The Cornell Physics Department

Recollections and a History of Sorts



Paul Hartman 1984 Revised 1993 Reprinted 1999

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Recollections and a History of Sorts

Paul Hartman

Cornell University

Informally Published in 1984 And revised in 1993

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THE CORNELL PHYSICS DEPARTMENT

RECOLLECTIONS AND A HISTORY OF SORTS

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Revised 1993

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Author's Note-1993

In the near ten years since this account was first printed and made available (some 350 copies, primarily for Cornell physicists, past and present, the supply now long gone), there have been numerous suggestions made that it be reprinted and made available again and to a wider audience. Late in 1993, Cornellian Erik Pell (Ph.D. '51, Xerox retiree '88) proposed to do something toward that end: reproduction of the 1984 printing, errors and all. Since there were numbers of errors, and numerous new facets to the story learned, it seemed better to correct and alter the text here and there. This we have tried to do. Some post-1984 events have been added but in no sense has the record brought up to the present day. The listing of Faculty, for instance, is carried only up to mid-1987 and the chronology of Department highlights extended but slightly.

Added is a summary of a long, 1869 letter from physicist Ogden Rood to astrophysicist Lewis Rutherfund, neither of whom had Cornell connections, the letter resulting from a visit to Rood by our first physics professor, Eli Blake, en route to his new post at Brown University. It seems perhaps a little out of place to include the letter in the body of the text herein but it apparently has not been published previously and is of sufficient interest for its look at our earliest beginnings and at what must have been the great frustration of Professor Blake (not to mention the opinion of Rood concerning "the Cornell"), that it merits inclusion some way.

Thanks are due to a number of people who have added bits to what we wrote earlier, to Dr. Pell for his initiative in the new printing, and to Sandra Scaglione, who has made the alterations to the printed text. Thanks also to the Physics Department, subject of the piece, for its help and support always. It has been a most harmonious and pleasant group with which to have been associated, a fact on which many outsiders have remarked.

P.L.H. November 1993

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INTRODUCTION

"If one looks through a book of college songs, most of them are found to be as shallow and frothy as the drained mug of beer which seems to be the central fluid of their verse. And so the Ivy and not-so-Ivy schools all have drinks at dear old Zink's¹, tables down at Morrie's, and so on. Another central fluid is in the tears of emotion for Alma Mater. And if the college is beside a river, lake, or the sea, then so much the better for the song content. As for my own Alma Mater, Cornell, I felt it to be the very best of the prestigious Ivy League. After all, she was not beside a body of water, she stood far above Cayuga's waters.

"I have indulged in this bit of tongue-in-cheekery since by contrast, Cornell did (and I hope, still does) have one song which along with its modicum of Rah-Rah, had a sense of vibrant living, a flash of an underlying philosophy of life and change. The Crew Song had [along with good melody] several wonderful lines:

Onward like the swallow going, roused in every nerve and sense, Oh, the wild delight of knowing, 'tis our power which does the rowing; Oh, the joy of life intense; rest was made for feebler folk. Onward, make her cut the water; onward, make her cut the water; And for fame of Alma Mater, stroke, stroke, stroke, And for fame of Alma Mater, stroke, stroke, stroke."

Thus wrote Donald R. Morey, once assistant to Ernest Merritt, and a denizen of Rockefeller Hall over a half century ago.² He goes on:

"The reference to the fluid or liquid state is partly by design, for the fluid state is one of the states of matter and of being; a proper subject in the field of physics, and it was with the Physics Department and old Rockefeller Hall that I spent several exciting and eventful years." Beyond the recitation of specific names and relating of happenings in his recollections, one senses "the intangible, philosophical--almost spiritual and unseen--things which make up the heart of a great institution."

The Physics Department of Cornell has had a long and illustrious history which is nowhere written down in any complete way. It was

¹ Famed, downtown drinking establishment, now gone from Aurora Street.

² Private communication, incorporating some of his own recollections of Rockefeller Hall.

suggested that the present writer, now on a part time "retirement" appointment in the Physics Department, with which he has been associated during a period going back forty-five years, put together a history of the department. With considerable hesitation and consternation, I made a start to see where it gets. It will be anything but organized, either chronologically or topically, an improper, helter-skelter mix of personal recollections and historical fact, with excerpts lifted from true histories and the recollections of others--the analog of a computer memory dump, Professor Stein would label it. Being neither historian nor writer, I really have no experience at the activity and did not see best how to go about it--how much of it should be dry factual information and how much anecdotal, how close to the line I should hew. Further, given some interesting material, must it be written in very formal fashion or can it be done in rather free style so as not to turn off a reader after the first few paragraphs. Is it fairly reasonable to embellish one's tales in the retelling? Not being a professional at history, I will try to go blithely along with my own way of recounting and merely let the chips fall where they may.

How can one convey the sense of appreciation and feeling one has for people, a place, and a past? In his few pages, Morey succeeds in a way the present writer cannot. Of Rockefeller Hall, Morey (and others) came to love the "homey brick structure, its plain tile walls and worn wood floors." He senses the "dignity in its massive wood beams (which can be seen in the attic) and in the walls," plain tile panels framed in dark wood risers extending from the floors to the wooden beamed ceilings. He recalls fondly the campus of his day, the houses still present, our proximity to the president's mansion:

There was green grass to be seen, and a place to park one's car and to play softball; and rows of elms meeting in graceful ballet over the campus roads. I could look out from the Physics library and see that well-known chemist, Wilder D. Bancroft (who loved to be a character), wending his way from his home [where the Statler now stands] up across the lawn in front of Rockefeller on his way to Baker Lab, clad in a brilliant, scarlet cap and gown. And just to the eastward was the Domecon cafeteria where one could at reasonable cost buy a good meal, with the added pleasure of seeing Home Economics majors at work behind the serving counters. [We will recall later herein, another character wending his way down a snowy Rockefeller slope.]

As to life in Rockefeller Hall, Morey reminisces:

While the building itself did not change much, there was a constant flux of new projects replacing the old; new apparatus being built, displacing the old (and relegating it) to the attic storage, and new graduate students filling the ranks of those who had finished their appointed time.

At night the basement became alive with the graduate students at their apparatus and desks, freed from formal classes, taken and given. There was spirit of camaraderie--of sharing in the tide of the "new" physics. And there was something to be learned from each other's specific thesis project, which had a broadening effect not found in the textbooks. And there were good times too--Carl Gartlein had a record player [turning smartly at 78 rpm in those days] and the basement was filled at times with tunes from "The Mikado" or from "Pinafore."

The quality of apparatus, the excellence of building and staff are certainly important to a good department. The present department is fortunate in having its new research library and administrative quarters (Clark Hall), but it is the inward flux of new students, and the outward flux of those who attain a degree and a job which is the heartbeat of a viable department. Perhaps the greatest values lie in the creation of lasting friendships and memories. Some of us will recall Tomboulian, with his recital of how he barely escaped the Turks by fleeing to a British ship--Sid Barnes and tennis--Ed Ramberg helping out in a German translation--meeting Paul Hartman on the day he set foot in Rockefeller--Percy Carr--Roy Barnes--Luther Andrews--just to name a few at random from the hundreds who have passed through and gone on to fill useful positions.

Morey hoped to "suggest the joy of life intense, stroke by stroke, down the river of time." And he does.

So also would the present writer wish to convey a sense of the good feeling one has for a place--Cornell, and the Physics Department in particular--for what it represents to him, for its people with whom he has been associated, for its life over the not so distant past, and for the scene of the action itself. Unfortunately, in these hands that desirable objective cannot properly be met; the feelings are there but the poetry is not. But beyond the intangibles, there is history, and that presumably is something one should be able to write down, however inadequately.

But where to start? There is a brief history of the department written by Professors Grantham and Howe in the middle 1950's which will be largely incorporated for the period up to that time. How much

elaboration of that, if any, will be undertaken, remains to be seen. There is a fair amount of material in the Archives of the University Library which pertains to the Physics Department, and more in the Physics office; the Howe-Grantham report is in the collection. There is an early brief history included in T. W. Hewitt's four volume Cornell University: A History, published in 1905, which includes an elaborate description of Rockefeller Hall, then a-building. And we have Morris Bishop's readable History of Cornell, with some of Physics. There is a large scroll--Moler's chart titled "The First Fifty Years of Physics in Cornell University," with the second fifty appended--on which is arranged chronologically all the people who were associated in some official capacity with the department through at least 1945; spread out and framed, this used to be a prominent feature of the Rockefeller Hall first floor corridor. Following World War II, the Physics Department simply grew out of hand to such an extent that it became quite impractical to keep this attractive record up to date. Nevertheless, it is a most useful source for the period preceding its demise. It is now an archive itself. There used to be a fine photo file in the old Physics library in Rockefeller Hall--the old, old Physics library, that is; the one on the west side of the first floor corridor just south of the north entrance, the library which preceded the "modern" Rockefeller establishment in the west end of the north wing, where the Arts College now has quarters. Hung on the library wall, hinged like a book were several large masonite panels carrying pictures of our prominent forebears. They have disappeared, sad to say. It may be that they were dismantled and the pictures turned over to the main library archives; a few portraits have been seen over there which ring a bell as having once graced that fine Physics library collection, but in the main they are still By way of note, there were on windows of the Rockefeller library lost. two beautiful, large, black and white glass transparencies of Colorado River canyon scenes done by old Professor Moler. One of these recently turned up as garbage on the Clark Hall loading dock, badly damaged by water (rain or radiator--both) and unsalvageable. Compared to our present Edna McConnell Clark Library of Physical Sciences, the old establishment was a modest endeavor indeed. Very informal and a peasant pace to study; it was easy to find journals in those days--there weren't so many of them, and they were thinner too.

To read an account of every person appearing in the department files would be utter boredom: it would not be possible anyway. Nor does it seem profitable to go into a complete evaluation and summary of all the many researches that have been done. Thus, the resulting history will be random, very sketchy, and obviously incomplete.

Before going into extensive research in the department and library archives, it seems easiest for me simply to comb my own memory to see what may be recalled of the "recent" past. This includes the forty-five years since I934 when I first appeared on the scene as a graduate student, seven years of which were, as in the case of many others in the department, spent elsewhere during World War II. Accordingly, I will start back then with personal things, not all necessarily germane to a Physics Department chronicle but yet somehow tied to the department, its residents, to physics and physicists, if sometimes remotely. It may serve to give some flavor for the life here during the past nearly half century.

RECOLLECTIONS

I guess I was fairly lucky that I ever got into physics, and into Cornell in particular. As an undergraduate at Nevada, I was an electrical engineer, class of 1934. With a physicist father (with strong ties to Cornell) and an astronomer mother, I had, however, taken what physics and astronomy courses I could manage in a full E.E. curriculum. At graduation, there was only one graduate who landed a job in electrical engineering; he got a job running a small power house on the Truckee River; and he had majored in philosophy. I found a job in the vegetable department of a new Safeway store. The district manager later told me I had a bright future in the discipline; he wasn't so sure about physics. But, anticipating a paucity in the engineering job market-and every other job market, it then being well into the great depression--I had applied to Cornell for admission to do graduate study in physics. Lloyd Smith, a new assistant professor in the department, knew of this and had encouraged it; his family were close friends of our family. Lloyd had graduated from Nevada nine years earlier, also as an electrical engineer, and had gone to work at General Electric. I believe my father was somewhat instrumental in persuading him to enter graduate study, and at Cornell, which he did with his Coffin Fellowship. My application forms, letters, and so on were forwarded to the Cornell Graduate School and nothing was heard until word came from Lloyd asking where all the forms were. One could hardly be informative from so great a distance, but they apparently got lost somewhere in the works, such even happening back in those simpler and less bureaucratic times. Somehow, Lloyd persuaded someone that it might be worth taking a chance on me. I was admitted and awarded a small scholarship. Together with that and a like amount of my own that had been invested in some fire engine company stock, I managed to live through the year, and not distressingly uncomfortably so either. A room at \$3.50 a week in a private home at the foot of Williams Street, a pint of milk delivered on the doorstep each morning to consume with some hot cereal cooked on a hot plate in the room, and particularly, those 23-cent lunches and 35-cent dinners at the Home Economics cafeteria in the new guarters of Martha van Rensselaer Hall, all went to making life on a small income quite possible. Memory

does not serve to tell what, if any, tuition had to be paid. I was given the large \$600 President White Fellowship the next year. A parenthetical note can point out that inflation was already making progress. Professor A. W. Laubengayer, of Chemistry, as a graduate student fifteen or so years earlier, roomed in a house across the street on Williams at Highland Place and paid weekly only \$2.50 for his quarters; it was a double room too and pretty luxurious.

So, for better or worse, I was admitted. I am sure there were times when both the department and I felt it was for the worse. I know I frequently wondered about the bright future in vegetables. This was my break out of the home environment. Unlike most students, who go away for their undergraduate years, mine were spent at home. Coming here, there was some shock and adjustment for me, but I suspect that I look back on my graduate years as most others look back on their years as undergraduates, a period of hard work and fun, very fondly remembered.

It was a long train ride from Reno to Chicago--and no reclining seats either, just right-angled, green plush. It was the year of the Chicago World's Fair--1934. One obviously had to spend a day or so giving it the once over. The YMCA was clearly the place to stay, cheap, and hard by the scene of the action. For years after, Y's in various places were my favorite hostelries. The last time spent in one was during a Physical Society meeting, over on 34th Street in New York, at 9th Avenue, not exactly the safest place to be in more recent times. I came in from New Jersey one night about 2 AM to find my roommate, Professor Silverman, putting himself to sleep with a Gideon volume.

Not much is recalled of the Fair. A whirling disc television demonstration was exciting and made an impression; one <u>had</u> to get in front of it and see one's small image appear through another disc whirling in front of a broad discharge light source modulated by one's own countenance. Some years letter, with Lloyd Smith on a visit to the RCA Laboratories and Vladimir Zyworkin, I first saw the all electronic system still under development by Zyworkin: a primitive iconoscope camera pickup aimed out the window, a very recognizable Camden Delaware River bridge and its moving traffic appearing on the kinescope. Somewhere else in the multitude of things to see at the Chicago show was a Cornell artifact--Professor Anthony's generator, said to be the first practical

dynamo built and used in America. If I saw it, it surely made no impression. Another exhibit is recalled, although not much was revealed. There was to be taken in the famous dance of Sally Rand, only recently deceased, but until death still reportedly performing behind her fans the routine for which she was noted. For all the promise, fairly tame. I only remember seeing it, not much of the performance. More memorable, from an earlier day, in company with a friend from undergraduate physics, was my first encounter with such goings on, at a burlesque theater (The Capitol?) in San Francisco at the time of a California-Nevada football game (Nevada lost). On stage, as a male soloist rendered the "Rose of Picardy" was a static tableau featuring a rose bower and bare-bosomed nymphs as statues. Pretty daring but in retrospect not unattractive. Tame today, apparently. Cost for the "hotel" that night? Fifty cents for two of us, with a rail spur and freight switching right outside the secondfloor window, somewhere south of Mission Street.

There is not much connection between such risqué adventure and the history of Cornell physics, one supposes, but it gives excuse to relate a story involving burlesque and real physicists, some from Cornell. At one of the spring, American Physical Society meetings in Washington, to which a sizable contingent of Ithacans usually traveled, seemingly more often than not starting off in a foot of snow, there were gathered one evening some physicists and their wives (I believe at that time there were but few physicists with husbands). The subject turned to what might be done after dinner during the evening. According to Lloyd Smith's story, told at lunch one day following the meetings, some staid soul suggested a concert, another a movie, and some really bright member proposed the burlesque down the street. It struck a happy chord and there was considerable enthusiasm shown. Professor and Mrs. Lee DuBridge (he, later director of the MIT Radiation Laboratory and still later president of Cal Tech, but at the time, a Professor at Rochester) were part of the company and Mrs. DuBridge was quite inquisitive as to what burlesque was. She had never been to nor, apparently, ever heard of it. An attempt was made to explain in a nice way. It was a sort of dance show with music, which could be likened to ballet. There was also the spoken word, some song, and light drama intermixed. All very well. And so they went. It would have been fun to have been there. I don't know whether it was the

first "comedians" or a solo "dance" performance and the response of the audience which awakened her to the reality. As I recall the tale, she took Lee right out of there and was going to write Mrs. Roosevelt a letter of indignation and dismay that such things went on in our nation's capitol. I don't know whether the rest of the troop followed them out or not; one suspects not.

Well anyway, I got to Cornell. I recall rather vividly rolling down West Hill on the Black Diamond of the Lehigh Valley R.R. and seeing the library tower (there was then only one tower which was particularly outstanding) and other buildings of Cornell across the valley under gray skies. (How many gray skies would follow! That first November--it got mighty cold--there were all of twenty-eight minutes of sunshine!) It reminded me of some poet's lines,

I saw the spires of Oxford, as I was passing by, The gray spires of Oxford against a pearl gray sky...

Although I had been in Ithaca once before, it was nevertheless quite a thrill. I was met at the station by my sister, who was a student in History with Carl Becker. She had been here one year and stayed on through the next, earning her Master's degree under the great professor. While he did not mean as much to me then as now, I had occasion to meet both him and his perhaps more illustrious predecessor, George Lincoln Burr, the latter meeting taking place at Telluride House, if memory serves.

In those days, there was still a streetcar system in town, so we took the car up State Street hill to my sister's place on Stewart Avenue. By evening I had decided on a place to live; at the bottom of Williams Street, pretty close to Jim's Place, a bar of some notoriety, now known as the Chapter House. "Oh, Bill's Alley," Professor Gibbs commented the next morning when I checked in and told him where I was putting up. No mention of Jim's. My room was a nice place, only one other student living there--a Chinese graduate student in Education, who had arrived a year or so earlier and who later rose to a prominent position in the Chiang Kai-shek government. His English was not so fluent but we managed to communicate easily enough. Coming over on the boat he had almost no English, but he found he could get along pretty well with what everyone

else aboard seemed to use: "OK." He tried his luck with it in the barber shop below deck. After his trim, the barber said something to him, to which he responded with what he knew best: "OK." A shampoo followed. Next a shoeshine. He decided that Americans did things right, and then he got his bill. One time after a trip to New York, he cooked a Chinese dinner for me and the elderly lady and her husband who rented to us. It was good; one dish drew special praise, but he would not tell us what it was. I've always suspected cat, but I don't know. A few years later another physics graduate student had taken up his room: John Trishka, now long a professor over at Syracuse.

Anyway, it was at the foot of Williams Street, a stiff climb every morning and, in winter, a frequently hazardous descent. But it was a nice quiet place to live. Jim's was not particularly disturbing and it was a couple of years before I got into the place, in company one summer evening with some other physics graduate students and Professor Leroy Barnes, whose wife was away. I was pretty abstemious in those days, but Roy went to the barkeep for something alcoholic he was sure I would like. And it was tasty, but I've always wondered if it wasn't just lemonade.

Saturday nights on the street used to get somewhat noisy as young customers in their cups headed up the hill for home. One Saturday night in my second year, I was studying away about 2 AM when the front doorbell rang below. Not wishing it to repeat and to disturb the landlord and his wife, I hustled down the stairs to see what was what. There on the porch, standing stiffly at attention in the light I snapped on, were three graduate student friends from Physics, George Scott, tall and lean, flanked on one side by big Charlie Baker, and on the other by little Willy Higinbotham. George was practically buried in Baker's huge raccoon skin coat and carried, military fashion, at right shoulder arms, an old Springfield, boltaction rifle they had found somewhere downtown during their evening's revelry. They weren't inebriated, but they were surely high enough. could not restrain my laughter at the sight and it was a pretty gay minute before we got settled down without disturbing the old folks upstairs. The three wanted me to keep the arms piece in my closet until the next evening when they planned to take it over to a house wherein lived a number of women graduate students in another department. Their house, known as Risley Cottage, was set near where the Prudence Risley bus stop

now stands. I don't recall the details of the delivery, why, or whatever became of the thing. I trust it got home, but I was not too happy for a day there keeping it in harbor. Charlie, incidentally, is probably the only person to earn a Cornell physics Ph.D., who was "busted" out of the institution as an undergraduate.

It was another Saturday night that I was walking away from Rockefeller to my room down the hill. It was spring, well after midnight, and I was between Stimson and Goldwin Smith halls, heading down toward Willard Straight Hall. As I neared the walk fronting Goldwin Smith Hall, I was surprised by an approaching automobile tooling along the pathway, lights piercing the dark, heading South, and filled with pretty boisterous college kids, who may have been at Jim's all evening. As they passed by me, a voice called out in the night: "Hey, boy! Is this the road to Buffalo?"

I cringe a bit when I think back on my first encounter with physics faculty. Smith was not in his office, so I walked down the Rockefeller corridor a few doors to where I heard his voice, and there I found him in some discussion with another professor. I barged in to break up the discussion. I often wonder what Professor Kennard--"Ducky" to all the graduate students--thought they had admitted to their halls; I was a little less brash subsequently. Smith took me to his office, and we plotted what I would be doing. I wanted Physics as a major--spectroscopy perhaps-with Astronomy as a minor. I talked to Professor Boothroyd after that and arranged a minor with him in the latter subject. It turned out that I would choose R. W. Shaw in Physics as my major advisor; an instructor, he was in spectroscopy and close to Astronomy. Indeed, he eventually became and remained head of that department until its later, postwar expansion. don't know if Smith was dismayed or comforted that I had not put him on my committee. That came later, after I had been advanced to the Ph.D. candidacy. So I wound up taking a course in Theoretical Physics (P-200) with Smith, in Atomic Structure (P-581) with Gibbs, head of the department, Astrophysics with Boothroyd, and Advanced Laboratory, Physics 106 it was back then, with Shaw. It was a semester of mixed success. I remember being exposed for the first time in the beginning lecture by Smith, to the vector and scalar products, tools of the trade these days in junior courses. Smith asked some trivial question, something about wind and a sailboat; I believe a scalar product was

involved. I spoke out boldly, but with exactly the wrong answer; I was clearly going to be shaken, and so would Smith. In the final examination for the term in that course I believe I received all of 45. But I did reasonably well in Atomic Structure, wrote a nice paper for Gibbs and received an A or a B. He was not the most inspired or fiery lecturer. As befitted his character, his lecture was rather plodding, closely following the script in his hands as he developed things on the board. The notebook I kept in the course indicates he covered the experimental evidence for the nuclear atom, black body radiation, the classical Bohr-Sommerfeld theory of hydrogen and its fine structure, some spectroscopic concepts, the vector model, and the like. No X-rays were met, but in many respects it overlapped considerably what Richtmyer must have covered in his two semester "Introduction to Modern Physical Theories," probably a more exciting course, but to which at Nevada I had been exposed, shall we say. Boothroyd's Astrophysics went well enough. But whatever my strength, it was in the Advanced Laboratory. There were two or three fields emphasized in the laboratory, housed on the third floor of Rockefeller, south wing, and in rooms along the central corridor. I was in spectroscopy with Shaw. Others were with Murdock in X-rays, or with Smith in high vacuum and the gaseous discharge. Shaw put me to work with one Henry Strauss, a senior in Physics. In our first get-together, I recognized how visually and easily we could make some adjustments that needed doing on the Rowland mounted grating spectrograph--one of two still used, by the way, in the present Advanced Laboratory course, P-510, in Clark Hall (one grating of which is inscribed on the back by R. W. Wood, the great optics experimenter at John Hopkins). I felt confident and Strauss was impressed. He was a nice, sort of bumbling guy in laboratory, who I was later to see at the MIT Radiation Laboratory during the big war, sometime before his early death. It was from him that I first learned of the great young European physicist who had been lured to Cornell, slated to show at the beginning of the new year: one Hans Bethe, already author of a couple of long and major articles in the Handbuch der Physik. It was rumored that, in cranking out the <u>oeuvres</u>, he simply sat at a table with fresh foolscap piled on one side and completed manuscript piled on the other, taking sheets off the one pile and transferring them finished to the other.

It was an exciting prospect; a young famous scientist with whom to be associated, one way or another.

From here I don't recall what spectrograms Strauss and I made with our spectrograph. The instrument was on one side of a plywood partition and the light source was on the other, much as we designed into the present Clark Hall laboratory. The location was the scene some years later of the only Advanced Lab fire they had up there, a fire trap if ever there was one. I do remember one rather exciting and extended class exercise. The wave numbers of the lines in the mercury spectrum were given to the class--there were three or four of us--and we were to deduce the energy level diagram. On a long strip of paper, the wave numbers were laid off linearly and the emitted spectral lines indicated by bars of height proportional to intensity. One then sought equal energy differences between bars and was led to the energy level scheme. Very instructive and fun.

In an Alumni News (January 13, 1904) description of Rockefeller Hall quoted by Hewitt in his history, special note is made of a large, 21 ft. focus, concave grating to be mounted (as per Rowland) in a great instrument on the third floor, south wing, for use in study at "highest precision" of solar and other spectra. The third floor of such a structure as Rockefeller seems an unlikely place for a large, vibration-prone, photographic apparatus; but there it was put, suspended from the ceiling beams no less. It was still there until extensive building modifications were undertaken in the seventies. But for years a number of optics experiments in the Advanced Laboratory were carried out beneath this imposing device, which hung like the sword of Damocles over the students working there, unaware of its presence. The laboratory area was dark, however, and it wasn't much noticed. I believe I never saw the main boom moved on its rails, and never heard of its being used, certainly on solar spectra, although I do recall a little enclosure in the corner of the big room which might have housed a heliostat and other sources. There was a photographic darkroom over in the other corner. Where the grating had gone, one would like to know. The "smaller diffraction gratings" also mentioned in Hewitt's description are in the two spectrographs alluded to above.

One of the memorable experiences from those days in the Advanced Laboratory, one for which the excitement still remains, was the summer morning that Otis Hendershot, a man at least twenty years our senior, showed me the Zeeman effect. In the room housing the above monster, he had an echelon interferometer set up with a modest spectroscope looking at some source in a magnet. The spectral line was red and to see it split into its various components as the field was increased left an indelible (at least deep red) impression. There we were, "diddling," so to speak, with something as basic as the inner workings of an atom. Hendershot was raised in rural Tompkins County, taught science and mathematics in high school before he enrolled at Syracuse University. He left an instructorship there to come here for his Ph.D., working on a crystal structure problem with Murdock. It was depression time, and I expect there was no physics future at a university without the advanced degree.

Besides Henry Strauss and another student, Danny Garland, who worked later with Carl Gartlein, there was one other remembered person in the group, one I still know well. She was then Charlotte Throop, granddaughter of E. L. Nichols, early head of the department, before Merritt preceded Gibbs, who had but recently come into the Merritt. position. Charlotte was surely "one of the boys," an admirable companion to all of us first year graduate students. She subsequently became Mrs. Shaw, but in our first year or so was very much a part of the gang. She was the only girl to eat with the circle of physicists who always managed to get one of the large round tables in the "Martha Van" cafeteria for lunch and dinner, somewhat to the distress and envy of other available young females in the dining room. She had an old car--a touring, four cylinder It was an occasional summer pleasure to set out late in the Erskine. evening with her at the wheel, under the promotion of Al Rose (later to develop at RCA the sensitive image orthicon, foundation for all of television pickup tubes for many years), with three or four others distributed between front and rear of the open vehicle, for the Hotel Cortland and a cup of coffee. It was then back to Rockefeller for an hour or two more of work. Al was not a great traveler in those days but he surely liked the journey to Cortland.

The third floor of Rockefeller was quite a different place in those days from what it is at present, and from what it was prior to the major

renovation of 1980-81. An appreciable period of my life has been spent on the floor. Besides taking the laboratory course in the south wing, Shaw started me on a Raman spectroscopy experiment, carried out in the north wing. He wanted to get a Raman spectrum of germanium oxide. I don't recall what the interest was, but considerable work on germanium had been done by Laubengayer over in Chemistry and Shaw had been given a lump of the glass-like oxide. That was a long time before transistors. Had we but known the possibilities! He gave me a room up there in the north wing, a room overlooking the shop end of the building. They were dark, musty, little rooms up there. I believe Professor Bedell had taught aeronautical science in the area during World War I, at least there were a lot of his aeronautical texts lying around. Some photography had been done in the back dark rooms, aerial work in World War I; Moler had been interested in photography. Even until the early thirties, a door carried the identification: U.S. Army Photography. And then there were these large, fuzzy flies darting in and out of the bright source illuminating the Raman sample, and flying around one's face. They still come out of the woodwork in the winter up there--at least they did, not so many years ago before the audio-tutorial teaching course took over and refurbished the whole north area. In the west end of that same wing was a sophomore laboratory for physics majors and premedics--rather a barn of locale. It could be as cold as Greenland in the large room, so each winter Buildings and Grounds trotted out big storm windows from the attic above and installed them at some risk to life and limb. Even then, as in much of the building, temperatures could occasionally get pretty frigid. Students frequently wore mittens during lectures. I only remember frozen pipes but once, and that following World War II. On a warm Friday afternoon the heat was full on and an uncomfortable student opened up the windows and left without closing them. And there were then no watchmen to catch such slips. Over the weekend, temperatures tumbled, snow flew, and pipes froze. There must have been a flood, else why would I recall it? When there was a flood on the third floor of Rockefeller, everyone below knew of it. In this old building, the space between the ceiling of one level and the floor above is filled with cinders, presumably to deaden overhead sounds. But cinders soak up water. By the time water had percolated to the basement, the intervening floors and the basement dripped for a long time until the

cinders gave up their soakage. It was a constant hazard, one which was obviated in the design of the student advanced laboratory area in Clark Hall, where all water leaks would be entrapped in curbing adjacent to any apparatus involving water cooling. So far as is known, there has been but one flood in the new lab, originated by the author but safely contained by the curbing. It did cost a fair amount of de-ionized water, however, but no drippery.

For the Raman experiment we had a very slow prismatic spectrograph, still used in the Advanced Laboratory. I contrived a reflecting elliptical cylinder in combination with a circular cylindrical reflector to put as much light as possible from the source, at one focus of the ellipse, on to the sample at the other focus immediately at the slit of the spectrograph, through which point the circular reflector passed, with its axis at the source. It was effective but exposures ran for days. I got only a smudge. Every time a smudge. Finally, we realized that was probably what germanium oxide did, and Shaw gave a paper on the results. I'm afraid it did not greatly advance knowledge, least of all transistor technology. The experience recalls a similar questionable smudge seen over great spectral range in a plate made with Tomboulian many years later at the 300 MeV synchrotron.

X-ray experiments in the Advanced Laboratory were located in a room on the east side of the third floor corridor. These were under Professor Murdock and were inherited in 1935 by Lyman Parratt, later department chairman for ten years, who, before his chairmanship, was in charge of the Advanced Laboratory course. There was a "Standards" laboratory immediately at the head of the south stairwell, which included, among other things, a lengthy photometer bench, long since departed. After World War II, we installed a Cavendish gravitational balance experiment on a wall of the room. It was of interest to note a marked shift in the zero reading when the weekly colloquium crowd assembled in Lecture Room B, a floor below. A flexible structure apparently.

Back up on the third floor again was a night watchman's room. Where the P-101 office is now located, slated (in the 1981 renovation) to be moved down one floor, there was a room given over to living quarters for two graduate students serving as building night watchmen. On alternate weeks, one or the other of these lucky fellows was supposed to

tour the building from top to bottom twice each evening--once at six o'clock and again at midnight or whenever his bedtime approached. And from top to bottom meant just that; from the attic to the basement and out behind the building to the Annex, which housed ventilating equipment, a battery bank, and some X-ray research. It was sometimes a bit eerie going down through the attic at midnight, a flashlight beam shining ahead; whistling, of course. I was fortunate to succeed AI Rose, I believe it was, when he left for more profitable climes at RCA; which accounts for more of my life spent on the third floor. It wasn't the most cheerful place of a Sunday morning, but it saved my \$3.50 a week rent for a couple of years before I went back to my old domicile on Williams Street. Breakfast was cooked up there and some of the more adventuresome holders of the "position" even put dinners together and had company, professors no less. It was a tenable operation, one which could well be emulated in the building and elsewhere on campus today.

Adjoining Lecture Room B on the second floor, with an apparatus and lecture preparation area between, was the big barn of an auditorium: Lecture Room A, at the head of the stairs, tiers of seats rising toward the back, under a great timbered ceiling with slate roof (now asbestos) immediately above, noisy in a rain storm, not to say leaky. There was also a preparation room for Lecture Room B behind its front wall, and above a similar room for like purpose behind Lecture Room C on the first floor. Original floor plans accompanying the specifications book for the building construction, still in department hands, show over the corner of these two rooms a circle, otherwise unidentified. Old hands will recognize this as the location of an iron spiral staircase connecting the two preparation rooms for handy transfer of apparatus. I doubt that much apparatus ever made the trip. I believe I traveled it but once.

There are two other artifacts of the building, still present but not ordinarily noticed. On the south side of the court against the brick wall of Lecture Room A, rising from ground level to above the roof, there is a chimney. The "specifications" spell out the construction, but what it was for is not known. Hewitt indicates that heating (as well as ventilating) was accommodated by the "low flat roofed structure" known as the Annex, so it is not clear for what the chimney provided draft. The other feature is more interesting. There is over the south side entrance to the building

and below the separation between the first two windows of Lecture Room A, a small window neatly set in the brick wall, complete with stone lintel and sill. There is no apparent counterpart on the inside. If one looks a little more carefully in Lecture Room A, however, one notices a latch fixed to the woodwork inside at about the right position. Indeed, there is a small door which will swing open on pulling the latch and which exposes the window. This was a feature not uncommon to Physics buildings of the period. It allowed for the aperture of a pin hole camera to be installed. Here, with this facility and the darkened hall, the scene back of the president's mansion could be projected onto a screen; not that the scene was usually all that great. The Ohio State physics building has such an installation. (Franklin Hall, across campus, had something earlier which served the purpose.) The story is told out there of one professor's consternation, on demonstrating in his lecture this camera obscura, to find, not inverted as is usual, but an image on the screen of a fellow cavorting around on the snow-covered ground, right side up. Someone "in the know," of acrobatic ability, had timed his act so as to be walking around on his hands in the scene when the demonstration was carried out. That recalls another demonstration, at Princeton I think, where the demonstrator, illustrating a principle of mechanics, fired some kind of a projectile to a skylight in the ceiling and a stuffed duck dropped down in front of him. That sort of thing can enliven what might be an otherwise to-be-forgotten lecture. (It was Herb Newhall's understanding that our little window was for the purpose of bringing in sunlight with a heliostat; he was given that impression by Professor Grantham. There seems to be no record of its having ever been used for anything.)

With the exception of a tiny office for Professor Trevor off the landing for the two lecture halls, and more commodious quarters for Howe and Grantham off the landing at the other end of the building (passage between landings being through the large front facing student laboratory, now, in 1981, by a corridor), the remainder of the second floor was given over to instructional laboratory space and TA desks in a side room off the north laboratory area, where Grantham and Howe could keep close watch on activity. Ceilings were fourteen or so feet above the floor, almost high enough to have made possible another floor level between. A strange division in elevation.

In the book of building specifications, there is a letter addressed to Prof. Moler. (On the envelope a modest, red two-cent postage stamp; Washington. Two cents! 1908! It was two cents well into my youth; these days it rises by that amount every few months.) The letter is from Carrere and Hastings and encloses a pencil sketch locating test borings that had been made prior to building construction. We may not be on all that firm a foundation. They had found "well compacted sand" somewhat above water level. This is what we are sitting on. So also Baker Laboratory, and Clark Hall, except that it is supported on a great number of concrete columns going through the sand to bedrock. The matter is of some interest, Back when the ice age was letting up, the especially to geologists. campus location was under(!) an upland lake, and Fall Creek built up a sizable delta in our immediate locale, which is the source of the present sand. The old university reservoir back of where Bailey Hall is located was on this sand bed. Baker Laboratory at one time had great problems with water in the basement which drained down to it from the reservoir through the sand. It is not recorded that Rockefeller had similar problems. With the abandonment of the reservoir, Baker's problem abated.

During two summers I fell heir to another "watchman" job, which sounded even better. It was to live in at the observatory, out beyond Balch Hall. One summer Marshall Holloway joined me, and for the other one it was AI Taylor, of whom more later. I had only to wind clocks weekly and generally see that things were not molested. I also had the use of the 12" refractor under the dome, for which instrument I had designed a rather too heavy double-slide plate holder for taking photographs. I'm not sure but that it is still around somewhere. My introduction to that "job" came at a fortunate hour. The first night I was gone from the Rockefeller quarters, there was a severe thunderstorm. Lightning hit Rockefeller Hall; an attic skylight fell in, hitting the automatic sprinkler system; all the alarms sounded; water began to play in the attic; and I guess it was a high old time that Willy Higinbotham had for a while there, he having replaced me in the position. I slept soundly through it all in my new basement quarters on the hill out beyond Balch.

The attic was a pretty useful place: athletics and storage. In those days there was a fair amount of space in the central portion, now subdivided into innumerable little storage rooms for this and that

professor. Skylights provided daytime illumination, particularly on sunny days. It was up there in winter's cold that various of us tried learning to ride "Kay" Frank's unicycle. He was a graduate student with an interesting past as circus-carnival performer. We were told that he rode his motorcycle (not uni-) at a good clip up the wall around the inside of a large hemisphere, a mangy lion at the center below. Needless to say he was pretty good on the unicycle. I don't know that any of us learned. Today's Prof. Sievers is another matter: he rides his in downtown parades. In the evenings or late afternoons, with all the lights blazing, handball was engaged in. There was a blank wall half the width of, and about a third of the way down the large space, oriented perpendicular to the main passage through. On each side of this wall, enterprising researchers of an earlier day had laid out a handball court. Lyman Parratt tells of working with Don Morey, Merritt's assistant, who instigated the operation, painting the wall and laying down the lines. He has said that it was necessary to use a hoe for removal of the pigeon guano that had accumulated. Morey himself tells how, on permission of the faculty, fifteen or twenty workers cleared the area of old apparatus and dirt and "we bought lumber from the Robinson yard down on State Street [folded in 1980]. Then with saws and hammers we put up the wall, and we marked out the lines. We could use both sides of the wall and so had two courts. During rainy days and in winter, when the outdoors could not be enjoyed, there was many a game played there. It was Physics I in elastic rebound and coefficient of restitution, with final exams in the form of elimination tournaments. remember some of us who were quite good players--Isadore Fankuchen, K. V. ("Kilovolt") Manning, and Horace Grover. They gave me good contests in terms of skill. But it was my very dear friend, Lyman Parratt, who was the complete opponent; for with us, not only was it a contest of physical skill in getting to the ball and hitting it properly, but also a contest of challenge of wits and strategy, of drawing each other out of position. Just as Lyman is 'wired up' somewhat uniquely in his approach to problem solving in physics and teaching, this characteristic showed in his approach to handball. Lyman has gone on to surpass me in contributions to physics, but there were times when I was able to outfox him on the court."

The courts were well used, even in the cold of winter, usually before dinner over at "Dom Econ." I don't recall about showering after such

activity, but there was an old jerry-built shower of sorts in the basement, largely for the use of the night watchmen, an old mildewed canvas curtain surrounding the shower basin. It could be warm, even hot, but was somewhat primitive. After the war, in the nuclear age, the area was taken over by Engineering Physics for a zero power reactor, which occupied the space until Ward Laboratory on the engineering end of the campus was constructed.

But to come back to the attic. In the south end, piled under and around the great wooden beams supporting the building, was the most marvelous collection of hardware and old apparatus that a "sealing wax and string" experimentalist could ever hope for. All manner of discarded research apparatus, vintage demonstration gear, old bound volumes of the Physical Review, multitudes of carbon filament lamps, all was piled in considerable disorder, somewhat like Tutankhamen's tomb. It also was a gold mine, when looking for parts with which to start an experiment. lt was not too easy to climb around it all, but it was usually worth it. Ideas came if not actual parts. However, it all disappeared in more recent times when inspecting firemen, aghast at the clutter and floor loading, ordered it out. Most of it went, to our own cost. The stored copies of the Physical <u>Review</u> did not, for many years they were a real treasure. Complete sets were available up to a certain year, and frequent requests came in for such, yielding a small but steady source of income for the department. (The completeness is not quite right; there were apparently no complete sets. Lauriston Taylor has told of Merritt's hiring him to organize huge piles of the journal, then in great disarray out in the dynamo laboratory-offering 25 cents an hour to do so and a complete set at the task's completion. Taylor took on the jumble but found only one complete set. "It's yours," Merritt told him on being informed.)Lead plates of Review figures and some pages were also to be found on the shelves, thrown into boxes. Dale Corson, later department chairman and still later president of the University, has mounted on his wall the lead plate from which was printed the cover of Volume One, Number One, of this today's important journal, which was founded at Cornell, as may be related in the early history, perhaps to follow. "A Journal of Experimental and Theoretical Physics," it says, "conducted by Edward L. Nichols and Ernest Merritt." The lead article is by that other noted Nichols, Ernest F., on "The Transmission

Spectrum of Certain Substances in the Infrared," followed by "The Relation between the Lengths of the Yard and the Meter," by one Rogers. Then comes another Cornellian, Benjamin Snow, with an alkali-halide paper (shades of a half century later!)--the infrared spectra of same. Another outside paper and finally one by insiders Bedell and Crehore (Bedell later the Review editor), on "The Geometrical Proof of the Three Ammeter Method of Measuring Power." There were some notes and book reviews. A beautiful and prized wall piece.

The plate was discovered by Roger Knox after World War II, when the Laboratory of Nuclear Studies was in need of lead for shielding purposes. Three thousand pounds of such plates were trucked out, melted down and recast as lead bricks; all, save this plate and one other--that of page one of the same volume, the latter hanging on former LNS Director Wilson's office wall somewhere. One day they may become part of the department or Physical Society archives.

Another attic problem was overhead, the roof of the building. This was not the most waterproof. Besides incorporating some large skylights, which frequently leaked, there were other random leaks through the slate It was not unusual to find numerous pails strategically placed shingles. to catch drippery during a long heavy wet spell. At one juncture there was even a system of gutters supported on boxes and stools to conduct incoming water from the faster leaks to containers larger than buckets, making for less frequent emptying operations. Without such ingenious ploys, it would not be long before dripping was occurring through the ceiling of the third floor. Recalcitrant radiators were another, but internal, source of unwanted water moving around over floors where it was not supposed to be. And in their modest effort to keep us warm, they frequently developed convulsions and took to banging loudly in wintertime as steam came on; lecturing was sometimes difficult in consequence.

The skylights were an original feature of the building, one later most practical for Chemistry in providing light when their freshman qualitative analysis laboratory was conducted up there for a period following the fire in Chemistry's home, Morse Hall. Our attic was of course unventilated, so that between the stench of hydrogen sulphide and the smog from ammonia fumes making it frequently impossible to see across the length of the quarters, it was a well nigh intolerable situation. But we saved the day for them. During the rather extended emergency, Chemistry also made use of Lecture Room A for the large freshman lectures, and B, opposite, for intermediate junior courses. After their fire, the second story of Morse was pulled down and the first floor and basement were used for laboratory instruction, and the catacombs, still further below, were used for storage, becoming Chem Stores after Baker occupancy. Subsequently, old Morse became an art gallery of sorts.

Pigeons were forever a problem in the attic. Somehow they would find their way in and it was impossible to recapture them to put them out. So there were yearly a half dozen or so pigeon casualties to be noted in the area. It was always disconcerting to go through and disturb some such trapped bird and send him or her flapping up against the roof skylight, seeking a way out. They were doomed. More will be recounted later concerning other wildlife in the attic.

On the first floor of the building were located the department office, the chairman's office and professors' offices. The library has already been mentioned, occupying two large connected rooms on the west side of the main corridor. These rooms are now all classrooms. The chairman's office was next to the south entrance of the building. As inhabited by Professor Gibbs, it was a sight to behold. But one was always welcome; Professor Bethe has said that if a professor went in to check some small procedural detail, for example, it was a two-hour visitation with the chairman before one got away. His desk and a table nearby were piled high with papers, somewhat in the manner of a haystack, but with little falling away. The remarkable thing was that if he wanted a particular paper, he would look at the appropriate stack for a bit, reach in, and pull out what he wanted. Magic. Next to his office was that of the departmental administrator. I don't know if that was her title or not, but AI (Aloysia A.) King was surely that. She held forth in there for many years keeping Gibbs under rein, or any other preceding or subsequent chairman, managing the department billings, and other matters, until she retired after World War II, after forty-five years of service. She long afterwards lived in Ithaca; she died in 1981. Next to her office was Nellie Lyons' office. Helen was her name, but to all she was Nellie. She came to the department "as a mere slip of a girl," as she used to say, when the department was still housed over in Franklin Hall. She retired in 1957

after fifty-seven years of devoted service. The mailboxes shut her off from the outside world for the most part. But through the wooden grillwork at the mail window, she would spot whomever was there, signal with her finger to come over for the latest bit of gossip, and whisper it off in all detail. But her heart was in the right place; she would give the professor needing one a slide rule turned in to Lost and Found, or other odd bits of hardware. Corson still prizes a stapler she turned over to him; it makes its own staples from a spool of wire and fastens all in one operation. Her retirement party was a happy occasion. She was decked out in all her finery, nicely coiffured, a red rose corsage at her shoulder, and was made much of, as indeed she deserved to be. Chairman Corson, referring to her as Secretary Emerita, presented her with her fine office typewriter, and she received message on message from alumni all over the country. They are in the Archives, and it is guite touching to read them. It was not long after her celebration that she was sent to Willard, a mental institution at the other end of the lake. I went in to see her once on a visit I made to a problem student there, but I don't think she knew me. She died not too long afterwards.

There was one other "girl" on the secretarial staff--young and pretty Dorothy Farrell. Gibbs was a bit concerned over the romance she appeared to be having with a ne'er-do-well young man, one Paul McKeegan. "You shouldn't bother with that fellow, he'll never amount to anything," he cautioned her. But she went her merry way and wound up being McKeegan's wife; and he only became Cornell's Budget Director--not too bad.

