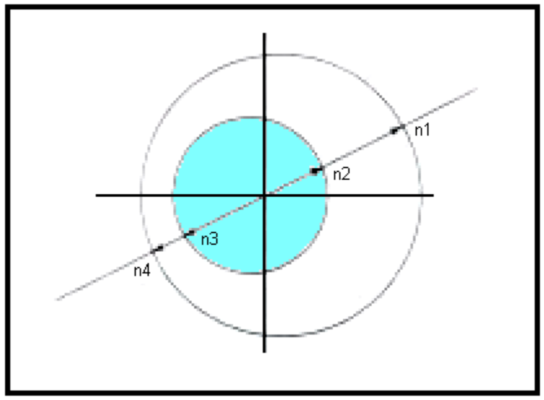
more to follow

6.2 Ring Laser Patent

In the period 1964 -74, when I was seriously considering the interaction of gravity and electromagnetism in terms of an effect on the polarization of an electromagnetic wave, it became apparent to me (after reading Fock [?]) that the four-fold degeneracy of the EM wave constrained by Lorentz constitutive equations could be broken in a topological arena that did not impose the Lorentz symmetries on the EM system. I went so far as to form a patent on such an idea and encouraged V. Sanders to construct a dual polarized ring laser [?] as his Ph.D. thesis. There was no explicit theory at the time, just my hunch, but the device worked! Then I suggested to Alton Schultz that he search for a theoretical reason based upon my lectures about topological electromagnetism and Post's extraordinary book [?]. His thesis considered several developments, but the most important was the solution to the Lifting of the Four-fold EM degeneracy. At that time an assumption was made in terms of a Coulomb gauge condition. Later I managed to solve the problem from a purely topological point of view , without the constraints of the gauge condition imposed in the above article.

6.2.1 Parity and time-reversal symmetry breaking.

This improved article [?] focuses attention on the mathematical problem of obtaining singular solutions to Maxwell's equations of electromagnetism for systems that do not necessarily have the symmetries of the Lorentz equivalence class similar to the vacuum. The methods employed are those suggested by Cartan [?] in his studies of exterior differential systems. Maxwell's equations are considered to be a system of global differential forms specifying certain topological properties about a differentiable variety. An intrinsic, physical distinction is made between the qualities of intensity (**E** and **B**) and the quantities of excitation (**D** and **H**). No assumption is made about the existence of a global metric on the variety. The singular solutions obtained are viewed from the perspective of Luneburg and Fock [?] [?] as being those point set domains upon which discontinuities in field amplitudes may exist. Propagating discontinuities are interpreted as signals. A dynamical perspective and/or the presence of matter is presumed through the existence of a constitutive tensor density $\chi^{\mu\nu\alpha\beta}$ [?] which acts as a geometrical constraint on the topology induced by the Maxwell system of forms. The symmetries and antisymmetries of the constitutive map lead to an explicit extension of the concepts of the Fresnel wave normal surface and the Fresnel ray surface, historically developed for anisotropic birefringent media [?]. Explicit formulas for these surfaces are created that permit the analysis of combinations of effects, such as optical activity, and Faraday phenomena, for which time reversal and parity symmetries are not maintained [?] [?]. The results are applicable to problems in which the four-fold degeneracy of the Maxwell-Lorentz system is broken, an example of which is given by systems employing four-mode ring-laser devices [?]. In such systems, the inbound wave speeds are different from the outbound wave speeds.



A mix of Optical Activity and Faraday rotation splits the 4 fold degeneracy of the Singular Solutions into 4 distinct wave speeds depending on polarization and direction.

6.3 Continuous Topological Evolution

A non-statistical theory of continuous, but irreversible, evolution can be constructed in terms of the Cartan calculus. The fundamental postulate, for an evolutionary theory which admits irreversible processes, is that the topology of the initial state will be different from the topology of the final state. Several fundamental theorems of continuous evolution are established, yielding a set of global conservation laws for reversible or irreversible processes. As examples, a comparison of the evolution of Topological Torsion and Topological Action is made for hydrodynamic and electromagnetic systems. The relationship between the evolution of Topological Torsion and a thermodynamically irreversible process is established.

arXiv:math-ph/0101032v1

6.4 Topological Non-equilibrium Thermodynamics

A major objective of this article is to establish a topological, non-statistical, link between mechanics and non-equilibrium thermodynamics [**?**], with the particular goal of describing the differences between reversible and irreversible evolutionary processes. The methods are based upon Cartan's techniques [**?**], which have been found capable of describing continuous topological evolution of exterior differential systems. In this review of thermodynamic concepts, for reasons of notational simplicity, the number of independent base functions is selected to be 4, denoted with the ordered pre-geometric array of variables $\{x, y, z, t\}$. The fundamental axioms of topological thermodynamics are:

Axiom 0. *The evolutionary variables of interest are deformation invariants, such as connectivity and continuity, related to properties of topological closure. They are expressed in terms of exterior differential forms, using the convenient fact that the exterior differential of an exterior differential form generates its limit points. The union of the set and its limit points defines topological closure. These topological properties are not geometrical properties, as they are independent from metric, connection, and distance scales. Note that differential forms are not necessarily tensors for they are well behaved with respect to differentiable maps without inverse.*

Axiom 1. *Thermodynamic physical systems can be encoded in terms of an exterior differential 1-form of Action Potentials,* $A = A_k(x, y, z, t...)dx^k$, *on a* $\geq$ *four-dimensional abstract variety of ordered independent variables,* $\{x, y, z, t...\}$. *The variety supports a differential volume element* $\Omega_4 = dx\hat{}dy\hat{}dz\hat{}dt...$ The 1-form of Action has physical units of angular momentum per unit source (mass or charge), with coefficients that behave as covariant tensors with respect to diffeomorphisms.

Axiom 2. *Thermodynamic processes are interpreted in terms of N-1 form current densities,* $J = (i(\rho\mathbf{V}_4)\Omega_4$, *with coefficients generated by real vector,* $\mathbf{V}_4(x, y, z, t...)$ (*or complex Spinor*) *direction fields, multiplied by a density factor,* $\rho(x, y, z, t...)$. *The vector direction fields behave as* contravariant tensors with respect to diffeomorphisms.

Axiom 3. *Continuous topological evolution of the thermodynamic system can be encoded in terms of Cartan's magic formula (see p. 122 in [*?*]). The Lie differential,* $L_{(\rho\mathbf{V}_4)}$, relative to a process, $\rho\mathbf{V}_4$, *when applied to an exterior differential 1-form of Action,* $A = A_k dx^k$, is equivalent, abstractly, to the first law of thermodynamics.

$$\text{Cartan's Magic Formula } L_{(\rho\mathbf{V}_4)}A = i(\rho\mathbf{V}_4)dA + d(i(\rho\mathbf{V}_4)A) = Q, \tag{1}$$

$$\text{First Law} : W + dU = Q, \tag{2}$$

$$\text{Inexact Heat 1-form } Q = W + dU = L_{(\rho\mathbf{V}_4)}A, \tag{3}$$

$$\text{Inexact Work 1-form } W = i(\rho\mathbf{V}_4)dA, \tag{4}$$

$$\text{Internal Energy } U = i(\rho\mathbf{V}_4)A. \tag{5}$$

Axiom 4. *Equivalence classes of systems and continuous processes can be defined in terms of the Pfaff Topological Dimension of the 1-forms of Action, A,* Work, W, and Heat, Q. If $Q\hat{}dQ = 0$, the process $\rho\mathbf{V}_4$is reversible.

Cartan's "magic formula" [**?**] representing the "evolution" of the 1-form of Action, A, with respect to the "flow" generated by the vector field, $\mathbf{V}$, is the cornerstone of the development. The Cartan formula does not depend upon the constraints of connection or metric imposed upon the base space of independent variables, and has been called the homotopy formula by Arnold [**?**]. The formula can be used to describe both those evolutionary processes which are homeomorphic and preserve topology, and those processes that represent continuous topological evolution. Hamiltonian processes are representatives of the homeomorphic category, and they are always thermodynamically reversible. Irreversible processes involve changing topology.

6.4.1 The First Law of Thermodynamics as a Statement of Cohomology.

From these definitions of Q, W, U, it is apparent that Cartan's magic formula not only represents the First Law of Thermodynamics in terms of the continuous topological evolution [arXiv:math-ph/0101032], but also demonstrates that the First Law is a topological statement in Cohomology theory, where the difference of two inexact 1-forms, $Q-W$, is an exact differential, dU.

$$Q - W = dU. \tag{6}$$

In this formula, Q is the heat added *to* the system, and W is the work done *by* the system, and dU is defined as the change of internal energy.