No doubt about it, Gibbs took a personal interest in his charges. Even after one left Cornell, he kept track. After my first year at Bell Laboratories, in 1940, I went to Nevada for a vacation, splurging by flying out in a DC-3, rather than by Greyhound as I had once done. This time I drove back with my apartment mate, Fred von der Lage, friend from Rockefeller days then teaching at Cooper Union, where Professor Anthony ended up. I was driving Fred's snappy Ford through Ames, Iowa, from whence Anthony came to Cornell--Highway 6--on our way back east. It was Saturday evening, and the farmers were in town slowing down the traffic. I managed to pass by one or two of them when a siren stopped us, and we were hailed to the courthouse. "Weaving dangerously in and out of traffic" was the charge. There was some waiting, the "judge" indicated

there was no point in a defense, and we paid the \$25. The next time I went to Ithaca, a few weeks later, Gibbs hailed me. "Well, I hear you had a little trouble out in Iowa." I was surely taken aback and wondered how he had learned that; I had been happy enough to keep it quiet. It seems his daughter, whom I had met, was living in Ames, and my arrest had made the local press.

But back to Rockefeller Hall. In the north wing of the building on the first floor were again numerous offices. Boothroyd of astronomy was down there, Professor Bedell was there, and besides the offices of Professor Boothroyd and Bendell, there was some experimental research going on in the west wing. Emery "Wheels" Meschter working under Lloyd Smith with a two lens electron imaging microscope observing electron emitting surfaces; John Hunter, a solitary black physicist, older than the rest of us, working on a thermionic emission problem, also under Smith; and Tom Goldsmith under Bendell, developing oscilloscope high speed sweep circuits. Directly below Smith's linear accelerator being constructed also in this area, was Lyman Parratt making low current measurements in X-rays, next to where the cyclotron accelerator was being constructed by Stanley Livingston, interference from both accelerators making low current measurments, difficult at best, almost impossible, even at more remote locations than Parratt's laboratory. Livingston had come to Cornell from Berkeley and E. O. Lawrence a few months before I put in my appearance. Down the north corridor toward what is now Clark Hall and where some Clark Hall offices are actually now located, was Bedell's dynamo laboratory. There were many machines therein, motors and generators, AC and DC, and large meter panels, all mostly from an earlier era. Included was an Alexanderson alternator screaming away in its non-electronic generation of 20,000 cycles/sec. Still, some of the machines found use up until the late forties. The DC generator for the building's 110 volt DC line was out there. It was a watchman's chore to shut it off each night, assuming no graduate student needed it through the night. And the cyclotron field generator was out there, a big, old, noisy machine, a hand-me-down from the Ithaca Railway System. Beyond the Dynamo Laboratory and in space now occupied totally by Clark Hall was the department machine shop. Roy (Fulkerson) and Murphy ran a relaxed and, in many ways, blissful operation. The work was

excellent, and they were two very congenial gentlemen, Murphy somewhat more rough cut than Roy who bore the mark of a gentleman "of the old school." The machines were not what one finds today. Many were run through an overhead pulley system from a common driver, as was the style of early-day machine shop practice. The student shop was something else again, and it was good to have gained enough of the confidence of Roy and Murphy in my second year to be allowed the use of one or two of their lathes.

The student shop was in the basement in a room opposite the main heat input utility to the building. It was always steaming hot in there what with a number of exposed steam pipes passing through. Equipment in this shop was truly outstanding; there was one lathe in the center of the room, driven by operator foot power somewhat less efficiently than the old-fashioned sewing machine. Over on the workbench was a decrepit drill press with bad bearings, on the wall possibly some hand tools of sorts. It was a real trial to use the facilities, but if one could turn out something useful he felt a sense of some accomplishment. It was only after World War II that a decent machine shop for students and staff, with shop supervisor in attendance, was put together out near the main shop. Outfitting such a student facility was opposed by some of the older faculty ("Students aren't supposed to spend their time that way--that's why we have a main shop!"), but the new "young bloods" carried the day. Today, it is a splendid facility by any standard, much used, and the envy of experimenters from almost any other institution, academic or industrial.

At the start, this new shop included a nice lathe and a good drill press, lots of hand tools; best of all there was a fine Van Norman milling machine, all of which I had some hand in selecting. It was with a sense of dismay that I came into the shop one Saturday afternoon a few weeks after we had installed the mill and found milled into the work table a 3/4" wide groove about 1/8" deep at one end and tapering off to nothing three or four inches further along. But the culprit came to see me about it. A good man. He was a young instructor from outside the department who should not have been using it in the first place. Milling a block of wood with the sharp mill not clamped securely in the collet, the cutter had gradually pulled itself down further and further into the work, finally getting to the bed and proceeding therewith to dig its way into our new

mill. He asked that I not let it be known he had done it, else promotion prospects might be compromised. I collected \$20 or \$30 from him and his name will go unrecorded. Suffice it to say, he is now a full and distinguished Cornell professor, and his milling operation still decorates the bed of our mill. But it remains a good machine serving well in the Clark Hall student shop.

It was in the basement that most of the department research was Here was located the "beating heart of the department," as Don done. Morey has put it. There were a few rooms in Bedell's area for his students on the first floor, but by and large the basement was for research. There was a stock room of sorts at the south end of the main corridor. A couple of small rooms behind the front room housed the prized possessions, some locked in a big safe; there was a dank acid room under the south side entrance steps reminiscent of "The Cask of Amontillado" and a surprise to some Buildings and Properties people discovering it recently. Here, in another room, was the department's prize clock--a fine piece that many up to recent times have had a hand at trying to make keep good time, beginning with William Anthony and George Moler, including C. C. Murdock and, more recently, Hywel White and myself. It now keeps time, attractively at least, in the department office in Clark. There is in the department files, dated January 29, 1895, a communication from the dean of Civil Engineering, E. A. Fuertes, giving Anthony a "history of your clock's rate" covering observation over a three-month period, "all corrected for level and collimation and all but the last two dates corrected for azimuthal deviation, though all are good enough for the object." Apparently Cornell practical astronomy was in hand by that time.

Continuing the tour of Rockefeller "facilities," X-ray research, as crystallography, was done under Murdock in the west end of the south wing (where Nichols' exhaustive fluorescence studies had been carried out) and as atomic physics, under Parratt (a National Research Fellow of one year's standing) in the west end of the north wing, adjacent to the cyclotron then under construction. Richtmyer had students doing X-rays in rooms on the east side of the main corridor and at very high voltages (100 kV or so) in the Annex, a low gray stone building boxing in the courtyard out in back. It was out there that Trevor Cuykendall labored; he was to become a longterm and charter member of Cornell's Engineering Physics, a concept still

ten years off from the time of which I write. Spectroscopy under Gibbs was done in the east end of the south wing, some in rather dungeon-like rooms. At the end of the corridor there, in one of these dungeons, was a constant temperature room housing one of the three grating ruling engines then in the country, the others being at Johns Hopkins and Chicago. I'm not sure that an acceptable grating was ever ruled with it, but attempts were made, first by Moler and later by Carl Gartlein and D. T. Wilber who both worked particularly energetically with it for a period. In the department expansion after World War II, the room served as office for John DeWire, the quietest office he ever had, he recalls fondly. Other rooms in the basement were occupied with the researches in electron and ion physics-electrons and ions in vacuum-largely under Smith.

Back of Rockefeller Hall, roughly where the Space Sciences building is now, were three tennis courts bordered behind the backstop by evergreen trees, some of which still line the present driveway. The courts were reserved for university staff subscribing to their upkeep. Smith was an avid and accomplished player, as was, naturally enough, handballer Lyman Parratt. My later nemesis in the French language examination, Agronomy Professor Wilson, was also a frequent and fast player. Lyman got his comeuppance after one particularly heated match. He wanted to see Richtmyer immediately after it and attempted to do so. Richtmyer had a somewhat elderly and pleasant maiden secretary who admitted visitors and looked after editorial chores. She took one look at Lyman--sweaty, briefly attired in shorts and undershirt--came near fainting, and ordered him out. Things have changed in forty years.

There were two other important Rockefeller establishments yet to be mentioned: the glass shop and the carpenter shop-liquid air production, both in the north basement and both under very definite persons, Harry Banta and DeWitt Calkins, respectively. "Mister" Banta--it was always Mister and not Harry when one talked to him--was a good and very clever, if a bit reckless, glassblower, red-faced and elderly. If you treated him half way decently, he would do anything to keep you going. He chewed tobacco continuously; not only that, he consumed it. No polished brass cuspidor for him; "If you're going to chew, chew and don't spit half of it out," he'd say. He came to us from the Bell Laboratories (then Western Electric), his recruitment from there being initiated by Lauriston Taylor.
Before he became a student here, Taylor had worked for a while at the Laboratories and knew the glass blower, who became delighted with his new life in the upstate environment. He always claimed invention of the well-known copper-to-glass seal given the name of one Mr. Housekeeper. In the technique, glass is fused to an appropriately oxidized, flared and feather-edged, copper cylinder, so thin that the copper has no difficulty in changing its dimensions to follow the expansion and contraction of the Many large, water-cooled transmitting glass as temperature changes. vacuum tubes featured the seal. Much of electron physics research was done in tubes constructed through the combined talents of Mr. Banta and the suffering graduate student. The student generally fabricated the hardware which the glassblower sealed into a glass envelope. Wondrous indeed were some of the experiments concocted for him to encapsulate: multitudinous electrodes of sheet metals, filaments kept under proper tension with springs, parts to be moved from the outside with magnets, and leads sprouting out from all over the glass envelope, each lead seal to be made vacuum tight under his blowing. A laborious process followed the final seal-in and attachment of the creation to a glass pump station for evacuation and preparation. Long hours of baking in an oven, flashing of filaments, heating of parts under or within an induction coil circulating RF current, testing for short circuits, flashing "getters", and final sealoff from the pump station--the whole process frequently consuming days.

There was considerable hazard associated with the bake out. One wanted to bake his tube at a temperature as close to the softening point of the glass envelope as one dared in order to evolve as much gas from it as possible so as to obtain the best vacuum. It was not infrequent that the experimenter would open the oven following the bake to get along with the rest of the operation, only to find that he'd been a little too daring, that the glass was neatly collapsed and folded down around all the internal hardware. This sad and dismaying circumstance entailed the loss of many hours of work and much of the patience of Mr. Banta. There was at one time collected in the building a fine assortment of these catastrophes, the whole being nicely arranged on a large display board and known as the "Higinbotham Collection." Willy Higinbotham undoubtedly contributed one or two himself to the collection. But Mr. Banta was forbearing. I only recall his exploding once. A student fresh to graduate

experimental work, George Scott, Jr., friend and collaborator and he of the coonskin coat and rifle mentioned earlier, brought in a working drawing to the glassblower for a piece of glass for him to fabricate; I believe it was a semicircular mass tube. The drawing was neat and carefully dimensioned out to at least the thirty-second of an inch. Mr. Banta took one look. "Jesus Christ! This isn't a machine shop!" Tolerances were quickly widened. He was subsequently joined by a younger blower from GE, Lee Hinman. Hinman was probably a better craftsman but less daring. Still, he was not afraid to try things. One would go in with a proposal to him and be told it was impossible. A few days later Lee would have tried it and managed quite well. After Mr. Banta's retirement, Lee ran the shop until, in a conflict of artistic temperaments between him and a new glassblower, he left for sake of his own piece of mind. He died a few years later.

DeWitt Calkins was another matter. He had been a fixture in the department; my father had known him and alerted me to his presence. I made myself known to him on one of my first days at Cornell. The carpenter shop occupied the north side of the north wing, just east of the basement entrance. He was seated at the console end of the liquefier, which large machine was loudly cranking away, compressing and cooling the atmosphere. It was a cantankerous affair, and it seemed always to be problematical as to whether cold liquid would issue forth from it, or not. Anyway, on this day it was doing well, and the liquid was dropping steamingly down into the dewar. I managed, over the noise, to introduce myself and relate my connection to my father. He was reputed to be a bear, but I think his growl was worse than his bite; he never growled at me that I remember. Chemistry's Professor Laubengayer tells of the apprehension he felt early in his career whenever he approached DeWitt for some of his liquid air, of which a considerable quantity was required in some chemistry research. He found the man a tough person to handle. But he learned how to soften up the old curmudgeon; an occasional bottle of homemade wine brought in to him did the trick. From then on it was sweetness and light between the two.

Along about the time that DeWitt was to retire he brought into his operation a younger man, Sam Weibley. I believe Sam was his son-in-law; he had worked in a factory--Boole Furniture--over in Forest Home

somewhere. He took over the operation--liquid air, carpentry, and general building maintenance--after DeWitt left. Sam was all right, but my recollection is that he enjoyed conversation about as well as he did his work. That and horseshoes. During summer months in good weather, he and Lee Hinman would always be found during lunch hour out pitching horseshoes, either at stakes implanted in the court or out back behind the machine shop. They were both pretty good, rivaling in their sport as Morey and Parratt did in theirs. Others in the shops were usually out there as well, kibitzing and egging them on. Probably Sam's greatest contribution to the department was the hiring during World War II of his assistant and subsequent manager of the operation, Louis Festa. Louie had been a vegetable wholesaler in town who tired of the rat race he found things had come to in that discipline. It's not clear that it was any less of a rat race in the Physics Department, but at least it was always in a spirit of cooperation and of competition only with nature. No matter how harried he was, he would always assent to come help one out in a problem in the basement--or elsewhere. A nice spontaneous tribute to Louie from someone was posted on a bulletin board during the construction of Clark Hall. Underneath a large photograph of the tremendous construction mess entailed in getting the foundation and support pillars erected from the excavation, there was typed a caption that went that went something like this: "Hey, Louie, we need you upstairs; something's gone wrong with the lights." "Just a minute, son; I've got to straighten out a few things down here." That typified his spirit completely. He was a rotund individual, and his attitude and spirit went with the figure. He has long since retired. There was a big party for him; he had found even physics was getting too much for him. He died in October 1982; pall bearers were all old Physics Department friends. His successors John Smith and crew have carried on the industry and good nature of the entire activity.

Besides these support people, there were on the second floor a laboratory apparatus attendant and a lecture assistant. I had not much to do with either, and so little comes to mind. At this point I recall but two incidents. I believe it was in the late forties--way ahead of where I am supposed to be in the overall picture--but so be it. There was this laboratory assistant--Peter Strok by name, a Czechoslovakian if I am not mistaken. He was the faithful servant if there ever was one. He had

minimal education but once he had set up a demonstration, he could do it again on demand for ever after, even without having any idea what it was all about. He was a rather lighthearted guy who got moody and somewhat depressed toward the end. Did he take his own life? I think so. Anyway, he had knowledge of his subject and was happy to apply it. One sunny fall noontime a group of students was playing football out on the north side of the building when some energetic punter hefted the ball high in the air only to have it come down in the great oak tree that used to stand there. Branches of another fine oak spread out from across the road that then ran up between Baker and Rockefeller and curved around to Bailey Hall. They were both unfortunately sacrificed for Clark Hall. (A slice of the one nearest Rockefeller is the top of a coffee table on the seventh floor of Clark, sadly covered with a thick, translucent plastic coating.) Anyway this football was well up in the tree, secure from various attempts to get it down. Watching the game from the second floor, Peter knew just the thing. There was in the freshman laboratory an experiment on determining the velocity of a rifle bullet, an experiment now retired before someone's aim had gone haywire. The rifle was fired at a wood block forming the mass of a ballistic pendulum whose maximum excursion was then read off after stopping the bullet. Peter got out the explosive end of the apparatus, loaded it, and from his second floor perch took a couple of pot shots at the wayward object before observers below saw what he was up to and discouraged him. Fortunately, he missed and no one out yonder was reported as having received his flying lead.

Peter was also aware of some facets in electricity and magnetism. He had noticed in the laboratory how poverty stricken laboratory assistants reactivated their frequently magnetized watches. One day a rather more affluent sophomore complained about his fine watch no longer keeping proper time after having come under the influence of a magnet somewhere. Peter knew how to handle that. He and the student took the watch and placed it in between the poles of an electromagnet he had connected through a switch to the AC line. Peter confidently closed the switch, there was a buzz inside the watch case, and the little machine ground to a stop as line fuses blew. As the story goes, Peter and the by now worried student pried open the back of the watch and poured the works out on the table. Undoubtedly exaggerated and perhaps apocryphal,

but it fits the picture. Emery Meschter has confirmed the story. He has said that Peter reported later: "Watch, he say oooh, boy feel bad."

Some janitors were very much a part of the "family." There has a been a long history, from the earliest days, of janitors moving into department activities. Ed Maret was one in the postwar era. He had come to his job before the war and graduated to become a good lecture assistant. He sort of went off his rocker near the end and he left. Another one, most highly regarded of the Rockefeller custodial crew, was Joe Massicci. He was janitor to the end. He was a very distinguished appearing Italian, a fine accent in his voice, always moving in a rather slow and stately fashion, but keeping up with the manifold chores which confronted him in the old building. It was sad when he retired, but it is always an uplift to encounter him on the street which one still does on occasion. He used to usher at the Ithaca Theater, and what a fine figure he cut, out of the dungarees and rough clothing of his daytime job. Good old Joe.

I don't know which of the service people George Scott told about who had been absent from duty for a day or so. On returning to his Rockefeller routine he was asked what the problem had been. "Too much of the bigeyed chick," was the weak response. He had feasted on roast owl.

But what about the professors and teaching staff, the graduate students, the postdocs (then known as instructors and fellows) back in those days?

First of all, I suppose, there was Ernest Merritt. At the time I arrived, he had only a year or so earlier divested himself of the chairmanship, which Gibbs then took on. Merritt and his wife were two just lovely people, both very charming, of good humor, very gentle in manner and voice, both of the "old school," so to speak. He was somewhat small, pixieish, with sparkling eyes, and never seemed too harried. I sat in on a Monday noon course he gave during my second semester. In fact, I well recall the final lecture--his last lecture before retirement; in Lecture Room B it was (turned 90° from its present orientation), his good wife watching from the back row. It was largely a demonstration course of phenomena in gaseous discharges. It was fun to watch him at work. He'd set the experiment to going; the pump would take off, the vacuum develop, then the first gaseous glow followed by whatever it was that

was supposed to happen. He'd look up at his audience and beam, seemingly as astonished as the audience at what was taking place. So it was in his theory courses one is told; he'd arrive at a conclusion and be as surprised as the students at what had transpired. I met Merritt on my first day in Rockefeller Hall, down in his basement laboratory where he was working with his assistant, Don Morey. They had done some magneto-optics on a new element. I later heard of this element as Ekacesium; Papish in Chemistry, over in Baker, had been involved. It was to be the last of the alkali metals, #87. The full story I have heard only recently from Lyman Parratt as told to him by one of the principals, Don Morey himself. Let Parratt tell it:

"In the late 1920's the search was on to find the missing rare earth elements. Professor Fred Allison at the Alabama Polytechnic Institute, hot on the trail in this search, had developed a new method of identifying elements by a supposed characteristic delay time for each in the Faraday effect. In 1930 Allison reported the discovery of element #85, Ekaiodine, which he named Alabamine in honor of the State of his birth. In many laboratories in the world workers immediately undertook to test the viability of this new effect and to use it, e.g., in the search for #87. At Cornell, Prof. Papish, having appropriate samples of rare earth, joined the Allison followers. But neither Papish nor others could come up with any positive results. Papish turned to Prof. Merritt, a magnetics and optics expert, to take a look. Merritt assigned the task to his assistant, Don Morey. Results continued all negative. By this time (1931), Allison had reported his discovery by his method of element #87, Virginium, another crowning achievement; and the worldwide community of physics and chemistry was seemingly ready to accept it. But Morey, a patient and dogged young physicist, was gradually building up a case that might be used to contend that the magneto-optic effect in toto was a sort of psychological illusion. At a high meeting of the National Academy of Sciences (a meeting held, as I remember, in Washington, D.C.) Morey listened to a long paper by Allison on his magneto-optic effect, and during the ensuing discussion asked for a little time. Then, with prepared slides, data and all, he attempted to rebut the entire affair. All hell broke loose, scattering around the world. It took several years to subside, finally with the discreet removal of Virginium and Alabamine from the textbooks and from the prestigious Handbook of Chemistry and Physics in 1950, the 32nd Edition."

Papish had been egged into the problem by Professor Dennis who, as a great innovator, had introduced spectroscopy into chemical analysis in

this country, initiating first a course in the subject here at Cornell. Using such methods Papish at one point thought he had the evidence for the new element; plans were made to announce Cornellium, but wisdom and some negative results prevailed. Later, after Allison's discoveries, Papish visited Allison's laboratory to learn more of his procedures. It is said that (as R. W. Wood had done earlier to apparatus in Paris showing the existence of N-rays, in his surreptitiously removing the critical prism of the apparatus, yet nevertheless allowing their discoverer to still see his radiation) Papish removed on the sly an optical element from Allison's apparatus, which likewise did not prevent Allison from still observing the phenomenon indicating the presence of his new element. However, it was not until Morey's discussion that the elements began their demise. In the Handbook of Chemistry and Physics for 1949 (31st Edition) Alabamine and Virginium are still present as elements, but the editors are hedging their bets, asking the reader to refer to elements 85 and 87 in the small print addendum at the end of the listing. There astatine and francium are noted. In the next edition the two pretenders are finally missing.

The story has further local interest. It was in 1939 that astatine was synthesized through bombarding bismuth with alpha particles in the 60" Berkeley cyclotron by D. R. Corson, K. McKenzie, and E. Segre. And we know where Corson wound up. Virginium (alias ekacesium) has been superseded by Francium, a natural isotope discovered by Mlle. Perey (Curie Institute), also in 1939.

The first collaboration of Morey and Merritt was in three years of work on textile fibers, of all things. In 1916 a German submarine had run the British blockade to bring a load of valuable dyestuffs to this country. When we entered the war the proceeds were impounded, finally to be disposed of by Congress in 1931 in the creation of the "Textile Foundation," the purpose of which was to sponsor research for the benefit of the industry as a whole. A dozen recent graduates in physics, chemistry, biology, and engineering were to be selected for carrying it out. Morey tells how it was:

"It was my great good fortune and honor to be selected as one of these. It was a condition that a suitable university and faculty member serve as advisor to each project,

but neither the Congress nor the selection committee ever really defined what projects would be basic research for this old and stable industry. And of course for good reason. Cornell and Ernest Merritt agreed to 'stand in,' and I can remember Ernest and myself pondering just where to start! That was fifty years ago; today with the technological-explosion in the synthetic fiber field there is no question. Ernest finally said, "Well, Morey, I think my grandfather once owned a share in a woolen mill, and that is the extent of my connection with the textile industry. I shall be happy to read your reports. Good luck!"

As it turned out, I spent three pleasant years on certain optical and physical properties of cotton, silk, flax, wool, remie, and rayon. None of this appeared in the <u>Physical Review</u>, but some very creditable papers were published in textile journals, duly noted as coming from Cornell's Department of Physics."

The Merritts frequently entertained at their house on Grove Place, about where Phillips Hall now stands, The weekend after I had arrived in Ithaca and had extended greetings from my family, they had my sister and me in for Sunday dinner. Present also were old Professor E. L. Nichols and his granddaughter, Charlotte Throop, mentioned earlier. Nichols had lived in a house near where the Law School is now located; it was gone long before I got here. The old man took Sunday dinners with the Merritts after the death of his wife; to the Merritt kids he was "Uncle Ned."

Merritt was something of a motion picture enthusiast. By that I mean he took his own movies. His interest dated from the days when Ithaca was a world movie capital--well before Hollywood was even a name. Old studios were in what is now the pavilion down in Stewart Park. I am not certain that I ever saw the results of one particular afternoon's filming he did. It was in winter after a snow or freezing rain, which left roads in something less than favorable driving condition. He told LeRoy Barnes that he was going down to the foot of State Street to take movies of hapless and frustrated drivers sliding down the hill with new degrees of freedom they knew not, and of other optimists slithering around, spinning their wheels in an attempt to get up the hill. I do seem, however, to remember someone's hilarious film footage showing the antics of automobiles in such grievous circumstances. It must have been his film. It was in the days before streets were salted down at the slightest provocation. Cars lasted longer too in those days.

I do recall one film he delighted in showing at one time or another. It was taken from a boat following the Cornell crew in some regatta. the foreground was hanging a rope fixed to the vessel on which he was traveling, the shell off in the background, somewhat aligned with the rope. It was interesting to see the back and forth motion of the racing shell relative to the fiducial mark on his constant velocity platform. As the crew dug in on its stroke, the shell would slip rearward, the oars would be lifted and swung back for the next stroke, the shell surging sharply forward of the mark. Back and forth she slid. It was on the basis of that exposure to crew dynamics that I suggested an idea (which was never implemented) that each oarsman take his stroke in turn somewhat like the firing sequence of an eight-in-line Stutz. One could even have weak spark plugs under each seat to time the crew, somewhere up front there being the rotating distributor turned by the cox'n. It would still be of interest to spring that system during a regatta and to compare the performance of a crew using such a sequential stroking system with the standard allatonce technique.

Next in the department was Professor Gibbs, chairman. There was apparently real debate associated with his being named chairman. I don't know the details. Professor Floyd K. Richtmyer apparently also wanted the job, or at least did not want Gibbs to have it. The two men did not get along, according to reports. I don't recall ever seeing them together, but it must have happened now and then. It could not have been avoided entirely, if nothing else at least at the department Spring picnic; it was always Richtmyer's job to brew the coffee: a great pot on the fire, throw in the grounds, boil so long, and then settle. It was a vigorous brew. While I may never have seen them together, there are at least tow photographs of Rockefeller inhabitants of the early thirties arranged in a group on the steps of the building showing both men in the front row sitting congenially next to each other. Gibbs, as Chairman, presided at the weekly colloquium held in Lecture Room C. But Richtmyer never once attended that I recall.

Gibbs was pretty suave in carrying out his chore there. He would introduce the speaker, resume his seat in the front row and shortly thereafter go to sleep, or apparently so. Perhaps he was really with it throughout. He always came forth with a pertinent question, after the

speaker had finished, to start off a discussion and series of questions. After all, it is always embarrassing after a talk on which the speaker has worked hard that there not be a single question from the audience. Gibbs certainly spared our guests that plight.

Before Gibbs's tenure as chairman the situation was not greatly different. Don Morey fondly recalls:

"At the time for discussion (of the colloquium), of course no one wanted to ask a stupid question, nor to appear, in front of the assemblage, to be less than completely understanding. As a result, very few questions came from grad students, and questions from the faculty were often framed to exhibit knowledge. With one exception. There was a gentle, small statured man, with kindly smile, who would say that he did not understand, and would the speaker please re-explain a certain point. He was unafraid to expose his ignorance of some aspects, unconcerned about a professional reputation. When I first arrived in Ithaca and attended these gatherings, I had a sort of shocked feeling--how could the head of the Department not know these things and say so? As time went on, and as I grew wiser, I began to realize that this man who physically was smaller than any other Department member, was the giant of them all.

Ernest Merritt, I salute you and thank you for what you gave me."

A bit florid there, but no question about Morey's (and other's) high regard for Professor Merritt.

Over the years Gibbs was certainly not the only one to doze at colloquia. Carl Gartlein was another who could generally count on a quiet snooze during the late afternoon hour. Today, colloquium speakers are still no less effective in putting some "listeners" to sleep. Professor McDaniel is generally soon gone; Professor Bethe starts out with the gamest of intention, taking notes assiduously--but it doesn't always work; presently he is observed head down, pencil stilled. But the funny thing is that he can be counted on for the key question--somewhat like Gibbs. Here and there are others in similar state. The writer is reputed to mask his own napping by covering his face with his hand as if in profound thought; who knows but what that's the way it is. Most sensational is the deep slumber of a younger professor--head tilted way back, dangerously so, mouth wide agape, really far into it; at the final applause, the head snaps up in surprise, eyes blink, and he too joins in the approbation. All

this reminds one of the tale told of Professor Browne of Chemistry. He was also a deep colloquium sleeper, one who always, awakening at the conclusion, loudly applauded the oral dissertation. On one occasion, however, when lights went on after a few slides, he awoke from deep slumber in the middle of the guest lecturer's presentation and, out of the blue, started loudly to applaud the only half-finished performance. Embarrassing.

Gibbs was a nice man. Sort of bumbling, for all his countenance of der Herr Professor. In reality there was none of it. But he was a stickler for correctness. After one student--George Sabine--had written and rewritten his thesis two or three times, poor George reported at lunch one noon that Gibbs "now wants to go over it with a fine tooth comb." But Gibbs left his mark on the department. It was in his regime that Stanley Livingston came as instructor (1934) and started construction of the first cyclotron outside of Berkeley, the first of Cornell's "high energy" machines. It was a miniature; the vacuum chamber was about 18" across. It was all homemade; even the high power radio frequency oscillator tubes to drive the "dees" were homemade. Mounted in a copper box, they can still be seen in the mind's eye, glowing in their filament light, red sealing wax securing a somewhat questionable vacuum, continuously pumped, of Nevertheless, some very good work was done with it. One of course. Bethe's crucial stellar, nuclear reactions was checked; in particular, Holloway and Livingston observed and measured the constants of the important $N^{15} + H^1 \rightarrow C^{12} + He^4$ reaction. Charlie Baker developed with it the first arc source for cyclotrons, now standard practice. By turning the arc on and off, it thus became relatively easy to turn the high energy beam on and off, making possible in the deuterium bombardment of beryllium, a pulsed neutron source. Charlie's thesis on the first precision slow neutron energy measurements based on time of flight, relied on the pulsed arc source. Boyce McDaniel came and joined Baker and Bacher in a program to do an extended series of accurate time of flight measurements of slow neutrons with it, making more capital of Baker's source. In a very real sense, that machine was the progenitor of the succession of high energy machines Cornell has seen since that time. A bronze plague at the site where it stood now testifies to the fact. The "dees" of this early

accelerator are now part of lecture demonstration apparatus shown to students, not as demonstration but as link to past Cornell physics.

Scott recalls (Baker does not remember it) that Charlie was walking by the energized magnet one day with a wrench sticking up out of his back pocket when the magnet took it over. Sailing in to the field, the wrench took a couple of ports out, creating some havoc with the vacuum system. Livingston later remarked that he hoped he'd "get a good graduate student next year." Charlie started his cyclotron arc work during a summer when Livingston was absent and beyond knowing. When, that fall, Charlie contracted TB, spending a year out at Biggs (our old County Hospital on West Hill) in recovery, Holloway and Livingston took up the arc work and brought it to a successful conclusion.

The arc was a very significant development to the growing number of cyclotrons. Baker remembered the day Lee DuBridge came down from Rochester to be shown it, among other things. Holloway was puttering around and Charlie was tinkering with the arc pulser when he came in. DuBridge was suitably impressed with the microsecond rise and fall of the cyclotron beam, so crucial to the successful slow neutron time of flight He waited around then to see the steady state beam, measurements. usually one or two micro amperes in preceding technology. Immediately there was ten micro amperes. DuBridge's eyebrows went up. Then Holloway adjusted the beam deflector voltage and the meter went to the end of the scale, "pegging" at 30 micro amperes. DuBridge left mightily Wehn he was out of hearing, Holloway patted the magnet impressed. fondly. "Good old bastard," he said. Eventually, they got beams up to 200 micro amperes.

One non nuclear demonstration shown with the machine was striking. The accelerator vacuum chamber was out of the magnet; a silver dollar (from Nevada--I had a few, and they were real silver back then) was put in its place standing on edge. The magnet was turned on and the large coin given a nudge. Still seen in memory is the slow motion fall as it lay slowly over on its face, almost as if it were immersed in an invisible viscous fluid. There was other nuclear physics going on beyond that involving the cyclotron. Joe Hoffman--gentle, shy, quiet Joe--did his slow neutron work utilizing a radium beryllium source. He graduated from Cornell and became a graduate student a year after I started. He had

already had a summer's experience in cancer research at the Buffalo Institute. While he did his work with Livingston and Bacher, he was something of a protégé of Gibbs's, to whom he felt rather close. Except during the period he spent at Los Alamos (naturally enough), he spent his research career on cancer which, in the end, was itself the cause of his death. His family wondered whether a contributing factor had not been his exposure to and work with radioactive substances.

Gibbs was particularly fond of the honorary society of Phi Kappa Phi and was probably responsible for Joe's being elected to membership. Joe had earlier alluded at lunch one day to some other student as being "Phi Kappa Phi material" and was not particularly pleased after his own election when George Scott twitted him on his own "materialship" for the Society. Joe took things pretty seriously; he notes in his thesis biography membership in the honorary organization and that of Sigma Xi ("Partners in zealous research"). Physics graduate students more often got elected to the latter society which was founded at Cornell. The department took nominating people for election to Sigma Xi rather seriously, but today it is almost if not entirely ignored. Joe was a very sweet person.

It was in Gibbs' tenure that Hans Bethe came. Certainly others, Smith in particular, were influential in getting Bethe here, but it still took some leadership at the top to push through the appointment of this European, noted though he was. What a fortunate event in the history of the Cornell department that was.

Then comes Richtmyer. On the outside, he was the best known man in Cornell physics. Among his many activities, he was the stern and able editor of the <u>Review of Scientific Instruments</u>, but not foolproof editor; he was quite miffed when he found that an article by Jesse DuMond at Cal Tech had been published which described a vacuum tight valve as "absolutely in a class with Caesar's wife." Many years later, I was on the Board of Editors of his journal, succeeding Bobby Pohl. I was given a paper on a channeltron secondary emission multiplier to review and could have passed a perhaps more flagrant violation of scientific propriety. Accompanying the article were figures, including a photograph of the device in question; for scale, in from the top of the cut, hung a woman's breast. The paper concluded in acknowledging the help of a "Miss Jones" in preparation of the figures. I turned it down, not for reasons puritanical,

but rather because I found the <u>Review</u> to be no place for that sort of thing. And anyway, the scale was still pretty uncertain. In resubmitting the article, the figure was altered with the particular scale reference eliminated, the authors requesting the editor to submit it to some other reviewer. I must have written something pretty uncomplimentary.

But to continue with Richtmyer. He was dean of the Graduate School. He was in at the founding of the Optical Society and editor for a time of its journal. He had a book to his name--Introduction to Modern Physics: it has gone through six editions and been "modernized" by others three times. (It was from the first edition of the text that I first heard of the Bohr atom in a course at Nevada in atomic physics. I well recall the question from a skeptical chemistry professor auditing that course as to whether there was anything to this "wave stuff" they'd been reading about in <u>Science</u> relating to the electron. I didn't know what it was all about. By that time the Davison-Germer experiment was six years old!) He edited the McGrawHill International Series in Physics texts, of which his was the first, Bacher and Goudsmidt's soon to follow.

Richtmyer was a big man, rugged. I can see him striding off across a snow-covered Arts quadrangle, on a clear but very cold afternoon, to his office at the Graduate School over in Morrill Hall. Gray suit, black felt hat, and black leather gloves; no overcoat, no overboots. He was a formidable sight that afternoon, and it stands in my memory for some reason. He could take it though; he was raised in upstate New York. My mother and aunt recalled him from high school days, each morning catching the train between Cobleskill and Central Bridge; of rather poor circumstances they said.

But so also was Professor Gibbs of not well-to-do background. Professor Bethe has recounted his understanding of how Gibbs came into physics. As an undergraduate at Cornell, he peddled maple syrup from his parents' farm. At one time a Physics professor, probably Merritt, bought quite a few jugs of the sweet, golden liquid. So Gibbs got acquainted with Merritt. In his junior or senior year, Gibbs needed a five-hour course to finish off some requirement; the choice was between Greek and Physics. Merritt persuaded him to choose Physics, which must have been done well, for upon graduation, he then offered Gibbs an assistantship, which was

accepted. It is not known why Richtmyer went into science--probably just because it was interesting.

Speaking of winter sights, as I have above, there was another one of different category from my memory of Richtmyer, yet similar. A small digression at this point. There came to Cornell physics a student named Al Taylor, son of the noted Naval Research Laboratory researcher, A. H. Taylor, reputed to have discovered RADAR. (This is something of an exaggeration; in the late twenties, Taylor and Young had a radio interference method for detecting vessels. Breit and Tuve at the Carnegie Institution pioneered <u>pulsed</u> radio ranging in their measurements of ionospheric height. Based on that technique, various countries were developing radio detection and ranging during the thirties. But old Tesla antedated them all; he suggested back in 1900(!) that radio could be used for detection purposes. Merritt made a similar suggestion to the Navy after World War I.) Al was guite a guy, something of a problem to live with. I managed it all right one summer out at the observatory where I had taken him in as associate watchman. At Wesleyan University, from whence he came, he and his roommate lined the walls of their room thick with insulating rock wool to deaden sound. It worked all right. George Scott, who was a classmate of Al's, said one's voice in the room dropped like a rock. There was a substantial repair bill at the end of that school year for refurbishing the place. And at another time he had more trouble, but with another roommate. A line was drawn midway through the room and each kept to his own side. That was all fine and dandy, but the roommate could not tolerate Al's swearing. So, in accommodation, Al invented a code for use in the eventuality that swearing was called for. "Nine! Six! Three! Two and five!" He must have been a problem child all One day down at a mathematics class in White Hall, he was riaht. obviously not paying attention to a proof Professor Agnew was developing. So Agnew stopped mid-proof and asked Mr. Taylor by what reasoning the previous step could have been written down. Al replied succinctly: "Womberger's Theorem." That took Agnew aback, for he himself was not well up on his Womberger. Hesitantly Agnew inquired, "And what is Womberger's Theorem?" Without batting an eye Al replied, "It is immediately obvious that . . . " Which did not sit very well with Agnew. I don't know that AI even finished the course.

But back to the winter scene. On this particular snowy morning Al was headed down the slope in front of Rockefeller headed for his Math class, probably that with Agnew. He was dressed like a modern prophet. He had on a long overcoat reaching almost to his ankles and big boots with chicken wire strapped around to keep him from slipping on the icy slope. He also had on, buckled against the storm, a leather aviator's helmet and had his eyes covered with aviation type goggles, a heavy beard masking the rest of his face. In his hand, buried in a big furred glove, steadying his progress down the hill, he held a long wooden staff. To complete the outfit, he only needed a placard strapped to his back: "The end is now." It was a sight.

We were really discussing Richtmyer. He was the big X-ray man at Cornell. He had a large number of students and several "postdocs" working with him and two prestigious National Research Fellows, Parratt and Charlie Shaw, who later went to Ohio State. Of course, Parratt subsequently became Cornell's man in X-rays and later became department chairman. Out in back of the building, in the Annex, was Richtmyer's high voltage X-ray installation where Trevor Cuykendall and one Matthew Jones hung out. It was not a very inviting laboratory, ill lighted and dingy. I never cared about going out there to check it out and shut off the Rockefeller ventilation machinery on my last watchman round at night. There was good work turned out under Richtmyer but the precision took a major step forward when Parratt came from Chicago with his high resolution instrumentation for looking at fine structure near absorption edges, line shapes, satellites, and the like, work which has been taken up today in many places in a renaissance of X-ray spectroscopy which synchrotron radiation has done much to bring about.

Before Bethe, Kennard and Smith more or less took care of theoretical physics, although Lloyd also had a flair for experiment; most of his thesis students were in experiment. He taught us the Introduction to Theoretical Physics course mentioned earlier, and he built up a popular course on the Mathematical Methods of Physics, a course still extant, albeit now given by the Mathematics Department. Kennard taught us quantum mechanics in our second year, a course more or less straight out of Sommerfeld. It was pretty dry stuff as he did it, and it was not until the second semester when Bethe gave us Spectra and Radiation that the

subject came alive and exciting. The explanation of the periodic system of the elements, first learned in his course, is recalled vividly. In retrospect, one has the feeling that Kennard was not exactly comfortable with quantum mechanics. Not that the mathematics was beyond him but, rather, that the ideas were a bit suspect. He spent a year in Göttingen during 1926 when things were pretty exciting in this fast-breaking field. He wrote Gibbs a letter quoted by Kevles in his book <u>The Physicists</u>, in which he noted that "Theoretical physics has reached a terrible state; new methods have to be learned every week, almost."

Bethe arrived in January near the end of my first semester in the I don't recall first meeting him nor, indeed, his first department. colloquium. There was no doubt he had his head on his shoulders, a head crowned with a shock of black hair standing out almost as though he were on one end of a van der Graff generator. He fitted right in, joining students at our big round table in Home Ec. We less affluent individuals stared wide-eyed at his 65-cent lunches and still more expensive dinners. When he first came to Ithaca, he stayed with the Smiths out on Upland Road. After observing the scope of Hans's lunches, Lloyd is reported to have one day asked his own wife, Florence, why on earth she didn't feed him some breakfast in the morning before he came over to the building, not always so early in the first days. Breakfast? Why, that morning she had only given him three fried eggs and five pieces of toast. Clearly, the man was hungry. For all his "high" dining he wasn't wealthy, although he has said he felt very rich. Corson has told how, when he became chairman, looking over some old correspondence, he learned that Hans had been salaried at \$3000 per annum. But taxes were low back then and inflation was minor. (I remember Professor Howe remarking, however, at lunch one day during the Roosevelt first term, to illustrate the president's rampaging spending policies, that the national debt had reached in dollars a numerical value equal to c in cm/sec.) Hans did not resort to hiding olives and pickles in his soup, as did some other physicist diners so the cashier would not catch them at the tally register. Nor did he engage in a somewhat more open but perhaps less objectionable subterfuge. The cafeteria made good coffee and the second cup was free complete with cream, the first having only cost a nickel. The smart thing to do was to get a cup of soup served in coffee cups, consume it, rinse the cup out with

a bit of drinking water and add a touch of coffee from the cup of someone who had taken coffee. It was thus no problem to go back for a "second" and so have a single free cup with one's dinner. I've always felt a bit guilty over having engaged in that practice, but, after all, they did give one cup of coffee away free, and we weren't rich. Still... At another time we had a physics undergraduate student serving at the ice cream counter. He was particularly kind to us in dispensing his well rounded balls of ice cream and his various sauces. I regret some of our behavior over there--like leaving filled water glasses upside down on the table. It was a good place to eat; we abused it no doubt. The University is the poorer for its disappearance. Servings were not enormous, but the food was very good and the dining rooms attractive.

Anyway, Hans fitted in very well. We generally rounded up a crew to wander over for lunch or dinner by going down to the cyclotron room and pulling the rope of a boxing rounds bell which someone had acquired and nailed to the corridor wall. It could be heard pretty well over the whole building, and so a group was easily rounded up to go over and capture our round table. Bethe usually ignored the summons but would join us later, bringing in his sumptuous feast from the serving line. They were happy days. I, at least, was impressed that such a noted person could be so unassuming in joining such as ourselves in daily routine. The light in his office burned late into the night. But he mostly managed to meet his early morning classes, granted a bit disheveled, shoes untied, belt end dangling. And yet he was always available for questions, stupid or otherwise. Well, so were all the others. It was a comfortable place.

After his arrival Bethe essentially became Cornell's Mr. Quantum Mechanics, rather completely overshadowing Kennard. It must have been a little hard on Kennard, but it was not obvious at all to any students. In fact, it is said that he very strongly supported Bethe's coming to Cornell. They both always attended colloquia as did almost everyone else. Colloquium has always been preceded by a tea. In the early years of which I mostly write, these were in the library rooms. Faculty wives were generally there and frequently the small children of the younger ladies; and wives supplied the cookies. Tea was tea; a bit of concentrate poured first into one's cup from a pitcher followed by hot water drawn from a tall copper cylinder in a wood frame, a relic from some past experiment in

heat, whose not known. There must have been a fire under something somewhere there, but that doesn't come into focus. Maybe the water was made hot and simply poured into the copper reservoir; no matter. But there was none of this modern chromium, electric urn business. After a half hour of pleasantries, we all strolled down to Lecture Room C leaving the ladies to clean up. I don't know that they were properly appreciated by the students.

These weekly teas were one means by which new students became acquainted with faculty and their wives. Today, this comes about mainly at the Christmas party and spring picnic, but mostly it doesn't happen; we have grown too large. For staff and wives to get acquainted with new appointees, "postdocs," and their wives, and reacquainted with each other, there is on a Sunday afternoon in the fall, a tea held on the seventh floor of Clark Hall ("The Top of the Clark"). Tea is mostly not present; rather wines, hors d'oeuvres, cheeses, and crackers abound; fall decorations brighten the tables. A nice annual affair, even if it doesn't manage to get everyone acquainted with everyone else.

Besides the weekly colloquium, there were and are seminars. It seems there have always been seminars. In the days of Nichols' leadership "Seminary" was held on Tuesday evening down at Nichols' home on South Avenue, near the present Law School. For both staff and graduate students it was and is a means for getting them to the frontier. In my first student years, there were an Electron and Ion Physics seminar and an astronomy seminar, both of which I attended. There is a picture around the place, taken at the steps of the north entrance out by the machine shop, of the group regularly attending the former. Bethe generally came to it, as he did to that in astronomy--at least he attended one astronomical session I recall uncomfortably well. I was reviewing a paper by Strömgren or Lindblad, I believe, on galaxy formation! I made some statement, and Hans asked how that could be so. I clearly did not get the question but thought that what he asked was pretty obvious and said so. Bethe was kind and did Fortunately, Robley Williams came to my rescue, not respond. straightened me out on the question, and volunteered the answer. I think that was the last talk I ever gave on galaxy formation, and it was certainly the last time I ever told Hans that something was obvious about which he wondered. Live and learn.

In present times, besides the Monday colloquium (it used to end the week on Friday), there are a multitude of seminars--theoretical physics, theoretical solid state physics, solid state physics, the nuclear Journal Club, and others in related fields, astronomy, surface physics, and so on. They all meet in the afternoon, mostly after 4:30, still generally preceded by coffee or tea and cookies or doughnuts, the latter nowadays purchased by the consumer. During Bob Wilson's directorship at Newman Laboratory, the Journal Club was presided over with an iron hand by the director. Of course he had various people in charge of rustling up speakers in different years. One year it was in the charge of Vana Cocconi, but she didn't last in Wilson fired her for a great breach in the tradition of not the job. allowing the speaker to go beyond 6 o'clock. She had as speaker Buckminster Fuller, originator of the geodesic dome and other architectural geometries. He was so engrossed in his topic that Wilson could not close him off, and he went blithely on full tilt ahead. That was the end of Vanna, according to the up-until-now accepted story.

Recent research has disclosed disputation concerning this incident. It seems that it was John DeWire who got fired; he had invited Fuller at Morrison's suggestion, Fuller happening to be in the territory at the time. So he came. DeWire lugged a load of geodesic models up for demonstration, and a real interesting discourse was anticipated. Wilson recalls Fuller starting in at Nursery School on an oral autobiography and going on and on. He believes the man never got to high school days before the lecture was in some way terminated; the geodesic models were not even referred to in Wilson's recollection. It is not clear how Vana got mixed up in the tale. Anyway, Fuller had bested Wilson, and it was DeWire who lost his commission.

One hears a lot of colloquia and special talks over the course of forty years. Most of them are forgotten, but they serve to build up one's general knowledge and familiarity with the subject. As Don Morey has indicated, there were memorable experiences, seeing and hearing "the great and the near-great," making science "the richer for framing the rigorous content with the personal side." He does not remember a lecture by Sommerfeld, only "his stocky figure, and bristling large mustache and heavy accent. And there was A. H. Compton, H. A. Lorentz, E. C. Kemble, and F. Hund, and scores of others who make physics live for me." True enough.

For me, a few over the years still stand out. One recalls one of Debye's on light scattering as being a clue to molecular shapes. Beautifully clear in delivery at the time, they were puzzles later when trying to reconstruct what had been said. (He always had models. Asked by Corson at an interview on his 80th birthday how he felt about current abstract physical theories, he threw up his hands: useless unless one can model it in some way.) I recall the excitement on hearing, following his extended visit at Kodak in Rochester, Sir Neville Mott explaining the photographic process. The ideas still hold pretty well. There was the colloquium of Professor Don Griffin on bats and their radar locating apparatus, during which talk he demonstrated on an oscilloscope the inaudible pulses of sound, and released his subjects to send them fluttering over the heads of the audience and out into the halls of Rockefeller. Presumably their sensors were operational; no one had hair entangled by one of his little creatures. One recalls Shockley and his transistor, demonstrating an oscillator powered by a dime, penny, and blotting paper soaked in a little saliva; that of Charlie Townes on his maser; one in which Wigner got stuck and had to be helped through by Bethe; a like situation with John Pierce discussing his traveling wave-electron interactions, in which he couldn't even get his own first equation out on the board. "Oh dear," I can still hear him moaning. He got a bit high after the affair before his dinner. There had been no help for him.

Today, Wigner and Pierce would have no trouble. Instead of working out their equations on the blackboard, they would have them all written out, in hand of one's own characteristic legibility, on transparencies laid on a viewgraph table to be projected up onto the screen. Some speakers go so far as to almost read their lecture from the viewgraph script. But at least they don't get stuck anymore. And the old, 3-1/4" x 4-1/4" glass lantern slide seems to have "gone West," replaced by the viewgraph or frequently by a 35mm automatic slide projector. Still, there was some advantage to manual projection; as projectionist, one was not very likely to go to sleep, he kept his eyes on the ball.

One colloquium well remembered by many of the older members of the department was one I arranged when I served as chairman of the Colloquium Committee. It was on teaching, something to which I felt some of us in physics could stand a little exposure. It was given by a

professor in the Education Department here on the campus. But his ideas and approach were a bit far off the mark for most of us. During the question period it is recalled that, to prove a point he was making ("If the learner hasn't learned, the teacher hasn't taught.") he asked the audience what was the height of Mt. Rainier. A Japanese member of the audience thought he was asking the height of a mountaineer, so he responded. "Six feet," he called out. "There you are," triumphantly cried the professor, "that just proves my point!" The audience howled. That was not a very successful session, but it is remembered, if only for that brief interchange.

A final anecdote involving colloquia, not here, concerns I. I. Rabi in a visit to Los Alamos, where he was to deliver an annual J. R. Oppenheimer Memorial public lecture. He had a full house in the high school auditorium and was introduced as I recall it by the then laboratory director, Harold Agnew. Or was it Bradbury? Anyway, Rabi started in on his memorial address, got through two or three pages of text, which was getting increasingly technical as he went along. Suddenly he stopped, looked at a few pages of text ahead and exclaimed, "My God. This is the wrong speech." He had been reading that which he was to give at the <u>laboratory</u> colloquium the next morning. There was scurrying about and some delay while the proper speech was located and delivered to him for his presentation. At the next morning's colloquium all went well, even if the first five or so minutes had been heard previously.

Mention above of Shockley and the transistor may permit a digression related to physics but also out of time and only peripherally related to departmental "history." Short of the discovery of metals by some "solid stater" in the dim past, there has probably been no other development in solid state physics as far reaching in its impact as this small device. Solid state physicists everywhere were excited by its discovery. Prior to the announcement, I had been told by Bell Labs friends to watch for announcement of an important development that had been made down there. And in due course, announcement was made of the solid state amplifier; the laboratories had a news conference by way of announcement. The New York Herald Tribune the next morning gave it good coverage, starting on the front page, if memory serves. But my paper, the great New York Times, had only a small two or three-inch item buried back

on the entertainment (!) page about a new amplifier. Some months later, the Physics Department received from Cornellian Ralph Bown at Bell, in a nice little case, two point contact transistors which are still in my box of assorted junk somewhere; they should wind up in the museum.

It cannot be said that the development, while exciting, was dumbfounding to solid state people. It had seemed reasonable to many that some day an alternative to the vacuum tube would be forthcoming. I recall one noontime during the war casually discussing the likelihood with Shockley. We were en route with others to lunch in the Village, where the Labs were located before the move to Murray Hill. It seemed possible that some sort of solid state amplifier might be the way. Solid state rectification was known. There was the copper oxide cell. There was the point contact on galena; many budding scientists in the days of my youth built radio receivers for a local station, using a galena crystal and a "cat whisker" contact to rectify for earphones radio signals built up in a resonant circuit. It always took some fishing around to find a sensitive spot on the crystal. Of late, Ohl at Holmdel had made significant progress in the genre, rectifying microwaves with silicon and germanium rather than galena. If we could in this way rectify, would we not some day take the next step, control it some way, and amplify?

After the war, there was a paper given by one of Lark-Horowitz's group at Purdue at an electronics conference run by Nottingham at MIT. The paper was on some very interesting effects around a point contact on germanium, as investigated by a probe in the vicinity. At the end of the paper there were some very pointed and extensive questions put to the speaker by Shockley. He told some of us a few years later in a visit to Cornell that he and his colleagues had been very concerned that they had been beaten to the amplifier after all; it had been known to them at the time that the point contact transistor worked. Purdue would surely have come to it.

Laubengayer, in Baker Laboratory, was much involved with germanium back in the twenties. My lump of the glassy oxide on which I took Raman spectra, mentioned earlier, had come from Chemistry. He had produced flat ingots of the semimetal having pretty high purity for those pre transistor days. Shockley, in a colloquium (not one I heard) given to the Chemistry Department, noted the contribution that had been made

here, a key factor in which had been the arsenic impurity which it had not then been possible adequately to eliminate. Anticipating measurements to be made years later on refined and controlled germanium, Bidwell in the Physics Department had made resistivity and thermoelectric measurements on this early material of Laubengayer's in work published in 1922.

Twenty-five years after the transistor invention, the American Physical Society held a commemorative session during the winter New York meetings. All the big-wigs were there to take part: my old boss Jim Fisk (by then the Labs president), Shockley, Bardeen, Brattain, and others. It was a good session. In presiding, Fisk alluded to the fact that here had been a development that at least had not added to our pollution. Afterwards, I saw Brattain and he commented that they had not counted on the acoustic pollution!

So much for transistors.

There was one other development in physics which I casually brushed before it came to reality. This was in nuclear physics. In the days when the department's undergraduate advanced laboratory course ran a student seminar (not very successfully), there was in one session a student talk on particle detection. The question of the drop formation in a cloud chamber came up. It was not clear why the drops formed. But no one seemed surprised that bubbles formed in a carbonated beverage. I pointed out the similarity, that in both cases there had to be a center for nucleation--in the cloud chamber, charged particles. I hazarded the guess that if one put an alpha source in a glass of ginger ale, he would probably generate bubbles. The next week, an EP student, Herb Spirer, perhaps realizing the implications in the suggestion better than myself, reported that he had tried it and didn't see anything. Too bad; he might have been Nobel Laureate Don Glazer.

We have perhaps wandered too far from the department and its professors--back to cases. One must mention Harley Howe, Guy Grantham, and Jacob R. Collins. Howe and Grantham handled the large service courses. Each had under his wing numerous graduate assistants. During his tenure, Howe, a wizened little man, had a lot of back trouble, and his wife was rather sickly. After retirement, before his death, he became quite spry, and she also seemed to take on new life. Howe had a keen nose.

He was pretty quick to detect the scent of frying potatoes and hamburger in a research room in the basement when a new graduate student, Fred von der Lage, with whom I was to live for a year or so when we both left and went to New York, set about preparing his dinner. Howe was definitely not in favor of setting up dining facilities downstairs, but I guess there was nothing he could say about the night watchmen's efforts upstairs. Howe was apparently a very good lecturer. I believe I never heard him and so rely for the opinion on what Don Morey says:

"I remember Lecture Room A, the large room with the rising tiers of seats, particularly for the meticulously prepared lecture demonstrations given by Prof. Harley Howe. These demonstrations were a delight to watch, and were so well coordinated with Harley's words. Harley Howe gave the introductory physics course; several graduate assistants, including myself, ran the lab sessions, but it was he who gave the demonstration lecture before the entire class. I sat in on these because it was such a pleasure to see and hear the clarity of exposition. (I am told it was also requisite; to run the labs one had to know what had been in the lecture. p.h.) There was a sort of academic pecking order in which the professors who ran large research programs with several graduate students working on doctoral theses under their direction assumed the top of the order, with the lower order falling to the underclass teaching. But in my book, Harley Howe was as good as the others. Guy Grantham filled the same role for the Sophomore Engineering class."

Emery Meschter who also taught with Howe echoes all this, adding, ("---in my book, one of the world's finest and kindest gentlemen." p.h.)

Grantham was also a great demonstration lecturer. No one who has seen his most famous demonstration, on the conservation of energy, will ever forget it. He stood with his back to the wall, displaced a considerable number of feet laterally behind the point of suspension of a long, heavy pendulum anchored to a beam up near the high roof. He would place the large spherical mass against his nose and let her go. The ball would swing far out over students in the front rows and then come sailing back at poor Grantham, coming to a stop neatly just at the tip of his nose. One has to have the faith for that sort of thing. He used to tell a couple of amusing tales of his war teaching experience. During World War II, there were crash programs here and there to educate our fighting men. There

was a Navy program here; V-12 was it? Anyway, Grantham was meeting a fresh class and sought to see what mathematics they knew. He wrote down a equation on the board like $x^2 + 3x + 2 = 0$ and asked the assembled warriors if they could handle it. There was a big burly marine in the back row who raised his hand. "You mean, Professor, if you take those three things and add them, they come to zero?" Patiently, Grantham said that was true, we'd like to solve it. "Why bother?" the big man asked. There was another marine at another time, or was it a footballer? He was majoring in Dance and minoring in Rhythm.

There is the amusing story of the poor freshman who was assigned to a Physics 112 recitation section and came to Grantham two weeks later with a tale of woe. Besides having the oldest TA he had ever seen, the student reported that the material being covered was simply gibberish; the TA was not preparing his stuff, he hadn't even mentioned F = ma yet, and the tie-in to the lecture material was not at all obvious. Grantham was somewhat perplexed but told him to hang in there for a bit longer to see if things did not turn around and get better with his own development from the lectures. The student did that but came back a week or so later and informed Grantham that, if anything, things had gotten worse. So Grantham decided to look into it. He checked with the student's assigned TA, who did not seem to be all that ancient and who informed Grantham that the student had not showed up in recitation at all; what was he talking about? It turned out that there had been a last minute room assignment change for the recitation section, the student had missed that information and had been attending Bethe's advanced quantum mechanics class for a month!

Grantham once ran an interesting, if brief, experiment. For a prelim he allowed students to bring any information they wished to use, provided it was contained on a single 3"x5" card. The results were a disaster. Students had seemingly tried to cram the whole textbook onto their cards. They spent the hour hunting over the cards for the right formula, twisting them around, turning them over, squinting at their fine print, largely to no avail. In the spirit of this experiment, at a later time and during a dressing-up of some Rockefeller recitation rooms when tile flooring was laid down over the old wood surfaces, Al Johnson, then administrative

assistant, had in one room the tile design inlaid at the entrance to the room so it read, in aid to forgetful freshmen, "F = ma".

"Jake" Collins was a versatile man specializing in heat and optics, researching in the infrared. He taught in the Advanced Laboratory course at the end of the war, in fact, he ran it. Parratt and I were underlings. He died quite suddenly the next year, and I was asked to take over his optics course on rather short notice. I had to learn thick lens optics in a bit of a hurry.

Theorist Kennard also taught in a sophomore laboratory course, at least he did one semester. He makes no note of the following incident in his own personal lab manual recently happened on in Tomboulian's sixteenlarge-box file in the Archives. I taught in the same section with Kennard. I had decided it would be worthwhile to try some teaching and so volunteered to take a section of laboratory. By then I was normally holding a research assistantship under Smith, working on his linear accelerator. On this particular day, Kennard was having great difficulty with a galvanometer a student was trying to make perform. It would never sit still for him. Kennard would reach up to adjust something, and the indication would go flying off scale. He asked me if I knew what was wrong. I watched him a bit and then asked to try my hand. No problem, no problem at all; it behaved just fine. I then suggested he go home and change his wool pants. Sliding around in them as he was, on a varnished stool seat, he was charging himself up to many thousands of volts and the poor instrument simply couldn't take it. Reminds one of the time a new graduate student found that a galvanometer he was working with behaved much better if he grounded one side of it, but that it was really stable, remarkably so, if he also grounded the other side; except that then it had no sensitivity. We learn.

Kennard somehow carried with him the aura of the typical "absentminded professor." Lauritson S. Taylor, who worked for Kennard a few years before leaving for the Bureau of Standards without finishing a Ph.D., tells how the professor arrived late one morning out of breath and very red of face. He habitually brought his young son and his own brief case up the hill in a push cart, depositing the boy at a kindergarten near the laboratory and taking the briefcase on over for his own day's activities. This particular morning, he apparently arrived at the kindergarten and

found the boy had been left home and an extra briefcase taken aboard instead. It all had necessitated another trip down the hill for the youngster.

Taylor tells how Kennard hired him as assistant on an experimental project on a Heckscher research grant; something to do with checking Kennard's theory on fluorescence excitation and emission characteristics. The fluorescence was very feeble, necessitating long periods of dark adaptation to make visual comparison at specific wavelengths with a standard source. They were thus led into photoelectric detection, then at a pretty primitive level, and on to sensitive current detection. This in turn led to a long period of development and construction of a quadrant electrometer after the design of Compton. Compton himself was helpful in this during a visit he made to Cornell. The Compton electrometer that was in use in 1946 in the Advanced Laboratory is thought to be that particular instrument and is the one now consigned to the museum. After spending two and a half years getting ready for their experiment, Taylor and Kennard worked feverishly for three days, after which something happened to the electrometer so that it lost its sensitivity. The Compton electrometer is a very temperamental gadget; no doubt of that. They are, of course, no longer used. Some years after the work (and a paper did come out of the three days' effort), Richtmyer asked Kennard what had been the principal thing learned from it. Kennard's reply was immediate. According to Taylor, he said it was to "never attempt another piece of experimental research."

Taylor, incidentally, was an early occupant of the third floor night watchman's quarters, which he describes as a little more sumptuous than has been done above. He was no less grateful for the privilege than I was. When he got married, he was invited to move his bride into the quarters. But he thought better of that and instead moved out into an apartment house run by two elderly sisters of parsimonious habit. Lamp bulbs were limited to 15 watts, hot water was in short supply, toilet paper was rationed. He managed to get around such strictures, but it might be imagined that there were times when he wondered whether he should not have thought still better of his move out. Taylor went to the Bureau of Standards without finishing the Ph.D. to become an outstanding radiologist, many of our radiation standards coming from his work.

There was another professor who should have been mentioned earlier: Carleton C. Murdock, C²M, as he was always known. Ramrod straight, long chin, rather long sloping nose, lean of frame and face, he was the teacher of electricity and magnetism, also teaching in the Advanced Laboratory. He researched in X-rays but with crystal structure his goal. He had married a student of his, Dorothy. A nice couple. He came to be dean of the faculty following the war but carried out some research even after retirement and his wife's death. He was studying the magnetized ellipsoid, something about a matter some standards group of the Physical Society wanted cleared up. But I think it never got finished.

Murdock had a hobby which seems somewhat out of character. He came from over near Cooperstown and had a cottage on Otsego Lake, so it was not unnatural that he should enjoy boating. But it does seem unnatural that it should be speed boating. He had an interest in propellers, how did the pitch, the shape, the rpm, etc., affect the mph of his craft, the turbulence, wake, and what not. Nothing was ever published from this interest so far as is known. He had, over two seasons, canoed the length of the Susquehanna River, from Otsego Lake clear down to the Chesapeake Bay.

A few years after Murdock's death, long after retirement and some years after Dorothy was gone, Professor Peter Carruthers brought into my office some books and a box of correspondence that he and his wife had picked up downtown in the Challenge Industries store run by the handicapped. In the box was correspondence of Murdock regarding the family genealogy, another of his interests. The two books contained notes relating to his research. The material, including a nice photographic portrait of a son, and a Christmas card ("Mother from Carleton") was apparently sent to Challenge when the family home on Wait Avenue (first from the corner at Thurston) was cleaned out after Murdock was gone.

There was another professor around the place who was something of a ghost, Joe Trevor. I don't know if he took part in department activities or not. One knew of his presence through his odoriferous cigars. His tiny office at the head of the stairs, opposite Lecture Room A, reeked. One surely knew where one was during a watchman's round, when he poked his head in that cubbyhole. Trevor was in thermodynamics; he had been in Chemistry before he came over to Physics. Apparently he was

independently wealthy at one time and gave his service. A letter in the Archives from the chairman to the president suggests some remuneration might be appropriate after various financial reverses had been suffered. I hope it was offered. He kept very much to himself in my recollection. Saturday evenings, however, he would wander over to Rockefeller to play string quartets with mathematician Wally Hurwitz and others. Only vaguely do I recall hearing them from elsewhere in the building.

Curiously, this almost painfully shy man was associated over in Chemistry with another independently wealthy person, Chemistry Professor Wilder Bancroft, anything but shy, he of the crimson robe of Morey's memory. Bancroft came from a famous Boston family, one of his forebears having been our first Secretary of the Navy. The professor was referred to as a "millionaire"--something in those days. He never collected his salary, was the first hereabout to own his own fancy automobile--which he never drove himself. A good thing apparently, for he broke everything he touched. With Trevor, he founded the now prestigious Journal of Physical Chemistry, and he kept it solvent with his wealth until it was taken over by the Chemical Society. Trevor, while in at the inception and early period, eventually dropped out leaving it to Trevor was a classical thermodynamicist; in contrast, Bancroft Bancroft. had little use for formal thermodynamics, and his Journal was a medium by which he could allow the subject to be disparaged by unfriendly and biased writers. He favored the descriptive mode and heartily supported the Gibbs's Phase Rule in that format. The differences between Bancroft and Trevor probably led the latter to seek his base, guietly, in the Physics Department.

He had a wife, a small wiry woman, who was a bridge addict. Withal her addiction, Mrs. Trevor was very pleasant, if persistent. Meschter, who lived in the house, remembers that she forever had problem bridge hands laid out on the living room tables, which he and other roomers in the house were encouraged to help solve on their way in or out. She would come around Rockefeller Hall evenings in search of a fourth for bridge--or even a third and fourth, not always unsuccessfully. Since they lived across the road on the north side of the building, under one of those great oaks, in one of a group of houses on what was known as the Circle--they all disappeared with the building of Newman Laboratory and later Clark Hall--

it was very convenient for her, and not too inconvenient for whomever she could find--if they liked bridge. I was glad not to know the game; came in quite handy during the period when I lived on the third floor. Another lady who called on residents of Rockefeller Hall was "Daisy" Farrand, wife of the president, living two doors to the south. She was a somewhat flossy lady, with not the conservative, reserved nature of her husband. Things frequently went awry in the old mansion, and it was rather more convenient and quicker to get a physics graduate student on the job than to wait for someone from B&P to come over to do it. It was never anything very complicated.

Mention of Mrs. Trevor brings to mind a few dog stories which might as well be interjected at this point. It must have been before Mr. and Mrs. Trevor lived on the Circle, but at any rate, somewhere they had as neighbor, Professor Browne of Chemistry, the way I understand it. He worked over in Baker near the Circle (doing some spectacular and widely known demonstrations in his general chemistry lectures) but he never resided over there. Anyway, wherever, one night, Mrs. Trevor was kept awake by the persistent barking of a dog, so she very firmly and irately brought the professor to task. At three in the morning, she called him: "Professor Browne, this is Mrs. Trevor. I wish you would stop that dog of yours from barking. He's been keeping me awake a long time." Browne: "Thank you, Mrs. Trevor, and I'm sorry. I'll see if I can take care of him." The next night, as the story goes, Mrs. Trevor's phone rang at three in the morning: "Mrs. Trevor; Professor Browne speaking. We don't have a dog." This Trevor story is to be presumed true until found to be otherwise.

The next dog story is true for sure; the scene was Rockefeller Hall. After the war, one night late, while working alone up in the third floor shop of the Advanced Laboratory, I was suddenly startled out of my wits when in the silence there came this awful, long howl, as of a dog, seemingly about eight feet from me, on the attic floor above. Nonplused, I could only wonder what in the world that was. Shortly later, it came again. And then again. There was no doubt it was real, and it sounded to be like nothing else than a dog in some trouble and up in the attic. So I took a flashlight and went up to investigate. I whistled, I called, I probed. Nothing was to be seen or heard. So I gave up. A few nights later, I was over again in another part of the laboratory and, again, this time from a

greater distance, I heard two or three howls as from the Hound of the I found one of the student watchmen and asked if he or his Baskervilles. partner had heard anything, and could we go up and look around together. Neither had heard a thing. But, sure, we'd look; he and I went up in the attic to search for an animal. No sign. Things were quiet as a tomb. I had probably been imagining things or it was from the outside. So it was; I began to doubt my sanity. However, the next day, the watchman told me that he and his roommate there on the third floor had been awakened about four in the morning by the howling of some animal, location inside or out not established. By then I was certain of my senses and went up once more for a thorough search. We never did find that dog. Some weeks later, a fire inspector came over to make a cursory check of things in the building. He wondered what was wrong with the sense of smell of the people in Rockefeller Hall. There was obviously a dead animal up in the He located it, in fact, back about where I had first searched but attic. between the main load-bearing wall and a false wall out from it, defining a narrow hidden space. It was possible for an animal to get into it through a small entry at some remove from where he was found. What was he up to? Was he tired of life and wanted to escape for a while? Or forever? I've often wondered.

It has always seemed appropriate to me that there be dogs around a university, a friendly place devoted to young people, a place of learning, and a place where a dog can meet other dogs. It's all fine so long as they behave themselves. Bishop's well traveled, streetcar-riding, bulldog "Napoleon" was one such. But there are always exceptions. There was in a more recent day a noted three-legged Alaskan husky, "Tripod" by name, who was banished to the North country for taking a nip at one or two persons on campus and, finally, for killing a cat. And there was the pair at the Bailey Hall speech of the former British Prime Minister, Clement Atlee. The place was packed to hear the statesman. Two dogs in the audience spotted each other from opposite ends of an aisle in the orchestra section of the hall, alongside of which sat Professors DeWire and Hartman. The dogs approached each other cautiously, met, circled each other growling and then went at it right at Professor DeWire's elbow during Governor Harriman's introduction of the guest of the evening. Fortunately, there were those who knew how to handle such a situation,

and they rushed in to separate the contenders and put them out on the street where there was plenty of room. But in general, dogs are good to have around.

But times change. In many buildings on campus these days, dogs have become personae non gratae. When the signs "No Pets" (and "No Bikes") first appeared on the doors of Clark Hall, there was some grumbling from inhabitants that Director Ashcroft had no appreciation of man's best friend. But we acquiesced in the main.

We were discussing Mrs. Trevor when this diversion began. So, let us return to the main thread. Besides the professors, and I hope I've covered them, there were assistant professors and instructors in the department. When I arrived on the scene there were Instructor Stanley Livingston and Assistant Professor Lloyd Smith. Bethe arrived as acting assistant professor four or five months later, and Bob Bacher came as instructor in the fall of 1935, eight months after Hans, Willoughby Cady Livingston thus came to Cornell before I did, his small after that. cyclotron was well started by the time I got here, and several graduate students got involved: Charlie Baker, Marshall Holloway, Gene Crittenden, and Ben Moore, and an undergraduate, Frank Genovese. Livingston worked hard and closely with them to get the thing going. And they did so within a year and a half, I believe. It had its ups and downs but was mainly operational for a long time. Improvements were made over the years, of course, so it became quite reliable. I suspect it was the smallest truly research cyclotron ever constructed. (That's almost correct; Illinois, under Cornellian G. Kruger, had one modeled after it.) Bethe made the claim for it that "per ton of iron" it had done more research than had been done with any other cyclotron.

Crittenden and Moore a few years later had a sailboat between them. The only time I went sailing with them we got becalmed and had to paddle home. That must have happened frequently for they finally bought a small outboard motor to help them make port in such strait. But that didn't last. They were out one day with a group when the wind died. So it was to gasoline propulsion. I believe it was Crittenden who gave a sharp pull on the starter rope; the motor roared into action and jumped off the back end of the craft and sank sputtering into the deep, not having been clamped into position. But Crittenden had his wits about him. Before they drifted

off, he leaped over to where the anchor lay with rope nicely coiled in neat, nautical manner. He threw over the anchor, the rope whistled out, the coil unwinding splendidly as may be imagined, and then the end of the rope itself disappeared over the edge; two things not tied down that day. There is an anchor out there somewhere for anyone who wishes to look for it. Not to mention a motor. But the story doesn't end there. I am told that after taking sights of landmarks on shore, they got another boat to drag for the motor. Heading out, down the inlet, they lost the propeller of that boat. So it was back to get diving goggles to search the inlet bottom for the propeller. In the diving operations they lost one of the goggle lenses. The propeller was ultimately recovered, but the rest, no. A less than satisfactory expedition.

Besides the construction of the cyclotron here, Livingston collaborated with Bethe in the last of an important <u>Review of Modern Physics</u> series on nuclear physics, still widely referred to. Bethe was joined by Bacher in the first of the three-part work and Bethe did Part Two by himself. Bacher came to Cornell as a spectroscopist but with the expectation of getting into nuclear physics. He started his work here in spectroscopy down in that end of the basement with Tomboulian, work on hyper-fine structure involving the nucleus-electron interactions. It wasn't long, however, before he involved himself in the work of the cyclotron, in large measure deciding what experimental work should be done, all probably to the discomfort of Livingston. After Livingston left for MIT to build a large machine there, the cyclotron was all Bacher's.

Smith also had interest in accelerators. He envisaged a low energy device of high current capability. He obtained a grant and started work on a linear accelerator in the space on the first floor just above the cyclotron. It was this project with which I got associated after admission to Ph.D. candidacy. It was friendly competition with the cyclotron, but they clearly won. The linear accelerator never was a real success. After a publicity release on the accelerator in the New York Times (with picture), Smith received an inquiry from Coty (a cosmetics outfit) about whether it could be used to pulverize powders to a finer consistency, leading to women of even greater beauty than we knew. And it did go into posterity, I am told, with a photograph I took appearing somewhere in the 1951 Britannica. I have never seen it. Smith had real

ability at theory and analysis but also knew experimental physics. He was quite a musician, being more than competent on the piano and slide trombone. He and his wife, Florence, cut a beautiful figure on the ballroom floor. And he had something of an eye for the ladies too, exuding charm and a boisterous good humor.

As instructors, there were also R. W. Shaw, my first research supervisor, and LeRoy Barnes. They both ultimately became full professors, Shaw in Astronomy and Barnes in Physics and later, until retirement, in charge of the Cornell premedical program. Barnes had done his thesis work in mass spectroscopy--ion emission from hot filaments-under Smith. Shaw had worked in spectroscopy under Gibbs and had taken part in an expedition to Arizona to check the efficiency of aluminized coatings for astronomical mirrors.

There is some contention between Cornell and Johns Hopkins as to which place first developed the evaporated aluminum metal film for astronomical instruments. Cornell says that it was Robley Williams, and Hopkins favors their son, John Strong. Anyway, to learn the advantages of aluminum, Cornell, under Professor Boothroyd, organized an expedition to Arizona's San Francisco peaks to photograph at good altitude some stellar spectra with the new coatings. On the expedition were Boothroyd and Shaw, and students George Sabine and Robley Williams, and outsider George Ketchum. Shaw was pretty much in charge of instrumentation. Boothroyd was in charge of organization, including food. The others largely functioned as Sherpas. Members of the expedition long remembered the constraints on their food. Boothroyd, a very kind and gentle soul, was something of a nut on natural foods and he provided them very well with such, not precisely to their satisfaction. Suffice it to say, they stayed healthy. They even got some good spectra, and aluminum came up to be superior to silver in many respects.

In an application of metals evaporation to biology, in which field Williams became noted, he and Crane at Michigan invented the shadowcasting technique for looking at biological (and other) specimens in electron microscopy. The bacterium is placed on a very thin film of celluloid which is evaporated over at large incident angle with metal of high density. The bug casts his shadow, which the electron beam subsequently images in the microscope. It is a universal technique today.

I recall a striking experiment of Williams in this connection. He had evaporated chromium onto a clean glass plate and had overcastted it with an evaporated film of copper. He had then soldered a copper wire to the copper film. The glass plate was supported over the open jaws of a vise in our aforementioned student shop, the free end of the wire below then being loaded with weights. The soldered end of the wire never did pull away; rather a chip of glass was pulled out, perhaps not too surprising, what with glass being rather poor in tension. But there was no question about the adherence of such films.

After some contradictory reports on the evaporation process from Science Newsletter, Merritt wrote Science Service to do better, to get firsthand information. He told the service that the evaporation method of mirror making was an old process. He had been told that Edison had even taken a patent on some variant of it. Merritt then went on to explain how Cornell people began in 1930 to look at silver and aluminum for spectroscopic purposes, learning of their use in the Reichenstalt. It was absurd to claim that Williams and Sabine or that Strong should get the credit for the procedure, and it was unfair to put men in that awkward position. He did say that he believed we were the first to point out and use the advantage in astronomy, but Strong's coating of a 36" mirror "gives him the size record."

Williams' great work, however, was in spectroscopy under Gibbs. He worked in the south Rockefeller basement on the fine structure of the hydrogen, Balmer alpha, spectral line and showed clearly, for the first time, the third component--from a deuterium discharge cooled in liquid nitrogen. From a careful measurement of the component line positions, he deduced that something was possibly not quite right with the theoretical Later. Pasternack at Cal Tech. surmised that the predictions. measurements could be reconciled by a slight shift in one of the presumed equal energy lower state levels, not, however, in accord with Dirac theory. It was not until after the war that the surmise was brilliantly shown correct in the work of Lamb and Retherford, for which Willis Lamb received the Nobel prize. And it was immediately after the Shelter Island Theoretical Physics conference where Lamb reported his confirmation, that Bethe gave the first semiguantitative explanation for the level shift. Today's guantum electrodynamics calculates the shift and other like
phenomena out to many decimal places. It is most fitting that an experiment on hydrogen and deuterium fine structure is still performed by students in the Advanced Laboratory course. Unfortunately, it seems that the interferometer used is not the historic device of Williams; that one appears to be serving in today's department laser researches.

There were, in addition to the faculty, research fellows and "postdocs" around: Parratt, Charlie Shaw, and E. G. Ramberg, for example. The first two have been mentioned; Parratt became a full-time instructor in January 1934. Ramberg worked in X-ray satellite theory under Richtmyer's auspices. He was a quiet, very soft spoken German, winding up eventually at RCA. A lot of Cornellians went that route. I recall Ramberg teaching me to do square root on one of the old style hand calculators; they were motor driven all right, but the carriage had to be transposed by the twist of a knob at the front of the machine. The routine revolved around the binomial theorem. It was a bit complicated. The machine loved to divide by zero under the hands of the unwary and uninitiated, and it was a real trial. The thing started off for infinity, and it was always a bit chancy as to whether one could persuade it to stop short of its goal without stripping the gears inside.

I should have said more about George Sabine above. He did his thesis on a study of the reflectivities of numerous evaporated films of the metals and nonmetals. It is still worthwhile referring to it. He was the son of a professor of Philosophy at Cornell. George was a cheerful, cherubic redhead, usually good for an amusing story. Besides his recollections of the Arizona expedition, I recall only one in which he himself was involved. It's perhaps gotten better over the years in the retelling. It seems that after a long day in the laboratory, George left Rockefeller late one dark night on his bicycle, without lights, for his home out in Cayuga Heights beyond the old golf course. His route took him out Triphammer Road. Somewhere out along the stretch there with no houses, where the golf course used to be, something struck him, knocking him senseless from his bicycle onto the ground. He came to, lying there in the dark, after how long he did not know. He seemed OK, picked himself up, and perplexed, pedaled the rest of the way home not too seriously damaged. He did not know what had hit him until some days later when he learned in conversation with a friend that the identical occurrence had

happened at the same place at the same hour to the friend while the friend was pedaling in to the University that same night! It all sounds pretty implausible, but I swear it, that's the way it was told to us. George went on to Kodak.

And then there were the graduate students--those already here when we arrived and well along toward their degrees, those who were our own peers, and those upstarts coming after. One heard of those who had left of There were Harvey White and Gerald Kruger, students of Gibbs in late. spectroscopy. White went to Berkeley and turned out numerous useful textbooks, including one on atomic spectroscopy, titled just that, in fact. Kruger went to Illinois and built their close copy of Cornell's small cyclotron, his research attracting Donald Kerst who there developed the first successful betatron. There was Sid Barnes, student of Richtmyer's in X-ray photo emission. He went to Rochester and constructed their cyclotron, at the time one of the largest outside of Berkeley. Hap Nelson was another; a student of Murdock's in electron diffraction, he went to Battelle. And others. There was one of particular note of whom I have never heard since. He didn't finish out two years but was an enterprising fellow who used his physics. I think I met him once. Recall that it was depression time. He worked one summer around the Ithaca rural area inquiring of farmers as to the state of charge on their lightning rods. He apparently convinced many that for a small remuneration he could put protection back into their system by giving the rod a good charging. They were tough times.

Finished but not gone was Freddie Hirsch. He had graduated from Cornell, got a Master's degree with Merritt and Bedell (on the dielectric constant of selenium under illumination) and a Ph.D. on X-ray satellites vs. Richtmyer's "double quantum jumps" under Richtmyer. He was independently wealthy and did his physics entirely for the fun of it, still working the X-rays until he left with his family for Pasadena a few years after I arrived. By a strange twist, many years later (1982), after Freddie had died and his wife was ten years a widow, she was to marry Dr. Robert Burt whose wife, Eleanor Bedell, had died a year or so earlier. Professor Bedell lived with his daughter and her Cornellian husband in his years out there as a widower. The last Mrs. Burt died within a year of her marriage to Dr. Burt, himself then some ninety years of age.

Of the "older" graduate students, perhaps the one closest to the group of youngsters was AI Rose. He was working with Smith on the effects in electron emission of varying work function from area to area over a non-uniform emitter. He went on to RCA and developed the orthicon and image orthicon, mainstays of the television industry for many years as the only suitable, low level, live scene, camera tubes. He gradually widened his travel horizons beyond the thirty miles and the Cortland scene. A number of graduate students made it to RCA, where Lloyd Smith had strong connections. There was John Ruedy, a spectroscopist under Gibbs; Ross Schrader, who X-rayed under Richtmyer; and Ramberg, as mentioned before. When things went wrong with his apparatus or a data run, as they always do in physics, Ross could run off the longest, most solid, purple, sizzling, ear-burning string of profanity in my recollection. Quite impressive to me, if not to his apparatus. Another of the older graduate students was Sidney Kaufman, student of Richtmyer's. He was one in the lineage of Rockefeller night watchmen, holding the fort before Al Rose. Sid went to Shell Oil and for years after World War II came by Cornell yearly to recruit people for Shell. After retirement in the seventies from the oil world, he left Texas and returned to Cornell to become a part of the by then prestigious Geology Department, with which he is still associated.

Of the second or third year graduate students around at the time I appeared, there were Willy Higinbotham, AI Fogelsanger, Horace Grover, Emery (Wheel's) Meschter, and Charlie Baker, among others. The first four worked for Smith, but Willy and AI never completed the degree. Al lived in Ithaca and for many years ran Ithaca Evaporated Metal Films, an outgrowth of the early Rockefeller basement work. Horace went on to Battelle Institute, and Emery went to DuPont.

As a graduate student, Charlie was not new to Ithaca. As mentioned, he had "busted" out of Cornell as an undergraduate (finishing at Dennison in Ohio). Even earlier, as a child, he had lived here where his father was serving as Baptist minister in town. The father had himself been a Cornell undergraduate; a big man, he did not "bust" out and at crew "pulled the sweetest oar," as Coach Courtney once said of him in commenting on his many crewmen over the years. Charlie must have been a worry to his

parents, whom I knew from the time when Scott and I spent a week or so up in Maine with them and their son during my first summer here.

The graduate students of the time and, indeed, most of the research fellows, were unmarried; how could we afford it? It was rather the exception when one showed up in married state, or became so while a student. Singleness had its advantages; there was not much else to do but hang around the building, days and nights alike. One got to know what colleagues and others were up to and learned from them.

Those graduate students fortunate and bold enough to be married usually had working wives who managed to find secretarial jobs around the University. An exception was the pleasant wife of an easygoing Manitoban who was working in Murdock's lab. She was diminutive, newlywed, and learning the secrets of housekeeping. They had Willy down for dinner one spring evening. It was certainly welcome, and all very nice. But for asparagus, she served only the butts, tasty enough but somewhat fibrous and unusual to say the least. "Honey," the husband hesitatingly inquired, "but--ah--where are the tops?" "Oh," she informed him brightly, "you don't use the whole thing; that part you throw away."

Willy was actually a pretty good cook himself. At Sunday night suppers, we could find him in starched tall hat back of the Willard Straight Hall short-order counter, whipping out the night's specialty waffle, omelets, and the like with calm efficiency. He could crack the eggs and extract the contents almost in a single motion using but one hand. He had a lot of talents; but he never mastered, with no hands, the unicycle that grad student Kay Frank had built and which was practiced on up in the attic. But he could and did run the liquid air machine; he repaired galvanometers (almost unknown today), constructed our early DC amplifier electrometers, and like many of us took turns nights helping Carl Gartlein out in the latter's barn during auroral activity. And there was his tour as night watchman.

He was a remarkable and valued colleague, a member of a remarkable family, who finally moved to Ithaca from Canandaigua after the father, a minister, died. Mrs. Higinbotham and the six kids frequently held large parties in the big old house on State Street at the foot of Mitchell. There were many such for Physics people, well remembered. Willy was farsighted and so had to wear thick glasses; he was short but made up in his

great congeniality, good nature, and loud good humor what he lacked in height. He was an accomplished performer on the accordion, always enlivening a party; he could take off on any tune, once or twice having heard it. Group sings were a usual part of our get-togethers, impromptu and otherwise, not so rarely in the basement of Rockefeller. How many times have I heard the many sentimental songs, the college hymns, the bawdy tunes that were popular, led on by Willy and his accordion. A proper writer could write an entertaining profile of Willy and the family. Some of the escapades they suffered were pretty hilarious, e.g., Moving the outhouse.

In my second or third year here, Willy's accordion got company; Joe Platt came from Rochester for graduate work in physics. He had and was good on a guitar; and he knew all the favorite songs. He also had a tremendous repertoire of limericks. A few have told of a particular accomplishment of his in this area of culture. There was a party that Sidney Krasik, who had just finished his Ph.D. under Lloyd Smith, hosted in celebration of the event. I missed out, having gone to Bell Labs. He invited his professor and numerous friends to join him over a keg of beer at his apartment in Collegetown. As can be imagined, it got rather noisy, so much so that someone called the police to quiet them down. The party moved out to the observatory; I don't know who was then serving as watchman. At any rate, out there some celebrant challenged Joe to recite 100 limericks, which challenge he accepted. He started in and, with some prompting, was going strong after 45 minutes, when the beer gave out. That stopped it but it was pretty clear he had met the challenge, many of his selections guite beyond printability. He became president of Harvey Mudd College in California. One wonders if his talent was recognized by the trustees in considering him for the post.

Willy went to the MIT Radiation Laboratory when war came and was later called to Los Alamos, at both places working in electronics instrumentation, for which he already had a flair in the basement of Rockefeller. He had not been long at Los Alamos before he made a very significant contribution which went well beyond the immediate application. Nuclear physics experimentalists were long-suffering in living with the instability of flip-flop electronic circuits so crucial to counters/scalers. Willy invented diode coupling ("steering") between

scales-of-two stages; this essentially locked the flip-flop in either of its two states of stability except on the application of a sizable fast trigger, which would kick it over into the other state. Life became very much happier in counting thereafter. The trick went well into the computer age, where the state of a flip-flop indicates either a 0 or 1, and we know how widely used that concept is in electronic computing.

Willy made another advance, the full impact of which is yet to be seen, but which by now has made a lot of splash. It seems that in the fall of 1958, Brookhaven National Laboratory, of which he was by then a staff member, was slated to host an "open house." For the kids attending, it was thought to have an exhibit with which they could interact, something more than the usual counters clicking away on a radioactive source with flashing lights giving the count. Willy devised a game--tennis--which could be played at an electronic "console," with the action displayed schematically on a five-inch oscilloscope. It was a sensation; there were longer lines for that exhibit than for any other. And so inadvertently was born the video game. No patent was taken out on the idea--no future was seen for it. It was interesting to hear Willy's voice in 1983 after all these years recounting the experience in a Public Radio broadcast interview.

The man has had other, more profound influence in our lives. At war's end, it was obvious that <u>the</u> bomb had changed the world. The May-Johnson bill in Congress proposed to turn the management of nuclear energy over to the military. Willy was the prime mover in Washington of a phalanx of volunteer scientists who came and lobbied successfully against the bill and for the McMahon Act. Our own professor-to-be, Bill Woodward, gave heavily of his time in the effort. In a very real sense, it was largely through Willy's efforts that we have civilian control of "atomic" energy. He subsequently went to Brookhaven and became part of the nuclear management and safeguards program, in 1971 receiving the first annual distinguished service award of the Institute of Nuclear Materials and Management.

The spring picnics seem forever to have been a standard department event. They were usually small affairs, and I don't recall much about them, except I do see the large figure of Richtmyer presiding over the fire and his large coffee pot. They were a far cry from today's monstrous

spring picnic when the department practically takes over the pavilion section of Stewart Park at the end of the lake, with sports and activities aplenty for young and old. On the barbecue pit must be at least three hundred chicken halves watched over by the men of the main Clark shop. Tables under the pavilion are laden with the rest of dinner, contributed to by all, beer and soda at a "bar" outside on the portico.

As graduate students, some of us occasionally threw an impromptu summer picnic, one of our own, quite outside the department calendar of scheduled events. I recall one such out at Taughannock Park where a few from a dozen of us divested ourselves of clothing and went swimming in the pool below the falls. A picture in my files, with the falls as backdrop, shows a rather motley crew, quite happy; another snapshot, on negative and never printed, of swimmers au natural. That was a merry affair. It could have been otherwise, as indeed it was at a well put together, scheduled Physics picnic held there a year after I left for Bell Labs. Marshall Holloway's wife, Wilma, and a graduate student, Henry Burstein, both drowned in the same pool. Appearing innocuously safe, the pool invited picnickers too close to the falls, quickly getting them in trouble. Mrs. Gerald Tape (her husband took "my" instructorship when I left) and a graduate student, Helen Hecht, were first in and were observed to be in difficulty by Burstein, who went in to help them from across the pool. He never made it. Mrs. Bacher and Wilma, coming up the trail, dashed in to help the two struggling women. Wilma went under. Hecht made it to shore exhausted. Before Mrs. Bacher was able to get into action on Mrs. Tape, a physical education man from Ithaca College, one Morris, got a tree branch and managed to get her out. It was two days before the bodies of Wilma and Burstein were recovered. Swimming there is now strictly forbidden. It was a sad occasion.

Some other early picnics I remember well were joint picnics held with the Rochester Physics Department. The two groups would meet up at Krebs on Skaneateles Lake, one of Central New York's famous eating establishments--all you could eat for the price. Manna from Heaven for us graduate students. I don't know how the picnic developed. I suppose Sid Barnes, by then at Rochester, was instrumental; there was Viki Weisskopf, friend of Bethe; Lee DuBridge was there, friend of Smith. Anyway, it was fun. Graduate students in each place got acquainted with those at the

other. On one such occasion, I.I. Rabi from Columbia was there. I recall that neither he nor Hans were too adept with a baseball bat. I don't know how proficient Weisskopf was at it. I do not remember who was on whose team or anything about a score but that there was amusement anytime the action came to either Hans or Rabi. Frankly, our man was not the most mechanically oriented in those days. I believe it is so that the boys in the cyclotron project helped him install the license plates on the automobile he acquired at some risk not too long after he arrived on the scene. It was probably at some such juncture that he remarked that he never knew which way to turn the screw to undo or tighten it. Holloway told him just to recall the mathematical operation, the vector product.