Remark 1 *For isolated systems, or closed systems with boundary, this law is interpreted as the conservation of energy. The interpretation is more difficult, if not impossible, for Open systems. (It is typical for chemistry texts to write the first law as Q+W=dU where W is defined as the work done on the system instead of work done by the system. This notation obscures the cohomological aspects of the first law .*

In this article this formal correspondence is taken seriously. The fundamental theme is to study processes that describe continuous topological evolution . Such evolutionary processes are not necessarily invertible and do not admit unique deterministic prediction of tensor fields from initial data. However, they do permit the deterministic retrodiction of tensor fields by means of functional substitution and pullback [**?**]. The magic in Cartan's formula is that it can be used to describe both evolutionary processes where the topology of the initial state is not the same as the topology of the final state, as well as for homeomorphic processes for which the topology does not change.

Both the heat and the work 1-forms as defined above are not necessarily exact, and therefore can lead to non-zero cyclic integrals. The symbol $L_{(\rho\mathbf{V}_4)}$ stands for the "Lie differential" with respect to $\rho\mathbf{V}_4$, a term evidently coined by Slebodzinsky [?]. The symbol dA stands for the "exterior derivative" of A. It can be shown that for the Cartan topologies, the exterior differential is a limit point operator . The symbol $i(\rho\mathbf{V}_4)A$ is used to designate the "interior product" of the process current density direction field, $\rho\mathbf{V}_4$, with the Action 1-form, A in the tensorial sense producing a diffeomorphic invariant. However, no constraints of metric or connection are applied a priori to the domain of definition. For more detail see [?].

For thermodynamic systems it is presumed that the fundamental base of independent functions (a variety) will be designated by the ordered quadruplet $\{x, y, z, t\}$. Most useful applications will be constructed from both differential forms and differential form densities defined over this pre-geometric base. Maps from this 4D base can be made to ordered arrays of functions of geometric dimension, 2n+1 or 2n+2. Note that this variety may consist of both "coordinates" and "parameters", and the notation is suitable for application of Fiber bundle theory. Exterior differential forms defined on this higher dimensional pre-geometric variety are functionally well defined on the 4D base space by functional substitution and the pullback operation. The choice of exterior differential forms (not vector and tensor fields) emphasizes the topological concept of topological closure, whose homeomorphic sets are deformation invariants.

7 Retirement in France 2000 -

7.1 Monographs

The purpose of my series of monographs is to present features of non-equilibrium and irreversible physical systems that can be understood in terms of applied topology. The monographs are not intended to be text books in Topology nor textbooks in Tensor Analysis and/or Differential Geometry, although the methods developed in these disciplines will be employed and modified in that which follows. The objective is to display and apply techniques of *continuous topological evolution* in order to gain a better understanding of non-equilibrium physical systems and irreversible processes . Classic equilibrium thermodynamics utilizes statistical methods, strongly influenced by properties of deterministic geometric evolution. These classic methods have limited (though useful) success when applied to non-equilibrium systems and irreversible processes. In this monograph it is demonstrated that fundamental thermodynamic principles can be extended to describe non-equilibrium systems and irreversible processes, when such concepts are described in terms of topological, not geometrical, evolution.

Evolutionary processes of the type that can be described by C2 differentiable maps from initial to final state will be at the foundations of the methods to be developed. When the inverse processes do not exist, or are not continuous, such irreversible processes permit description of topological change, but they do not admit representations in terms of linear groups of motions. A projection from a space of (N+M)-dimensions to a space of M-dimensions is an example of a non-invertible, but continuous map. These processes of continuous topological change are not diffeomorphisms. Diffeomorphisms, which form a differentiable subset of homeomorphisms, do not describe topological evolution. A basic axiom is that thermodynamic irreversibility requires topological change.

The historical use of a geometric diffeomorphic approach (tensor analysis), with emphasis on uniqueness, symmetries and conservation laws, to solve problems in physics has heretofore constrained, if not eliminated, the stated objective of understanding non-equilibrium systems and irreversible processes. However, geometric methods, borrowing the words of Eugene Wigner, have been "unreasonably effective" in understanding physical phenomena - at least for phenomena that can be approximated by isolated-equilibrium systems and statistical averages. The geometric methods developed historically (and based upon geometry) are time reversal invariant. However, thermodynamic irreversible continuous processes require that the topology of the initial state and the topology of the final state are not the same. Paraphrasing Eddington:

Remark 2 *Aging and the arrow of time have slipped through the net of geometric analysis.*

Most of the references to my earlier publications have been compiled for convenience in Volume 7 "Selected Publications", which is available in paperback form, or in PDF file download format. See http://www.lulu.com/kiehn.