There was another eating establishment which was a joy to us graduate students. It was a small outfit, set in an ordinary house on Tioga Street, where there is now a parking lot. They offered a Sunday evening smorgasbord; a bit expensive for us--50 cents--but well worth it, for one could return for replenishment. And we did. I remember one night the proprietess suggesting gently to Charlie Baker that perhaps he had had sufficient as he went for a third plateful--or was it a fourth? It was not sumptuous as such things go, but it was good and there was a lot of it.

In those days before the war, and immediately thereafter, department parties were more frequent than they are now. There was generally a fall party, a Christmas party, a spring party, and the picnic. One memorable party involved some shenanigans in connection with the fall 1936 Presidential election--Roosevelt vs. Landon vs. Browder; Norman Thomas must have been in there somewhere; he usually touched base in Ithaca during his campaigns. At this particular party there were spokesmen for each party, save the Socialists. Charlie Baker spoke for the Republicans, belittling Roosevelt's alphabetic agencies, NRA, TVA, WPA, SEC, et al. What would we do when we got to ZZZ? The Democratic spokesman seems to have been a loss; we don't recall him. For the Communists, there was Hans Bethe, of all people. But he had recently published for the light elements, on the basis of known transmutation reactions, a table of New Masses. Thus, who else for the renegades?

Christmas parties were usually held over in Warren Hall on the Ag Campus. There was a very pleasant spacious room up there for such affairs. During Merritt's chairmanship, they were held at his house, down

on Grove Place. Christmas always used to mean a Santa Claus; one year I was somehow roped into the role. One had to dole out appropriate gifts to the various and sundry members of the department, principally the faculty. I recall giving Howe a brace of cigars--he was pretty down on tobacco--and stealing and lighting one up for my own use as I continued to dole out the surprises. Unaccustomed as I was to various hazards of smoking, I have to assume that the whiskers were of the nonflammable type.

Today, the Christmas parties (and other social functions) are held on "The Top of the Clark" overlooking the lake, campus, and inlet valley, a very fine facility. Skits are more or less mandatory, put together by various department thespians, students and faculty alike. The 1981 party will be long remembered for the excellence of the skits and particularly for an "underground" movie produced by enterprising department movie makers. Featuring Hans Bethe, Neil Ashcroft, and others noted in the local acting profession, it purported to catch on candid camera film actions of some of our notables; the elevator on the seventh floor opens and a hesitant Ashcroft peers furtively out--all is clear and he comes forth dragging by the leash a reluctant dog, a practice strictly verboten by his own edict for laboratory personnel; one Bethe comes down the seventh floor corridor, glancing nervously about--he comes before his own bronze bust underneath the clock in the foyer and quickly brushes it off with feather duster in hand. This strip must be preserved at all cost.

It was after World War II that the shop people in Rockefeller started having informal parties at Christmas. Participants generally contributed; shop soda pop sales during the year sufficed to finance the presence of a bowl of eggnog, generously fortified. The machines were background, serving as tables. In the meantime, the workers in the Laboratory of Nuclear Studies began having their own Christmas parties. Pretty lively affairs they were, Santa Claus distributing amusing gifts to various personnel. At one such party, Roger Knox, LNS Administrator, was the butt of a clever and good-natured joke. He was quite a square dance caller and had a radio program in which he played and discussed reels of various sorts. The tape recorder was at the time a new gadget; Corson had one. One Sunday, Corson taped Roger's radio spiels and subsequently, with the aid of another recorder and some assistance, excerpted phrases, words, maybe sentences out of what Roger had said, recording it in rearranged

fashion onto another tape. It was a howler when played at the particular party, from all reports. Laboratory populations have grown so that a downtown hall is now hired for the LNS affair, and that in LASSP has become a lunch on the top of Clark Hall, a smaller, more informal function being held earlier in the day by the machinists down in their domain amongst the machines. Population growth has advantage: there is considerable turnover in personnel, so that the frequent retirements and departures are observed with parties, both sad and happy occasions.

Before the "modern era" there were other sorts of parties in the department. A program committee usually had to work something out as entertainment before refreshments and dancing, not so much folk style as There was, at one affair, a treasure hunt through enjoyed today. Rockefeller Hall. It must have been an eye-opener to some of the faculty wives and some professors as well; most students got around. The hunt ran competing teams, from the basement to the attic, and the place rocked with screams of surprise and laughter as one team after another raced through the building for clues. At another, I believe after the war, there was a guiz program; two sides against each other somewhat in the manner of the then popular form on television. There was a question--it must have been of Phil Morrison's concoction--what is the denier measure of the cables on the Golden Gate Bridge? We leave the solution to the ingenuity of the reader. (Hint: some dictionaries define the unit, usually restricted to the textile industry and well known to women, as a measure of the thread in their nylon hosiery, for example.)

Speaking of Howe's Christmas cigars above recalls an incident that Parratt remembers all too well. I don't know how Howe managed in early department faculty meetings, usually held in late afternoon, for at such meetings, there was at least one member not to be put down from enjoying his cigar during the sessions. When he attended, presumably before Gibbs' tenure (the evidence is that he never attended any thereafter, save the one here to follow), Richtmyer generally lighted up. Old Joe Trevor was also an inveterate cigar smoker, but he never came to meetings. One time, at an important and somewhat tense department graduate faculty meeting which Richtmyer had been persuaded to attend, Richtmyer drew out his cigar, and then yet another, offering it to Lyman, who by that time had been made an Assistant Professor. While not a smoker, being a brave

individual and perhaps wishing to pour oil on the troubled Gibbs-Richtmyer waters (indeed, Lyman has said he persuaded Richtmyer to attend, that a proposal to establish a Graduate Conference in the Field of Physics was on deck, and it was most important that Richtmyer go along with it), he took it and lighted up. Well, the meeting progressed; so did the cigar, and so did Lyman. He started getting green, greener, and greener. He finally had to make a dash out of the meeting. I believe he made it okay to the men's room around the corner. He came back pretty pale, there was general amusement, and the meeting went on smoothly enough and successfully to its conclusion. Thus was born our Graduate Conference. But I'm not sure Lyman has had a cigar since.

Summers in the department were less active than during the regular school year. Some professors disappeared, but effort was made to keep the place exciting and attractive for summer students. Professor Gibbs initiated a program in an attempt to bring noted people here for the six weeks of the summer session. In my first summer in the department, we had the pleasure of having Professor James Franck with us. He gave a course in atomic physics with emphasis on experiment. While he had visited Ithaca before (see snapshot--Gibbs, Franck, and Merritt in the Merritt archives), he was new to the country; if I recall it correctly, Cornell was his first stop en route to a Johns Hopkins appointment, and then to Chicago, where he remained. The nightmare in Europe was beginning to have its effect in American science. He was a very amiable, rather slow-moving, dignified gentleman, with a big bushy mustache. think he enjoyed the summer too. After summer session, a group of us decided a symposium was in order--something not guite for real. Franck regretted his not being able to attend, but one or two professors did. Livingston was there. I seem to recall Tom Goldsmith (later Director of Research at DuMont) giving a paper on the "Determination of Cosmetic Nebula in G-minor." I gave a description of a new high voltage generator involving cats trying to escape hollow insulating spheres. There were others equally dumb. But we had a small Home Ec dining room to ourselves and it was well attended--by the usual gang of students needing a little outlet.

The next summer Franco Rassetti was in residence. His course covered Raman spectra and molecular doings.

After summer session was over, the campus got very quiet; Martha Van closed down. We were reduced to going over to the cafeteria in Willard Straight Hall. The food was not so good, but at least one could eat outside. The once open terrace has since been built over for the Ivy Room, Okenshields, or some such. Evenings were particularly nice. We'd go over about the usual hour, get a table outside, and dawdle around until we could spot the first star to come out. Baker was always spotting stars, well before sunset. I think he really thought he saw them, although none of us was that anxious to get back to work.

After he became chairman, Gibbs tried another innovation for the summer; I think it lasted only three years. There would be a two- or three-day symposium on various topics in physics, at which the leading lights in the country would be invited to participate. The first one was in the summer of 1935, just before Independence Day. It was a revelation to see titans in battle. The subject of the symposium was electron physics; at least very considerable attention was given to some of the complexities of electron (thermionic) emission. Among others present were GE's Nobel Laureate, Irving Langmuir, and Bell Lab's scrappy Joe Becker, both having worked extensively in the field. One scene stands out in memory. Joe had given a paper I believe, and Langmuir was up there at the front of Lecture Room A belittling "Becker's Alice-in-Wonderland view of the world." And then Becker comes striding down the tiered aisle. "Oh, I've read the quantum mechanics all right. I've heard of the wave function," he calls out as he starts his argument en route to the front. It was illuminating to a freshman that two real authorities could so violently disagree. Physics was alive and not all so clear-cut.

It was fearfully hot during the sessions of that symposium. Windows of the old auditorium were wide open throughout. On the day of adjournment it looked like a thunderstorm, but it did not materialize. By the next day, Sunday, fortunately most visitors had departed. For some recreation, I borrowed a bicycle and pedaled out to Taughannock Park. It was still very hot, so I stripped to the waist before I got to the foot of the hill downtown. Today, one would not even notice. That day, a police officer stopped me and told me to get dressed. I'm sure I peeled right down again when I got out of town. Anyway, I got to the park all right. But the weather began to look threatening again, so I rather promptly

headed back to Ithaca. As it got darker and darker, and thunder began to roll, I put on more and more steam. I barely made it to my room on Williams Street when the rain came down. And did it come down! Torrents, from afternoon until dusk, and all through the night and into the next morning it rained hard. In the morning with my window open, I could hear that things were lively over in Cascadilla gorge, a stone's throw to the north. It was the great flood of 1935. Roads were washed out, bridges gone, the Lehigh Valley Railroad a shambles. At the building in the morning, Sam Weibley reported Fall Creek had been practically up to the Triphammer Bridge! Hardly that, but the falls by the hydraulics laboratory were not dropping vertically but, rather, were shooting out at about 45°. The various drops below the dam were almost hidden in the onrushing muddy water. The <u>Ithaca Journal</u> was full of pictures of the devastation. The trail in Enfield gorge was largely washed away; a big overhanging chunk of rock at the lip of Taughannock Falls broke away, altering the symmetry of its drop over the brink. Eleven lives were lost county-wide; 400 refugees spent four nights in the Drill Hall. The following Sunday I took the same bicycle ride; quite different, and with a fair amount of portaging over the several washouts on Highway 89.

There was another, but less major, weather disaster in the spring of 1936; Ithaca was having a tough year of it. No loss of life this time, merely inconvenience. There was a great ice storm. The air was still, cold but sharply stratified, and a light drizzle kept coming down and freezing to things. I can still hear from the watchman's quarters up on Rockefeller's third floor, sounds like shots in the night as tree limbs broke under the load. One knew that the campus would look different in the gray morning. And it did. There was great damage; the city was isolated for two days. We ate by candlelight at Home Ec; how they cooked I don't know; I expect they used gas. It was a curious storm in that the freezing took place above a very certain elevation. One could see on individual trees, down by the inlet, the line of demarcation--ice above, all clear but wet below. To a person from Nevada, the Ithaca weather took some getting used to. I have yet to manage it.

The flood was a bad experience all through the Southern Tier. Over at Corning, the second 200" Palomar reflector disc was cooling. The first one had developed a flaw during the pouring, and it was decided to pour a second. The first one is now a fine attraction in the lobby of the Glass Museum over there. The Chemung River rise during the flood threatened to inundate the cooling and annealing oven, which was programmed to lower the temperature of the huge mass of glass a few degrees a day. To meet the flood threat, and it was good that they did, workmen sandbagged around the whole affair up several feet to keep out the rising water. The Physics Department arranged a tour of the Glass Works for students and faculty later that summer. There we saw the cooling oven and could look in a port to see the great disc, still at dull red heat. They were in the process of dismantling the pot from which the melt was taken and poured into the disc mold. There were lumps of the melt lying all around the rim. So it was that I come to have a chunk of the Hale 200" mirror on my desk.

The second topical symposium was on nuclear physics and remains in my memory chiefly for the image I have of Enrico Fermi. Certainly Bethe was there, as was Konopinski, who by then had arrived at Cornell to work with Hans. Rabi was there, many others. But I don't recall much of it. Except for the image of Fermi. Beautiful. He reminded me of a happy peanut vendor. His eyes sparkled, he had the nicest warm smile, naturally a fine Italian accent, the most patient manner in explanation, and he knew everything; he was utterly beguiling. It was almost enough to turn one to nuclear physics. There was still a lot of it left to do, in spite of the great series of slow neutron experiments he and his group in Rome had completed a few years earlier and probably touched on here. Uranium had been and still was a puzzle. Fortunate, considering the coming events. (Many years later, on looking thoughtfully for a few moments with a colleague at a bas-relief of himself on the side of some building, seeing it for the first time, he remarked, "There's the man who never discovered fission.")

For this conference, there was no flood thereafter. There was, however, during one afternoon, a severe thunderstorm with much crashing and banging around of the heavens. One wondered that it might not be a repeat performance of the year previous. Bethe recalls Gregory Breit delivering a talk during it all, overriding the noise of nature by dint of virtually shouting his speech. There was in those days no public address system in Lecture Room A to fill its cavernous volume with a lecturer's voice. Today, we have such equipment which, in spite of the advanced

state of the technology, is barely adequate. More often than not, it seems an unwary lecturer can induce strange and annoying interference in his microphone wireless connection to the amplifier, or, if wired connection is relied on, he gets all tangled up in the cord connecting him in. There ought to be a solution somewhere.

There was a third symposium, I was gone that summer and don't know what it was all about. It was the 1935 symposium which was the inspiration for our own mock symposium after summer school, noted above.

Ithaca was a pretty place, and still is, in summer. It was pleasantly quiet after all the summer school students had departed. That first summer of mine here saw the end of the streetcar system in Ithaca. It had already shrunk from its one time extent. It came up State Street, up Stewart Avenue, over the bridge to head up Thurston around in front of Balch and then across Triphammer Bridge down along back of Goldwin Smith, across what is now the Engineering quadrangle and across the Cascadilla bridge--now a wooden foot bridge--down back of Cascadilla Hall onto Eddy Street and thence to State and down the hill to the railroad station. I think it was double track up and down State. College students were somewhat troublesome; pulling the trolley wheel from the overhead wire brought things to a standstill. I don't think this happened very often; I never saw it. Ice could be a problem and frequently was. I recall being awakened once or twice with a vacuum pump clacking away in a dream. It was only a streetcar heading up Stewart Avenue past Jim's, with a slightly flattened wheel or two, incurred in sliding down State Street I am told.

Morris Bishop, in his Cornell history, credits our first big name professor, William Anthony, with the design and supervision of the installation in 1887 of the Ithaca Railway short line, an electrified, paired-trolley, streetcar system, running intermittently from the R.R. station to the Ithaca Hotel, where the large department store now stands on the Commons. The trolly was powered from an unsightly generating station in Fall Creek gorge near the Stewart Avenue bridge, the station stack indicating that other than water power could be used for driving the generator at times of low flow in the creek. The generator, incidentally, eventually came to the Physics Department, where in the mid-thirties it

was pressed again into service to wind up its days, driven by a large electric motor, energizing Livingston's cyclotron magnet. In an expansion of considerable bravery later, the line was extended up steep State Street to the campus and a terminus in front of the library.

As I say, the system was dismantled that first summer. One evening during the process of first removing the overhead wire, there stood in front of Rockefeller, on the tracks passing in front, a small four-wheeled vehicle with a wood tower reaching up to the trolley wire so workmen could reach things. The temptation was just too much for someone. The next morning the contraption was found overturned down by Cascadilla It had been simply shoved along the tracks, still in place, until the Hall. Cornell slope took over. It must have been a sight to have seen it tearing down past Sage Hall, across the foot bridge, to come to its bad end down by Sheldon Court and Cascadilla. Still, the wild abandon of this piece of rolling stock careening down the hill must have been as nothing compared to the early performance of another piece--an old trolley car--which was shoved off the Stewart Avenue bridge over Fall Creek for the entertainment and edification of early moviegoers in the "Perils of Pauline" era. The undercarriage was there to be seen well into the thirties according to my authority, being swept away over the Falls in the aforementioned flood.

The Ithaca scene and Cornell was then rather different than today. There were many houses on campus. To the northeast of Rockefeller Hall and across Reservoir Avenue, which was the road on the slope between Baker and Rockefeller, curving around Bailey, and so named, one presumes, for a reservoir that was once back in there, we had no Savage Hall, no Newman Laboratory of Nuclear Studies. Rather, there were houses along a drive in and around a dead-end circle. There was no Engineering quadrangle; rather, a wide open greensward widely used for playing fields before the war. After the war, there were quonset huts disfiguring the landscape until Engineering moved in from the north end of the Arts guadrangle. The Johnson Art Museum was very much in the future. lt stands now on the site of old Morse Hall which burned; that is part of ancient history. There were two or three houses along Central Avenue where later was built Gannett Clinic. The Armory (the old gymnasium) stood where Hollister Hall now sits. A fraternity house was south of it,

overlooking the gorge. There were other houses along East Avenue where the Statler was built and where Phillips and Upson and Grumman Halls are The Merritt's house was down there. now located. The two Merritt daughters (Virginia married one Emlen; their son is up on the Ag campus working in bird navigation, so there is still a Merritt connection at the University) tell of faculty kids roller skating on the long stretches provided by the Arts guadrangle walks. In the winter it must have been over to the ice on Beebe Lake. Some playground. The dean of students for women had a house back of Sage Chapel, just west of where Day Hall is. As described above, the streetcar system still went through campus. In summer, both Central and East Avenues became green tunnels under tall graceful elms bordering and arching over the thoroughfares. Trees were prominent around the quadrangle. In place of Olin Library, there was Boardman Hall, seat of Government and History; seat of government in more ways than one--the university faculty meetings were held there up until the time that Olin Hall (Chemical Engineering) came to be used, and now lves. There was no Campus Store worth the name; what there was was crowded into Barnes Hall. That was long a sore point with both students and faculty. There were dividends one could collect at year's end, but it was a privately run outfit, profit accruing to some faculty shareholders, I believe. After the war, as Cornell grew, there was frequent discussion about a new store, where it could be located, etc. To put a new store next to Barnes Hall would cost some parking space and disfigure the landscape; putting it somewhere else would make it non central. And so it went. This is relatively recent history. After one such faculty discussion, I wrote Tom Mackesey, Vice President for Planning or some such, asking why they didn't go ahead and put the building north of Barnes where it was advantageous, but bury it; no one is looking at the scenery while he is perusing a book or making other purchase. Interesting, but that was it. A year or so later there was more discussion, and I wrote him again, insisting that it still made good sense to bury it. And they did. Unfortunately, they hit bedrock too soon and so the building did not go down the three or four more feet that was planned. Some years later I wrote Mackesey again, suggesting something else but volunteering that they were probably too busy to pay it much heed. He wrote back an acknowledgment, reminding me "We built your building, didn't we?" So I

guess some blame or credit, as one prefers, devolves onto myself for its presence.

The other striking thing about the appearance of the campus is that the student population is more than double what it was. Students are far more casual than then, less finely garbed, less closely shaved and shorn (men), women, more frequently than not, now in pants. And professors no longer lecture in coat and vest, watch chain looping across from one pocket to another. Indeed, open collar, no tie is the usual; Levis are not uncommon. And in physics, I think graduate students have tougher courses now than we did back then. I have been lucky all through my life; I've just gotten past, by the skin of my teeth, hurdles which those coming after were expected easily to get over. However, there was more laboratory in the old days and of what there was, I think more was expected of the student than during, say, the last decade, by which time only one rather than two semesters was all that was required. More's the pity.

In these days, a great many graduate students are married, in marked contrast to the prewar days, noted earlier. And today it is not uncommon, from all that is said, that male and female cohabit without the benefit of marriage, and without stigma, perhaps only a raised eyebrow. Even on campus, the sexes are now mixed in dormitories. One recalls a <u>cause</u> <u>célèbre</u> in the early fifties when a male graduate student, not in physics, was expelled from the university when it was revealed that he was living in unmarried bliss in Collegetown with a coed from Cortland. Closer to physics, in my first year here, there was the undergraduate physics major boasting to some of us of his spending the better part of the previous night with a girl in the ladies room on the third floor of Rockefeller. Brazen, and not very gallant either.

Even with all the work it was a very congenial place to be, and I think most students and staff found that to be the case. I hope we still find it so. There was one sad instance when that proved otherwise. There came an instructor, I believe from Chicago; he had a wife and child. I don't know what went on, but a fairly Bohemian former lifestyle came into it. At any rate, one evening he went into the back hold of the stock room and drew the potassium cyanide. He was found later that night, and it gave the place quite a jolt.

There was at least one undergraduate physics major who also found his experience with us less than satisfactory. This would have been in the He wrote the chairman a long, vitriolic letter castigating in sixties. particular Professor Lee and myself. Professor Lee was inept, incompetent, and a total disaster. I was a menace, and had physically threatened him. I must admit I was extremely provoked with him at one point. In the Advanced Laboratory, measuring the rotation of the plane of polarization of light traversing a sugar solution, he had spilled his sugar solution all over the work table, so I had a nice mess the next morning to clean up on finding it as a thick syrup which he had left for others to worry about. Seeing him later, I indicated that I could have wrung his neck, illustrating with a sharp twist of my two hands on an imaginary neck. Our regard for each other was mutual I am afraid. Lee and I felt rather complimented with the letter. There have not been many students over the years for whom I have taken a dislike; I don't know how it has gone the other way, except in this instance for sure.

In my last year at Cornell before the war, I was named an instructor, teaching under then Assistant Professor Diran H. Tomboulian, with whom and whose family I became fairly close. He earned his degree sometime after I got here, was at one time a Rockefeller watchman, inhabiting the third-floor den. He was a firm disciplinarian--no monkey business--and that went for student and staff alike. I well recall one evening's violent and rather nasty argument between him and another instructor, Leonard Pockman, when we were grading a sophomore physics prelim. I don't recall the issue, but the words became very caustic and brittle. It was not a pleasant scene. Pockman was quite a radical politically, and it came out in practically any conversation. He did not stay long at Cornell but went back west to work for Varian or Eitel-McCollough in electron tubes, a route similar to that I was subsequently to follow. He died during the war.

Tomboulian almost killed himself in a demonstration lecture on Xrays. In darkened Lecture Room A, while carrying out a fluorescence demonstration, he reached over for something and hit the high voltage. It belted him severely, knocked him away, and burned a hole in his shoe. But no one in the audience was aware that anything had happened, and he finished out the performance. Demonstrations frequently entailed some

hazard. Peter Strok also got involved with a hot wire, that of a big electrostatic machine. Sparks flew all over; he emerged scared but unscathed. Such machines are not too dangerous.

Safety has only in recent decades become a matter of prime importance. It is surprising that there haven't been more mishaps. Mercury we used to handle like water. Spills on the old wood floors were but a nuisance. Today we vacuum it up, even to cracks between boards. High voltage was treated rather lightly. Hap Nelson and a colleague, both working with Murdock on electron diffraction, had a most impressive high voltage set up, with foil covered glass plates as filter condenser, and big transformers occupying a small room near their apparatus. The safety interlock on the door to the room was bridged the first day it was installed and the door never closed thereafter. The 50 or 75 kilovolt output was taken across the corridor ceiling on 1/4 inch copper tubing to the hot side of their camera, all completely exposed. Recall that one worked in the dark, better to see the dim fluorescent images on their screen. As Emery Meschter has said, one had to conclude that someone must have gone to church fairly regularly. There were set ups in the advanced laboratory course on Rockefeller's third floor that were not greatly different, even up to the early fifties. But things have changed by now with University safety people even coming around to check for hazards.

Tomboulian was an interesting character. He had fled, a young refugee, from the Middle East, was in Smyrna at the time of fighting and conflagration there in 1921, a scar on his swarthy face testifying to an encounter in that troubled period. He was a dark, rather heavy-set person, perspiration seemingly perpetually glistening on his face. And he was very methodical; he kept everything it seems. His files in the Library Archives are to be found in sixteen large boxes. Old scribblings, reprint requests, manuscript drafts, galley proofs, class notes, problem solutions are all there, even including a thin folder relating to the "Coffee and Cream" problem. (A conclusion therein: "In other words, add the cream FIRST!") His laboratory in the basement had a room, which after his death, I was given to clean out. It was his private stock room. Any piece of apparatus lying around, which no one claimed at the moment, had wound up in there. Money was tight, always had been, but he saw to it that he was

not left wanting. It was amazing what was in there. But it served him well. He was a tireless worker, had many graduate students and lots of publications, most importantly in soft X-rays. He started in hyper-fine structure analysis but early on got into the soft X-ray and far ultraviolet regions of the spectrum, necessitating vacuum spectrographs. Looking through his file, one is impressed at what he accomplished. He wrote the authoritative article in the <u>Handbuch der Physik</u> on soft X-ray spectroscopy.

It was my good fortune after the war to get involved with him in some work with the synchrotron, an ideal collaboration between the "nuclear" people and the "solid staters," the latter appellation one I think that Tommy never went much for. I was down talking one afternoon with Leonard Jossem, one of Parratt's students who later went on to become head of Ohio State physics. Lenny had been around an inordinately long time. I recall Smith being particularly piqued at Jossem's bringing his thesis to him, after a residency very close to the ten-year limit, and requesting that he read it that evening so the examination could be carried out safely the next day and just within the time limit. Something like that. Anyway, I was talking to Lenny about vacuum ultraviolet sources. Was it completely impossible to think of using bremsstrahlung in a high current X-ray tube? He wondered why I didn't consider using the synchrotron, light from which had been observed a few years earlier at the GE betatron. That sounded exciting. I went down the hall to Tomboulian's lab, found him, and suggested that it ought to be tried, how about it? I can't believe he had not thought of it; but he was a pretty private person about his affairs, scientific and otherwise, so one does not know. At any rate, he liked the suggestion and agreed we should look into the possibilities. I talked to Bob Wilson; he and the synchrotron group were more than willing to let us have a go at it. We first looked at the radiation in the visible and near-UV with a small guartz spectrograph and phased shutter so that spectra at various energies could be а photographed. Over the limited spectral range, all appeared to be in accord with what Schwinger had earlier calculated. So we made preparation for looking at it in the soft X-ray region, for which Tomboulian had the instrumentation and experience. I did the mechanical design and got things ready; he did some analysis on what to expect; those

complex, fractional order Bessel functions were troublesome. We got in on the machine one Saturday and prepared to make some exposures that night. After a little vacuum trouble with a large Wilson (R.R.) seal on the valve opening their system to ours, we made an exposure and trotted over to Tommy's dark room in the Rockefeller basement to develop it. There was just a great long smear streaking across the plate. A light leak? Tommy knew just how to check that. He had some thin beryllium and aluminum foils mounted properly from previous work he had done. We put one behind the slit of his spectrograph, closed it up, evacuated it, and opened it again to the synchrotron vacuum and took a second exposure. That was exciting. On development, the dark streak was no longer so uniform. There were the sudden changes in density indicative of aluminum absorption edges. There was no doubt of the radiation from the synchrotron; in the Cornell machine, it did extend down to about 40Å. We got a nice paper out of it, and he went on planning to use synchrotron light in his soft X-ray researches, but with the Harvard-MIT synchrotron. He spent a sabbatical period at Cambridge helping to design a facility there to exploit it. Why he was not contemplating doing so here, I don't know. At any event, sudden death intervened, and the first real exploitation came at the Bureau of Standards with Codding and Madden. One of Tomboulian's students, Dave Ederer, continues in the work down there, and synchrotron radiation is a big thing now in numerous laboratories, the fixed energy storage ring making it a really practical and potent source of radiation. It was interesting after all these years to find recently in the Archives' Tomboulian file, one or two of our old notebooks detailing some of the work.

The power distribution system in Rockefeller Hall was a marvel-for the early 1900's. At numerous places throughout the building, there were situated large, marble-faced switchboards with rows of orderly arrayed spring clip contacts at the surface. Connection was made by pushing in flat metal sorts of reeds at the ends of the connecting wire, the metal ends being insulated from the "switchperson" with a wooden or bakelite handle. Power was moved from one switchboard to another through interconnections made with these jumper cables at the appropriate switchboard, and from there to miniature editions of the switchboard in each research room. Exposed wires on white insulators ran

all over the place, mostly along corridor ceilings. The building had its own DC supply, the ordinary 110 volt AC, and three-phase 220 volt AC, the latter added during Gibbs's regime. There was of course the usual array of pipes for water and steam heat. Newly added a year or so before I got here was an extensive sprinkler system for fire protection. It was charged with air, as I understand it. Were a sprinkler head to become overheated, a seal was broken allowing air out and water to fill the system and spray out over the flames, an alarm sounding down on the first floor, with flag indicator showing where the presumed blaze was located. It did function properly on a few occasions; one hopes that it would serve in the case of a major conflagration. Of course, it is supposed to keep a small blaze from becoming major. It is still in use, at least standing by. (1986--The mechanical flag signal system has been replaced by a flashy looking bank of signal lights at the north basement door, which port of entry is where a fire engine would come first to gain building access anyway. The single old audio alarm is superseded by piercing buzzers at various sites, which frequently cause building evacuation in fire drill.)

DC power in the building was used for arc sources in spectroscopy but particularly for charging storage batteries. The storage battery is a very quiet source of low voltage, high current DC. It was the rare research room that did not have a station for charging batteries, each with its series glo-coil to limit the current, imparting some heat to the room in addition. On watchman's evening rounds, one always looked in the dark for the tell-tale dull red glow of this limiter, usually at the room switchboard, indicating a battery charging somewhere in the room. One had to pull the line since, with the DC turned off later, the battery would simply discharge through the generator until it was started again in the morning by the shop crew. The clever researcher resorted to a simple relay to take care of a mistake. The storage battery was a necessity for those doing small current measurements--those in X-rays and in ion and electron physics. The old FP-54 electrometer tube had filament heated with storage battery. That was the way to measure minute currents in those days. (The name of Lee DuBridge is associated with a particular circuit involving the FP-54, which he developed at Washington University in some photoelectric measurements, before he went to Rochester.)

To those who parked their automobiles in the courtyard, winter brought special hazards. After a deep snowfall, woe be it to the unwary who parked his car close by the building under the eaves. Many were the hood and top stove in by heavy snow sliding off the slate roof above. Equally hazardous, and this to pedestrians, were the front entrances to Rockefeller Hall. Great icicles hung threateningly over them, particularly on the south side. An early report by Nichols to the president mentions the danger. For a long time, a tunnel was in place up the stairway to provide protection, but this was abandoned ten or twenty years ago, perhaps for esthetics. Whatever its beauty, it was very practical, taking care of ice from above, as well as that underfoot, which without it accumulates to an alarming thickness. Only in 1979 did Buildings and Properties do some remedial work on the roof and gutter and installed railings allowing one to gain entrance in some safety. It seems to have helped.

Merritt was also very worried about the ice conditions overhead at the entrances, writing a letter to Dean Ogden of Arts and Sciences about it. There was even a theory as to why the conditions were so much worse at the south end than at the north, geometrically about identical. The theory was, he explained, that the inside stairwell at the north end only went to the third floor, while that at the south end, opposite the entrance, went clear up to the attic and roof. Thus the roof was warmer over the south entrance than at the north end, so the ice collection was much more severe. He expressed great concern that a person could well be killed by falling ice. It is not known if this is what prompted the appearance of the now abandoned tunnel.

Research rooms were not as convenient as those we have today. Water spigots were at the corner sink; electric outlets were not distributed all around the room for getting power conveniently, as is the case today. The use of lamp cord (twisted, cotton covered pair--"old green and yellow") was prevalent. I recall moving into the research room vacated by Tom Goldsmith when I started work on the linear accelerator. My recollection is of a veritable jungle of cords dangling from the ceiling and looping across the gloomy room. Together with the clutter mounted on the floor, it was a discouraging sight.

It was later in that same room that I experienced another sight which shook me thoroughly, coming as it did on awakening from a nap at my post. George Scott and I had brightened and dressed up the room considerably, and he had there an ion source apparatus built up, and his desk, alongside of my own. His experimental set-up had magnet coils associated with it, pumps, and water cooling for these and other parts. It was a warm afternoon, bright sunshine outside, and I fell to dozing at my desk by the window. When I first dazedly opened my eyes, I found them fixed on Scott's apparatus. I could not believe it. I blinked and shook my head, but no change. My immediate impression was of a huge snake crawling out of the sink, down onto the floor and up into and around his apparatus. It was Laocoon, with the father and two sons replaced by the experimental set up, the great serpent entwining it all in its coils. It took a few seconds in my confused state to realize that the gum rubber water hose was on the point of bursting. It had expanded from its usual 3/8" diameter to something nearly two inches in diameter. I finally came to, dashed over to the sink, and turned off the supply. It took a long time for that hose to drain itself. What had happened was that, as usual, they were digging somewhere on the campus, this time repairing a water main break up on the Ag campus. The pressure in Rockefeller had fallen very low during the morning hours, so everyone naturally turned up the spigots. By noontime, the leak was repaired, unbeknownst to those who had done this. Of course the input had now become much larger than the hose would accommodate, so it did what any normal rubber would do in the circumstance. It was an amazing spectacle on which to awaken.

That was a nice research room after we got the litter of the previous tenant cleared out, rather bright, and, needless to say, airy. Somewhere in the building I had found a big shelf model Atwater Kent radio and separate external speaker with which we kept up on what was going on in the world. The Sunday afternoon symphony was usually tuned in and appreciated. In the last years came the ascendancy of Adolf Hitler, and some of his hypnotic and frightening oratory; frightening even though we could not follow much of the German. There were the equally hypnotic and disturbing Sunday talks after the symphony, given in his smooth, oily manner by the demagogue, Father Coughlin. And the sad abdication speech of Edward VIII. And the dirigible Hindenburg disaster. The usual diet was

provided by the local radio station, now WHCU. At the time it had call letters WESG (Elmira Star Gazette) and for on-campus Cornell broadcasts had a small studio cottage at the north end of a nice sloping flower garden where Malott Hall now stands. Already the outfit was a CBS affiliate, extolling such as the virtues of Carter's Little Liver Pills ("Right! And when you don't feel good, try--they do the work of calomel without the danger of calomel--harmless vegetable pills, etc."), rather "homespun," not with guite the erudition which was to come later when a Saturday afternoon local WHCU announcer could tell of his next week's program, offering "two concerti by Brandenburg." The antenna for the receiver (in those days it was almost axiomatic that you needed a long outside wire) ran up the outside of the building from our first floor window to one on the third floor, there being no tree handy and ice a winter problem. One night I even came close to home, picking up KOH, the CBS station in Reno. That fine old set is recalled with affection. It always smelled nice, warm, and electronic inside, tubes softly glowing, performing their appointed function.

Speaking above of Hitler's German, in those days the Cornell graduate student had to pass a reading examination in two languages--for physics, usually French and German. Never having had any German, I audited a German class down in Goldwin Smith Hall for a while, bought myself a German dictionary, got hold of a German physics text (Kohlrausch?), and started in, looking up word after word until some words started to repeat. For many months, this was the usual thing before going to sleep at night. It was while so engaged one night that I noted the bed shaking and realized we were having an earthquake, the only one I've experienced around here. Anyway, by hook or crook, Collins passed me as being able to read German. French I thought would be a snap. I'd had some French. So I went confidently up to Professor Wilson in Agronomy to pass my French. What did he do but reach up on his shelf for a French book in Agronomy. I didn't even know the French word for the subject. But somehow he passed me. Today language other than English is not required, the requirement disappearing in the 1960's.

The physics examinations were manifold. We had to pass an oral qualifier. I remember nothing but the question Professor Bethe asked. It was on the diffraction grating. I did badly, but he nursed me through it,

and they let me qualify. Then, a year or so later, there was a written examination in the student's major subject and in each of two minors. I was confident enough in my major, with Smith, on experimental physics. That went fine. And I was pretty confident on the Astronomy. But Boothroyd fooled me; he gave me only one question--in practical astronomy--spherical trigonometry, if you will; that was my mother's field. In practical astronomy, I had only measured for him previously where Fuertes Observatory was located, as if it were not already known. Latitude and longitude determination confirmed that it was indeed in the vicinity of Ithaca. For the written examination, he gave me that on a certain date an aviator observed that Sirius, Capella, and perhaps Canopus were all at equal altitude above the horizon. Where was he and what was the time of day? Something like that. No astrophysics there. I really didn't make much headway with it in over a day and a half. But I could visualize the situation and so conceptualized some about it. At the oral examination, taken after all the writtens were done, he commented, when his turn came to quiz me, that I must be more of an experimentalist than theorist. How well he knew me. I had designed and built and used the double slide plate holder for the I2" refractor at the observatory and had performed the optical tests on the 25" mirror for a projected telescope Shaw was gradually building. It is now the objective mirror in Shaw's open tube at the Boothroyd-Hartung Observatory out on Mt. Pleasant, the largest telescope in New York State.

I worried most about Bethe's exam. I had previously inquired of him as to how much hydrodynamics I should know. He said it was enough to know that there was such a subject. It was not very many years later that he drastically changed his mind on that. But I was glad for the opinion. He gave me a series of reasonable questions, and I made some impression on them, enough to satisfy him, kind person that he is. During the oral, he asked me that same earlier question on the diffraction grating. This time I could do it.

There was one other somewhat amusing experience of the graduate student days, reminiscent of the difficulty students had in the 1980 national election. My experience came about as the 1936 elections approached. I was a strong FDR man, and my fellow graduate students were largely Landon men. Landon! We'd argue loud and long at lunch over

the various merits of each. Now be it recalled that I lived at the foot of Williams Street, on the south side, in fact. It turned out that a ward boundary line ran straight up the middle of the street. Across the way, on Highland Place lived physicists Baker, Holloway, Moore, and Eugene Baroody in an apartment. There were some pleasant evenings spent over there. Anyway, they were all for the Republican standard bearer. That sets the scene.

The first week of voter registration, I went to the firehouse on College Avenue to register. This would be the first election at which I could vote; I was pretty excited. There sat three or four elderly ladies, pretty obviously Republican. I may have indicated my preferred affiliation. I must have. But I don't wish to impugn their sincerity. When I told them I was a graduate student wishing to register, they indicated I was out of luck. Students couldn't vote, graduate or no. Well, that was interesting; disappointing and a little unfair I thought in my case, for I had not been home for a year and a half. But I accepted the verdict. I dropped over to Highland Place that evening and informed my friends that all our argument had been for naught, that none of us could vote. There were raised eyebrows and some disbelief. Now, being in another ward it so happened that their registration place was in Rand Hall on the Cornell Campus. To which place they traipsed to register. No problem. They registered! Well, it was clear my ladies had made a mistake, so I went back on the next Saturday to register, telling them of their error and how some in more questionable circumstances than my own had been registered to vote for their man. "Sorry," they said, "they should not have been allowed to do so. You better go see your Democratic Committeeman, Dan Crowley." (So I had told them my party preference). Which I did. He was an old time political hack all right. He said, "Yes, the ladies were right, but you tell me the names of your Republican friends who have registered and I'll have them challenged at the polling place on Election Day." That hardly seemed cricket to me. But then he added the happy thought. "You better check in with Professor Bretz up on the hill." Bretz was a history professor who was always running on the Democratic ticket for Congress--and never getting to first base. But he was glad to see me, hear my tale of frustration, and he encouraged me to go back to my firehouse the next Saturday and register. He would in the meantime speak

to the election commissioner downtown and get it all straightened out. He did, and I did. But something went wrong somewhere. They still would not let me register, and the time was getting short. I think there was one more Saturday in which it could be done. So it was back to Professor Bretz. "Listen," he said, "it's all nonsense. Of course you can vote. I'm going to Albany this week, and I'll go see the attorney general and get a ruling." And it happened. The next Saturday I went down, and the disappointed old ladies registered me. I felt fine.

Actually, I still wasn't quite home free, even with the ruling of the attorney-general. Having come from a state beyond the confines of New York, it was assumed that I was illiterate. I was required to take a reading test; so on the spot I did--something about the Statue of Liberty, which I could handle. I have recently been reminded of this by my friend Scott, then student colleague, Republican, and also in my election district. He was under no such handicap, even though he was from Auburn up the way, and frequently got home. It is true that he was employed (as an assistant), and I was on a fellowship, but I doubt the ladies recognized the difference. He played the angles, however, pointing out to them that he had given to the Community Chest (a full dollar, after being told by the department, probably Grantham, that since he made his living here and was resident, he owed the community at least that much); he was therefore obviously an Ithaca citizen. That was accepted as clear proof of something, so they let him register. And, being from New York, he did not even have to know how to read! But no matter, there had been no great uncertainty in the decision at stake; my man won, hands down. But he is long since gone; Landon is still alive, active and kicking, his daughter now a Senator.

There were two or three graduate students of the era of particular note. One was Ralph Myers, a theorist. He was in the theoretical physics course, and it was clear he was the brightest in the class. In fact, he became Bethe's first graduate student, did a nuclear physics thesis in good time, and went off to the University of Maryland, where he remained a mainstay of theoretical physics there. In the second or third year of my student days, there came another bright theorist, Bob Marshak. He worked down the corridor from the linear accelerator and used to come wandering in at night to find out what we were up to and to explain theory to us. He

also worked with Bethe, of course, making calculation on various models of stellar nuclear burning, a concept that Bethe had only just recently shown could be used to explain the energy production in stars. This has frequently been described as something he worked out on a train ride from New York. Bethe disowns (1980) the tale and thinks it is time that it be scotched. He did do something, also very important, on another train ride much later, as may be recounted in due course, but this was surely not it. He developed the stellar theory during four intensive weeks of consideration and calculation, right on the first floor of Rockefeller Hall. The result of the work is well known. There was, however, yet another train ride, the short one down from Rochester, during which he engaged himself in modest diversion. He had visited someone up there who had told him of a problem he was working on with a couple of graduate students, the solution to which he would communicate to Bethe after the couple of months it would take to complete it. During the hour and a half run back to Ithaca, Hans worked it out and gave it to his class the next day as the week's homework problem.

Marshak went on in theory, making quite a name for himself, after years at Rochester winding up as president of City College in NYC, from which he resigned in 1979 to become a professor-at-large at VPI. He was also to become president of the American Physical Society.

A few more words on Lyman Parratt are perhaps in order before terminating this lengthy reminiscence. Parratt had come from the laboratory of Compton and Allison at the University of Chicago as a National Research Fellow. At this time, he was all research. It is recalled that at a Christmas party, talking to one of the new batch of graduate students in our "class," he regretted that he had no time to get acquainted with graduate students and was appreciative of parties for making it possible to a degree. One can see Lyman huddled by his evacuated spectrometer tank in a semidark room, watching the circular light spot moving over his galvanometer scale, taking the measure of an Xray intensity. Or again, in the glare of an overhead light, green visor pulled down on his forehead in the manner of a city desk editor, old newspaper man that he was (Salt Lake City Tribune), batting out on an old typewriter the next paper on his research. Indeed, it turned out that at the time of his remark to the graduate student above, he had already

resigned his fellowship to become part of the faculty, and while his interest and devotion to research never flagged, he developed deep interest and devotion to teaching and to getting acquainted with graduate students. When, very much later, he became chairman of the department, the research was essentially put aside, and he became as single-mindedly engaged with students and department routines as he had earlier been with his research.

Lyman was for the most part outwardly pretty imperturbable, very much in control. This characteristic was perhaps best shown me in an amusing incident occurring considerably out of the time of most of this story. It was shortly before he took over chairmanship of the Department, when I was an underling to him in the Advanced Laboratory. Lyman and his wife Rhea had, besides a little Volkswagen, a big blue Oldsmobile which had a small leak in the radiator at the time of this tale. So Lyman took it out to the then fairly new Triphammer Shopping Plaza to have it fixed at the Olds service garage. Parratts could retrieve it that evening if they wished. But came the time, Lyman found it would not be quite ready as promised; he could get it the next day. Fine, they had the spare vehicle. So after dinner the following evening they drove out together, and although Olds service was closed, they had no trouble finding the car out in the lot. Together they drove the pair of vehicles home, a couple of miles off. The next day, Rhea had a luncheon engagement with a number of her friends, and she would drive them home in the big blue Oldsmobile. In her travels about with her friends after the lunch, she began to smell things getting warm in the car and noticed the temperature indicator well When smoke started billowing up, she promptly pulled in offscale. steaming at the first service station she came to, that at the foot of Buffalo Street at Aurora, told the man that something seemed perhaps to be awry, would he look. So the good man obliged and raised the hood. "My God, lady! Where's the radiator?" His garage attendant took in the commotion and came over to see what was up. "I'll be damned! What'd she do with it?" It doesn't take any explaining to know what had gone on up at Triphammer Oldsmobile. I was in Lyman's third floor office during our laboratory instruction period when his phone rang. "Parratt here." Brief silence. Then slowly, "No ray--dee--ay--tor?" One can imagine during the silence, Rhea's breathless, "Lyman, I'm downtown and the car has no

radiator!" Well, it all turned out all right; the block was not damaged, but Ithaca Oldsmobile was very much embarrassed. I can still hear Lyman's equanimity: "No ray--dee--ay--tor?"

His composure presumably was also shown at an earlier time when he awoke on a camping trip to find a rattlesnake in his sleeping bag. And at another time when he awoke at great heat to find that his electric blanket-one of the first-had short circuited on him, and they were going up in smoke together. He was willing to try such gadgets and advances. He owned one of the first streamlined cars--a 1935 Chrysler, which had the profile of an overgrown postwar beetle Volkswagen. That Chrysler was not a popular model, although it made considerable sense.

Lyman understood the financial marketplace and still is probably as astute a financial expert as most bankers. Nothing he goes into or has gone into has been done halfway; it is all or nothing. During the days of this early period of the late thirties, he played with commodities and buying things on margin. He had to scramble at one point, however. He received word that his 1,000 bushels of wheat were being shipped; where did he want them. I told this story once, and it seems I had it wrong; it was two carloads of lard. Anyway, there was a tight situation in there for a while, whichever way it was. I can't tell where or how it was that he disposed of his lard (wheat?).

It was of interest to find in the library Archives in Ezra Day's stuff, the letter from Richtmyer as dean of the Graduate School to Day recommending the promotion of Parratt to an assistant professorship of Physics. It seems odd that it did not come from Gibbs, the chairman. It was the custom for promotion letters to go to and through the college dean to the president. Maybe it was another manifestation of Richtmyer vs. Gibbs.

Well, the graduate student days came to an end. Responsibilities began to change after the Ph.D. was earned and I became an instructor. During a leave of Professor Smith, as substitute adviser I got involved in an ion source thesis with Ted Forrester, one of Smith's students. After the war but long before the advent of lasers, Ted was with <u>his</u> graduate students at USC to do a noted and difficult experiment in light beats, mixing light from Zeeman-split atomic states, detecting the beats in the excitation of a microwave cavity. It was an idea conceived by Bill Parkins

and himself; Bill was another Smith student and valued younger colleague and friend of the foregoing graduate student days. As a graduate student one's responsibilities are largely to oneself--not entirely so, of course. But as part of a faculty, even as instructor, one's responsibilities should then turn very much more heavily to his students; or at least it seems so in my experience. It is probably for that reason that the graduate student days from this vantage point seem to have been among the happiest of a rewarding life. There was a lot of work and at times discouragement, but in sum, it was a great time; and it probably grows greater as I grow older. It was, I think, not atypical of the experiences of other graduate students in the department at the time.

My prewar period really came to an end when, one day, Gibbs came down to the laboratory and said he'd had a call from M. J. Kelly, president of Bell Laboratories; they were looking for some people. Was I interested? I had never really faced up to what I was going to do. I had always sort of thought I would wind up in a university somewhere, but it was not a fixed objective. I had a year earlier visited at Columbia, and Pegram had offered an instructorship, but I didn't see the advantage in it over what Cornell had offered me. And, besides, it was in New York City. Now, a year later, another New York possibility, and this time with industry. What could I lose by going down and looking at it? Why they ever thought to take me on I do not know; I had a terrible "crick" in my neck during my day of interviews. I couldn't straighten up my head all day, which could hardly have conveyed the impression of one long for this world. And I certainly did not delude them about whatever theoretical competence I may have had. But they still offered me a job and I took it, deciding that some years in industry could not hurt in a university job were I to decide later to do that and were it ever to be forthcoming. Salary was all of \$3,000 per annum with a two-week vacation.

As replacement for my vacated instructorship, the department hired another young Ph.D., one Gerald Tape. The war took him away before very long, and he wound up at MIT Radiation Laboratory, going on then to Illinois and subsequently becoming a U.S. Atomic Energy Commissioner and leading light at Brookhaven.

This change of scene came at a time of not just a single World's Fair but, rather, when there were two of them--one in New York and one in San

Francisco. The nation's railroads offered a round-the-country excursion for \$90 (!), allowing one to take in both fairs. One could go by any route one chose, even including boat from New York to Galveston and thence onward by train. I did just that; a "stateroom" under the stairs at \$1 a night for five nights and food were extra. But it was worth it. I added the fillip of my first airplane ride, in a DC-3 over the Sierras from San Francisco to home in Reno. So I had a short vacation before heading out into the world. I was standing with my family in the early dawn at the Southern Pacific platform waiting for the train to take me to Chicago and back east, when a paperboy with the <u>Nevada State Journal</u> came shouting the news: Hitler had invaded Poland.

INTERMISSION

Reminiscence is not necessarily history. What has been written thus far has been put down principally to give a flavor for life in the department largely, but certainly not exclusively, as a graduate student in the late thirties. It has spilled over in many places to modern times. And it is a view of the scene which is purely personal and obviously nonobjective. It is not certain that I can do otherwise with what should be history. And any "history" will not be entirely devoid of reminiscence.

Of the years before 1934 and of the years during World War II, I cannot speak knowingly of the department; I wasn't there. After 1939, for a year or so, I did come back to Ithaca now and then, particularly during the first year away, maintaining contact with a girl I had come to like. (It was during one such visit that I first met Boyce McDaniel over at the Home Ec cafeteria, where Physics was still maintaining its dominion over a round table. Mac had his Master's degree from Case which he had earned under Eugene Crittenden and was here to get his Ph.D. on the cyclotron.) Any history of the early days and of the war years will be mainly from the record and from what others have recounted. Of the postwar years the narrative will of necessity be quite different from what has been set down to this point. My viewpoint and perspective had changed, and my responsibilities were entirely different, both private and professionally. It will all be much closer to factual summary.

The Physics Department (along with the university) has at this time some 110 years behind it. A "history" of the department can rather conveniently be broken into three parts: there are the Early Years, say the first thirty-five or forty, breaking at roughly the time of the occupancy of Rockefeller Hall, occurring more or less midway through Professor Nichols' chairmanship; there are what might be termed the Middle Years, taking us through the chairmanships of Professors Merritt and Gibbs to the end of World War II, when dramatic growth in the department set in, in the beginning of what we might call the Modern Era. That was more than thirty-five years ago, which, in retrospect, seems perhaps not so modern a time after all. But they are convenient breaks. Many of those in the department making the move to Rockefeller Hall from Franklin Hall were

still around when my own interaction with the department began in 1934. And my permanent association with the department begins in 1946, shortly after the end of the war.

While the three periods may be reasonable categories into which to divide a history, I will not necessarily adhere rigidly to the division in chronicling events or citing names. If appropriate in one period to bring in something or someone from another, it will be done. But in a rough way, the periods seem natural. So let us consider the following to be a department history of sorts and see where it takes us.
EARLY YEARS

In the beginning, there was Physics. This may be said of the creation, and it may be said of Cornell University. In his History of Cornell, Morris Bishop has a footnote describing a trustees' meeting in Ithaca upon the return of Andrew D. White from a European recruiting and buying trip, some two months before the first students entered the new university in October of 1868. Present at this meeting besides Ezra Cornell and White, were trustees and professors, including one Professor Blake--Eli Blake--relative of Eli Whitney and the first head of the Physics Department (Salary: \$1,500). That Physics should be in at the beginning is not too surprising. Cornell, a practical man, had a bent for things mechanical and was well aware of what electricity could do in at least one area--that of communications--what with his activities in the development of the telegraph. He was more interested in applied technology than in science theory. For example, he had thought to have here the largest telescope in the world; forget how the Universe is constructed and evolved--just look at it. White, on the other hand, was a scholar, knew the value of pure science. In his speech on Inauguration Day, he speaks of "scientific study," but in spite of his German experience, in which country the university as a research center was established, he makes no mention of research, which today is as important as teaching in the University mission.

In Bishop's footnote on the above trustees' meeting, "Mr. Cornell reported delay on Building No. 2 (White Hall). The erection of a temporary wooden building was authorized." This presumably was the second "physics building," noted as "wooden building" in the Moler wall chart, "The First Fifty Years of Physics in Cornell University." During the first year, Physics along with Chemistry was taught in Morrill Hall which also served as a dormitory. Chemistry "laboratory" was in the basement, but machinations there were so foully odoriferous to those attending lectures on the floors above that the department was sent packing off across campus, with Physics in company. Our "wooden building" across campus was apparently that standing to the north of the approximate location of Goldwin Smith Hall. It housed the chemical, agricultural, and photographic

laboratories, according to Morris Bishop. "The more knowing visitors were impressed by the arrangements and equipment of the laboratories for student work." This was rare; by "1872 only six colleges had adopted it." Much of the equipment was picked up by White in the above mentioned trip to Europe. Bishop reports that a "stream of packing cases flowed to Ithaca, containing chemical laboratory apparatus, anatomical. architectural, and engineering models--and whole libraries of books." Grantham and Howe quote White from his autobiography: "As to equipment, wherever I found valuable material I bought it. Thus was brought to the university, physical and chemical apparatus from London, Paris, Heidelberg, and Berlin." They go on to guess that "White's purchases must have included some of the ancient pieces that still gather dust in the Rockefeller attic, e.g., copper vessels designed by Regnault (ca. 1860) when he was performing his classical experiments in calorimetry, gaseous expansion, and other work in the subject of heat." Sad to say, what was so in 1950 is no longer so; the attic is now mostly bereft of such.

Chemistry, with which department our own fortunes have been intertwined geographically and scientifically, was also in at the start. They had two professors, Caldwell and Crafts, the latter of whom was to become professor and acting president of MIT. Much of what President White purchased in Europe for Chemistry was from lists compiled by these two: standard European texts, journals, and apparatus. Professor Caldwell has written of the very early days; it seems worth including here his picture of 1868 Cornell life. He lived down on Buffalo Street--probably fairly far down since it must have been pretty much pasture up near the university tract. Caldwell's description is taken from Hewitt's history:

To reach the university it was necessary to climb a hill without sidewalks; to skirt Cascadilla, passing an old weather stained mill which stood behind it, and avoid skillfully the debris around these buildings; to descend into the gorge by ladders, and to risk one's life in crossing planks; to wind through the woods upon the north bank, and then pass through fields and over two successive ravines, and clamber over fences before the solitary building which constituted the university was reached. The new professor found his earliest task in the manual labor of unpacking these European purchases. The first chemical laboratory was established in the basement of Morrill Hall, in the large room on the north side of the entrance. The private laboratory of Professor Crafts, for

his own and special work of his students, consisted simply of one short table at the other end of the room. All the water supply was brought in pails, and the waste received and carried out in jars. The only ventilation was through chimney flues, and what did not escape through this uninviting exit ascended to the library room, which was directly above. Lectures in agricultural chemistry were given in a small basement room adjoining this laboratory, and the lectures on general chemistry in the large room on the other side of the middle hallway.

Caldwell describes the move across campus with Physics to the temporary wooden building which had been put up in the first winter. It was shared also with Mechanical Engineering and Botany. Gradually the other three departments moved to better quarters so Chemistry had it as its own for nearly ten years rather than the expected four or five, at which point Civil Engineering took it over.

So Physics started off under Eli Blake, somewhere in Morrill Hall, moving a year or so later with Chemistry over to the temporary building across the guadrangle-to-be. As we can see from Caldwell's description, things must have been a little rough, with primitive accommodations, domestic and professional. Blake stayed only a year (according to Hewitt, Moler's chart indicates three; no guarantee is made for the chronology in here) leaving to go to Brown University, where he remained until he He was apparently a man of strong mechanical tastes and retired. aptitudes, fond of experiment. He published one paper which attracted some attention, on a method of photographing acoustic vibrations. He was succeeded here by one John J. Brown, who went to Syracuse after but one year, during which year he served students as Methodist pastor as well as physics teacher. His successor, Francis E. Loomis, also lasted but one year. However, because of illness, he neither taught physics nor preached the gospel.

In Vol. Xi of Scribner's "Dictionary of Scientific Biography" following the entry on Röntgen, is one written by Daniel Kevles on Ogden Nicholas Rood. In Vol. XII is an entry for Lewis Morris Rutherfurd, an independently wealthy astrophysicist and spectroscopist, for whom Columbia's Rutherfurd observatory atop Pupin Hall is named. Rood himself was a professor of Chemistry at Troy University before he went to Columbia as professor of Physics, where he did good experimental work in optics,

electricity, and acoustics. He was a talented painter and wrote on color in his "Modern Chromatics," something of a bible for impressionists, particularly of the pointillist school. The work of such artists he decried: "If that is all I have done for art, I wish I had never written that book."

Anyway, in 1869 Rood wrote a lengthy letter to Rutherfurd, of which a copy of the hand written missive has come to the Cornell Library archives. It is of considerable interest for the light it throws on our early trials and is not totally out of place in Physics Department history, and seems not to have appeared elsewhere.

Rood had had a few days previously a visit from Eli Blake, who stopped by on his way to Providence and Brown University to start a six month paid leave of absence from "the Cornell Univ.," his resignation "ready as soon as the trustees are willing to accept it." He has related to Rood "a number of items of their performances which were quite refreshing." There was the purchase of apparatus. After much consultation, the trustees "voted \$3,000 for the cabinet of apparatus which was to be superior to anything else in the world, and as the President is strictly a literary man without any scientific culture, Blake innocently supposed that he would be allowed to take this matter in charge." The President, however, on heading shortly for Europe wrote to Blake asking what apparatus he should bring home. Blake replied that there was only enough money to get a scanty set of those necessary articles which could be best obtained in Boston, and requested him to bring nothing except a few small articles, one of which was a plane mirror of speculum metal. The President went to Paris and bought Duboseq's (prestigious French optical house, p.h.) complete projector apparatus at a cost of \$2,400, "but as he did not buy a battery and as they couldn't afford to run one if they had it, the apparatus though quite ornamental is not of the slightest use. Meanwhile, the case of small articles ordered by the President came, and was unpacked in the presence of several of the faculty. Blake's horror was great when he saw the speculum! It was not of the kind usually found in physical cabinets, nor of a nature which would allow of the use in his lectures; the President had ordered the article from an apothecary!--If Mr. Cornell gets run down, and wants a place, you might take him into your work-shop as an assistant. He is interested in mechanical matters, and determined that his students shall have as good a mechanical education as he had himself--so the future work-shop and self supporting students are a great hobby with him. Blake talked to him about the time necessary to make a good workman, etc., to which Cornell replied: "Mr. Blake you altogether overestimate this; all that I know about tools I learned in two weeks! The

University is, as is usual, set on a hill and stands out alone by itself, being two miles away from the town of Ithaca. Quite near it, but separated by an impassable ravine is the famous Cascadilla House, which barn has been hired as a big boarding house for the students and professors. There they live together much like respectable pigs, though of course not nearly so comfortably. No female servants are allowed on the premises, so the professors' wives who have babies take them to the table, where their infants act with perfect independence and freedom from restraint; the students are the waiters and those who are able to conquer the tough viands do so."

And then Rood describes the problems associated with Goldwin Smith of Oxford, the service of whom President White had secured. Where to put him, certainly not in Cascadilla House. Mrs. Agasiz told White that he could board at the hotel just as she and Mrs. Blake did. No, that would never do; but there was this Chemistry professor, Crafts, "who had married a rich New York girl, bought a house and was living in style; Mrs. Crafts must take Goldwin." Not as a boarder, but as a friend. Mrs. Crafts handled the President all right on that one; she had company coming for the next n months. But in the end, Goldwin wanted to get "acquainted with the young working men" of this country anyway. So, as Rood goes on,

"He (Goldwin) was allowed to go to this grand piggery, where by the way the President lives, but Goldwin could not be permitted to eat with the students--that would be too democratic. So the halls were carpeted for Goldwin, to the immense indignation of the ladies then resident, who had in vain implored for a single strip of carpet in the middle of the great dining room."

Then there was the matter of Smith's lectures. He was to lecture to all the students and a thousand townsfolk but "with the meanness characteristic of the whole affair the President had the audience charged 30 cents a head" (antedating Physics Prof. Anthony's like practice later, p.h.). It was a flop; Smith had no voice; and the audience felt cheated; following audiences got thinner and thinner. Smith was pretty sad; had he done the right thing in coming here? Then White had the bright idea of having him lecture on Oxford. A free lecture.

The Ithacans crowded in, and pleased with the idea of a free lunch, actually applauded when he appeared on the stage. This acted on him like punch, the strings of his

tongue were loosed, and he is said to have made a fair performance. He was pleased enough that he ordered his library send over from Oxford. Rood had apparently seen Smith in New York. "I had a long talk with him about the Cornell the other night at the Century--poor man, he needs punch, always punch, much punch, and then he only gets up about to the level of ordinary cheerfulness. Such however has been the matchless system of advertising and puffing with which the Cornell entered on this existence, that Goldwin is constantly receiving letters from young Englishmen asking whether it would not be advisable for them to come over and enter the Cornell, instead of going to one of their own institutions! The professors receive various salaries ranging from \$1,000 up to \$2,000; they made a bargain with each man and got him as cheap as they could. Hence it actually happens that the highest salary is paid to the professor of horsedoctoring, a gentlemen used from his associations to the rather sharp practice. His wife is said to be a woman who no respectable housekeeper would be likely to hire as a cook; 'Sure and ye turned my Johnny out of the room did ye; and its myself would like to know what right ye had to do it, when my husband is the biggest man among ye, and gets the highest salary,' was the remark to Mrs. Wilson, who had put Mr. John out of the room Christmas night for conduct which had become quite unbearable."

As to courses: "With regard to the course of instruction it may be sufficient merely to state that Agasiz lectures to the Freshmen in zoology while the Sophomores listen to Prof. Evans on Quaturnions! (sic). The manual labor scheme has proved a gigantic failure, and young men who have spent their all in paying their expenses to lthaca are starving there by the fifties. The whole concern is ruled down to minute particulars by Cornell, who has enough ignorance and stupid ideas to overstock six ordinary men, but having with all great tenacity of purpose keeps them perpetually in hot water with nature."

On the Ithacans: "The Ithacans were described to me by Goldwin as being very 'Arcadian.' They are firmly convinced that the scenery around Ithaca is quite superior to that of Switzerland, use kitchen stone-china at their parties, build very large barns, and live comfortable lives."

And then before discussing with diagrams a problem in physics relating to the "refraction" of a heat front moving across an oblique boundary between two dissimilar materials, Rood ends his remarks on the University with his version of a student prank also described by Morris Bishop.

At the end of the first term, in the middle of a very cold night, having locked the bell ringer in his room, students built a big bonfire outside Morrill Hall and then rang the college bell. "Fire companies hustled up the hill and Brother Cornell, roused by his wife, rushed out endangering his lungs and limbs. His rage was great, and he told his friends that he was on the point of ordering the firemen to go into the building and clean the young men out and turn them adrift to seek such quarters as they could find. The next day he reportedly said he was sorry the whole thing had not burned down and in the Trustee meeting offered a reward of \$5,000 'for the detection of the perpetrators of this outrage!' With which I think it will do to leave the Cornell, this child of the unpaved districts, and pass on to something else."

Thus Rood. It is of some interest that our man, E. L. Nichols, would in 1909 write for Rood in the National Academy of Sciences Biographical Memoirs. Whether he ever knew of the latter's opinion of "the Cornell" and its founders is not known.

End of disgression; on to our history.

Chemistry dominated Physics at the beginning; they had more and better equipment than did Physics, which had mostly optics. We did have a photographic laboratory in the old wooden building. In it lived one Frederick E. Ives, developing a three color half-tone process. Morris Bishop describes Ives's contribution to the first campus humor sheet:

The first humorous magazine was <u>Cocagne</u>, which existed only from April to June 1878. This publication is perhaps less remarkable for its contents than for the printing of some of the cuts, done by a photographic stereotype process invented by the University's photographic technician, Frederick E. Ives. This self-educated countryman refused an instructorship, migrated to the Great City, and invented the three color printing process, the modern form of the binocular microscope, and, it is claimed, the half tone screen.

We might also add that he sired a son, Herbert E., who wound up at what was later to be Bell Laboratories, where he demonstrated first the reality of Einstein's transverse Doppler shift and did some early work in wired television, demonstrating a system between New York and Washington in 1924. Whirling discs. The son established the Optical Society's lves Medal in honor of his father. The latter had heard Anthony's

lectures, had close personal contact with him, and was much influenced by him, according to lves the younger.

The real beginning of the department starts in 1872, with the arrival of the said Anthony--William A. Anthony--from a professorship at lowa State. He had been planning a summer of study "back east" when the lowa trustees required his presence to supervise the plumbing of a new He did most of it himself. building. Morris Bishop notes that "This experience persuaded him to accept a call to Cornell." It would be interesting to know who placed the "call." He was to stay here for fifteen years. In the same year, the one-man department moved back across the "quadrangle" from the temporary wooden building, leaving it to Chemistry. "We" were apparently housed in McGraw Hall and subsequently in expanded space in White Hall for about eleven years, until 1884, when we moved into Franklin Hall--Benjamin Franklin, "The First American Electrician," it says under the bas-relief of Ben over the entrance. This fine structure. emblazoned over first and second floor windows with the names of great workers in physics (Newton, Galileo, Galvani, Volta, Oersted, Ampere, Faraday, Young, Brewster, and others now covered with ivy. No Maxwell?) and with bas-relief busts of great chemists (Priestley, Dalton, Berthelot, Lavoisier, etc.) above the second floor, was clearly designed for Physics and Chemistry. Physics had the first floor and Chemistry the upper two floors (in the hope that their fumes would stay aloft?).

The late Professor William Ballard once informed Professor Joe Rosson (both of the E.E. School--the school inherited Franklin and occupied it for a long time) that two brick piers in E.E.'s old, open, second-floor laboratory once had mounted on them galvanometers of the Physics Department. The piers, capped with marble, presumably went to bedrock to avoid building vibrations and are probably those to be seen today rising in the middle of the west studio. (Perhaps further reason why Chemistry was sent to the "third" floor. There are so many levels and odd stairways in the edifice it is difficult to tell at what level any "floor" lies.) Rosson says he was also informed that there was water motive power (!) delivered by belt from the gorge up to West Sibley; Bishop also refers to this. From West Sibley there was presumably a pulley shaft extending underground over to Franklin. Except possibly for floor ducts in Sibley, there is not much, if any, evidence left of this.

The structure has gone through several appellations. The 1883 edition of the student yearbook, "The Cornellian," features on its frontispiece an engraving of "The Physical and Chemical Laboratory of Cornell University." Some two years later one finds, on searching through trustee records, that we took the name Lincoln Hall in honor of the man in whose presidential administration the Morrill Land Grant was enacted. Some six months later apparently that name was given to the Civil Engineering building across the quadrangle; it still bears that name but is now devoted to music. It was with that name shift that our building became known more appropriately as Franklin Hall, and the stone slab over the entrance announcing it as such was set in place. In October 1980 it was announced that the venerable old structure was to be renamed Tjaden Hall, in honor of a prominent lady architect, Olive Tjaden VanSickle, apparently noted for her work since graduating from Cornell in 1925. It's going to be a lot harder to pronounce. Clearly, the marble slab over the entrance proclaiming it to be Franklin Hall will someday have to come down and be replaced with something more suitable for the honored architect. Perhaps it can come over to us in Clark. We will not have lost all connection with the edifice, however, with all those bas-reliefs and names inscribed above the several window openings.

Chemistry and Physics went their separate ways when Chemistry moved from Franklin across the road to Morse Hall, a deplorable red brick structure resembling nothing so much as a 19th century grammar school building; the art museum now occupies the site. This left Physics alone to occupy the whole of Franklin Hall. Morse subsequently burned, fortunately, or else we would still have it--even so, its remains still existed as something of an art gallery until at least the mid-fifties. So, from 1916 to 1923 Chemistry had to make do with spaces in various places. It moved into Baker Laboratory in 1923, the laboratory being dedicated and the anonymous donor identified on the day Livingston Farrand assumed the presidency.

Physics stayed put there in Franklin, albeit uncomfortably put, until 1906 when it moved into the new, elegant, and specially designed Rockefeller Hall up on the hill, at that time one of the nation's outstanding laboratories for physics. While not architecturally great, it still presents a pleasing external appearance, and it was a great improvement over

Franklin and no comparison at all to the first three real buildings we occupied, Morrill, White, and McGraw. Professor Goldwin Smith, an Englishman, considered those three redeemable "only with dynamite." Today they are National Treasures.

How Franklin prevailed over Priestley in the naming of our first real "home" is not known, but the affinity of Physics with Franklin is clear. He was America's first electrician and the interests of physics in the late 1800's were largely in electricity--at Cornell, very much in applied electricity, motors and generators included. In a lecture on Franklin to the American Philosophical Society (which Franklin founded) on the 200th anniversary of the inventor's birth, our E. L. Nichols describes an electrostatic motor devised by Franklin; the cagey inventor saw no problem in building one "capable of carrying a large fowl, with a motion fit for roasting if set before a fire." Applied physics, including heat. In the same lecture, Nichols cited Franklin's invention of the so-called Franklin plate--really a planar version of the Leyden jar, with electrodes on either face of glass sheets. With these, Nichols went on, Franklin demagnetization, and reversal of magnetization. observed the magnetization of steel needles near the discharge current from such condensers. He was getting close; the deflection of magnetized needles by a current would not be discovered until 1820 by Oersted, seventy years Franklin was all right; in optics he invented bifocal spectacles; in later. heat, his stove; in acoustics, a glass harmonica, in which glass cylinders were frictionally stroked continuously by discs of leather. (Previously they were bowed or rubbed with wet fingers.) A German maker added a keyboard of sorts, and Mozart composed a rather ethereal sounding concerto for the instrument. (Listen to it at the Corning Glass Center.) Professor P. G. de Gennes, an Andrew D. White professor-at-large, in his first 1980 lecture gave Franklin credit for making the first reasonable estimate of a molecule's size by determining how much oil was required to cover the surface of a lake with its film. Not bad. So independently of his electrician status, it was fitting enough that our first real home was named in his honor.

Anthony carried on the electrical interests of Franklin and is best known for building, with instructor and former student George Moler, the first Gramme ring dynamo in America--at least the first self-excited

model. It was designed from a published description of the European machine. With a senior student, Harris J. Ryan (later a faculty member) as assistant, and, undoubtedly, Moler as contributor, he also built an impressive "precision" current meter, capable of handling up to 250 amperes--a large tangent galvanometer housed in the "magnet observatory"--a non magnetic shed which stood about where the west end of Rand Hall is now. The "observatory" was held together with copper nails and the stove was even of copper. The installation was a source of departmental pride, being mentioned in its announcements up to the year 1900-01. The usefulness of the instrument came to an end when the Ithaca Railway System ran its track too close by--along East Avenue and Parts of the great device are in the across Triphammer Bridge. Rockefeller Hall "museum." It was the best of its day. Calibration data are in a notebook in the library Archives. This instrument seems to have been more highly regarded by the department in those early days than was the generator. A description of it was part of the Department Announcements. In the October 1885 issue of The Electrician and Electrical Engineer, Anthony describes it in some detail in an article "The Mammoth Tangent Galvanometer at Cornell." It may be recalled that the principle of the tangent galvanometer rests on determining the angle a magnetized needle takes when acted on by the earth's field together with a field at right angles to it set up by the current to be measured. The galvanometer was mammoth; an engraving with the article shows it to be operated by a two man team, one man at the telescope and the other on a platform above the observer. For precision measurements, the earth's field cannot be taken as constant from day to day; it would appear that the second operator is helpful in the determination of the earth's reference field. For those who never encountered the instrument in freshman physics (all too likely these days), this can be done absolutely without knowledge, or indeed need, of (Refer to that war-horse of old experimental physics, any current. Glazebrook, Dictionary of Applied Physics, II, 532 ff, on magnetometer measurements.) The comparison field produced by the current to be measured was produced in the behemoth by three pairs of coils arranged in the Helmholtz configuration, and thus of different diameters, the larger pair two meters across, each a single turn of copper rod nearly 1.9 cm in diameter. The coils could be used variously: separately, all in series, or

differentially, so that Anthony could measure currents from a few milliamperes up to his 250 amperes.

Curiously enough, the article immediately preceding Anthony's in the Electrician is a defense against a criticism leveled by one Professor Blake.

For further details on the instrument, the interested reader may hunt up a copy of E. L. Nichols' own book, <u>The Galvanometer</u>, a text no longer required of our students in physics. Indeed, the status of galvanometers these days may be surmised on learning that within the month I was asked by a laboratory technician and a third-year graduate student (!) what the object was they were holding. It was a very clean, late model, Leeds and Northrup, Type R galvanometer--a really pretty specimen. Sadly, their object seems to have been fashioned now into a desk lamp.

Anthony came from Rhode Island, graduating from Yale (Scientific School) after three years at Brown. As mentioned, he came to Cornell from Iowa State, before that having been at Antioch and Delaware Literary Institute (!) in Franklin, N.Y. He had the highest salary then paid to any faculty member at Cornell and had President White's assurance of departmental support. This support was not all that great, but his lectures were so popular that he gave them to the public downtown, charging admission, which enabled him to augment the departmental support. He must have been good. His successor, our E. L. Nichols, wrote of him: "With his first lecture, a miracle happened at Cornell. He won his students in the first ten minutes" by the introduction of demonstration experiments with the lecture.

Thus began the demonstration lectures which have "so long been a cherished tradition of the Department" in the words of Howe and Grantham. Laboratory experiment was also implemented, in spite of scarcity of apparatus and of space. His "laboratory" was a bit of territory under a McGraw Hall stairway, according to Howe and Grantham. The building, along with Morrill and White Halls, was both classroom and dormitory. Physics later got larger space in White before the move into Franklin. Handy to have one's quarters so close to the lab.

While his best-known achievements locally were undoubtedly the construction of the dynamo, with Moler, and of the tangent galvanometer,

also with Moler, probably more important to the Physics Department and to physics generally was the enthusiasm he engendered in his students who went out from here to spread the word. He must have felt great obligation to do right by his students. There was an Anthony son who drowned while swimming, but Anthony never missed a class.

In 1885, he proposed a curriculum in Electrical Engineering. There was some opposition from the trustees, but White was enthusiastic (Howe and Grantham indicate that it was White's idea), and it was instituted as part of Physics. In that first year of its existence, we granted the first E.E. Ph.D. in America. One can surmise that requirements were considerably different than is the case today. It was a popular discipline, and it has remained so, contrary to the view of Anthony's contemporary, Professor Lovering of Harvard, that the current (no pun) interest in electricity was "only a spurt." Prior to that, Anthony had recommended that the university invite the Edison and the U.S. Lighting companies to test their machines at Cornell, and in the first year of Electrical Engineering there were three machines sent by manufacturers for test. Here, as Bishop writes, "The modern concept of contract research was faintly forecast."

The newly established discipline of Electrical Engineering, as Howe and Grantham report, was essentially a second wing of the fledgling department, and it was not until 1889 that it became a separate department under Harris J. Ryan, who was an Anthony student and an instructor in physics during the second year of Nichols' leadership. After separation from Physics, Electrical Engineering was housed in West Sibley. Ryan ultimately went to Stanford. But the tie between Physics and Electrical Engineering was so close for so many years that Physics "may properly claim a fair share of credit due Cornell for its many prominent Electrical Engineering graduates," to quote a 1930 department document.

There has been some uncertainty in the past over whether the discipline of Electrical Engineering began at Cornell. There is a long letter in the Archives to Merritt from Anthony's one year colleague, D. C. Jackson. In it he discusses the question, addressed to him by Merritt. It seems fairly clear that MIT was one year earlier with their program which, like Cornell's, was an offshoot of the Physics Department before it

went off on its own. However, in Tokyo, Jackson says, Ayrton and Perry (what were <u>they</u> doing in Tokyo?) had started something in applied electricity about a year before MIT's development.

The late E.E. professor H. G. Smith (during his Cornell graduate education preferring a Physics assistantship to one in E.E.) has written a history of his school, in which is discussed its origins. The authoritative history of the general development of Electrical Engineering out of Physics, largely at MIT and Cornell, was published in an issue of <u>Physics</u> <u>Today</u> (October 1983) by Robert Rosenberg of Johns Hopkins. More detailed than that presented here, it rather supports the present brief outline.

By the year 1885, Cornell engineering in the guise of Mechanical Arts and of Civil Engineering was well under way, if not harmoniously so. The mechanical side (a curriculum including shop practice, drawing and "draughting," the steam engine, etc.) was under the department "deanship" of one J. L. Morris. At the time there was considerable hullabaloo over the man's qualifications and performance. This is revealed in letters in the Sibley College file of the Archives. There was a committee to look into it: there were letters pro and con both from former students and from faculty. Apparently, not much came of it; he stayed on until retirement in Of interest to us is a letter to President White from Anthony 1904. bemoaning the presence of Morris, chiefly for his incompetence in matters in which Anthony felt a dean should not intrude. He should not change grades awarded students by other professors, nor excuse students from others' class exercises, nor pass on theses written on subjects entirely outside branches he teaches and of which he has no knowledge. "Why should a thesis written by a candidate for the degree of Mechanical Engineering upon an electrical subject, the work upon which it was based having been performed entirely under my direction, go to Prof. Morris at all?" he asks. It was most embarrassing to take Warner of Cleveland (Warner and Swazey?) through the shops and to have him disgusted with the slovenly condition of tools and the old-fashioned "slop shop" way of doing things.

Not very well known is Anthony's contribution to Ithaca transportation. In 1887, the year he resigned, the Ithaca Railway Company came into being with the short-run trolley system from the Lehigh Valley Station up to the Ithaca Hotel, a system which Bishop says was "designed

and supervised" by Anthony. He was also into telephony. A year after its debut at the 1876 Philadelphia Exposition, where his own generator was also to be seen, Anthony got a pair of Bell's instruments into the department; he and Moler conversed over one and a half miles of wire "as easily as in the next room," according to Bishop. And two years later there was a campus system of sorts in operation. At one point after this had been accomplished, Anthony connected the phone in Comstock's house up on the hill to a concert hall downtown and brought "music" into the home.

One sees in Anthony a practical man--perhaps more the engineer. His concept of teaching is revealed in a letter to the president in 1873. Bishop writes of this in his history: "He [Anthony] insists on the practical applications of science and points to dreadful errors made by builders and industrialists through ignorance of physical laws. He demands a proper laboratory, wherein the whole class can perform experiments in areas of physical concern: friction and lubricants, strength of materials,--" right on down to "heating power of fuels, condensation of steam in iron pipes,-and photography. Although he pays lip service to theoretical physics, his program seems to have been determined by practical needs, and to be concentrated on applied or engineering physics."

Supporting the view that Anthony was primarily interested in practical matters, Kevles, in his book <u>The Physicists</u> quotes an article in the proceedings of the AAAS written by Anthony in 1887: "In this country, men devoted to science purely for the sake of science are and must be few in number. Few <u>can</u> devote their lives to work that promises no return except the satisfaction of adding to the sum of human knowledge. Very few have the means and inclination to do this."

Still less widely known is what Anthony did for the Ag College. Agriculture at Iowa State was in such turmoil that one Professor Roberts resigned. Anthony, having been there, "recommended Roberts for the agricultural professorship here, and the future of the Cornell department was assured," to quote Morris Bishop. From the department came the college; hence, today's Roberts Hall, now facing demolition or refurbishment.

There is little doubt that Anthony <u>should</u> be best known for the impetus he gave to the establishment of one of the country's best physics

departments, which in turn was very influential in the spread and growth of physics throughout the country. Before Anthony's day, physics was taught from textbooks and from textbooks alone. Anthony emphasized how physics deals with real concrete phenomena and thought the student should have the opportunity to see and work with them himself--thus the practical laboratory instruction he introduced. MIT and Cornell share the distinction of having introduced that methodology. Cornell, under Anthony, was also one of the first places to establish a course of demonstration lectures on physics; charging for lectures to the public was perhaps a further innovation. However, innovation notwithstanding, in a long absence of President White, during which he was replaced by Vice-President Russel, the institution declined; there was a fair amount of student complaint. Of this Bishop writes:

With some of the complaints the faculty could only agree. The Physics Department, well staffed but shamefully housed and insufficiently equipped, was a disgrace to a university with a special regard for technology. Anthony wrote White (6 June 1880) that Cornell's offerings and requirements were below those of Stevens, M.I.T., and Pennsylvania. "We are twenty years behind the times," he said.

After having been here but a short time, Anthony took on an assistant, M. M. Carver, about whom nothing more has been found. When he left, two and a half years later, in 1875, an Anthony student, George S. Moler, was hired as an instructor. He stayed with the department until his retirement in 1917, when he became Emeritus; he died in 1932. He was made an assistant professor in 1880 and a full professor in 1911; promotions came more slowly in those days than is usual today. There were other students of Anthony who became important, not only to physics at Cornell, but to the growth of physics in the nation at large. Besides Moler, two others of local interest were E. L. Nichols and E. G. Merritt, both later to become Cornell Physics Department chairmen (Nichols being Anthony's choice to succeed him as head of the department), both men influential in the education of many leading physicists.

Howe and Grantham indicate that the teaching was done only by Anthony and Moler. But there was Carver, and Moler's "Fifty Years" chart indicates that in Anthony's last year at least, they were joined by an instructor, D. C. Jackson, who became head of Electrical Engineering at Wisconsin, subsequently at MIT. He was here one year, another one year instructor, J. H. Pratt, taking his place during Nichols' first year on the faculty. As with Carver, we don't know what happened to Pratt.

Anthony resigned the "Chair of Physics" in 1887, after fifteen years at it. President White had resigned two years earlier and was replaced by his protégé, Charles K. Adams, who was not enthusiastic about supporting scientific work. Adams came in expressing distress at Cornell's lack of "scholarly learning"--the time for the Classics and the Humanities had come. Anthony and Adams just could not see eye to eye. Adams was apparently somewhat secretive, in contrast to Anthony; rather than consult such friends as White, Anthony just resigned. This is how Jackson saw it in writing to Merritt many years later about it. We'll refer again to this letter. Anthony had become more and more interested in electrical engineering and he thought it was time for new blood to run things, having Nichols in mind. When his resignation became known, Professor Gage bemoaned the fact to him. Anthony told him that he thought the Physics Department would go much further under Nichols than it ever could under Nichols, by that time, had four years of graduate study and himself. experience in Germany, had returned to this country to work with Rowland at Johns Hopkins and with Edison in New Jersey, had taught in Kentucky (Central University) for a couple of years and at the University of Kansas for four; he was a natural to take over the department leadership. Anthony left to work in the electric industry (Mather Electric Co., Manchester, Conn.). He did consulting engineering for a period but went back to teaching in 1894--at Cooper Union, where he taught until he died in 1908.

Things were not too plush in Adams' regime, a circumstance which may be gleaned from the following communication to Nichols after Anthony left. It is from the President's Rooms, dated November 1, 1889, signed by the president.

Dear Sir:-

The following are details of appropriations made by the Trustees for the Department of Physics for the year 1889-90:

Janitor, gas repairs

\$1800.

| Additions to equipment in workshop | 150. | |
|------------------------------------|-------------|--------------|
| Photographic apparatus | 125. | Apparatus in |
| electrical engineering | 300. | Apparatus in |
| General Physics | <u>325.</u> | |
| Total appropriation | \$2700. | |
| | | |

Very truly yours, C. K. Adams

Closely coupled with the name of Anthony has been that of George Moler. As may be guessed, he is best known for his partnership with Anthony in constructing that first "practical" dynamo in America. The priority is suspect. Cornell used to claim in no uncertain fashion that it was the first of any description, but a voluminous correspondence between members of the department (mostly Bedell) in the late twenties (see <u>Science</u> during 1928) seems to indicate that there had been some earlier very small machines utilizing permanent magnets instead of the self-excitation of the Cornell version. At least Cornell's was practical and was used for serious purpose. Here is how Grantham and Howe describe the development:

Moler had no more than graduated when he joined his professor Anthony, in the construction of a ring armature dynamo, a project begun immediately upon the receipt in this country (in 1875) of the details of a successful machine built by Gramme, in France. In 1876 this (Cornell's) Gramme dynamo was exhibited at the Centennial Exposition in Philadelphia, where it was operated by Mr. Moler. (The dynamo was driven by a steam engine taken along, from Ithaca. When the steam engine broke down, Moler became the "prime mover", using a hand crank. He could thus get the armature up to just enough speed that a small wire, used to short-circuit the whirling machine would glow briefly.)

The Gramme machine, known to those who associated with Professor Moler in his later years as the "Moler Dynamo" or the "Moler Motor," has been the department's prize exhibit since its display in Philadelphia, followed by its long-time and varied use on the Cornell campus. For several years it operated arc lamps that lighted the campus, thus creating a new "need"--street lighting by electricity. The local belief, long held, that this dynamo was "the first dynamo built in America" seems not supported by the available evidence (see Moler's departmental folder), though it was certainly "one of the first," and probably the very first Gramme dynamo built in this country. However,

there seems little doubt that Cornell's outdoor electric lighting was the first of its kind in this country, probably the first anywhere, certainly antedating the electric street lighting in Paris or London.

The Cornell dynamo supplied current for various departmental uses, such as producing oxygen and hydrogen by electrolysis, and charging "storage batteries" for laboratory use. Later, when more powerful dynamos became available, this versatile old machine functioned as a motor to run the lathes of the physics shop. An astonishing novelty at Philadelphia, it was an historical show-piece at the Louisiana Exposition (St. Louis, 1904) and at the Century of Progress Exhibition (Chicago, 1933 [also 1934]). The machine remains, even unto this day (1957) a treasured piece of lecturedemonstration apparatus, being each year shown in operation as both dynamo and motor to hundreds of students in Cornell's introductory physics courses.

Unfortunately, no longer do hundreds of students have it demonstrated. It occupies a center place in the department "museum." There is talk of placing it in the Smithsonian Museum, along with some other artifacts (including the two great coils from Anthony's galvanometer), but no decision has yet been made. After exhibition at the Chicago World's Fair, there was serious consideration given to placing it in the Museum of Science and Industry out there, but it was decided not to do so. There was almost solid unanimity against this move in the department.

Grantham and Howe say that the generator was used eventually as a motor for driving the lathes of the Physics shop. In his own discussion of it, Nichols proudly indicated that it was still serving the department, driving the machines of the students' shop. What he called the students' shop cannot have been the establishment we knew as the student shop, else it had seriously deteriorated. We would have been happy to have had an electric motor of any vintage driving our primitive lathe down there.

With the dynamo such an important element of our early days, something further might be said regarding it. Upon completion, it was first set up in Morrill Hall and was driven by a 4 HP "petroleum engine," then moved to McGraw Hall, where it was driven by steam, and then to the Sibley Laboratory, where it was belted to the water power(!) supply driving the shop. The first lighting was from two lights in front of Morrill Hall, after which a single arc lamp was installed on the steeple of Sage

Chapel, no longer rising heavenward, to light travelers on their way from the Cascadilla bridge to the central campus. It was pretty dark down there; two gas lamps were to be installed at the bridge, not then the lovely stone arch we have there today. To farmers on West Hill, the flickering beacon above Sage Chapel was a sign in the night for several years that our institution was still there. The power line from the generator was underground--that was the first such in use anywhere, I believe. Some legend has it that the copper wires inside the gas pipe were wrapped with the silk of garments donated by Department wives, including the wedding gown of Moler's wife. The pipe was filled ("pumped" in Nichols' description) with tallow, still in good condition twenty years later when dug up. How the filling operation was managed is not made Professor Gage tells of remembering Anthony in 1875 as a clear. "practical man in an old laboratory coat and a pot of grease in his hands pouring it into the pipe carrying the electric wire to the spire in the chapel." This must not have been done indoors, for Bedell writes in the Sibley Journal on "The Beginnings of Electrical Engineering at Cornell," that the "cable was made by winding (wrapping) bare wire with muslin [silk indeed!] and then passing the wire through gas piping, which was then filled with tallow poured in through temporary T-connections at each pipe joint." An awkward business--but it seems to have worked, and Cornell was in there very early with the technology. Insulated wire was not available in 1875, and the wire of the generator itself they also wrapped with cloth (silk?).

The flickering beacon atop Sage Chapel was "an open arc lamp, and its carbons were not copper-cased pencils but flat plates, so that the arc, instead of traveling continuously around and around to fit irregularities in the wear, jumped back and forwards (buzzing and spluttering) along their edges, from one point to another of approximate contact. Polychromatic flames resulted, startling to behold." (Charles Hull, '86, quoted in the <u>Alumni News.</u> October 22, 1931.) Arc lamps were considered an important development in the Department, several turn-of-the-century annual reports alluding to the importance of arc development.

The dynamo, rated at 20 amperes and 150 volts, was driven at the Philadelphia Exposition by a straight-in-line engine built in the Sibley shops by Professor Sweet and his students, one of whom was a certain Smith, later acting president of the university after J. G. Schurman.

It was certainly not the first lighting system anywhere that we had here on the hill. It is not even clear that the lighting was the first in America; Cleveland seems to make some such claim. At least we can say (we think) that we had the first illuminated campus in America and the first underground "cable." The first real working use of the machine was in the electrolysis of water to produce oxygen and hydrogen for "lanterns" of the University." Lecture room lanterns (read projectors) were supplied with "lime light" for which oxygen and hydrogen had to be prepared, for a flame presumably. Lime light? A sentence in my Encyclopedia Britannica, under Lighting (Gas Lamps) informs me that in "1838, W. H. Fox Talbot (of photographic fame) discovered that even the feeble flame of a spirit lamp will heat finely divided lime to incandescence." The later Britannica of the Clark Library does not mention this, but under Stage Lighting, it says the lime light was invented by Drummond in 1816 and was in wide stage use by 1860; no indication what it was. Apparently a flame is directed at a block of calcium oxide, bringing it to incandescence, not unlike that produced when the oxide of thorium is heated by a flame in today's Welsbach gas mantle used by campers in their gas lanterns.

The <u>Ithaca Journal</u> of December 5, 1878, reports on an electric lamp (must be arc; Edison's contribution did not come for another year) put in Wms. Bros. Machine Shop; the place was crowded with spectators, who were a bit disappointed at the luminosity. How our generator got involved is not said, but the report tells of Anthony's obtaining the drawings from abroad. The reporter seems not to have heard of Faraday: "Our readers who are 'up' in physics know that all machines used for generating electricity by friction are essentially the same in construction, differing only in some unimportant 'feature' and in name."

At the 1876 Exposition, the machine was new, important, and a novelty. At the St. Louis Exposition in 1904, it was historical. A jury there studied the various claims on priority and decided to award the Silver Medal to Moler and Anthony. Some doubt was later raised. This troubled Bedell; he had been on the jury, thought they had really studied the evidence, and so settled for the Cornell machine. In his 1928 <u>Science</u>

correspondence, it becomes clear that it was probably not first; at least Merritt would no longer claim that it was.