7.1.1 Points of Departure

Herein it is demonstrated that these concepts of thermodynamic irreversibility can be captured in terms of continuous topological evolution.

Remark 3 *Geometric based, diffeomorphic, processes (describing invariant topology and invariant geometry) and group based symmetry methods (with their inherent inverse operations leading to unique invertible solutions) are not explicitly useful in describing the non-equilibrium systems and irreversible processes of interest herein.*

As the development progresses, it may come as a surprise to many readers to find that the theoretical basis of thermodynamics and electromagnetism indicate that these disciplines are topological, not geometrical, physical theories.

In this monograph, a perspective of topological evolution and change is subsumed from the outset. Topological properties and features of physical systems and processes are emphasized, and their evolutionary change becomes the point of departure from classical physical theories. Physical properties of size and shape (though useful and interesting, indeed) are intentionally suppressed, in favor of topologically coherent deformable features. Such topologically coherent, but deformable structures, appear to self organize themselves during thermodynamically irreversible processes of topological change. It was recognized by Tisza (1961) [?] that metrical based properties can not be used to distinguish between the two classes of intensive and extensive thermodynamic variables. Thermodynamic features appear immediately in terms of the topological properties of isolated-equilibrium, closed, and open physical systems.

Caratheodory pointed out that a thermodynamic physical system in isolated-equilibrium admitted description in terms of a Pfaffian form constructed from at most two independent functions (but with arguments over perhaps N geometric variables and parameters). Such Pfaff systems are said to be of (Pfaff) topological dimension 2[1], and are uniquely integrable in the sense of Frobenius. Such uniquely integrable systems consist of a single topologically connected and topologically coherent component. In other words they are systems of a single phase. Once the integrating factor for an isolated system is specified, the Pfaff topological dimension is reduced from 2 to 1, which defines the state of equilibrium. It is remarkable that the Cartan topology constructed from an integrable Pfaffian 1-form is a connected (but not necessarily simply connected) isolated topology.

On the other hand, irreducible, non-equilibrium thermodynamic systems are of Pfaff topological dimension 3 or more. The Frobenius theorem of unique integrability fails. Even more remarkably, the Cartan topology for such systems, of Pfaff topological dimension greater than 2, is a disconnected topology and may have many components (mixed phases). Another way of describing such a topologically disconnected system is that if solutions exist, there may be more than one solution (non-uniqueness) at any geometric point, leading to the notion of envelopes, Huygen wavelets, and edges of regression representing stability limits and the possibility of thermodynamic phase change. Pfaff topological dimension 3 (or more) systems are non-equilibrium systems of multiple topological components. Pfaff dimension 3 systems can be chaotic, but the chaotic processes can be reversible in a thermodynamic sense. However, Pfaff dimension 3 (in general, 2n+1) systems, always admit a unique extremal vector direction field which can be interpreted as long-lived kinematic evolution - neglecting topological fluctuations. Such extremal fields do not exist in domains that are of Pfaff topological dimension 4 (or 2n+2). Such four-dimensional (2n+2) topological spaces are the domain of

[1] A simple method to determine the Pfaff topological dimension of any 1-form is given in Section 1.6.

thermodynamic irreversible processes. Self organized topologically coherent structures are the domains of Pfaff topological dimension 2n+1.

The topological perspective of thermodynamics used in this monograph is based upon Cartan's theory of exterior differential forms, which can be utilized to describe continuous topological evolution. A fundamental example of continuous topological evolution is described by the evolutionary change of Pfaff topological dimension. The topological perspective is founded on the idea that thermodynamic physical systems can be encoded in terms of a 1-form of covariant Action Potentials, $A_k(x, y, z, t)$, on a four-dimensional abstract variety of ordered independent variables, $\{x, y, z, t\}$. The variety supports a volume element $\Omega_4 = dx\hat{}dy\hat{}dz\hat{}dt$. It is also assumed that thermodynamic processes can be encoded, to within a factor, $\rho(x, y, z, t)$, in terms of contravariant vector direction fields, $\mathbf{V}_4(x, y, z, t)$. In printings of this monograph starting in 2005, it was appreciated that direction fields representing thermodynamic processes should include classical Spinors, as well as Vectors. Spinors, which behave as vectors with respect to affine translations, do not behave as vectors with respect to rotations (see Chapters 1 and 4).