Moler was certainly the most active department photographer of the He gave a laboratory course for both researchers and early days. amateurs, and "shot" film. This was in the days before there was a film company which, as Howe and Grantham remark, "did everything but press the button." There is of his, a fragile, old cellulose nitrate base, 35 mm film in the library Archives, of waves breaking over a pile, a crew rowing toward the camera, etc., according to the scrap of paper folded with it, perhaps in Moler's writing, or that of Professor Stainton of Speech and Drama who donated it. Moler's photographic laboratory, which was rather extensive, was in the north end of Rockefeller's third floor, where the audio-tutorial course is now located, which Arts and Sciences took over in 1982. Temperature control must have been difficult, or maybe they didn't worry in those days about such niceties. He made the first movies in Ithaca well before "The Perils of Pauline." He anticipated Disney. Professor Stainton tells of Moler taking movies of a human skeleton he had rigged and manipulated by strings, executing a dance with the head bouncing off and on. He took daily pictures on movie film of the Run forward through a projector, they construction of Stimson Hall. showed the building going up at a furious rate; conversely, run backward, the structure disappeared to the ground. Photos of Moler himself convey no sense of this sort of mischief in the man. He took the first X-ray pictures in Ithaca, immediately after Röntgen's discovery was learned of. Mrs. Ryan's hand, with bones and a covered coin, is of his doing.

He was in charge of the dynamo laboratory in its earliest days in Franklin Hall. He was inventive, frequently proposing improvement in apparatus at various places in the building, and helping graduate students in design and construction of their own apparatus. He devised an electrical control for the library tower clock, largely ending its erratic behavior in its chiming and time keeping. He was active in such ways for his fifteen years after retirement.

The <u>Ithaca Journal</u> of May 20, 1932, tells of his death in New Jersey where he had gone to live. Some other items from the same issue may be of interest for what was going on in the world at the time: some suspect negotiations with the Lindbergh kidnappers (headlined); a light beam

carries the voice in air-earth tests with the dirigible "Los Angeles;" Amelia Earhart is all set for the first Atlantic solo flight by a woman, she has reached Newfoundland; so also is the big sea plane, the Dornier, twelve engined, 100 passenger monster, D0-X, poised for its flight home on Lake Constance. Closer to our home: Six Cornell students are ousted (!) for stealing books from the library; some 150 volumes they have removed. No physicist seems involved; it is only Byron, Keats, Shelley, etc., which have been targeted. Said one student: "This is a more righteous institution that I believed it to be." Another claimed to have acquired the idea for the heist in class, and he wanted "to taste it in practice"; his was a Sophist's argument--"right is what you can get away with--truth and morality are only relative," he explained. The practice has not abated. The Clark Science Library is said by the librarians therein to lose upwards of \$30,000 a year in material removed permanently from their precinct. Electronic equipment was finally installed in 1981 to detect material leaving without having gone through established signout procedures. The loss should drop.

Moler provided the continuity in the department between Anthony and Nichols. He was not the luminary that Nichols, Merritt, and Bedell were to become, but he played an important and necessary support role in the workings of the department for many years. Two others who in a similar way contributed much to the progress of the department were John Shearer and Ernest Blaker. Both were Cornell students (Shearer, Ph.D. 1902; Blaker, Ph.D. 1901) who stayed on as faculty members and became very active in the undergraduate courses. Shearer supervised the large introductory courses--recitations and lectures--there were no general labs for nearly thirty years. He shared this with Nichols and Merritt; they took turns, often lecturing in the demonstrations on successive hours to sections of the same course. All students, Arts and Engineering, got the same treatment; there was consequently a high mortality of the nontechnical students. Shearer looked after the liquid air production in Rockefeller Hall and became interested in radioactivity and X-rays, using a large static machine for the production of radiation and doing X-ray photography for local doctors, the activity taking him to France in World War 1 where he was in charge of X-ray work for the American Army. He died a few years after the war, still in Cornell "harness." Blaker taught in

the sophomore courses, tangling there with engineers. He was not easy on them, and the work was tough so that there was frequent bitter criticism of our courses from Engineering environs. He also gave intermediate courses and initiated laboratory work of advanced nature where it long remained--in the south end of third floor Rockefeller. This laboratory course of Blaker's is today's Advanced Laboratory course which has been alluded to innumerable times, and which will yet be heard of in pages to come.

The growth of the department really begins with Nichols. Moler's wall chart starts proliferating with names; more space is taken up with new positions, which the chart was able to accommodate up to about the time of World War II. Thereafter, it became unmanageable, the chart system was abandoned, and typewritten lists of names were simply appended for each succeeding year. In the mid-1950's even that grew beyond bounds, and unfortunately no systematic record of department personnel was thereafter kept. The chart has been turned over to the library Archives, but copies are available.

Edward Leamington Nichols was born in Leamington, England but came to this country with his parents at the age of five or so. His father was a music teacher who gave that up for a career as a painter. He and the family spent some time in Italy where he studied painting, and he was not bad apparently; some of his work is still extant. There is no indication that the son then had an interest in science, let alone physics. As a matter of fact, by his junior year at Cornell he had thought to be a chemist. It was at about that time that Anthony excited him with physics. After graduation, he spent four years in Germany: at Leipzig with Wiedemann, at Berlin with Helmholtz and Kirchoff, and at Gottingen, where he got his Ph.D. with a thesis--"Von gluhendem Platin auggestrahlte Licht." They re-awarded him the degree fifty years later. On returning to this country, he approached Andrew D. White about a position here. But there was a depression on and no room at the time; however, White wrote Johns Hopkins about him with the result that he went there to work with Rowland. There he repeated Rowland's famous European experiment on the magnetic effects of rotating static charges and showed that the effects were largely frictional. Any real magnetic effect was extremely small. He then went to work with Edison at Menlo Park, particularly developing

photometric methods for use with the incandescent lamp. After a teaching position in Kentucky, and another at the University of Kansas, he came finally to Cornell. So he had acquired a lot of experience, and it is quite reasonable that Anthony should see Nichols succeeding himself as head of the young department when he left for Connecticut.

When Nichols took over from Anthony he inherited as his staff one man, Assistant Professor Moler. Two men were enough for those of the one thousand Cornell students who took physics courses. But rapid growth set in and Merritt, Bedell, and Harris Ryan were added, the last named becoming professor of Electrical Engineering before he left for Stanford, the "Cornell of the West." Quarters expanded. Chemistry moved from Franklin Hall to Morse Hall so we had the whole of Franklin. But we outgrew that and got Rockefeller Hall in a \$250,000 gift from the There were then two full professors, four assistant philanthropist. professors and ten instructors and assistants. For their researches in luminescence over a long time the names of Nichols and Merritt became linked almost as though they were a single person. In a biographical memoir on Nichols written for the National Academy of Sciences Bulletin (1940, 21, 343-66) by Merritt, he has this to say about the man:

Nichols was a pioneer in several branches of physics. Much of his work called for manipulative skill of high order; all of it called for ingenuity in meeting new problems. But when an investigation reached a point where high precision was called for he was ready to go to something beyond...

One of his most important contributions to American physics was the indirect influence he exerted through the students who received their inspiration from him and who later entered the field of college teaching or industrial physics. At the time of his retirement (1919), the heads of departments in thirty-five colleges, fifteen of them state universities, were men who had received their physics under him.

Nichols was quite a forward-looking man for his time. His views would please revolutionaries and environmentalists alike today. In his retiring address as president of the AAAS in 1909, on "Science and Practical Problems of the Future," he sees three or four of our problems in the solution of which science will be needed. He deplores the waste he sees in the destruction of the bison, forest, fish, etc.; the prevention of such and "other manifestations of corporate greed" will be solved through laws, but science will be called upon. He sees the eventual exhaustion of our coal and petroleum supplies. He wants to see the utilization of solar energy in ways other than by storage of nature. Science is at work on all three today. But he wants the seeking of fundamental knowledge to go ahead; practical results come; the work of Joseph Henry is used as an example. He complains that when a man becomes productive, he is shoved off into a presidency or directorship. He hopes for discipline and reason in the front offices, quoting the "testy remark once made by an eminent scholar: 'You can't run a university as you would a saw mill.' " And: "We need not merely research in universities but universities for research." This was a view at some variance to that prevailing at various periods in our department history. At the 114th anniversary of Ezra Cornell's birth, Nichols gave the Founder's Day address, summarized in a Scientific Monthly shortly thereafter. He deplores tradition--tradition as such. He was not unhappy over the student activism common in Europe but "Nobody wants a university full of anarchists. They are a nuisance--certainly a nuisance--harmless, but still a nuisance." Why aren't our students active? Tradition. Rather, let's have reason.

Perhaps the remarks he made at his retirement dinner, on the occasion also of the semicentennial of the university, are the most pertinent for us today. He tells of his "dream." Cornell had had half a century showing the way in a daring experiment in education and "... we glory in that fact. Yet she, who has boasted of her freedom, is fettered like her sister institutions throughout the land, by one bad tradition; the tradition of school masterdom. My dream is of a Cornell that shall be first to break away into glorious freedom that surrounds us into the glad Bohemia at our very doors. We get whiffs, we are not dead, we struggle feebly." Productivity is what the world needs. The universities are the workshops for the tasks--immediate, urgent, imperative--man's job-titanic effort--no dilettantism. We need freedom, devotion, and the It must declare the scientific spirit--they define the university. advancement of knowledge to be its prime, its supreme function, and make it go. And then he winds up. He hopes to be around and still curious, having "the privilege of watching the wheels go around, for that is all I feel I can do or ever have done. It has been delightful-unspeakably

delightful--that life which comes from the study of science. What I would like to say, among the thousand things I would like to say and can not, is that you must not be content with the things the generation that is passing away had to be content with. It is for you to do greater things, and more important things than we have ever done. The things are crying to be done, and the world is crying out to have them done. If Cornell is to be what we all hope and believe she is to be, it can only be through endless strivings of the imagination, through ceaseless labors and great creative art. It can only be by the highest efforts of everybody who has a mind to do anything whatsoever. Then we can look back upon the crude efforts of those who went before and while we smile we may at least believe that they looked forward to the things which they could not accomplish but which you shall accomplish."

The modest retirement dinner was a festive affair held in the Home Economics cafeteria--long before Martha van Rensselaer Hall--with some 230 former students, colleagues, and friends in attendance. (Tickets: \$1.10; entree: roast beef with brown gravy. Roast chicken could have been provided at \$1.50 (!) a plate. The arrangements committee were clearly on the side of frugality.) Nichols and his wife, who died nine years later, were presented with a silver tea service and a check for the amount the gift fund was oversubscribed, \$230. Nichols himself died in 1937 in Florida, where they had for many years spent their winters.

He received in 1928 the Rumford Medal from the American Academy of Arts and Sciences of Boston, in 1927 the Cresson Medal from the Franklin Institute, and in 1929 the first Ives Medal of the American Optical Society which Herbert Ives established in honor of his father, our early photographic innovator. Planck also got a Medal that day at the Franklin Institute ceremony and spoke on "The Reality of Photons"; he had come around by 1927.

In presenting the lves Medal to Nichols, Optical Society president, Richtmyer, spoke of Nichols' lectures: ". . . students carried away something far more valuable than information imparted. There was about his lectures a quiet dignity, an unaffected simplicity, a respect for and love of all knowledge, which could not but be infectious."

There is in the Nichols' file in the library Archives a copy of a talk that Nichols gave over in Albany to some Albany Institute in 1896, a few weeks after Röntgen's discovery of X-rays. There are reproductions of Xray pictures of keys, gears, etc., taken by Moler here in the days following the announcement. There is in the Merritt Collection of photographs in the Archives, besides these, the negative and print of the X-ray Moler made of Mrs. Ryan's hand atop a 25-cent silver piece; exposure 15 minutes, three feet from a small hole in front of the tube anode. Lots of places had the means for doing this sort of thing; gas discharges and high voltage induction coils were common. It was Röntgen's good fortune to accidentally find an effect from his discharge tube when it was covered with thin black cardboard; a "paper screen washed with barium-platinocyanide lights brilliantly and fluoresces equally well whether the treated side or the other be turned toward the discharge tube." Lenard had observed effects on the outside of a thin aluminum window through which he was attempting to pass cathode rays. He undoubtedly had X-rays there, Nichols said, had he but known it. He could shadow objects as one could on the inside of a discharge tube. But Röntgen had the imagination and insight to try bones and found a differential absorption. Things successful medically are still good for publicity and funding, not to mention Nobel prizes. It must have been an exciting period following the discovery; in three months they were being used in a Vienna hospital. Cornell was not alone in "playing" with X-rays; but it was to become a major field of research in the years to come in its Physics Department.

Somehow it was the understanding of some of us that Nichols was involved in the founding here at Cornell of the science honor society, Sigma Xi; that he and Professor Dennis of Chemistry were initiators of the enterprise. But in a first guarter century history of the Society published in their journal, no mention is even given to either Nichols or Dennis; students (non-classicists, i.e., ineligible for Phi Beta Kappa) provided the The date was in fact 1886 and Nichols did not assume his initiative. professorship until a year later, so we have been wrong. But it was a pretty shaky start apparently, and it was nearly a decade later after the chapter at Yale was installed that the organization began to grow. However there was certainly Physics Department support and interest, for it was Professor Bedell who installed the Yale chapter. (Today, the Department displays little interest in Sigma Xi. Dennis' colleague, Professor Bancroft, with whom we are by now acquainted, refused to join,

labeling it a "mutual admiration society" rather than the "Partners in Zealous Research," which its motto proclaims it to be. A bench in front of Sibley Hall commemorates the 50th anniversary of the founding of the Society, an occasion to which physicist and Cornell alumnus, I.I. Rabi, contributed the major speech.

Nichols was for two years (1913-15) Dean of the College of Arts and Sciences, the first physical scientist to hold the post, following by fifteen years the first Dean, Thomas Frederick Crane, remembered as "Tee Fee" Crane in the Cornell song, "Give my regards to Davy (Hoy, the registrar), Remember me to Tee Fee Crane, etc." Gibbs was acting Dean for a period in the twenties, but it would not be until 1986 that another physical scientist, Geofrey Chester, would take on the Deanship in full measure.

Nichols will also long be remembered in American physics for the founding of the <u>Physical Review</u>. Here is Grantham and Howe on the founding of the <u>Physical Review</u>:

Professor Nichols was hardly settled at Cornell when, concerned over the lack of an American journal devoted exclusively to physics, he persuaded the University to furnish financial backing for such a publication, to be edited by him and Professor Merritt. Thus came into being the <u>Physical Review</u>, now so familiar to all American physicists. Bedell, among the first group of men to receive the Ph.D. degree in physics at Cornell (1892), was added to the editorial staff before the first copy of the <u>Review</u> appeared in 1893. For twenty years the three men managed and edited the journal. When this Nichols-Merritt-Bedell Cornell enterprise became self supporting, it was turned over, outright, in 1913, to the American Physical Society. After that, Bedell served for still another ten years as managing editor, from his office in Rockefeller Hall.

It may be that some credit goes to President J. Gould Schurman for the initiation of the <u>Review</u>, that he may have planted the seed that took hold with Nichols and his colleagues. Schurmann himself, according to Morris Bishop, was the editor of two journals and encouraged his faculty to assume such obligations. Nichols took him up on it, at any rate, with the results known. It would almost seem these days in the immense proliferation of physics publications, that the days of the <u>Review</u> as we know it are numbered. In fact, the days as we <u>knew</u> it <u>are</u> gone. It used to

It now comes every couple of weeks, is broken down into an issue on this abstract field and the next one two weeks later in that very special field, some biweekly issues measuring almost an inch or more in thickness and running to four and five hundred pages. The day will come when our publication will appear only on microfilm or magnetic disc.

The <u>Review</u> used to carry on its mast head the note that it was founded in 1893 by E. L. Nichols. This came about as the result of a letter in 1935 from Professor Merritt to John Tate at Minnesota, then editor of the <u>Review</u>. In the letter, which he also sent to others, he suggested that, in common with most other learned journals, and he listed a number where the custom was followed, there be a note on the cover such as "Founded in 1893 by Edw. L. Nichols," which indeed it was. The suggestion was taken but "Edw." was simply replaced by "E.," which led to some confusion in due course. It was in the sixties that the writer noticed that the initials were E. N., not E. L. There seems to be no record of an E.N. Nichols working in physics. It is presumed that someone intended that it should be E.F. Nichols, mistakenly getting the wrong middle initial. Indeed, there was a rather noted E. F. Nichols, who joined the radio spectrum to that of the infrared, but is best known with Hull for, independently of Lebedev in Russia, measuring the pressure of light.

Our Nichols, E. L., tells how one winter evening in 1885 he gave a lecture on experimental physics in the chapel at Kansas State College in Manhattan. Three years later, the event forgotten, two men appeared at Cornell to undertake graduate study in physics; they had heard that lecture, and it had decided them on physics. With their undergraduate work now behind them, they had come east to Ithaca for more. One of the two was Ernest Fox Nichols. He got his degree here after an intervening two years in Berlin (no clue as to what happened to his companion). He was appointed a Cornell "Fellow in Physics" for one year between 1891 and 1892. Anyway, after spotting this error on the part of the <u>Review</u>. I called editor Sam Goutsmid's attention to it, and the middle initial was corrected in subsequent issues of the journal. He was pleased to find "that at least one of our subscribers reads the <u>Physical_Review</u> from cover to cover." What prompted the cover change a year or so earlier is not known; it was not changed on the mast head appearing at the top of the

first page. But now any reference to <u>any</u> founder seems to have been dropped. We should urge its restoration.

E. F. Nichols really moved around after his period here. First to Colgate and then to Dartmouth, where he did the famous experiment with Hull. Then it was to Columbia and back to Dartmouth as president. He left that for Yale, and that in turn for industry at Nela Park. MIT called him to its presidency but, through ill health, he served only a few months after his inauguration. After recuperation, he returned to Nela Park and a less strenuous life. He died addressing an audience at the National Academy.

In Berlin, he had worked with Reubens on residual radiation-that which remains of the black body spectrum after suffering repeated metallic reflections--long wavelengths. His thermopile demanded a galvanometer of great sensitivity; his mirror and needle weighed but 48 mg. It was the ancient weak-field instrument, a tiny version of Anthony's monster, suffering the vagaries of the earth's field, which frequently were accompanied by auroral activity--the slightest trace of northern lights (and it was good and dark at night back then) was a sign to forget work that night. He was the first to definitely detect the heat of a star; Arcturus gave him a 1 mm deflection--10⁻⁸ candle at one meter. He later did a beautiful experiment which our Nichols rated as second only to that of Hertz in connecting radiation to electricity and magnetism. He found that the reflectivity to residual radiation of a cross grating ruled on a metal film very much depended on the grating space. His little square film resonators, roughly a wavelength on a side, were doing what was expected of them.

Cornell Physics was not responsible for the founding of the American Physical Society, as it was for the <u>Review</u>, but Cornell physicists were much involved with its 1899 inception. Nichols, Merritt, Bedell, and Shearer from Cornell were at the organizational meeting held in May at Columbia to consider the possibilities. There was growing sentiment that a separate society distinct from Section B of the A.A.A.S. was needed. The honor for having the inspiration seems to fall on Professor A. G. Webster of Clark University in Worchester. Merritt was the first and for many years secretary of the Society and in consequence, could write an article for the <u>Review of Scientific Instruments</u> (April 1934) about the founding. There is in the Merritt file a long letter from

Professor Magie of Princeton whom Merritt had asked about that first group discussion on starting a society. Webster instigated the meeting and Magie says that he, Magie, practically wrote the constitution himself, modeling it on that of the Mathematical Society. He recalled Webster's promoting the first officers: Rowland, president; Merritt, secretary; Nichols to go on the council. Somewhat isolated from Physics up there in Worchester and rather discouraged, Webster was a suicide in 1923. Fifty years after its founding, Bedell wrote of it (Physical Review 75, 1949) (as phrased by Howe and Grantham):

Merritt now had a dual role, as editor of the <u>Review</u> and Secretary of the Society. During its early years, Merritt, as Secretary carried the load of its operation, arranging programs and issuing the quarterly bulletins. As Secretary during the critical years, then as president and as councilor, he served continuously as an officer of the Society for over fortynine years.

For his R.S.I. article, Merritt wanted a photograph of Webster, which, indeed, he got and included in the piece. In a letter about it to Mrs. Ware, Webster's daughter, he speaks warmly of his friendship with her father and of his own days in Berlin. Merritt tells her that when he was a student there, he used to meet the Kaiser almost every day on Unter den "I always took off my hat and he acknowledged by saluting, but Linden. that was as near intimacy as I ever came," he wrote. As mentioned earlier in these recollections, it was in Merritt's house on Grove Place (houses and woods there now replaced by Phillips and Upson Halls), on my first Sunday in Ithaca, that I had the pleasure of meeting Nichols. I don't recall anything of the conversation. It was a sunny afternoon, and I have a picture of the venerable old gentleman, neatly bewhiskered with a white (rather than the earlier black) beard, sitting in his chair, not doing much of the talking. Merritt, on the other hand, although near retirement, was full of life, sparkling wit and conversation, enthusiastic, bubbling over. His wife was just as charming.

Merritt, as surely may have been surmised by now, was a student of both Anthony and Nichols, graduating in 1886, the year before Anthony left. He became an instructor in the third year of Nichols' headship, having been "F.P." on Moler's chart (Fellow in Physics) the year before that. He

was thus active in the department for nearly half a century. In some ways the days of Merritt seem not so long ago. There is still up in one of the Rockefeller attic storage rooms a collection of bottles containing salts, the fluorescence of which Nichols and Merritt investigated. Save for some discarded uranyl salts (radioactivity!); the collection would appear to be intact, amazingly, something not thrown out when fire marshals ordered an attic clean-up, other artifacts have not been so well attended.

Unlike Nichols, Merritt had leanings toward science at an early age. The Indianapolis Journal in 1879, with an article entitled "The Boy Astronomer," reports on the fourteen-year-old Ernest "whose eager study of the stars may some day make him famous." He was inspired while ill at one point "to translate its wonderful language (that of the heavens) wherein are recorded the mighty secrets of the universe." He had purchased a small telescope from a local astronomer and put it in his modest observatory equipped with "transit, desk, and stove, and a stereo plasa, an invention of his own," not patented one presumes, nor preserved, and thus lost forever. Earlier on, he had an inclination toward journalism, of less florid sort one hopes than that of the Journal reporter. At the age of eight, the article informs, he edited, scripted, and illustrated "The Sea Breeze"(!), his own paper about the size of a man's hand. Later, with a small hand press in place, the sheet was somewhat enlarged, "printed, stitched, and bound" under a new name, "The Mountain Echo"(!!). All this in Indiana.

At graduation from the Indianapolis Classical School, he was awarded the mathematics first prize and subsequently received a note from a professor of Mathematics at Cornell, where he was to matriculate, commending him on his success. When journalism became astronomy and astronomy became physics is not evident; his daughters tell of his flunking his first physics course but falling in love with it in spite of that. His obituary indicates his undergraduate degree was in Mechanical Engineering, so his physics education came mainly after that it would seem.

Long after he became a member of the faculty, for some gathering or publication Merritt prepared a short dissertation on liquid air, a copy of which is in the Archives. He tells of a local emergency in which our liquid air machine performed yeoman service. There was a typhoid epidemic in

town, probably that which Bishop comments on as arising from unsanitary conditions along Six Mile Creek. At any rate, in this particular epidemic, the hospital ran out of oxygen, which was used in the treatment of typhoid patients. It was a weekend, and no trains ran between Syracuse and Ithaca on Sundays (one used to be able to make that trip by train), and the oxygen supply was in Syracuse (why not Rochester--and with our Lehigh Valley R.R. connection?). The university was asked for possible help. Chemistry set about trying to make oxygen by breaking down oxides, a pretty slow process. Nichols suggested cranking out liquid air with our machine (cranking out is approximately the correct term for it if one knew the early machines) and then allowing the nitrogen to boil away, leaving the oxygen which could then be pumped into cylinders. And so it was. The liquefier ran full tilt, the product was put in pails and allowed to boil largely away, the remainder being sent in cylinders to the hospital, tiding them over until the following Tuesday.

In the same dissertation, Merritt relates a tale that would seem fitting for a Marx Brothers movie sequence. It seems there was to be a popular lecture on liquid air given by someone over in the lecture room of Franklin Hall. It was indeed popular. So crowded was it that Merritt could not gain entrance to the large room, and so went around to the back in the apparatus room behind. There he could barely see the lecturer, but enough of what was to transpire. There was a fire extinguisher hanging from the wall and immediately below it, not yet disposed of, was a pail of old lamp bulbs (carbon filaments, no doubt) which a janitor had but recently taken from the room lighting, replacing them with new bulbs. All was now bright and cheery. One obviously interested spectator, sitting on an available space on a table near the wall, squirmed about somehow, the better to see the demonstration, and managed instead to knock the fire extinguisher off the wall, dropping it onto the pail of bulbs, upsetting it and spilling the contents. The fire extinguisher went into action, spraying a mix of H₂O and carbonic acid about in all directions. It was clear to the audience that the liquid air was loose and about to freeze everyone. The spectators scrambled away, tramping the old lamp bulbs lying on the floor. The resulting explosions indicated to the audience that the liquid air was indeed going off and more panic resulted. Positive feedback. In the pandemonium, no one was injured, fortunately. Merritt notes that "I never

succeeded in seeing or hearing the lecture except this, but it was sufficiently interesting."

There was another incident in that same room which perhaps might have had more disastrous consequences. Merritt was lecturing on heat and was demonstrating heat capacity in a familiar experiment. There is a series of balls, all the same size but of different materials and thus of differing heat capacity. These are heated to the same temperature and then placed on a block of paraffin to observe the descent of the balls Hot water was not quite hot enough; boiling oil was the through it. medium in which the balls were heated, and to keep the vapor out of the lecture room, this was carried out in the back apparatus room by DeWitt Calkins, mentioned earlier, the lecture assistant. Merritt started the experiment and sent the collection of balls out to the back room and continued his discussion while they were being heated. He came to the end of what he had to say, but the balls had not reappeared. Wondering what was causing the delay, he cracked the door to look behind. There was DeWitt backed into a corner battling with another fire extinguisher a sheet of flame from <u>burning</u> oil. With great presence, Merritt nonchalantly inquired whether he needed any assistance. Assured not, he came back to his audience and improvised, not wishing to cause another panic. With the help of yet another fire extinguisher in the hands of a passerby whom he hailed, Calkins got things under control, and the experiment went ahead, indeed showing that the sphere of copper descended more rapidly than that of lead, and further, that boiling oil was not the wisest way to heat balls to the same high temperature.

Merritt was to be department head for fifteen years. With department "chores" growing over the years, he found less time for his research, to his distress. He thought that half a professor's time should go to that activity. But the business of running the show, of consulting with graduate students, and of giving his lectures to freshmen, which he both felt an obligation to do and delighted in, left little time. His first fifteen years in the department were spent in various research activities (with publications on such as glow lamps, flames, photoelectricity, batteries, electric waves and oscillations, cathode rays); his second fifteen were spent with Nichols on luminescence, and the last fifteen in the department chair. Mid-course, 1909-14, during the second fifteen

year period, he also served as the first dean of the Graduate School, anticipating the role of Richtmyer years later.

He was a good teacher apparently. Even in his theory courses he made use of the trick of apparently being surprised at what had resulted in an analysis, much as I had seen him show in his demonstrations of the gaseous discharge. Very engaging. I can still see him down there.

Merritt had wide-ranging curiosity. I've mentioned his movies of the rowing crew. In a remnant of his early astronomy interest, he got involved in an eclipse expedition, wondering about a change in radio propagation at totality. Others had undoubtedly done similar measurements; like cosmic ray investigations in the early days, there was usually the chance for a trip to exotic places. Unfortunately, unlike an eclipse expedition that would take F. K. Richtmyer to the South Seas with the National Geographic in the thirties, this one of Merritt's only took him out of the house. In fact, practically the whole department was caught up with it, for the path of totality was right across the Arts guadrangle. There is a beautiful picture of old Boardman Hall, at the south end of the quadrangle, snow on the ground, with the totally eclipsed sun and corona directly over the building. Was Moler the photographer? For January 25, it was an unusually bright day; 25" of new snow; temperature, a cool -25°F, as Merritt noted in some correspondence. Lots of 25's in the event, which lasted all of two minutes officially, for weeks actually, according to Merritt. "What a show," he noted. Whatever was learned has not become part of history--at least this "history."

He had a continuing interest in radio wave propagation, dating from the World War I days, if not earlier. In 1928, anticipating the work of Gartlein, he noted an aurora during a large change in radio receiving conditions, and, following up the observation, inquired of the Coast and Geodetic Survey about whether there had been a magnetic storm concurrently. He had a Heckscher grant which permitted the erection of a wood tower some fifty feet in height on upper alumni field. Pictures show it to be a rugged structure with a shack surmounting it where the receiving and recording was done. At a National Academy meeting in Schenectady, a <u>Times</u> dispatch reports that he gave a paper on the interaction between the sky and the ground wave. In the same column, Richtmyer is reported as thinking that X-ray satellite lines may be due to
the simultaneous jumps of two electrons within the atom. More interesting, a paper by R. Gurney discusses nuclear spontaneous disintegration and barrier penetration, "a natural consequence of the new wave theory of matter." This was first understood by Gurney with Condon, and independently by Gamow.

Further correspondence in the Archives' Merritt file shows that in 1926, with a handsome salary of \$8000 attached and \$2000 for an assistant, a full professorship was offered to Max Born, "an authority on crystal vibrations and photons." Wave functions and probability were yet to come. Born and his wife had visited Cornell, when he had given a lecture on the worst day of Ithaca's winter a few months previously, staying and being entertained by the Merritts. He wired to President Farrand his acknowledgment of the honor of receiving such an offer but put off a decision until he found what Gottingen was going to do about his position there. They clearly did what they should; in a subsequent wire he regrets not being able to accept.

In a letter to Born after his visit and before the offer of appointment, Merritt expressed his pleasure at having had Born visit Cornell, sent him pictures of a more attractive Ithaca than he had seen, and enclosed a strip of film "of Millikan, of a new kind being developed in a laboratory not far away from here (undoubtedly Eastman Kodak); the voice is reproduced as well as the picture." Millikan had been here giving the Messenger Lectures; talking movies were made. He had half filled Bailey Hall, which was probably not bad for the day. In a 1980 Bethe Lecture, Freeman Dyson filled the hall.

There were other approaches made to illustrious Europeans. In 1926 Merritt wrote to Debye encouraging him to undertake a permanent stay in this country, in particular in Ithaca. While Debye had visited the country before, Merritt suggested a temporary appointment to give it a try, details to be worked out later if he were at all interested. We did get him ultimately, of course, but across the way in Baker Laboratory. When Kennard got back from a sabbatical leave spent with Born at Gottingen and with Bohr at Copenhagen, he was all fired up over the idea of getting Heisenberg here for a semester as had been done with Lorentz just previously. In his report to the president for 1926-27, Merritt indicates he has put the idea to the Schiff Fund Committee. Nothing seems to have

come of it; having just supported Lorentz, they perhaps felt another physicist was unfair distribution. In any event, there is no record beyond this. Kennard wanted a permanent fund set up to draw big names here for such extended visits.

These were the days when American physicists went to Europe, where new physics was breaking fast. In a memo to Arts Dean Ogden, Merritt discusses the problem of why European universities were ahead of us in the field. Several reasons: they give small time to undergraduate courses, they have small teaching loads, no administrative chores, they have excellent material equipment and financial support. Remedies: increase salaries so we can compete with industry, increase the nonteaching help, enlarge the faculty, modernize the equipment. No doubt such steps would have helped, but it seems the situation could not be quite that simply remedied. In a report earlier, he worries about why we don't get more physics students and points out that possibly students go to engineering to discover physics too late. We should stress physics as educational more than informational, get across the spirit of science.

In 1931, Merritt was in Europe on a sabbatical leave, checking the territory, visiting around--Leiden, Gottingen, Berlin--his eye out for people. And he made some interesting notes. In spite of Kammerlingh Onnes' death, the low temperature work in Leiden is going along fine; under Keesom and deHaas, he was shown solid helium at 0.8°K (and 60 atmospheres), first obtained a few days previously. Spent a day in Haarlem with Prof. and Mrs. Lorentz--"very pleasantly indeed." From Göttingen: thinks Born's decision is final, even if Franck were called to Ithaca; thinks Franck might be even more valuable--but "it would be grand if we could get both." Saw excellent lecture, corresponding to Physics 3, by Pohl at 7 AM! Was delighted, "Pohl is doing nice work on crystal conduction produced by light" (before or after the great experiment on the Hall effect in diamond done with Gudden(?)--in which the unclamped electromagnet pole pieces slammed together crushing a huge borrowed He is favorably impressed with the younger men such as diamond). Oldenberg and Rupp. "Did not meet Jordon. Pauli is impossible." In Berlin he had dinner at the Planck's; von Laue was there, but Merritt was not impressed -- "he talks too fast -- even the Germans can't understand him." Berlin and Göttingen are much interested in Schrödinger's recent articles

on quantum theory; thinks that Planck regards it as the most promising step yet. It was an interesting discussion at Planck's between Planck, von Laue, and Franck; the last sees Heisenberg as largely formal, Schrödinger's approach more capable of physical interpretation, but the real solution will be something different still, not yet reached.

But we are getting ahead of ourselves. Let us turn back to more ancient of days. The final noted personage of the early days in Cornell Physics was Frederick Bedell. Bedell got his undergraduate education at Yale but decided that for his graduate work Cornell was the place to be. He tells how there were no set course requirements, there was no graduate faculty, no dean--"I simply did as I pleased, taking my oral examination (with thesis) in 1892, the first year Cornell gave that degree in Physics." And he stayed on.

Of the early faculty in the Physics Department, Bedell was probably the most innovative, his research the most far-reaching of any of the others. His contributions to the use and understanding of electric power were the equal of any electrical engineer of the time, even including Edison, with whom he experimented on a three-wire system from a single transformer. He was one of the very early expounders of AC circuit theory and in large measure was responsible for the fact that today, almost universally, power is distributed as AC rather than as DC, which Edison favored. (When I first went to New York City in 1939, the apartment was in a large westside section of Manhattan still served then with DC.) Bedell understood the transformer, three phase circuits, and he could measure AC power in the days before wattmeters (see his paper in the first issue of the Physical Review, cited earlier). He introduced the use of vectors and the circle diagram in analysis of the transformer and synchronous motor, writing a paper with our H. J. Ryan on the action of the single phase synchronous motor, measuring phase difference between the generator and motor by simple optical means. He presented with Crehore in 1892, the first paper to the Institute of Electrical Engineers, a 72-page opus clearing up much misunderstanding as regards AC. The first real textbook in electrical engineering was written by Bedell and Albert Crehore, Alternating <u>Currents</u>, and was translated into many languages. It was for years the standard reference and text on the subject. Direct Current and Alternating Current Manual followed. He wrote The Principles of the Transformer and others: The Airplane and The Airplane Propeller. outgrowths of his course on airplanes during World War I. He wrote with GE's Steinmetz in 1894 the first paper on reactance. With Crehore, also in 1892, he called attention to the limitations of the telephone in long distance communication because of the difference in the "decay" of high and low frequencies, which they pointed out could be reduced through the introduction of "self induction" in the line. The theoretical deduction was substantiated later by Pupin at Columbia; he did just that and is generally given credit for making long distance telephone possible with the "Pupin Loading Coil." Bedell understood the importance of wave form in AC and was led eventually to the use of the electronic oscilloscope, first demonstrated by Braun. (Bishop credits Hannibal C. Ford, '03, a Physics undergraduate at Cornell, with building the first cathode ray "oscillator" in America for wave form "and power" determination. Our man, Harris Ryan, by then gone over to E.E., obtained an important patent on the magnetic deflection of the oscilloscope electron beam for the display of wave shape. He was among the first students admitted to the Electrical Engineering program when it was established by Anthony under Physics, and was involved with him in that big galvanometer.) He was the first to use the multiple-trace oscilloscope. The linear time base (from the voltage rise on a charging condenser, stopped by the breakdown of a neon glow tube across it for the flyback and re-start) for the oscilloscope was known, but Bedell and his student, H. J. Reich, were first to stabilize the sweep, which was to make the instrument (they were first to call it oscilloscope) useful in providing stationary wave forms on the screen. (Reich was also the first to propose, importantly, from here in 1928, the use of 450 kc as the frequency for IF amplifiers in superhetrodyne receivers. It is now standard but the idea took a considerable time to be adopted by radio manufacturers.) This was patented and the Burt Scientific Co. of Pasadena--Burt was his son-in-law--manufactured an oscilloscope incorporating this feature. There were a few of these around Rockefeller when I first got here. The oscilloscope was in a wood cabinet with folding front doors, including beside the sweep circuit glow tube, three vacuum tubes, two of which were in use at any one time. Through a peephole in front, the operator adjusted tube filament temperature visually. The specifications included with the UX-201A tube (a version of

the UX-20I, the first commercial vacuum tube for home radio set owners) cautions: Filaments should always be operated at the lowest voltage which will give satisfactory results; AVOID PUTTING THE PLATE VOLTAGE ON THE FILAMENTS (!) Beam focusing in early cathode ray tubes was achieved with ions created by the beam from residual tube gas. In consequence, oscilloscopes displayed only relatively low frequency signals. In the mid-thirties, DuMont in New Jersey sent Bedell some new, highly evacuated tubes in which electron lenses played a major role--ion focusing was almost nil. But this time radio tubes had gone well beyond the UX-201A. Bedell's student, Tom Goldsmith, for the new tubes created fast sweep circuits, making possible the display of high frequency signals. No more 201's; no more neon glow tubes. Goldsmith went from his Ph.D. to DuMont, whose commercial scopes became almost the standard up to the time of World War II. Being hard of hearing himself, Bedell invented a bone conduction hearing aid, and the Deaf Speaker, a loudspeaker for the hard of hearing, using the same principle. He was among the first to apply the principles of impedance matching to bone conduction devices. Professor Merritt himself was quite deaf and benefited from Bedell's researches. The latter built Merritt one of his Deaf Speakers, a little box connected to the radio, out of which came a stiff cable with a clip holding at the far end a disposable tongue depressor. Held in the teeth, one could hear the radio. Merritt got quite a lot of pleasure from his unit.

All in all, he seems to have deserved the raise he suggested for himself in a letter to acting President Smith in 1920. He "hesitates to refer" to his accomplishments, but he thinks he deserves more than just the average. Merritt recommended to Dean Ogden that Bedell's salary go from \$4500 to \$5500. In 1896 he was elected to the Physical Society of London; the only American member at that time was Henry Rowland.

At the 50th anniversary of the first "pioneering and classic" paper presented to the AIEE, his work was cited at the Institute meeting in Victoria, B.C. Bedell's contributions were enumerated and he reminisced a bit: "In 1890, alternating current was just plain freak; it did not follow Ohm's law and 'clogged' itself in the circuits. Everyone was afraid of it." The first installation had been a 4000 foot line carrying 500 volts, which he contrasted to the 275 kV lines in construction for Boulder Dam. Voltage had been stepped down to 100 volts with a transformer, itself

then new. Seen in the distribution of electricity through a community at 500 volts or more "was grave fire and life danger." A bill was introduced in the Virginia legislature limiting AC voltage on lines to 200 volts because AC was regarded as "More deadly than DC." He told how the early workers were worried about wave form, "whatever that was." Harmonics in AC could be dangerous; inductive effects were worse so one could get high voltages--and often did. The paper with Crehore answered the question of why current was "clogged" in some circuits while voltage jumped to distressingly high values in others; it treated both the transient and the steady state; in the paper, \underline{J} was first introduced into AC circuit analysis.

He enjoyed recollecting the old days. He tells how at the 1876 Exposition, where he saw Cornell's generator generating, and Bell's telephone speaking, he never imagined that he would one day be associated with Bell, Edison, and Anthony, later to be at a laboratory "with Edison verifying the possibility of having a three-wire system from windings on a single transformer" or "listening on headphones with Bell in New York as he first telephoned Ryan in California," and with Anthony "using equipment he had built." His involvement with the <u>Physical Review</u> and the American Physical Society is recollected in the <u>Review</u> article of 1949 previously referred to by Howe and Grantham. In a letter to Howe from Pasadena (where he retired at the Burt household), written in connection with the Howe-Grantham history of the department, he recalls some of the early days and gets on to the <u>Physical Review</u>: "... brings me to the unsung heroes, Nellie Lyons and Aloisa (Aloysia A.) King, to whom we owe so much. Can you imagine how the department and the Physical Review could have been run without them, with only an occasional secretary for a few hours to help out and no organized office? Looking back to a period in the early years, Merritt I believe in Germany, and Nichols beyond his limit and for a while away, dumping the <u>Review</u> in my lap in my rooming house on Buffalo Street. It is hard to see how it could be done even with me working nights and holidays and when on a vacation with the Review in my trunk. But survive it did. Then what a blessing was Rockefeller Hall with Alice [he slips] and Nellie."

He served as chairman during a couple of Merritt's sabbatical leaves, so there is a fair amount of correspondence back and forth. Bedell was

always quite formal: Dear Mr. Merritt; Dear Mr. Gibbs; etc. Sincerely, F. Bedell. There were some pleasantries and then the business at hand: he has "recommended the appointment of L. P. Smith in Theoretical Physics;" he has made some assistant appointments "the last two B. Credle and H. Smith [both later professors in E.E.], excellent men, declined a \$1500 fellowship in Electrical Engineering to take the \$700 position in our Department;" and so on. The choice of Credle and Smith is understandable perhaps when Dean-to-be Dexter Kimball reminds Merritt that he and Bedell were joint members of Mechanical Engineering not only for close relationships but, better, because students could get more from them than from the Engineering College.

Bedell had an eye for the dollar and he was pretty sharp. He was frequently called in as consultant, and he had a number of patents to his name that must have netted him something. He recalled how at one point in his early career he was called in as an expert witness in a patent case, not one of his own. This was on a stepdown transformer, for which the patent had not been granted; any fool knew that you couldn't get more current out than you put into it. One assumes that Bedell handily earned his witness fee. His contribution to oscilloscopy was not negligible, and he had some income from his textbooks. But his Cornell salary was still not all that great; he must not have received that \$1000 raise. There is yet another letter in the files, this time from Bedell to Merritt, suggesting that he thought it would be fair if his salary went from \$5000 to \$5500, and a follow-up letter from Merritt to President Farrand supporting the request. I hope he got it. According to one retired professor, who was a student in Bedell's course on AC circuits, Bedell would come to class loaded with bundles of reprints as required reading, selling them off (!) at a price thought to be a bit steep.

Bedell was active in the department when I arrived on the scene, and he remained so until he retired, apparently in 1937. I have no recollection of a retirement "doings" for him; I went home that summer and may have missed it. I find no record. He still had a few students; Tom Goldsmith, later Director of Research at the DuMont Laboratories, whose much more sophisticated oscilloscope relegated Bedell's to retirement, is especially recalled. He had a nice southern drawl. There was another, a lanky redhead with an even broader drawl.

I got to know Bedell fairly well. He used to come into the research room when I was down in that end of the Rockefeller first floor working on the linear accelerator. I recall his explaining how one located objects in hearing, how not only the slight phase difference between what one ear hears and what the other hears is important, but also that the folds and shape of the ear flap itself play a role.

I took Dale Corson to meet Bedell in Pasadena, where he was then living with the Robert Burts, when we were both on a sabbatic leave in the Los Angeles area in the spring of 1954. Bedell hauled out his notebooks on the "Electrification of the Street Car System for Rochester." Neat, concise, and thorough. It was a pleasant evening. He died a year or so later. In 1961, at the twenty-fifth anniversary of Engineering Physics (also the seventy-fifth of Electrical Engineering), Corson recalled the visit and entertained the wish that we might get some of Bedell's notebooks and correspondence. That sounded like a good idea, so I wrote the Burts (Mrs. Burt was Bedell's daughter, Eleanor), inquiring about the possibility. I got a very nice but rueful letter back; they were in fact currently rummaging through his stuff and had only a week or so earlier promised it to the Cal Tech library. Too bad.

Crehore, who wrote with Bedell, was an instructor for one year here, when I suspect he got tied up with Bedell. Or maybe it was earlier; at any rate, Bedell married Crehore's sister, Mary. In the early twenties, in a sabbatical leave, after leaving the <u>Physical Review</u>, long before navigable automobile roads, let alone transcontinental highways, Bedell and wife Mary, in a big touring car laden with tent, camping equipment, and repair gear, made a circuit of the United States. She later recounted their adventures on the journey (for it was just that) in an entertaining book she wrote, "Modern Gypsies."

Another name of the period which rings a bell is that of C. D. Child. He was first an assistant and then an instructor for three years, leaving in 1897 for Colgate University where he became head of the Physics Department. His name is associated, at least in this country, with that of GE's Irving Langmuir for the well-known(?)--at least it should be--three halves power law governing space charge limited currents drawn across a thermionic diode. Langmuir published a paper deriving the law and in a footnote remarks it was derived two years earlier, in 1911, by Child in

connection with ion flow between plane electrodes. In German texts it is referred to as the Langmuir-Schottkysches Gesetz; but Schottky seems to have come in a year later even than Langmuir.

There were two Stewart brothers associated with the department about this time, both serving as assistant and then instructor: O. M. went to the University of Missouri, and G. W. to Iowa, later devising acoustic filters and writing an early book in acoustics. O. M. himself wrote a general physics text. Both became heads of their respective departments. When he went there in 1940 as an assistant professor; Dale Corson met 0. M. at Missouri in Stewart's last year before his retirement; a very kindly gentleman he was, Corson recalls. Cornell must have turned out a lot of such types.

Another name of the era, which should be better known to Cornell physicists than it is, is that of S. J. Barnett who earned his Ph.D. degree in Connect the name with the Barnett experiment, the first 1898. avromagnetic effect to be detected. It was followed a bit later with the inverse effect found in the Einstein-deHaas experiment. Maxwell had anticipated the phenomenon, looked for it, and failed. Rotate an unmagnetized iron rod; the randomly oriented magnetic moments, like gyroscopes, tend to become somewhat oriented in closing the angle between their angular momentum axes and that of the rotating rod, so magnetizing the rod. Such effects are small; the electron has a small mass and moment. E. F. Nichols, who apparently relished looking for tiny effects like the pressure of light, had some years earlier tried unsuccessfully something similar in an electron inertia phenomenon.

In the service area of the department, there were several Calkinses, the first being old DeWitt, met with earlier. He goes back well into Anthony's period, working first (1883) as a janitor, and becoming the first lecture assistant when Nichols took over. In turn he became a mechanician and engineer, finally coming to lord it over the Rockefeller north basement shop and liquid air machine until he died in 1936; fiftythree years he was with the department.

There was an 0. Calkins, first a janitor and then engineer. In between the regime of DeWitt as janitor and that of 0., there is no indication on the record that there was a janitor. Franklin Hall must have been quite a place in the three-year interregnum. Then overlapping 0.

there came F. as janitor; he graduated to lecture assistant when DeWitt became mechanician. The department apparently liked Calkinses, but none stayed with us as long as cantankerous old DeWitt.

While she was not the first female associated with the department (that honor goes to one Miss L. Mack, who was the first stenographer, for three years 1896-99), Miss Helen Lyons--Nellie--served the department even longer than did DeWitt. She joined up at the start of the century and stayed with us until she retired in 1957. She could recall when there was but one telephone in the building, and she had to chase all over the place hunting down this or that professor called on the device. For a number of years, she was a mainstay of the <u>Physical Review</u> when it was run by the department. She was joined about ten years after she came by Miss A. A. King--"Al" to everyone in the department--who was both department clerk and stenographer. Together, these two kept the department on an even keel for the half century. Al retired some years before Nellie did, lived a long time in Ithaca, frail and a little slow but seemingly as sharp as the day she left us; she died in the spring of 1981.

There were two other nonacademic types worthy of mention for these early days in the department: F. C. (Fred) Fowler, who with Moler constructed Anthony's galvanometer. He was a skilled instrument maker, coming to the department in 1884, when Anthony was still its head, staying on for thirty years. He is said to have been very helpful in designing and making apparatus for various research projects. The other was a character, W. D. (Bill) Stevens, general helper from 1906 until 1927 (dates from Moler's chart, not quite in accord with Howe and Grantham). Howe and Grantham describe him as "handy man and sometimes stockroom attendant, was a 'jack-of-all-trades' and most certainly master of none, despite the fact that he sometimes bore the title of assistant mechanician." There is in Nichols' file a long statement about an explosion which Stevens experienced on the job. Apparently it involved hydrogen, but it was never understood. The hydrogen itself was clean; tested afterwards, it burned quietly; no bang. (Chemistry, over in Morse Hall, had an explosion in hydrogen associated with their electrolysis generator installation [Anthony's generator?]. They piped the gas and oxygen all around the place. There is no indication that this operation was in any way involved in their later disastrous fire.) During the last half of his

tenure, which followed the explosion, Howe and Grantham report that Stevens "seemed to spend most of his time repairing his Model T Ford car, the parts of which often lay around in the north basement" (Rockefeller).

Another name appearing in the Moler "Fifty Years" is rather well known to the writer and is of more than passing interest to me, if not to others; I may be pardoned for even bringing it up. The name of L. W. Hartman is noted beginning in 1899, apparently as a scholarship holder for six months until he became an assistant. There is a lapse of three years, after which he appears as an instructor for one year between 1904 and 1905. In the department report of 1899-1900 to the president, it is noted that Hartman is involved with Blaker, Shearer, and Stewart in the Junior Laboratory. A report in 1905 indicates that Hartman has been again in the Junior Laboratory (antecedent of the Advanced Laboratory?). And one dated two weeks earlier says, "Dr. Hartman, who returned to this department as Instructor after completing his studies in Germany, has been appointed Professor of Physics in charge of the Department of the University of Utah and there are indications that I shall be compelled to report the loss of other experienced and efficient members of the teaching force as the result of offers elsewhere at increased salaries." A universal constant and perennial problem; more support is necessary.

It is strange that seventy-five years later that report to President Schurman was reason for me to correct my own understanding of my father's history. I thought he left here and went to Kansas State for a year and then took the job at Utah. I did not understand the above Utah reference until I looked up an old Who's Who. Sure enough. He took the Kansas job after he earned the M.S. degree here following his Cornell graduation. A year later he went to Penn for his Ph.D. It was after that, following a year abroad, that he came here as an instructor. Utah and Nevada followed in succession a couple of years later. Whatever persuaded a native-born upstate New Yorker to go to a place such as Reno must have been in those days I can't tell. At that, it had some advantages over present-day Reno. Anyway, I'm pleased that he did. It was in the 1920's that Lloyd Smith attracted his attention and was somewhat influenced to come to Ithaca for his graduate study; and Lloyd's influence on my own life has not been insignificant. My father never lost his attachment for this place. That is reflected in the modest gift he willed

to the university for a Physics fellowship in alternate years; no longer adequate for that (at the 6% (!) return the university earns on investment), it is at least a token.

Parenthetically, it was at Utah that he became a close friend of James Gibson, Mathematics, father of Rhea Parratt. She and Lyman had married while he was at Los Alamos during World War II. The connection became apparent when she asked me on our first meeting if I had anything to do with a Nevada Hartman.

In spite of the disappearance of the collection of portraits of people associated with the department over the years, which collection once was a feature of the old Rockefeller library, there are a few interesting pictures about, some of which ought to be included in this record. (Thev There is that taken in 1902 of Lord Kelvin, with white beard, aren't.) standing in front of a fuse box in the old dynamo laboratory in West Sibley, flanked on his right by Edward Nichols with black beard and on his left by J. Gould Schurman, president of the university, with no beard. Nichols was a distinguished looking man, tall and imposing. In a couple of group pictures of department faculty and students (it seems), date unknown but early, the chairman of the department stands out. In comparison, the others are as ruffians or scalawags. Identification is for the most part not made; clearly, however, Nichols, Merritt, Bedell, and Moler are present. There was a large group picture taken at the south entrance of Rockefeller Hall in 1932, or thereabouts, and another a year or so later on the north steps. Many faces are recognizable and most are identified. These photos are in the department "archives." Time was when the whole university faculty would assemble in front of Bailey Hall for a group portrait. That has gone by the board; so also the recording in group of the department personnel. It is the practice these days, however, and a wise thing it is indeed, that individual photos are made and posted of all the graduate students, faculty members, and service personnel. In a community the size of the present department, it is the only way to find out who is who around the place. And of what interest for the department fifty years hence!

It seems appropriate to conclude this section with a brief history of the acquisition of Rockefeller Hall, an event which marked the end of this early period in the department's history. The acquisition of Rockefeller

Hall was an important step for the department. Even though Physics had the whole of Franklin after Chemistry went across the road, it was severely cramped, both for the research and teaching. In Nichols' reports to the president, this theme is often repeated. "I beg to call attention once more to the fact that if the work in physics is to be kept upon a modern basis it will be necessary in the near future to provide quarters for this department in a properly designed laboratory building, the apparatus of which shall be suitable for the carrying on of physical laboratory work of the highest grade" (undated report, probably 1898-99). A subdivision of some space in Franklin is then proposed as an interim The next year, describing further the crowding, he suggests that step. plans for a new lecture room to be added to Franklin Hall should be abandoned "in favor of the erection at an early date of an entirely new and modern laboratory." The report concludes: "To this end I would urgently present the question of the advisability of building, as soon as possible, a new laboratory for the Department of Physics."

The report of 1901 heats things up. "There is urgent need," it starts right off, "first of all for a new laboratory, the cost of which has been estimated at \$250,000." It describes the overcrowding. Franklin was designed to accommodate 80 students in laboratory and twice that number in the lecture room--ample. Registration in the beginning courses had reached 400; lectures had to be given three times (later, even four times). It goes on to justify a new building. "The position of Physics among the sciences is unique. It is the <u>fundamental science</u> upon which all technological and industrial progress depends. Every important invention of modern times may be traced back (to) researches in the laboratory, often so far removed from any obvious useful application, as to have attracted no attention from the greater public. Thus the arc light" (Cornellian L. B. Marks developed the enclosed arc lamp, standard before the incandescent type.)

In the next year's report (1903) nothing is said about building; crowding is worse; Blaker and Shearer should be promoted, three of the five-member staff have been out for weeks--"A trying year on account of the fever epidemic." A new era in the <u>Physical Review</u> is entered; the American Physical Society is to cooperate in its support.

The next report, in 1905, hopes "that the share which the Department of Physics is to take in the new programme (of Sibley College, i.e., Engineering was adding courses) may receive adequate support from the University. Rockefeller Hall will provide admirable quarters for such work but it must be properly equipped." Rockefeller had apparently come through with his gift, but with no fanfare in the department reports, nor, seemingly, any note of the fact made in the <u>New York Times</u>.

No report for 1904? It does not seem to be in the Physics archives. It is strange that there is no report for the year, but department records are not all that complete. Parratt has written that when the Board of Trustees in 1904 approved the construction of Rockefeller Hall, Professor Nichols explained to the board that "the intrinsic nature of physics, both teaching and research, and the basic design and proposed construction of the proposed Rockefeller Hall, were such that the Board should expect to replace the building in 25 years." That prophecy came to pass, if replacement did not. Hewitt's Cornell History mentions contract letting for the construction, in the spring of 1904. The building cornerstone on the north wing only mentions the architects: "Carrere and Hastings--1904" is all it says. So great is the current remodeling on the interior, there perhaps had better be another cornerstone on the south wing: "Hoffman, O'Brien, Levatich and Taube--1982"(?). Carrere and Hastings did Goldwin Smith Hall at the same time, incorporating much the same timber frame construction as in our edifice. Interestingly, they also did the more renowned New York City Public Library building.

The report of 1906 indicates that the Sibley College "programme" has made life tough; that there has to be money to carry on original work, citing the importance of the enclosed arc lamp; that, in spite of Blaker's promotion, his salary should go from \$1500 to \$2000. No mention of having occupied a new building, but five new service positions are needed, not counting the two or three new janitors that a new building requires. There are now fifteen instructors, including F. K. Richtmyer and R. C. Gibbs, both to figure heavily later in department affairs.

The July 1906 <u>Cornell Alumni News</u> reports on the dedication ceremony. It does not seem to have been reported by the <u>New York Times</u>. The building was dedicated on June 29, during a meeting of the AAAS held here. There were addresses during the meeting by the presidents of the

Carnegie Institution and of the AAAS. At the dedication, Nichols made some remarks, reprinted in the <u>Alumni News</u>, and there was an address by Elihu Thomson of GE. It was intended that Anthony and Moler both be present, but Anthony had been advised by his physician to stay home. The Ithaca Daily Journal carried a story on the building dedication, headlining: Immense structure with 220,000 square feet--485 (!!) rooms--Commodious lecture rooms--Well lighted laboratories--Impressive It reported the construction to be fireproof (!) with steel ceremonies. girders (wherever they might have been hidden), iron stairways (identical in pattern, railings and risers, to those still extant in Goldwin Smith), and concrete floors (in the basement (!) and at stair landings). Merritt read portions of a letter that Anthony sent in his stead for the occasion, telling how he had come to Ithaca and found the Physical and Chemical Laboratories in a "large barn-like wooden building" and physics taught only through lectures; no attempt at laboratory work. He found McGraw Hall just completed with a physics lecture room at the south end of the building; the seats were raised so much toward the back of the room that there was sufficient space between to provide a fair apparatus room. There endeth the reading, or at least the Journal's report. Presumably he used this to house laboratory and demonstration apparatus. Nichols reminded his listeners that it was the largest building for physics on the continent (478(!?) rooms, he claimed), spoke at length about the dynamo, and indicated the influence of Cornell Physics on physics elsewhere in the country. For example, at the St. Louis Exposition he said there were not only Anthony pupils on the electrical jury, but there were pupils of his pupils, and indeed, even a pupil of a pupil of a pupil of his "unto the third and fourth generation." So also the influence of Cornell Physics generally.

The ground had been broken for the building on August 1, 1904. No mention seems to have been made in the Ithaca paper on either August 1 or August 2 of the event. The only item spotted for those days of interest to physics was one noting that "Professor J. E. Trevor and his family arrived in town today."

<u>The Cornell Sun</u> for September 27, 1906, describes the new building briefly, noting that through a failure of the furniture contractor, old desks and chairs will have to be used for a month. The building had already been used during the Summer Session.

So far as the well-known John D. story goes--that he came for dedication exercises, took one look, and left in disgust--it is of interest to read in the New York Times for May 31 of that year, that he and his wife had embarked on a European trip; he was reported to be as "lively as a cricket all the way across the Atlantic," while his wife felt the weather and stayed below. A few days later the paper reported him to be offering--presumably from the high seas--25 cents for every snake killed on his Pocantico Hills estate! On July 29, one month after building dedication, the paper reports his return from Europe; he is said to have been the most popular of all the passengers on the Amerika; he shakes hands with everyone and leaves for Tarrytown and the snake-free estate. And the next day he is reported to have lived simply while he was abroad. There is no item on Rockefeller in the paper between 1904 and 1907 which reports him as visiting Ithaca; and they kept close tabs on the man. That should fix the tale, but it probably won't. It makes a good story.

Kermit Parsons in his book <u>The Cornell Campus</u> details some of the problems encountered in the acquisition of Rockefeller Hall, both financial and geographical. It seems that President White, on a boat trip home in 1900 during his German ambasadorship, found at his table the Rockefellers, John D., his wife, and their son. They were very pleasant, and he spent a fair amount of time with them on the voyage, coming to like Mr. Rockefeller "better and better the more I see of him." Within a year, John D. had given the university the quarter million for building and maintaining a new physics building on condition that the university raise a like amount for other construction. Actually, the trustees ultimately matched the fund themselves with the financing of a building for the "academic(!) departments," i.e., the future Goldwin Smith Hall.

White, even before the gift, worried where to put the building, writing President Schurman, "I would be glad to know where you propose to put your Physical building in case you can get it, for it seems likely that we are to be greatly embarrassed by lack of a proper site." White proposed running it east and west between Lincoln and McGraw Halls, dividing the quadrangle in two! Nichols objected; it was too close to the trolley line, and his electrical experiments would be disturbed. Later he would object also to any connection to Lincoln Hall for the interference their machine vibrations would have on delicate experiments in the department. Good man. He wanted the building to be on the hill above the guadrangle and to the east of East Avenue. For over a year plan after plan was developed concerning siting. It could be placed extending north and south along the length of the guadrangle, this was all right with Hiram Sibley, who had given Sibley Hall and who had objected to closing off the view of his building by a structure cutting across the quadrangle, east and west. For a time it seemed the building for the "academics" would go up on the hill which Nichols favored for Physics. But they objected; it was not conspicuous enough, it was too far from the library and too closely associated with planned locations for technical departments nearby (including Agriculture housing!). After a year of deciding this and scrapping that, only then to decide that and scrap this, the administration made the decision to leave the quadrangle open, putting Physics up on the hill and the academic building down on the guadrangle opposite McGraw, fifteen months of effort coming around finally to Nichols' original suggestion, all of which must have been most frustrating to Carrere and Hastings, the architects selected for both "our" building and that of the "academics." Bids were taken in one contract for both, a better "buy" In Parsons' opinion, neither structure represented the expected. architects' best work. Budget problems hindered them in the frugality to be associated with Rockefeller Hall. It was said we wanted some equipment out of the deal; some interiors were rumored to be left in a semi-finished state. Parsons feels it fair to compare the Rockefeller exterior to that of a turn-of-the-century "American secondary school" (albeit a cut above the usual), where I have reserved that view for old Morse Hall, now extinct.

By 1907, the department was settled in the new building. "The present year is memorable for this department as the first in which the work of the department has been conducted in its new laboratory, Rockefeller Hall." Moving began in June 1906, largely done by janitors. In spite of the difficulty involved in and immediately after a move, the added space more than compensated. (The same could be said when the next move came, in 1965, to Clark Hall, except janitors did not do the moving. The individual research units were moved mainly by the researchers and building maintenance personnel. The only teaching unit to move took all summer to shift some sixty or seventy experiments of the Advanced

Laboratory from the third floor of Rockefeller to operational status on the third and fourth floors of Clark, and the moving was done primarily by Nick Szabo, who was the regular laboratory technician, two paid graduate students, and one professor. Nick at the time had had only a few years with the department but by this writing has served the laboratory for some twenty-five years, being largely responsible for keeping it operational and on an even keel. A tough job.

The new Rockefeller Hall was a source of great pride for the department, for its purpose a facility unsurpassed in the nation. Suggestion that space for research may have been underestimated in spite of the planning, does not come until the report of 1908, when it is already reported that in "a suite of some forty separate rooms--for original investigation," all but four are in actual use, and it "seems likely that it is this important portion of the work of the department that lack of space is likely first to be felt." Researchers turned out a "scientific output unequaled in amount and not surpassed as to quality by any Physical Laboratory in this country." Presaging the future, in the forty papers listed in the report, there was a paper on the "Electrical Conductivity of Silicon at Various Temperatures" (Miss Wick) and one on the "Influence of Electrical Oscillations upon the Edison Effect" (Orin Tugman on what was in reality thermionic emission. Tugman, incidentally, went on to the University of Utah, where he many years later had as student one Lyman Parratt, whom he encouraged to follow physics. There also Parratt tangled amicably with a Dean Gibson of Mathematics, father of Rhea, years still later to be Lyman's wife.) Always there are papers on fluorescence and luminescence. Apparatus is still very much in short supply.

The report continues in the same vein but makes the additional and noteworthy statement: "The country is filled with our graduates who are teaching physics and these men have had a profound influence upon the educational standards in this science which they would not exert had they not been for a time under the influence of a body of men in this department who are enthusiastic investigators as well as faithful teachers." A list compiled by Grantham and Howe makes this point perhaps even more tellingly. In spite of its straddling well into what I am calling the "Modern Era," I append their list, though it is very incomplete. It may serve to indicate something of the extent to which Cornell Physics has

played (and continues to play) an important role in the education of physicists elsewhere.

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PARTIAL LIST OF CORNELL TRAINED PHYSICISTS

WHO BECAME TEACHERS ELSEWHERE

| Frank Allen* | 1902 | University of Manitoba |
|------------------|------|------------------------------------|
| S. J. Barnett | 1898 | University of California |
| Charles Bidwell* | 1914 | Lehigh University |
| A. A. Bless | 1927 | University of Florida |
| Thomas Brown* | 1916 | George Washington University |
| C. D. Child* | 1897 | Colgate University |
| A. L. Foley* | 1897 | University of Indiana |
| L. W. Hartman* | 1899 | University of Nevada |
| Percy Hodge* | 1908 | Stevens Institute |
| R. M. Holmes* | 1923 | University of Vermont |
| H. L. Howes* | 1915 | University of New Hampshire |
| C. Y. Hsu | 1933 | Lingnan University (China) |
| Charles T. Knipp | 1900 | University of Illinois |
| W. N. Lowry* | 1929 | Bucknell University |
| S. S. Mackeown | 1923 | California Institute of Technology |
| Louise McDowell* | 1909 | Wellesley College |
| C. R. Mingins | 1935 | Lowell Polytechnic Institute |
| E. F. Nichols* | 1897 | Dartmouth College |
| Paul Northrup* | 1926 | Vassar College |
| E. D. Palmatier* | 1951 | University of North Carolina |
| J. B. Platt | 1942 | Rochester University, |
| | | Harvey Mudd College |
| H. J. Reich | 1928 | University of Illinois, Yale |
| L. A. Richards | 1931 | Iowa State University |
| R. W. Shaw* | 1934 | Cornell University (Astronomy) |
| G. W. Stewart* | 1901 | University of Iowa |
| 0. M. Stewart* | 1897 | University of Missouri |
| C. W. Waggoner* | 1909 | University of West Virginia |
| H. E. White | 1929 | University of California |
| Frances G. Wick | 1908 | Vassar College |
| R. C. Williams | 1935 | University of Michigan |
| (Astronomy), | | Berkeley (Biophysics) |
| P. I. Wold | 1915 | Union College |
| | | |

^{*}Indicates that the person named became the head of a physics department at the school listed.

THE MIDDLE YEARS---ROUGHLY 1905-1945

Upon completion and occupancy of Rockefeller Hall, the Physics Department entered what, in 1983 for want of a better name, we are labeling its "Middle Years." Nichols would still be chairman for another fifteen years. The building of Rockefeller Hall was probably the most significant local accomplishment of the department in the Nichols' regime. As noted earlier, however, the expansion of quarters and staff was essentially forced on the university by the demands placed on the department by other expanding units in the growing institution.

Moving into the new building were faculty members Moler, Nichols, Merritt, Bedell, Shearer, and Blaker, along with numerous instructors, including Richtmyer and Gibbs. Shearer and Richtmyer died years later "in the harness," Blaker resigned and went to work for Goodrich, the others eventually became emeritus. When his own retirement came, Merritt particularly liked that: Professor E. Merritt, Professor Emeritus. Joseph Trevor, the thermodynamicist, was around, but in Chemistry until 1910, when he became affiliated with Physics, staying until his retirement in 1940. In 1909 came two new names: students C. C. Murdock and Harley E. Howe, followed by Charles Bidwell in 1910. Before Nichols retired ten years later, in 1919, the names E. H. Kennard, J. R. Collins, and G. E. Grantham had appeared, again as students. Bidwell left as an associate professor in 1927 to become chairman of the department at Lehigh; Grantham had disappeared for several years to the Postgraduate School of the Navy and came back permanently in 1928 when Bidwell left. Howe. who had been "sent" to Cornell for his graduate study by our 0. M. Stewart at Missouri, also had a period away--at Randolph Macon--before joining the department professorial ranks. (It was understood that Howe and Grantham would likely not be researchers--their main endeavors would be in the teaching but they would be no less rewarded for a good job than would be the research types. There was some sentiment around later from varied intimation let drop that that had not exactly happened.) All were associated with Cornell for many years, Kennard resigned to continue his World War II work at David Taylor Model Boat Basin, and Collins died shortly after that war. As Grantham and Howe have pointed out in their

writing, the department was largely built around its own graduates, and this situation obtained until late in the period, before World War II, when Livingston, Bethe, Bacher, Cady, Parratt, and Rossi appear. Thereafter, in our "Modern" era, the roll was increasingly made up of people trained at places other than Cornell.

Names associated with the Nichols' period, other than those we have mentioned, are a number who went to the early Bell Laboratories: O. E. Buckley, Ralph Bown, P. Mertz,³ H. Pidgeon, Joe Becker and J. C. Schelleng, all of whom were active in the laboratories when I went there in 1939 and with whom I interacted one way or another (save Buckley and Mertz). Buckley rose to become head of the outfit; Bown became their Director of Research. Schelleng was my supervisor at the Deal, N.J., labs for a short period in my first months of employment, being in charge of trans-Atlantic radio telephony development which involved high power electron vacuum tubes that I was to be working on in New York. I recall well the morning he and I learned of the sudden death of Richtmyer. Schelleng was an accomplished cellist and had been Chimesmaster here while an undergraduate. In his late seventies, he wrote an interesting article for <u>Scientific American</u> on the physics of the violin string, illustrated with experiments he had done in his own basement. An attractive person with a nice way about him, he was somewhat worrisome but with a pleasant sense of humor. Earlier, in his chief scientific work with H. W. Nichols (there seem to be a lot of Nicholses around here), he developed the theory of radio wave propagation, taking account of the earth's magnetic field, which theory explained a number of observed phenomena in radio transmission.

There was a group who went to the National Bureau of Standards: Coblentz, noted for radiation measurements, F. G. Nutting, E. K. Plyler, and E. C. Crittenden, Sr. E. C., Jr., was a graduate student in my own days as such, did a cloud chamber thesis with Bacher, and owned that boat with Ben Moore we heard about earlier.

Another notable, one-time graduate student and instructor in the department, was H. G. Dorsey; not N. E., who with Rosa at the Bureau of Standards measured the ratio of esu to emu to check out c. But our Dorsey

³ Mertz, the last surviving of this group, died in the summer of 1982, leaving the Physics Department, by his will, the sum of \$3,000.

also went into government service, winding up as chief scientist of the U.S. Coast and Geodetic Survey and inventing the well known Fathometer for determination of ocean depths, profiles, and such, which have been of prime importance in pointing geology to plate tectonics.

There were undoubtedly names of undergraduates of the period who became well known which should be cited. One forever to be remembered in physics is that of Lester Germer. He graduated in 1917, flew an airplane in the war, went to Columbia for his graduate work (anticipating the route of I. I. Rabi) and then to the Bell Laboratories (in those early days, it was part of Western Electric), and became associated with Clinton Davisson. The two names are connected with the first observed wave diffraction of electrons--from a damaged nickel crystal. There was always some sentiment that Germer had been slighted in not also sharing in the Nobel prize, with Davisson and G. P. Thompson. Davisson was close to retirement when I went to the laboratories; I sat in on an out-of-hours course in electron optics that he gave. His son, Dick, was a very bright graduate student in Cornell physics after World War II, working with Greisen on a large cosmic ray experiment. Germer liked Cornell; he returned to his Alma Mater for his retirement years, bringing his Bell Labs donated apparatus with him to Engineering's School of Applied Physics to continue work on low energy electron diffraction, the field to which he had returned after working for many years on contacts, an item of some concern to a telephone company. Quite a character he was. At his fiftieth college reunion, he walked in from Binghamton. Of a cold winter's morning, he and Professor Jay Orear would scale the ice up Ithaca Falls. Crazy. He died of a heart seizure while rock climbing in the Shawagunks some years after he took up retirement residence.

It was near the end of the Nichols' regime that the country became involved in World War I. Unlike the situation thirty-five years later and the upheaval it spawned, the first world struggle did not change physics nearly as greatly as did the second. This was particularly the case in this country which entered the war rather late in its course. Chemistry was the science greatly affected by the first conflict, and Chemistry expanded rapidly thereafter (although it did not take off until late in the twenties; jobs were scarce in the first years following the war). Nevertheless, physics was involved, if only peripherally. There was a committee, headed

by Thomas Edison, which undertook the task of identifying the areas where physics and technology could make a contribution. Little can be said of its benefits. A letter from Merritt, however, protests the absence on it of any physicists from academia; Millikan concurs in the protest. University physics departments were not greatly changed. They were of course involved in training programs undertaken by their institutions, and there was minor war related development. At Johns Hopkins, R. W. Wood, beyond the development of a secret signaling system, was proposing the use of dolphins in the hunt for submarines. It was a time for radio to blossom, and the airplane was taking flight. The Cornell Department was active in each area. Bedell gave a course on the airplane; not how to fly it but, rather, how does it fly. His two books on the subject came out of that. It is of interest that airplanes were built in Ithaca as well. Radio was a department interest; for many years after, two radio antenna masts stuck up above the ventilators gracing the Rockefeller roof as reminders, one still present (now gone in 1983). A number of graduate students left the department for work in the radio (non entertainment) field, some appearing years later in the upper echelons of industrial communications laboratories.

Kevles, in his book The Physicists, has much to say concerning the involvement of physicists in the war. There were serious problems: the submarine menace, again to be seen in World War II; the location by audio ranging and triangulation of enemy artillery batteries. And these were matters about which physics might do something. The big names of the period in this regard were Ellery Hale, the astronomer who seems to have been the most prominent (at least vocally) scientist in the country, R. A. Millikan, A. A. Michelson, and Frank Jewett, among others. Of course, the pragmatic Edison was in there, heading up the Naval Science Board of Inventions, but he was hardly a physicist, technologically prolific though Nichols in his reports to the university president, indicates that he was. the number of Cornell department personnel had undergone severe reduction because of the science war effort. We seem, however, to have extensive records only about Merritt's involvement. After the United States was engaged, he went to New London, where submarine detection work was pursued. In the Archives there is a large amount of correspondence and reportage relating to his period at New London. There

are letters from and to Millikan, Michelson, and Jewett; nothing seen of Hale. The name V. Bush appears signed to some correspondence; his was the big name twenty-five years later. There are reports of tests carried out in the New London waters; these involved hydrophonic detection and magnetic induction techniques, and they were not unsuccessful. In fact, some two decades later, after Pearl Harbor, Merritt writes Frank Knox, Navy secretary, asking why the harbor was not protected by an induction cable on the floor of the harbor entrance; it was not a totally useless tool. Merritt is later pleased to learn that a cable was in place, it did detect submerged vessel penetration, but in the chaos of the day there were too many other things demanding utmost attention; at that, one or two enemy submarines were sunk.

But overall, the contribution of physics to the first great war was rather small, and little more need be said of it and the Cornell department.

It is to be presumed that the Merritts were particularly unhappy at the turn of events which had happened in Europe in the second decade of the century. Mrs. Merritt's heritage was of European, her native language Swiss German, and while he came from un-German Indiana, Merritt himself had many German friends, having done graduate study over there with Max Planck and others twenty years earlier in 1893 and 1894, which was six or so years after he entered physics. He has many pictures in the file of German physicists, all familiar names. There is a particularly fine one of a glowering, powerful Helmholtz before a blackboard filled with equations, taken at the end of the last lecture he gave before his death, as it happened. Many of the photos, however, are on small thick cards about the size of a playing card, printed up with the name below of the professor portrayed. One wonders if they were regarded as were the cards of baseball heroes American kids used to collect and trade.

To physicists, the Merritt file is of interest quite beyond our local history. There are the notes that Merritt took of Planck's courses "Warmtheorie" and "System der Physik," all neatly bound together. There is a New Year's greetings from Planck, 1939 the year, when things must have been most gloomy for Planck. He stood up to the Nazis; one of his sons was later to be shot for involvement in the unsuccessful plot against Hitler. It was quite characteristic of the Merritts; during the war they were active in the Bundles for Britain aid program, after the war with the

American Friends Service Committee in relation to relief for friends in Germany. In particular, with Professor Cope of Pennsylvania, they made an effort to get in touch with Planck and his family, to learn of their welfare, and to get CARE packages to them. This they managed to do. There are many letters from Planck's wife, Marga, and son, Hermann, expressing their appreciation for what had been done for them. Planck himself does not write in this period. In fact he is very feeble and failing. However, there is a letter to Merritt from Yale's Walter R. Miles, which describes what must have been a tremendous and moving experience for the old man, in connection with a Newton Celebration of the Royal Society in 1947. Planck had taken to his bed and considered his life over; he did not expect to get up and out again. At the celebration in London, a page in loud voice announced the honored guests, one by one in turn, names appearing on the official list. Each got up, shook hands with the president of the society and then returned to his place. The list was finished and the page then went on and announced "Professor Max Planck--from no country." There was a standing ovation as Planck slowly arose and was helped up to greet the president. The society had made special arrangements to get him there, going so far as to charter a small private Miles regrets Merritt's not being there with his friend and airplane. former professor. The "from no country" was the page's error; Planck was not listed. This was rectified the next day with the citation "from the world of science." Miles goes on to tell of a small dinner he gave for the Plancks to which he invited Bohr; Planck had hoped for a conversation with Miles writes that things went pretty slowly at first but that they Bohr. warmed up and overall it went very well.

At the end of this correspondence file is a large black-edged notice. Frau Marga Planck has lost her husband, March 1, 1949. It is an experience somehow to enter in this way into the private lives of such notables. There is a fine portrait of a much younger Planck, somewhat reminiscent of Murdock if he had sported a mustache. At the bottom is a greeting to Merritt. A copy of this is now in our own department file.

Two minor items are in a scrapbook the Merritts kept: one, pertinent to Cornell Physics Department society, is an engraved invitation: Mrs. Ernest Merritt Mrs. Ernest Blaker at home Saturday November fourteenth from three to five o'clock Thirty-nine East Avenue

(Year unknown)

showing that we are somewhat less formal today with our social functions. The other item is not even related to physics and is of interest only to one who has read of Arctic exploration. There is the program of the Sage Chapel memorial service for Ross Gilmore Marvin. Ross Marvin was the Cornell geologist who perished on Peary's last expedition, on which Peary claimed to have reached the North Pole. Peary himself spoke at the service, citing the fact that Cornell colors had been farther north than had been any but an American flag, a somewhat dubious distinction it would seem.

While the Merritts were of Quaker persuasion, they were by no means peace-at-any-price pacifists. In 1915 Merritt had written Navy Secretary Josephus Daniels, proposing means for signaling to and from submerged vessels and urging the establishment of a Naval Research Laboratory. The station at New London was about as close as it came during the period. When America got in the war, he wasted no time in urging that physics be used in combating the submarine and in locating concealed artillery. In World War II, he is quick to write Vannevar Bush asking him to let physicists know what needs to be worked on so they can get at it, and he writes Lee DuBridge at MIT of various techniques and devices possible. Three months after the end of World War I, he suggests radio as a means for locating vessels in the fog in order to prevent collision.

At the next important milestone in the history of the department after the occupancy of Rockefeller Hall, we have the retirement of Nichols in 1919 and Merritt's assumption of the chairmanship of the Department, which he held until Gibbs took over in 1934. (Technically speaking, the early heads were "heads of the department," appointed by the dean with some connivance of department members; from Gibbs on, they were

department "chairmen," elected by the department members and concurred with by the dean. We are not worrying about such niceties herein.) It would seem that my admission to Graduate School occurred in the Merritt regime. Chairmen held on to (more likely, were confined to) the job for longer periods than is the norm these days. In present times, it must certainly demand much more "dog" work and bureaucratic shuffling about. The department, and the university, has become an organization <u>much</u> more complicated, as indeed has life itself.

Nichols' retirement came at the time of the university semicentennial. There was to be a celebration carried out during the graduation weekend, June 19-23. The quadrangle statue of the founder was to be unveiled on Sunday with Graduation exercises coming the next morning. The Physics Department organized for itself a concurrent celebration. There is in the Archives (Merritt file) a printed program for the affair.

Program of

The Physics Conference and Reunion In honor of Edward Learnington Nichols upon completion of 32 years of service and retirement from active duty as Head of the Department of Physics

Thursday morning, there was the inspection of the Rockefeller laboratories. Early in the afternoon a seminary reunion, a gathering of members of the Physics Seminary during Nichols' 32 years, following which, late in the afternoon, was to be a meeting of the seminary with dinner coming after. The seminary program included three papers by former Cornell graduate students: Barnett on "Electromagnetic Induction," Captain Ralph Bown on "The Vacuum Tube and the Development of Wireless Telephony," and George W. Stewart on "Binaural Hearing and its Application to the Location of Air and Water Craft." The next day the university and guests heard addresses by President Schurman, Governor Hughes, and Governor Alfred Smith and Judge Hiscock, representing the trustees. Of these notables, Hughes was to wind up Chief Justice of the Supreme Court, and Al Smith was to be beaten soundly in the 1928 campaign for the U.S. presidency by Herbert Hoover. Captain Bown would become head of research at Bell Laboratories; I would come to know him thirty years later.

Three weeks prior to the retirement celebration, the Board of Trustees met on May 31 and adopted the following rather unusual resolution, in tribute to the contributions of Nichols:

On the retirement of Edward Learnington Nichols after serving Cornell University for thirty-two years as Professor of Physics, the Board of Trustees and the members of the Faculty desire to record their appreciation of his attainments as a scholar, of his influence as a man both within and beyond the University circle, and of the wisdom and hospitality of mind that have made him a leader and a beloved colleague.

He was graduated from Cornell University in 1875, spent four years teaching abroad and one in this country in advanced study and, after teaching in several institutions, returned to his Alma Mater in 1887 as head of the Department of Physics. Professor Nichols has conducted his department with conspicuous success and has seen it grow from three members to a staff of thirty-eight teachers and investigators; he planned and secured the erection of a great laboratory for teaching and research; he has understood how to subordinate administration to scholarship and, while carrying his full share of the duties of teaching, has enriched science not only by his own contributions but by the contributions of those who received their initial inspiration in his laboratory. Outside the University he has exercised a profound influence in both pure and applied science; he founded the <u>Physical Review</u> and was for twenty years its editor; in the councils of various scientific bodies, as in the deliberations of the Faculty, his well-considered, broad views have carried great weight.

Professor Nichols has exemplified in his career a striking combination of attributes; courage united with gentleness, tenacious adherence to conviction with tactful patience towards opposing minds, progressiveness with tolerance, perseverance in seeking new knowledge with a conservative regard for old ideals and approved traditions. Relieved of the burden of routine duties and surrounded by his friends, we wish him many years of happy and fruitful study.

It is perhaps of some interest that at this same trustee meeting a gift of one hundred thousand dollars for an endowed chair in History was read into the record. Today this Stambaugh Professorship is held by Pearce Williams, Historian of Science. Coincidentally, also at this same

board meeting, Onthon Guerlac was advanced to tenure and full professorship of Romance Literature and Languages. His son, Henry, in due course also became a Cornell professor in the History of Science and is now retired. The space limitations of Rockefeller Hall were beginning to be really felt, but no solutions were advanced until the early thirties. There are in the library Archives, drawings of a proposed (1932) modification of the structure, converting it to upstate-Gothic, a squarish tower rising above the roof line. It would have had far greater floor space for research, teaching, and facilities. That was it: plans.

Too many students from Engineering were failing physics, a complaint that goes back well into Nichols' time in fact. A <u>Sun</u> editorial of April 1909 is titled "Physics 8, 10, and 14." There were only two instructors respected by students, the editorial lamented, the editor concluding by thanking a "kind providence that he's never had to work as so many have in Physics 8, 10, and 14." So it goes.

In the same vein, one reads a 1930 report of the subcommittee on Physics and Mathematics in the College of Engineering concerning Physics 6 (8, 10 and 14 seem to have vanished). Only 62% of the boys had passed. "It is the opinion of this Committee that the primary cause of student failure in Physics 6 is the lack of sufficient study of the assigned work on the part of the students." That's an admission all right. The broad scope of Physics 6 is cited- "55 different fundamental topics in the relatively short period of 15 weeks" (not the present thirteen plus). Dean of Engineering, Dexter Kimball, in traveling around the country keeps hearing how tough Cornell is; there is no point in a normal man's going there and "particular reference is made to mathematics and physics."

The department underwent no great staff expansion during Merritt's chairmanship. Blaker had recently left. Howe had returned in 1918 as an assistant professor following his six or so years professing at Randolph-Macon. Gibbs and Richtmyer had become professors, Murdock an assistant professor; Kennard became an assistant professor the year Merritt took over, and Collins moved from an instructorship to assistant professor three years later. Nichols had hoped to promote Kennard a year or so earlier. He wrote Merritt a letter in 1917 when the latter was at New London on his war activity. Reporting on local matters, he tells that "Kennard has broken down and has to live out of doors for a year." TB

probably. That was the popular cure of the time, widely practiced by a sanitarium at Saranac Lake. So his promotion is delayed. Kennard was apparently a good teacher, although from experience I was not inspired by his wave mechanics. Later, Merritt, in recommending to the president a raise for Kennard, tells how Kennard gave a recent lecture on relativity, a model of clarity, getting across more in his one lecture "than Silberstein had in five."

In a sense, the permanent personnel did expand, but some of those who were part of the expansion had been here earlier. There was one Forrest G. Tucker appointed assistant professor in 1923, but he lasted only three years. Henry Barton, remembered for his role in the American Institute of Physics, was an assistant professor for two years, 1930-32. So the permanent staff did not grow much. Perhaps the best known name tied to the department during the Merritt period is that of H. A. Lorentz, who held a specially endowed Schiff professorship in the department for the fall semester of 1926. A very nice picture, long in the department, shows him lecturing in old Lecture Room C, leaning over the desk at the front of the room. Another department photograph shows him in company with Merritt and someone else in the top row of Schoellkopf stadium at a football game, blanket over their knees against the chill.

In the library Archives, in a Merritt box, is a folder labeled "Lorentz"; there are more pictures of him in Lecture Room C, all together the best his daughter had ever seen, she wrote Merritt. There is correspondence between Lorentz and Merritt concerning arrangements for his visit, Merritt's letters typed and those of Lorentz in neat, fine script, and in English. He has been to America before but would like to come again and do less traveling around, staying more or less put in one or two places. How fortunate that one of the places to which he'd like to return is Ithaca. He has to put it off a year but is to come then in the fall of 1926 for ten weeks and will give lectures. In the spring Merritt writes inquiring what they shall be titled. There is a two or three month delay in a reply, for which Lorentz apologizes; he has been involved with the League of Nations Committee on Intellectual Cooperation and was "absorbed in hydrodynamical problems that are connected with the projected draining of our 'Zuider Zee' and with which my government

charged me." Lectures were on problems in modern physics, the electron, and such.

Not known perhaps is his poetry. There is a Christmas card for young Howard Merritt with the following, handwritten on the back:

May the New Year so turn out That you become a good boy scout As healthy and strong as a boy can be These dear Howard, are our wishes for thee.

> and signed by H. A. Lorentz A. C. Lorentz-Kaiser (his wife)

Further along in the file is another of those black-edged cards, this in a black-bordered envelope addressed to Merritt in 1931 from Holland, announcing the death of Lorentz. Besides his wife surviving there is listed a daughter, deHaas-Lorentz. Was her husband that well-known physicist? I don't know but it could well be.

In the year 1926-27, the name of Lloyd Smith appears in the department record, as a Coffin Fellow from General Electric, starting his graduate study. He was made an instructor a year later. Apparently instructors then and in the years preceding were also graduate students. The category was reserved for those holding the Ph.D. after 1935 or 1936.

How Smith came to be a graduate student at Cornell is interesting. His first choice of school was Princeton. From GE he applied to Princeton for a fellowship. He received, in reply, word that in no way could he have a fellowship; he did not have the necessary background in the classical languages--Greek and Latin. Somewhat dismayed, he then thought to apply for GE's own Coffin Fellowship. If he managed to win it, he'd go to Princeton on that. He had done well in GE's famous test course and had become acquainted with people in the Research Laboratory, including Princeton's K. T. Compton, who was in residence for the year. He did win the fellowship and triumphantly sent word to Princeton. Word came back even more restrictive; he couldn't even be admitted, not being able to read the classical literature! That was it; even Compton said they couldn't get past the hurdle of the then dean for admissions. Whitney, director of the laboratory at GE, with a daughter here at Cornell, suggested that Smith and he take a trip over here together to see the place. And they did. Smith found there was no problem with being admitted, the program looked interesting, and he came. He felt he had done the right thing when, in the fall, he went to his first colloquium in Lecture Room C. An old dignified gentleman was hanging up his hat outside the room (in those days that was safe enough) and turned to Smith to introduce himself. "Good evening; I'm H. A. Lorentz," he said. After his Ph.D., Smith had a year at Cal Tech as a National Research Council Fellow and another in Europe, including time at Munich with Hans Bethe, which was to prove of great importance.

One name which is rather curious is that of D. T. Wilber. He appears on Moler's chart first in 1914, over the Code identification "R," Research Assistant. He had graduated in Chemistry from Cornell in 1911. I wasn't even born! He worked with Nichols, Merritt, Howes, Wick, et al., in fluorescence and luminescence. In 1918 he became an Instructor for two years and then reverted to "CRA," Carnegie Research Assistant, for the rest of the time until he left for the duMont Laboratories as chief chemist, nearly thirty years after he first appeared on campus. He earned his Ph.D. (in Chemistry) in 1932. He must have had some other source of income, or so one hopes. He was here for a few years after I arrived; to the graduate students he was something of a "lone wolf" in the department; much older than the rest of us, he never seemed very close. He was, however, always the first one in the community each year to get into Beebe Lake for swimming--usually sometime late in March or early April, if memory serves. A hardy soul. There is in the Archives Nichols file a 500-some-odd pages long, typewritten manuscript summarizing the years of work here in the luminescence field. The copy is so fresh that it seemed it must be something of Wilber's doing. A look through the Science Library catalogue indeed reveals a 350-page volume written by Nichols, H. L. Howes, and Wilber, a report for the Carnegie Institution of Washington that was published in 1928. The draft is that from which the report is copied; it looks as though it had been typed yesterday.

Of the many people who had served the department, say up to 1935, and perhaps even to this day, none was involved in more outside activities than Floyd K. Richtmyer, touched on previously. He graduated from Cornell and got his 1910 Ph.D. here, beginning his Cornell career while a graduate student. He was in the sophomore engineering courses with Blaker, took

over when Blaker went to Goodyear. Understudies for him were Bidwell (who subsequently went to Lehigh) and Grantham, who in due course took over the engineers. Richtmyer always insisted that if something is to be taught, it must be taught well; and he did teach well. His interests there led to his being an organizer of the American Association of Physics Teachers; he was also an organizer of the Optical Society, and member of several others. It was not mere membership in this or that body or committee; he was <u>active</u> in fifteen professional bodies. He was, at one time or another, president of four of them: the American Physical Society, the Optical Society of America, the American Association of Physics Teachers, and Sigma Xi; he was vice-president of four others. He was the editor of the <u>Review of Scientific Instruments</u> and of the <u>Journal of the</u> Optical Society. Besides all this, he was a life trustee of the National Geographic Society and active in church and a couple of local clubs. He had 94 papers and 60 meeting abstracts to his credit, mostly in X-rays. lt. was from his X-ray measurements on absorptivities that the famous lambda Z⁴ law was obtained. How he could do all this is remarkable. He was a professor of Physics after all and, on top of that, like Merritt earlier, dean of the Graduate School for several years! One gathers that these duties were all well done. Many have testified to his excellence as a teacher. There were those around who felt he was too much of the big time operator to take his share of department chores and that when Gibbs was named to the Chairmanship over him, the right choice had been taken.