Variational principles are not used to define "equations" of motion. Instead, continuous topological evolution of the thermodynamic system and its system of differential forms is encoded in terms of Cartan's magic formula (see p. 122 in [**?**]),

$$L_{(\rho\mathbf{V}_4)}A = i(\rho\mathbf{V}_4)dA + d(i(\rho\mathbf{V}_4)A). \tag{7}$$

The motivation for this departure from classical theories is that the Lie differential, when applied to a exterior differential 1-form of Action (per unit source), $A = A_k dx^k$, is equivalent *abstractly* to the first law of thermodynamics. Hence, the first law of thermodynamics is a topological, not a geometrical idea. Remarkably, physical systems and processes can be put into equivalence classes defined by the concept of Pfaff topological dimension. These concepts will be presented in detail in the chapters that follow.

Discontinuous processes and statistical methods are, more or less, ignored. However, it is important to remember (and for some - a surprising fact) that continuous evolution in a topological sense can cause discrete changes in the topological properties of a given system. Indeed, an important topological property is the number of disconnected parts, which in this treatment of thermodynamics will be related to the mole number, n.

7.1.2 Results

The original motivation for this monograph was based upon the goal of developing analytical methods which can decide if a given physical system was an equilibrium system or a non-equilibrium system. If a specific analytic process was applied to the physical system the methods should be able to decide if that process was thermodynamically reversible or irreversible. It is remarkable that by using a topological perspective and the axioms for continuous processes, given in detail below, these goals have been achieved without the use of probability or statistical methods, and without the use of metric constraints and linear connections. The topological method, constructed on a Cartan system of exterior differential forms which are inherently antisymmetric, emphasizes the antisymmetric properties of a physical system, where the more geometric and statistical methods, based upon quadratic metric forms and symmetric averages, tend to obscure the antisymmetry properties. Perhaps one of the most significant properties of antisymmetric matrices (associated with exterior differential 2-forms) is the fact that their eigendirection fields (spinors and vectors) are related to real eigenvalues which are zero, or to pure imaginary numbers. The eigenvectors which are associated with the complex eigenvalues have complex components whose squares add up to zero. That is, these complex eigenvectors are what have been called null isotropic vectors, and are the generators of Spinors.

Remark 4 *The Cartan method of exterior differential forms incorporates Spinors in a natural way, without allusions to microphysics or relativity theory. Spinors play a role in classical physics and dominate the theory of minimal surfaces.*

It is further remarkable that the Jacobian matrix of the coefficients of the 1-form of Action (per unit source) - for those non-equilibrium turbulent physical systems of Pfaff topological dimension 4 - leads to a universal thermodynamic phase function represented by a polynomial equation of 4th degree. The universality is related to the singularity theory of non-degenerate systems which are equivalent under (small) deformations. The Phase function is constructed in terms of the symmetric similarity invariants of the Jacobian matrix of the component functions that encode the 1-form of Action (per unit source), A. The resultant Phase function brings attention to thermodynamic phases that have equivalent (symmetry) structures other than those depending upon size and shape. In general, the exterior differential form method focuses attention on thermodynamic phases that have equivalent deformable topological structures (equivalent Pfaff topological dimension), and which are the result of continuous topological evolution.

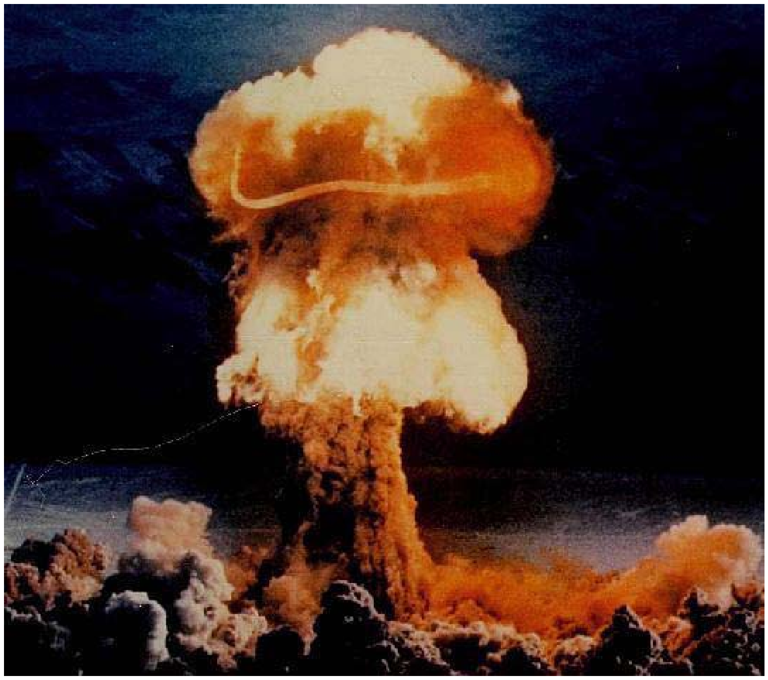
This resultant universal fourth order Phase function result matches the concepts of Landau Ψ^4 mean field theory and phase transitions on one hand, and on the other hand makes contact with the non-equilibrium expansion of the universe described by "inflation" and dark matter and dark energy concepts due to a "Higgs" quartic potential below the critical point of a deformable van der Waals gas. The concepts of surface tension (or string theory) can be related to the mean curvature (induced by the molar density) of the universal phase surface, the concepts of temperature and entropy are related to the quadratic or Gauss curvature (induced by the molar density), while the concepts of pressure (of either sign) and interactions are related to the cubic curvatures (induced by the molar density). The theory as presented herein is far from being complete, yet the methods offer a new perspective for analyzing thermodynamic problems. Moreover, the techniques appear to solve the problem of making a marriage between mechanical dynamics and thermodynamics; the methods can be quite useful in the design of new applications previous excluded by assumptions of equilibrium and uniqueness.

The historical limitations of geometric (metric-size-and-shape) and topological (deformation) invariance usually imposed upon theoretical descriptions of nature (especially in relativity theories) are abandoned herein in favor of studying those properties that are homeomorphic invariants in odd topological dimensions, and yet permit description of topological, as well as geometric, change relative to continuous transformations in even topological dimensions. The methods which are presented herein are based upon Cartan's calculus of exterior differential forms [**?**], [**?**]. Exterior differential forms are objects, which, in contrast to tensors, are well behaved with respect to differentiable (continuous) mappings that do not have an inverse (and therefore do not preserve topological properties), and are also well behaved with respect to diffeomorphisms, which are differentiable invertible continuous mappings (and which preserve topological properties). Evolutionary processes will be defined in terms of the action of the Lie differential with respect to vector direction fields acting on differential forms [**?**]. The Lie differential acting on differential forms is not confined by the diffeomorphic constraints of tensor analysis, and can treat problems of topological change. The method goes beyond the more standard "extremal" techniques based upon the calculus of variations. In most of that which follows, the functions used to define the physical systems will be assumed to be C2 differentiable. The functions that describe processes most often will be assumed to be C2 differentiable as well, but certain C1 processes (inducing tangential discontinuities and wakes) and C0 processes (inducing shocks and first order phase transitions) are of physical interest.

A fundamental result can be expressed by the statement:

Remark 5 *Topological change is a necessary condition for a continuous thermodynamic process to be irreversible.*

Irreversible processes, related to the arrow of time and the biological aging process, require topological evolution and topological change. Current physical theories that describe evolutionary processes (for example, Hamiltonian or Unitary dynamics) usually are formulated in terms of homeomorphisms that emphasize geometrical properties, but do not permit topological change. Hence all such homeomorphic continuous processes are thermodynamically reversible.



In 1957, while working at Los Alamos, I volunteered to help out the scientists of group J10 to conduct diagnostic tests on above ground nuclear explosions at the Nevada test site. One of the wonders of a nuclear explosion that has haunted me from 1957 onward is the fact that, in all that turbulence and irreversible upheaval, a robust ring of ionization current, located at the scroll points of the mushroom cloud, will persist for relatively long periods of time. That extraordinary visual observation has motivated me through out my scientific career. Contemplating those past experiences, I now realize that there are now not too many of us left alive who have witnessed above ground nuclear explosions. Feeling the intense heat on my face, with no initial noise, from a distance of about 7 miles from ground zero of nominal 25 kt atom bomb is awe inspiring. I remember watching the shock wave approach at the speed of sound, ruffling up the debris and sand from the desert floor, and then the noise and shock wave hit.

At that time I had no explanation for the long lived ionized ring, and, although I had been exposed to differential geometry by Dirk Struik at MIT, I did not know of an analytic formula for the mushroom shape. It now appears to me that the toroidal ring is a topological defect or limit set of Pfaff Topological dimension 3, occurring in a non-equilibrium turbulent plasma of Pfaff Topological dimension 4. The mushroom is due to Kelvin-Taylor hydrodynamic instability, for which I found exact mathematical expressions some 40 years later. Volume 1 (and the other volumes in this series) may be downloaded from (http://www.lulu.com/kiehn). Most of the references to my earlier publications have been compiled for convenience in Volume 7 "Selected Publications".