In 1928, his Introduction to Modern Physics was published, coming out of his best-known course. This was one of the first texts on the subject, certainly in America. For many physicists it was their first exposure to "modern" physical ideas. That its impact has been considerable many be gleaned from the fact that it is still in print, having gone through six editions, a first revision having been made by Professor Kennard. Lauritson (Tom) at Cal Tech made a second revision, and there has been yet a third by a Cornellian, John Cooper, a graduate student of my years. Richtmyer's editorship of the McGraw-Hill International Series of Physics texts came concurrently with his own book.

Richtmyer was raised in or near Cobleskill. After earning his undergraduate degree, he was instructor at Drexel Institute for a couple of years, before returning to Cornell for his Ph.D. Except for leaves, and a period in the Signal Corps during World War I (Major Richtmyer), he remained at Cornell--at least for a good fraction of his time. But one must surmise that he was not entirely happy in the department. When I got here in 1934, he was not taking much part in department affairs; it was rather common knowledge, even to students, that Richtmyer and Gibbs did not exactly see eye to eye. Gibbs had by then assumed the chairmanship. The source of the friction is not known, but it went back many years apparently. They had arrived here themselves at about the same time and had risen through the ranks together. But there is also a hint of Richtmyer's unhappiness even with Merritt in some Bedell correspondence in the Richtmyer file over in the library.

In 1923, while in the west, he was offered by Whitney at the GE Laboratory "an excellent position in his laboratory." Richtmyer so wired Bedell from Berkeley. Bedell was substituting for Merritt during a sabbatical leave. Could Bedell consult Farrand--"advise by day letter collect regarding opportunities position and salary at Cornell present and probable future." Poor Bedell found Farrand gone on vacation but wired him recommending "retaining" Richtmyer" if possible ... present salary four thousand five hundred." Farrand wrote back to Bedell that he "is loathe to see him [Richtmyer] leave," his future is "second to none;" and he raised his salary to \$5,000. The response persuaded Richtmyer to stay at Cornell, and he wrote to Bedell to that effect. Bedell wires him:

Letter received. Gratified that you consider sacrificing so favorable opportunity elsewhere to remain here, thus admirably indicating desire to loyally work with conditions as from time to time exist. I think your criticisms are largely past history or misunderstanding. I thoroughly approve administration of present permanent head. All desire betterments where possible. When they are not possible all must work in cordial cooperation under such conditions as obtain.

Frederick Bedell

Bedell subsequently writes Richtmyer during the latter's swing around the country en route back to Ithaca. He hopes that Richtmyer will enjoy getting back to Rockefeller:

... nowhere can one pursue his work in greater freedom. Careful consideration on your part must lead you to the same conclusion, despite any contrary feelings you may

have had. I feel convinced that in the Department no one is less hampered than you. Successful cooperation calls at times for sacrifice of individual views and from this limitation no one of us, including the head of the department, is exempt.

Bedell knows that Merritt "acts impartially and unselfishly." "You and I can't shift responsibility in what is the matter with Cornell. You and I have been Charter members of the standing Committee on Organization." There seems to be no record of what was bothering Richtmyer. But Bedell is not taking any guff.

There was a time when it was suspected that besides the K, L, M, N, etc. X-radiations there was also a still higher energy than K-radiation, namely, as might be guessed, J-radiation. Barkla thought there was evidence for such. Richtmyer provided contrary evidence in failure to find absorption discontinuities at supposed J-edges.

He also got involved in a couple of <u>National Geographic</u> expeditions. For one, he was a consultant and visited the camp of the 1934 manned high altitude balloon flight of Captain Stevens, who, incidentally, on that ascent took the first photographs showing directly the earth's curvature, giving a talk on the ascent at the 1936 APS Washington banquet. Richtmyer was on the 1937 Canton Island eclipse expedition, where he studied polarization of the corona. He was preparing for another at the time of his sudden death, two days before that of retired President Farrand. There is no indication of any coronal interest he may have had in 1925, when the eclipse was in Ithaca.

His association with the Geographic and their eclipses, led to the society's funding for some years the auroral program which was initiated by Carl Gartlein in the late thirties, actually well after Merritt's retirement. His name first appears on the Department Roll in 1925 as an assistant; he becomes a graduate student instructor and remains that until 1936 when he is made "curator," something of a jack-of-all-trades in department research, a position somewhat akin to that which Moler apparently held in his later years, a position that seems to have no counterpart in one man today. It involved the supervision of the service personnel in the Rockefeller basement and being the ever-ready consultant on various research problems and techniques. In the ten or fifteen years
before his sudden death, the auroral program, however, was clearly his first priority.

Mentioned earlier was the ruling engine set up in the constant temperature room in the basement of Rockefeller, at this time one of three in the country. It was natural enough that the other two were at Chicago (Michelson) and at Johns Hopkins (R. W. Wood). Moler had worked arduously with it, and now Gartlein and Wilber spent a fair amount of time with it in an attempt to make it rule first class gratings. I don't know if it ever did make anything very good; there used to be a lot of scrap examples of its work in a box down in Gartlein's research area. In those days, the performance of such an engine depended in large measure on a very precisely machined screw, besides other vagaries encountered during the ruling run, which could take days with a large grating. Things have improved greatly, first with interferometric control of the displacement of the ruling diamond, and today, with the production being carried out essentially photographically with lasers and holography.

Interferometry and precision machinery, screws in particular, bring to mind an incident which fits in here, although somewhat out of time. It was in the early sixties and the government was lending more and more support to science. We had put in a request to the National Science Foundation for a really first class cathetometer and a research grade Michelson interferometer with various attachments, to be used both for research when needed and for teaching purposes in the Advanced Laboratory course. They were ordered from the prestigious optical house of Gaertner. It was a proud day when they arrived, but it was some time before I could bring myself to let an ordinary student measure the wavelength of the mercury green line with the beautiful interferometer-a real joy after the previous clunkers we had (still around and still useful, however). I finally assigned its use, and the student came up with a value for the wavelength which was off by some 1%--an intolerable performance in the hands of a good experimenter, which this student was. So I had him do it over. The same result transpired. I tried my hand at it; still off about 1%; something was wrong. I sensed that a part was slipping. The main mirror is driven by a screw turned through a graduated bronze worm wheel by a worm also with graduated dial. One hundred turns of the graduated worm dial should have advanced the main mirror by

exactly one millimeter. It was definitely doing a shade less. I wrote Gaertner to find out how I should take it apart to see what was slipping inside, or had they done it badly? They responded quickly with a drawing, indicating that there was not much to slip, and they certainly had not done it badly. So I got it apart and, in truth, there was nothing there to slip. I finally counted the teeth on the worm wheel; it came up one tooth more than the one hundred it should have been! Gaertner could not believe it. I had to send the worm and worm wheel to them, and they confirmed it, reporting that the example had thrown their shop into a real turmoil--how could it ever have happened? How many similar instruments they had made with the same error built in, they never indicated. I, of course, soon received a proper worm and worm wheel along with abject apologies, and the interferometer has been in use to this day very satisfactorily. It has been used but once in research, I believe.

The reputation of the department in Merritt's time was good. We were widely recognized and many famous figures in physics came by. Lorentz we have seen; there were Sommerfeld, Kramers, Born, Millikan (for a few months), others. The visit of Born resulted in his being offered the professorship cited earlier. In addition, in 1927 Professor Gibbs initiated the interesting experiment (described in the first section) of bringing distinguished physicists here to lecture and work over the summer. Grantham and Howe provide the following list of summer visitors during this period:

| K. T. Compton | Princeton | Electron Theory of Matter |
|----------------|---|---|
| W. F. G. Swann | Bartol Institute | Relativity |
| A. H. Compton | Chicago | X-rays |
| E. C. Kemble | Harvard | Wave Mechanics |
| C. D. Ellis | Trinity, Cambridge | Radioactivity |
| W. V. Houston | Cal. Tech. | Modern Theoretical Physics |
| E. 0. Lawrence | Univ. Cal. | Theory of Electrical |
| | | |
| P. S. Epstein | Cal. Tech. | Modern Thermo dynamics |
| J. Franck | Johns Hopkins | Atomic and Modern Physics |
| F. Rasetti | Rome | Matter and Radiation |
| F. Bitter | MIT | Magnetism |
| | K. T. Compton W. F. G. Swann A. H. Compton E. C. Kemble C. D. Ellis W. V. Houston E. 0. Lawrence P. S. Epstein J. Franck F. Rasetti F. Bitter | K. T. Compton Princeton W. F. G. Swann Bartol Institute A. H. Compton Chicago E. C. Kemble Harvard C. D. Ellis Trinity, Cambridge W. V. Houston Cal. Tech. E. O. Lawrence Univ. Cal. P. S. Epstein Cal. Tech. J. Franck Johns Hopkins F. Rasetti Rome F. Bitter MIT |

In 1930, the department hosted a summer meeting of the American Physical Society, much enlarged from the first years. There was unhappiness that the meetings had become so large that the intimacy and informality of the old gatherings seemed gone. It was hoped to restore a bit of the former style with a summer meeting at Ithaca. It was small enough that Professor Vladimir Karapatoff of Electrical Engineering ("owners" of Franklin Hall by then) gave an evening plano recital. (Besides being an "electrician," Karapatoff was guite a musician and had invented a five-stringed cello so that he could play compositions written for the violin. He used to eat lunch in an eatery hard by Franklin Hall, Sibley Dog it was called, down in the basement of Sibley Hall. There was a piano upstairs under the dome where university concerts were then held. After eating he would occasionally wander up there and play on the instrument, and people would sit around and listen. Eventually, he even provided program notes for his impromptu concerts.) The society summer meeting was also small enough that a group picture was made of the conclave, in which one recognizes a lot of familiar faces. In the library print of the group photograph, individuals have been numbered but not identified; under #131 I have noted the face of one J. R. Wilson who, ten years later, was to be over our magnetron group as head of Bell Labs' department of vacuum tube development. In planning for the meeting, the program committee considered various themes: the nucleus, astrophysics, and biophysics. Even at that late date, Merritt writes K. T. Compton that "our feeling is that the nucleus does not offer the interest or possibilities of the other two [subjects]." The meeting was a great success according to all comments in the files, but the invited papers were not confined to astrophysics or to biophysics. Sir William Bragg, slated to give the commencement address at MIT, was persuaded by MIT's President Stratton to come to Ithaca with him and give an invited paper. Stratton wished to visit with President Farrand and presumably accompanied Sir William here. A. H. Compton also gave an invited paper. So there were X-rays on the program, if not the nucleus.

There was probably a picnic held in connection with the meeting; Ithaca is well situated geographically for picnics. It is today almost foreordained that such meetings will feature a picnic at a nearby park. Spring picnics for the Department had somehow come into being. A letter

from Merritt to Richtmyer tells of one rained out at Enfield Park, now Robert H. Treman State Park. The affair was therefore held at Bedell's big house (!) and 'lit was the best ever." Festivities were followed by a seminar report given by L. H. Germer "on some work with C. J. Davisson."

As the years advance, more and more names familiar to old-timers appear. Sid Barnes appears the year Gartlein does; he went to Rochester and in the late thirties initiated the cyclotron construction and program up there; also good at tennis. H. J. Reich, worked with Bedell, stabilizing that oscilloscope; went to Yale (from Illinois) and then to Berkeley, wrote a well-known book on applications of electron tubes. Don Morey was here; when he left, after I got here, he went with Kodak in textiles. There is Harvey White; worked with Gibbs in spectroscopy--isoelectronic sequences. He also went to Berkeley where he remained; a prolific writer of books, perhaps the best of which were his Introduction to Atomic Spectra and his text with Jenkins, on optics. He synthesized in some rather pretty photographs the electron distribution in several of the energy states in hydrogen--illuminating for one to whom the mathematics does not always speak too well. He pioneered a morning television physics course in "Sunrise University." H. R. "Hap" Nelson went to Battelle; he was killed in a senseless boat accident. L. A. Richards, who went to Iowa State, was joined here by his brother Sterling who did something in soil physics--they were both here when I arrived; Percy Carr--went to Iowa; L. L. Barnes stayed here through his retirement in 1965; G. K. Schoeffle (Kent State); C. R. Mingins (Lowell Polytechnic); Felix Yerzeley (always wanting to make the big invention-he had a patent on one, a device to keep one's paper napkin from blowing away in a breeze); John Ruedy, E. Ramberg, E. G. Linder, Ross Schrader, Al Rose, L. Malter--all went to RCA. Malter did part of his thesis at RCA; he had discovered the "Malter Effect," a pretty phenomenon, not widely known nor somehow ever put into application. He found that a caesiated aluminum oxide film on aluminum when bombarded by electrons (secondary electron yield greater than unity) would continue to emit electrons long after the bombarding beam was removed. The caesiated front surface of the oxide film got charged positively such that a high electric field developed across the insulating oxide, sufficient to initiate field emission from the underlying metal, the electrons making it through the oxide into the vacuum and to the collector--another

remembered seminar with demonstration. The name of James Webb, appearing around 1928, is of interest chiefly to the writer. One summer day during work on the linear accelerator in the late thirties, in wandered this character, red-faced and white-haired. He started talking, where had I come from and what all. It turned out that he had gone to the Mary S. Doten grammar school in Reno, as indeed had I, and as had Lloyd Smith. Strange. He went to Minnesota and finally wound up at Columbia, where I later heard of him during the war, before losing track of him. Somehow, he and his wife lived apart, but he showed up summers here where she was, with the clinic, I believe, where also Dr. Muriel Cuykendall worked. R. W. Shaw came in about this time (as mentioned earlier, he was my first committee chairman and research supervisor); he subsequently went to Astronomy and was the chairman of the tiny department before Thomas Gold came in 1959 and the picture changed. There were Diran Tomboulian and others who have already been mentioned earlier in this recital.

The name Richards is of interest--the two brothers. In 1979 there was an Astronomy seminar speaker--a Paul Richards from Berkeley, who had done (is still doing) some beautiful work on the spectrum of the remnant 3° black body radiation pervading the universe. He was introduced as an old Cornellian; he was said to have lived at one time in Ithaca and, although he could not remember it, he had been with his parents on a number of Physics picnics as a baby. I went up after his talk and told him that I had been on some of those picnics. The resemblance between him and what I recalled of his father, Lorenzo, made the connection clear.

These were the days long before government funding. Research was supported by the University itself and by private grants. Richtmyer, for example, had received apparatus from General Electric for his Xray research. Bell Laboratories and Western Electric similarly supported work through equipment grants. There was also private funding. This fact was noted in a listing of grants made by the university under Heckscher Research Foundation funds, money earned from New York real estate. Richtmyer had two such, of amounts \$1,800 (for a one-year assistant) and \$1,000 for apparatus (to augment that from G.E.). In 1920, Mr. August Heckscher made a gift to Cornell to promote research, not only in physics. Grant #1, in fact, went to Professor J. Q. (John Quincy?) Adams for relief

of teaching for half a year so he could complete his book <u>The Life of</u> <u>Shakespeare</u>. Physicists made good use of the fund, and apparatus may still be found around the department which was purchased under its auspices. In his letter formally presenting his gift, the donor wrote:

As research in America suffers from the exhaustion of professors by teaching and other duties, it is my desire that professors and instructors possessing the talent and training necessary for research shall, under such conditions and for such period of times as the University authorities may prescribe, be liberated partially or wholly from those duties and enabled to devote themselves in all the freshness and fullness of their energies to productive investigation and scholarship.

Another source of funds came from the Carnegie Foundation. Andrew Carnegie, earning his keep in steel, put much of his wealth into support of science, besides his many public libraries. He set up the Carnegie Institution in Washington and invested funds in smaller amounts in numerous research activities in many universities, including Cornell and its Physics Department. Various researches in the Department benefited from his munificence and for years it was common to run onto a piece of apparatus numbered Car.__.

There was another source of funds in the department known as the Gage Fund, given by Simon Henry Gage and his son, Henry Phelps, in memory of Gage's wife, Susan Phelps. Gage had been a physics assistant back in 1908 but was first and thereafter a biologist. He wrote an early widely used textbook on the microscope, which went through edition after edition, even unto the thirties. He had somehow been exposed to microscopist Professor Caldwell of Chemistry and had been won over by microscopy. (Caldwell was one of two Chemistry professors appointed by President White in his first four faculty appointments to the new university; he essentially introduced chemical microscopy to this country, coming to the university with an early microscope in hand.) As of many biologists of the day, sadly, we cannot say Gage understood or accepted Abbe's theory on the role of diffraction in what one sees in the eyepiece. He sure waffles on the subject, at any rate, even as late as the 1932 At his ninetieth birthday, there was a celebration at which edition. Merritt spoke, claiming him for physics. Susan Phelps was the first

woman in the world, Merritt said, who had received laboratory instruction in physics, presumably at Cornell. Endowment from the Gage Fund is still helpful to Physics.

In later years significant support to the department came from other sources, the American Philosophical Society, the Research Corporation, and industrial laboratories which occasionally donated out of date equipment no longer of use to them. Near the end of World War II, Stromberg Carlson sponsored a war project in the department the equipment from which would remain in Cornell's hands at war's end, at which point funding changed dramatically, as all of us are aware.

The fields of research during Merritt's regime had not broadened There was spectroscopy, X-rays and crystal structure, significantly. luminescence, and the like. In 1926, there was very serious discussion about developing the research side of biophysics. Merritt wrote President Farrand in that year about promoting biophysics, noting that except for Gibbs' work on ultraviolet absorption, "which has an important bearing on the therapeutic use of ultraviolet radiation," no man in Physics is much Richtmyer could be brought in through his X-ray work; bio-oriented. Bedell's "special experience in electrical matters could be valuable," and so on. He suggests that two new men be taken on and \$10,000 be added to the department budget, to grow larger as the program develops. There is physics enthusiasm for any proposal in which such research could be stimulated and supported. Merritt hopes that "things will develop in such a way as to give us a chance to make good."

Farrand responds in fine presidential tone. Thanking Merritt for his letter, he goes on: "I am very deeply immersed in this whole problem of fortifying science here at Cornell and I hope in the not distant future we may obtain some sorely needed financial support in that great field." Sincerely yours, Livingston Farrand.

As may be supposed, things did not move very fast. But the concept grew. In 1929, R. C. Gibbs, and C. C. Murdock were members of a joint Biology-Physics-Chemistry group discussing the possibilities. Gibbs wrote a three page memorandum to Farrand giving the gist of the considerations, noting that he thought "the interest in this area is real and vital and will continue to increase, particularly if we can get an early start on some of these problems." There were seen three definite lines of

work: the effect of X-rays on plants, animals, eggs, and seeds, etc.; the study of various effects on living things of radiation in isolated regions of the visible and near ultraviolet parts of the spectrum; and chemical analysis, organic and inorganic, and spectral analysis was seen a need in connection with biology. It would seem to have still been many years before anything very much directed toward biophysical and biochemical problems came into being. Today we have in the university a large department of Biochemistry, and Biophysics is an area of research in the School of Applied Physics.

The cyclotron in the late thirties was the scene of the only biophysics experiment I know about seriously conducted in the Physics Department. But it was done by a young lady seeking an advanced degree from somewhere up on the Ag campus. In this instance, the cyclotron was not making high velocity ions. Indeed, the accelerating chamber was removed; no vacuum was necessary. She was investigating the effect of high magnetic fields on the growth of yeast, and so exposed her cultures to the ten or so kilogauss field of the magnet. She simply placed her dishes in the magnet gap and the "cyclotron boys" turned on the current. Memory does not serve to recall exposure times or the results. What one can say, however, is that today neither is bread raised nor wine fermented in the fields of strong electromagnets.

It was during Merritt's chairmanship that one other event took place, significant for the future, although not for Physics especially. Professor Boothroyd, representing all that Cornell had in Astronomy, for the most part practical, was in 1933 moved out of Civil Engineering to his own one man department in the Arts College. Space was made available to him in Rockefeller Hall, where I first dropped in to see him when I came as a graduate student a year later, there in his office at the north end of the main Rockefeller corridor. Boothroyd was clearly a practical astronomer as I found when I took my Ph.D. examination from him. But with the move came an ever strengthening emphasis on astrophysics. The tiny department increased 100% when, a few years later, R. W. Shaw joined it. And so one comes to the end of Merritt's tenure, both as head of the department in 1934, and as professor a year later after Gibbs had assumed the chairmanship. In May of 1935, on a pleasant, warm, spring evening, there was a retirement dinner honoring him and his wife, Bertha. There is

no recollection of a symposium as there had been at Nichols' own affair, sixteen years earlier. The dinner was held in the Memorial Room over in Willard Straight Hall and many former associates of the department were there. My sister and I were both present. LeRoy Barnes' wife, Lucy, played violin selections, accompanied at the piano by Marion Howe, daughter of our professor. (Why Roy wasn't in there is not clear; he was presumably not so bad on the cello. Or they could have gone quartet; the Barneses were pretty active in that format.) It is a pleasure to look back and realize that one has been in small way part of the formative days of the department, overlapping the tenures and lives of some of the important and early characters involved in it. A small booklet was put together for the occasion:

Cornell Physics Issued on the occasion of the retirement of Professor Ernest Merritt Head of the Department of Physics 1919 to 1934 Ithaca, New York May 4, 1935

In it were names and addresses of those who had held staff positions or had received advanced degrees in Physics at Cornell. There surely was a gift of some sort presented and Merritt had some gracious and whimsical remarks to make, among which was the expressed pleasure at finally being able to use "Emeritus" following his name, E. Merritt.

And so we come to the chairmanship of R. C. Gibbs. Like others in the Department, he also graduated from Cornell; he earned his Ph.D. in 1910, and was thus a contemporary more or less all the way with Richtmyer, which is perhaps some reason they did not get along. He held all possible academic ranks in the department, from undergraduate student right on through to department chairman and finally to emeritus professor. He started his teaching career with Shearer, taking over from the latter much of the undergraduate teaching administration, training others like Murdock, Howe, and Collins. He gave a course in atomic physics and graduate courses in spectroscopy--his own field of research.

Following his retirement after World War II, he continued a decade of activity in another chairmanship, that of the Physics Division of the National Research Council in Washington.

Richtmyer must have been pained at having Gibbs take on the role of department chairman. Except for continuing the teaching of his modern physics course, and cursorily consulting with one or two students and with the fellows and research associates associated with him in X-rays, he essentially dropped out. There was but one faculty meeting thereafter which he is known to have attended, a session important in department annals for having given birth to our Graduate Conference. After Gibbs became chairman and with Richtmyer at the helm at the Graduate School in Morrill Hall, this ploy was invented to ameliorate dealings between the department and the Graduate School. There would be a separate body--the Physics Graduate Conference--to direct the graduate program in the department, and more important, for reduction of friction, the chairman of the conference could not be the chairman of the department. Parratt has reason to recall certain aspects of this important meeting. He has told how he was in large measure responsible for the concept. Being closer to Richtmyer than others, having been associated with him as a National Research Fellow in X-rays, he had persuaded Richtmyer to attend this meeting of the physics faculty directing graduate research. The session was somewhat tense, leavened considerably by Lyman's experience with Richtmyer's proffered cigar, which under the circumstances, Lyman felt under some obligation to accept. We have seen how that turned out. Things relaxed a little following the amusement over Lyman's lack of expertise in the use of tobacco. But Bethe still recalls that when Gibbs directed a query to Richtmyer, the latter would only respond to the chair: "Mr. Chairman, would you please inform Mr. Gibbs that such and such is so and so." He must have been a troubled person. Anyway, we still have Parratt, and we still have the Graduate Conference, but the role of chairman of the conference seems to have drifted back to the department chairman, with no untoward disadvantage.

In further promotion of department harmony, there was established a committee of three, ostensibly to insure that Gibbs kept to the "straight and narrow." Bethe himself was named to the committee in his second year here, playing a role similar to that which he would play twenty years

later on a committee of three to steer the course of the department with its new Laboratory of Nuclear Studies. It must not have been easy for Gibbs to have this feud around his neck, but he was a very patient and kind person, with the department welfare at heart. How else could he have stood up to these rather insulting actions directed at his abilities and Lauriston Taylor tells of the time Gibbs was exhibiting to a motives. large class (engineers?) in Room A, the absorption of X-rays in a demonstration experiment. The X-ray machine was at some distance from a charged gold leaf electroscope, the leaves of which would close together from the ionization produced by the radiation, fairly rapidly so in the direct beam, as he showed. He put first a thin aluminum foil in the beam, the leaves still closed at moderate rate. Then he put in a sheet of lead and they closed not at all. Next he interposed his own considerable bulk between the source and the electroscope and the leaves again barely moved. "And now what does that show?" he asked. He shouldn't have; some voice from the back of the big hall called out: "You've got lead in your britches." Which broke up the lecture. Gibbs may have been a bit bumbling and ponderous, but he was a nice man and, while his demeanor and bearing rather invited the characterization, it was not in any real derision that some students referred to him as "the great white father." Smith remembered a picnic at which somehow a foot race was arranged between Richtmyer and Gibbs, both pretty large men. There was enough competition and feeling between them without that. But anyway they ran, Richtmyer outdistancing Gibbs, who just at the finish line fell flat on this face. Outwardly, he was good natured about all of this. The department owes much to his forbearance.

Physics was not the only department to have its internal feud. Chemistry had one too, although apparently less intense than that between Richtmyer and Gibbs. Over there it was Professor Dennis and our friend Professor Bancroft who did not get along with each other.

It must have appeared, at least to some faculty, that the research being done in the department was going to reach a dead end. Fluorescence and luminescence had been worked and worked; spectroscopy, visible-UV, far-UV, and vacuum-UV; X-rays, crystallography--all would seem to have lost some of their glamour. Electron and ion physics were active fields, but again, it was a matter of working out details. It is not quite clear

from whom the impetus came, but discussions on new areas of research must have begun during Merritt's regime and carried over into that of Gibbs. Certainly it fell to Gibbs in large measure to implement what was apparently decided. There is correspondence given to the possibility of biophysics. But it was nuclear physics that was decided upon. It was clearly a growing field, and there was none of it at Cornell.

A letter from Gibbs to Richtmyer seeks his opinion on taking Hans Bethe into the department; both Sommerfeld and Bragg have written strong supporting letters. Smith has precipitated the need to decide; the latter has a good offer from elsewhere and is likely to leave unless the department is so strengthened. Kennard is much in favor. While there is no response in the file, Richtmyer was apparently opposed to the appointment of Bethe and, indeed, even to the department's going into nuclear physics. He was the big man in Cornell research at the time and wanted all the monetary support he could get for his X-rays. He was not persuaded by the argument that support for the growing field of nuclear physics would bring money into the department and would advance his Xray work probably more than his own fund appeals for work in X-rays would accomplish alone. He held the view that research was done to provide theses for students not that there should be, for their own sake, ongoing university research programs into which students could enter, theses resulting; something quite different in philosophy. This was a bone of contention "chewed on" by the then new Graduate Conference in seeking to define the department's research mission.

So it was that in the fall of 1934, when Gibbs became chairman, M. S. Livingston came as Instructor from Berkeley and began the construction of his small cyclotron. This was an event larger than any Cornellian could have imagined at the time. From that beginning, nuclear physics grew at Cornell, leading without stop (save the War) to subsequent maturity and the succession of machines to the large effort and great high energy machine we have here today. After Livingston, four months later, came Bethe from Germany, by way of Manchester, where he had gone during the rise of Nazism. Then came Bacher, doing spectroscopy but hoping to start work in nuclear physics

In a three-part profile on Bethe in the <u>New Yorker</u> (December 1979, now a book), Jeremy Bernstein writes on some of this. During 1931-32,

Bethe was at Munich with Sommerfeld, having had a period in England. Knowing English, he was given the American and English postdoctoral people to look after. Thus it was that he became acquainted with Lloyd Smith, who worked on a Hartree calculation with him. Bethe, and letters in the Archives, indicate that it was Smith, who a few years later pressed the department and chairman under threat of his own resignation, to offer Bethe the position of acting assistant professor with the chance to move It was Smith who, in 1932 (the year Chadwick's discovery of the up. neutron made the newspapers), introduced Gibbs to Bethe--in Rome where Hans was working with Fermi. The part Smith played in getting Bethe to Cornell was surely significant. That and his active role in getting the Department into nuclear physics were enormously important to the future of Cornell physics. Bacher, in a 1985 talk on the origins of Newman Laboratory, also stresses the point. The favorable decision to do so was probably the most momentous ever made by the department. The Bernstein story makes nice reading. The offer came to Bethe "right out of the blue" while he was at Manchester, waiting for an offer from Mott to go to Bristol. We had quite a reputation over there. Read Bernstein on it:

Bethe knew next to nothing about Cornell when he accepted the job, and what he heard about it during the next few months hardly reassured him. "I met a physicist who had been there. He said, 'Don't go to that place. It is a terrible place. It is so straitlaced that you have to go to church every Sunday. You won't like it at all.' I accepted anyway and when I was offered a salary of three thousand dollars a year, I considered myself immensely rich. I came in early February of 1935. What I found was a department full of ambition. A new chairman, R. C. Gibbs, had just been selected, and he explained to me that the department was changing from one in which research was done to provide thesis topics for graduate students to one in which graduate students could participate in ongoing research. Not everyone agreed with this new emphasis on research, and there was some disagreement on which fields to expand into. It was the progressives versus the conservatives. The progressives had won the fight and now had the backing of the administration. My appointment--I was a theoretical nuclear man, and a foreigner to boot--was one of the signs of change. The year before, it had been decided to build a cyclotron--the first to be built outside of Berkeley--and Gibbs brought in M. S. Livingston for this purpose, because Livingston had assisted E. 0. Lawrence during the building of the original cyclotron, in 1930, and is generally credited with having made

it actually run after Lawrence had the idea for it. The third man to be appointed in nuclear physics was R. F. Bacher. The Cornell Physics Department was a very friendly one, and I immediately became part of it. I felt perfectly at home...

"I found my colleagues at Cornell terribly eager to learn, but not very knowledgeable," he went on. "The courses that I gave to the graduate students were a lot of fun, but on the whole the students didn't have as much background as I had expected. Livingston, who had done a lot of nuclear physics before, had a big card file of all the papers that had been written on nuclear physics. Imagine having that now! But he didn't really understand many of the basic ideas. So I explained them to him, and then I explained them to Lloyd Smith, and then I was invited around the country and explained some more nuclear physics here and there."

So he decides to write what he knows. Thus the three great <u>Reviews</u> of <u>Modern Physics</u> articles, the first with Bacher, the second by himself, and the third with Livingston. Bernstein quotes Bacher as saying how Bethe in writing the articles would take a sheet of blank paper from one pile, fill it up and pile it up over on the other side of the desk, a completed manuscript resulting. I heard that description before Bethe ever arrived at Cornell. But that's the way it was. Bacher was just finding it out. Bethe recalls his first department faculty meeting. It was devoted entirely to the question of putting soda pop vending machines in the basement!

Bacher was soon followed by Willoughby Cady in spectroscopy, son of the famous Wesleyan professor who pioneered in piezo-electricity. Parratt became an assistant professor, pushing his work in X-rays.

Bethe attracted a number of noted research associates and students: M. E. Rose, mentioned later in connection with the limits of the cyclotron; Emil Konopinski of beta-ray decay; Bob Marshak of solar nuclear reactions and fundamental particles, later at Rochester and then the CCNY Presidency; and G. Placzek, doing nuclear things. Bethe was not exclusively a nuclear physicist when he got here; there was the <u>Handbuch</u> article on the physics of metals. His first student here was Ralph Myers, who did work in nuclear physics. And there was Fred von der Lage, my roommate for a year in New York, who completed his thesis calculation on metallic sodium with Vannevar Bush's mechanical analyzer at MIT, during our domestic partnership.

Bethe, without doubt, is our most famous department personage. Beyond his Nobel prize, his contributions, both in physics and in the public domain, are many and outstanding. Most are serious of purpose, of course, but some show him not always thus; that there is a nice sense of humor in him, however subdued. One recalls the paper of Alpher, Bethe, and Gamow, probably inspired by Gamow, who was well known for such things, but entered into happily by Bethe and Alpher.

During Bethe's first year in the department, he gave a seminar on the work of one F. Klingelfuss who, during the years 1927-30 in the <u>Annalen</u> <u>der</u>, and <u>Zeitschrift für</u>, <u>Physik</u>, had described some remarkable measurements he had made on the sparking potentials observed between spheres in helium. From his data, he could calculate the electron charge, constants of Planck, Stefan, Boltzmann, and Rydberg, and the doublet separation in the fine structure of hydrogen, all in close accord with what was known. Amazing; utter and complete bunk.

I expect Bethe also included in that seminar a review of his own paper of similar estate, which he wrote with Beck and Riezler, but I don't There was this famous theory of Eddington's, whereby the recall. astronomer arrived at the fine structure constant essentially through numerology and mysticism. As first derived, he obtained I/a to be 136. When the experimental value subsequently appeared to be closer to 137, Eddington conveniently found how he could slip in an added digit, unity. As a joke on the theory, Beck, Bethe, and Riezler wrote from Cambridge a paper relating the degrees of freedom in a crystal to the absolute zero degrees of temperature. The argument went something like this: In a crystal lattice, absolute zero is to be attained when all internal motion is frozen out; except for the motion of the electrons in their Bohr orbits, which doesn't contribute to the heat content. Each electron, according to Eddington, possesses I/a degrees of freedom. For neutrality, of course, we have an equal number of protons, which Dirac says we can consider as a hole in the electron gas; an equal number of degrees of freedom is thereby to be added in. This total is to be lessened by unity to take care of the motion of orbital revolution. Thus, 2/a-1 degrees of freedom are to be removed from the lattice to reach absolute zero. And so they obtain T_0 = -(2/a-1). Taking T_0 to be -273°, one arrives in this way to a value for 1/a of 137. Neat.

The paper was published in <u>Die Naturwissenschaften</u> (1931, II, 29) under a "Remark on the Quantum Theory of the Zero of Temperature." Sommerfeld read it and had Riezler report on it in a visit he made to Munich; which he did with a perfectly straight face, according to Bethe. The great theorist sat there impassive, probably scowling, and at the end commented that degrees of freedom were not equivalent to degrees of temperature, certainly in Celsius measure. At that point, Riezler could no longer contain himself, broke down in laughter and owned to the levity of the theory.

Sommerfeld wrote Bethe an angry letter; he thought it not funny to mislead people. Furthermore, the journal editor, Arnold Berliner, had procured for Bethe the postdoctoral fellowship on which he had lived for two years. This Bethe had not known. Berliner was also pretty angry. Well, apologetic letters were written and things smoothed over.

A few weeks later came a serious article written by some Indian on regularities he had observed in some spectral series which were related again to numerological significance. <u>Naturwissenschaften</u> published it; this brought forth a letter to the editor from George Placzek to the effect that he thought we were done with jokes. Poor Berliner. When Bethe returned to Cambridge from a European vacation, Mott asked him for a reprint of his "most famous" paper. Everyone at Cambridge seemed amused except Eddington. It still amuses.

Bethe's first public renown perhaps came with his theory of stellar energy production, for which at the end of 1938 he received the New York Academy of Science's Morrison Prize of \$500. It is good that there is no such thing as double jeopardy in the awarding of prizes, else this might have usurped something a bit better many years ahead. The theory was explained to its readers a few months later by widely read, brassy <u>Time</u> magazine in an article entitled "Hot Stuff." the article poses the problem, and the question is asked: "Just what atomic processes enable hydrogen to be utilized as fuel?" It goes on: "At a meeting of the venerable, rich American Philosophical Society in Philadelphia last week, gray, gentle Astronomer Henry Norris Russell of Princeton explained what he considers the most reasonable modern theory." Bethe's. Then, clearly elucidated the <u>Time</u> article. There was with it a picture of our youthful theorist. Captioned <u>Time</u>: "Cornell's Bethe; About the Sun he is Reasonable." The

magazine sized up the situation fairly well: ". . . demure Dr. Bethe at Cornell has increased his repute as an atomic physicist like a snowball rolling down hill." President Day, not blind to the importance of keeping Bethe at Cornell, sent over a letter of congratulations about the prize, and followed with many other favorable actions in years to come. In October of 1943, in the depths of the war, Day appointed Bethe to the John Wendell Anderson Professorship of Physics, succeeding Lane Cooper of Comparative Literature and Carl Becker before that. He noted for Bethe that he was following in an "illustrious succession."

Bethe of course got deeply involved in the war effort, first with the work of the MIT Radiation Laboratory, and then more importantly at Los Alamos, where he headed up the Theoretical Physics Division, as Bacher headed the Division of Experimental Physics. It was his deep concern over the use of nuclear weapons and his great experience that involved him in national committees and the like.

In the flurry of activity after Russia's Sputnik in 1957, a Scientific Advisory Commission was established in presidential Washington. Bethe was a member. The commission set up a subcommittee on disarmament. In the subcommittee, Bethe suggested that a ban on bomb testing be studied. He thus became chairman of an interagency panel doing just that. They came out with a report saying that it would be possible to monitor tests carried out in the atmosphere and under water; that it was not inconceivable that even underground tests could be monitored. Eisenhower accepted the report. A favorable time for suggesting something to the Russians came when Khruschev taunted the president about our peaceful intentions: What about all our testing? Eisenhower responded, suggesting an international conference on the general testing Thus came about the Geneva Conference of 1958, a really problem. successful technical conference on the possibility of detecting nuclear explosions. It was a fine conference, the first such between the U.S. and Russia leading to an agreement report.

There were three official U.S. delegates: Fisk, Bacher, and Lawrence. The delegation was headed by Jim Fisk, by then president of Bell Laboratories; earlier he had been my group leader in magnetron development for four or five years. Twenty important advisors accompanied the delegation, among whom were Bethe and the later

presidential (Carter's) science advisor, Frank Press, who knew about earthquakes. Corson, who with his wife happened to be in Geneva at the time, tells of their running into Bethe on a walk the evening before the conference was to convene. Never had they seen Hans so troubled; he was not to get much sleep that night--"the world seemed to be on his shoulders." Well may it have been.

Bethe has been a strong proponent for the use of nuclear energy in power generation--opposed by colleague R. O. Pohl, who was as strongly supportive of the contrary view. The two appeared on a few occasions in friendly public debate, pro and con, on the issue.

His contributions in physics range over a wide area: solid state physics, nuclear physics, atomic physics, and astrophysics. His Nobel Prize came officially for his work on stellar nuclear burning and theory of nuclear reactions. Other work could have been cited. There is his important contribution to the understanding of the Lamb shift laying the basis for quantum electrodynamics. As the work in the Laboratory of Nuclear Studies moved to higher and higher energy, he let his younger colleagues take care of the theory in strange particles; he continued his own interest in many body nuclear structure theory. He became emeritus at 68, but it seemed to make little difference in his department association. He works every day in his third floor Newman office, as though nothing had changed. Retirement does allow him to leave during the spring for extended periods of study and work at other institutions. He has been a consultant at Los Alamos ever since the war days. It was a happy circumstance that brought him to Ithaca.

It was an exciting morning over in Newman, on our hearing of the announcement of his Nobel Prize; the champagne was flowing not long after breakfast. A few months after that there was a fine testimonial dinner given by the university in recognition of his achievements. A few years earlier a similar Nobel notification had been received at Cornell. Linus Pauling, unfortunately not one of our own (although his <u>Nature of the Chemical Bond</u> came out of the Baker Lectures he gave here during one year) was here on campus to give a Chemistry colloquium when he received news of his award. He came in to give his lecture, beaming and flustered, shortly after hearing the news. He received a standing applause, of course. The lecture I remember, it was on the genetic defect

causing sickle cell anemia; it was the first time I had heard of those delightfully labeled bases, gyuanine, cytosine, adonine, and thyamine, so useful to DNA.

But we must get back to the Gibbs era and drier stuff to wind up these "Middle" years. Rockefeller Hall had gotten more and more crowded. Another building renovation study was undertaken and plans drawn. The engineers were still complaining about the numbers of their students who were failing physics, seemingly an ever-present problem until recently. Little is heard these days on the score. Have we gotten soft? Or are students better prepared--probably the latter, perhaps some of both.

More important, in 1938, to stay at the forefront, members of the department made another long and thorough study of the status and needs of the department. A report was drawn up. This long and comprehensive study is of some interest. In many ways, it indicates a change has taken place in the department philosophy.

Comparison studies were made with other physics departments in other institutions. Out of seventeen departments elsewhere, eight were considered superior to us in equipment and funding, five were equal; in theoretical work, we stood high--only two were perhaps better and two In the early seventies, a national survey put the perhaps equal. department in the upper ten of American physics departments; so we have held our own in the competition. (Still more recently, in 1982, our graduate program was rated first in the country.) In the report of the study which Gibbs sent to Richtmyer, the chairman of the Trustee-Faculty Committee, which committee apparently initiated the study (universitywide), our current areas of research were X-rays, spectroscopy, crystal structure, electronics and ionics, nuclear physics, and theoretical physics. Areas which could be broadened to advantage would be X-rays, nuclear physics, and the auroral program. New housing was essential, plans were ready--there were always plans being drawn for expansion of physics.

The Department Report, written by Gibbs, was a summary of individual reports submitted to him by the various faculty members. The individual reports gave the objectives of the various researches, the current status, and future plans--with costs. Some of the researches under way: study of energy levels (or bands) of gases, liquids, and solids through X-ray emission and absorption lines (Parratt); interferometric

spectroscopic studies of the fine structure of Balmer lines in hydrogen and deuterium, ionized helium and -- "if sufficient quantities can be accumulated from nuclear disintegrations"--of H^3 and He^3 (Gibbs); hyperfine structure research--the interaction of the atomic nucleus with the extra-nuclear electrons (Bacher and Tomboulian); development of a high intensity positive ion source, experimental test of Saha theory of ionization at hot metallic surfaces (Smith); exact determination of the range-energy relation for alpha particles (Livingston and Holloway-resulted in an important paper); development of a high intensity capillary arc source for the cyclotron (Baker and Livingston--also important); the multiple scattering of electrons (Bethe and M. E. Rose); maximum energy obtainable from the cyclotron (ditto); probability of forbidden beta disintegrations (Bethe and Konopinski); calculations of electronic energy levels in metals (Bethe and von der Lage). The projected costs seem modest by today's standards; roughly \$35,000 per year and an initial outlay of \$32,000.

Richtmyer's individual report to Gibbs is of interest in that it points up the difference in outlook between the "old way" represented by Richtmyer and the "new" espoused by the younger faculty, as revealed in Gibbs' marginal notes on Richtmyer's Prefatory Remarks. Gibbs' notes, showing irritation, are here put in parentheses:

Prefatory Remarks

1. The primary function of a university is to train men at both the graduate and undergraduate level. The primary purpose of research in a university is to contribute to the training. (?) The scientific results of such research, however important they may be scientifically, are secondary to the main purpose and are to be regarded as byproduct. (!)

II. A more important purpose of research by faculty members is the providing of suitable subjects for Ph.D. theses. Subjects for such theses must come from research programs actively carried on by faculty members. Ph.D. theses subjects cannot be "found in books." (Gibbs: This would follow as a matter of course under a more significant objective. Do important and significant research of highest quality.)

III. In the logical development of an integrated program for any department, one should start first with the interest in research of the individual professors and others who make up the permanent staff of the department. In order that each department

member may contribute most effectively to the department's work of training men, his research interests should be prompted and supported to the extent of the department's resources. (Gibbs: No one questions this. We are wasting time debating it, but if one has the above noted type of objective, new fields will come and old ones go out.)

IV. Each of the several fields of research in which the various members of the department are interested should be carried out along scientifically logical lines. This will occasionally mean a major expenditure for assistants, apparatus, or both, in order to pass some hurdle. Some of the research problems in a given field are too complex and time consuming to be done by graduate students. Other problems involve work which is too much of a routine nature. Special assistants should be provided for such problems. (Gibbs: Just what many of us have long since recognized.)

V. It is axiomatic that wherever the interests of two or more members of the department overlap, either in research fields or apparatus or both, cooperation should be developed in the interest of both economy and research efficiency, but it should be kept in mind at all times that the primary unit upon which a comprehensive research program should be built is the individual's interest. (Gibbs: Nothing new, has long been followed here.)

VI. Since in last analysis, the primary purpose of any university is the training of men--that is to say, teaching--and since in reality our several research programs in physics are contributory to teaching, there should be an adequate balance between attention given to developing the teaching program proper and the research program. (Gibbs: Just what we have been doing.)

VII. In outlining below the needs for the immediate future in the field of X-ray spectroscopy, I have included what I think we <u>ought</u> to do to carry on the program effectively. (Gibbs: Good.) The recommendations of course fall far short of what one would <u>wish</u> to do. (Gibbs: Naturally.) They are somewhat above the irreducible minimum, namely <u>zero</u>. (Gibbs: Why has he waited four years to present them?)

Whether anything came of this nontrivial effort in department planning is not known by the writer. The war would intervene.

Parratt is less sure than I have been in the contention that Gibbs deserves full credit for starting the department along the nuclear physics trail. He believes that Merritt was the more likely promoter. As we have seen, Merritt certainly was concerned about keeping the Department at the forefront. In any case, the path was pretty well set by 1934 when Gibbs took on the chairmanship of the department. The "magnetic resonance"

accelerator and the linear accelerator would be built, under Livingston and Smith, respectively. The department was not of one mind about this new direction. Richtmyer surely was not in agreement. Nor was Collins, according to Parratt. They did not like the idea of big programs involving many people; that was too likely a way toward dominating a department. Gibbs and Smith strongly supported the direction. There is the reference that Smith would leave if the Department were not greatly strengthened, a factor in Bethe's appointment.

Parratt, although a close colleague of Richtmyer, also supported the trend. However, he soon had misgivings. His research room was then immediately adjacent to the cyclotron room in which the new machine was taking shape. In due course, he found that his ion chamber current measurements were in trouble from some interference, source unknown. Measurements down to his 10⁻¹⁶ amp limit became impossible. He was apparently unaware of what was transpiring next door. He queried Murdock, doing X-ray crystallography at the other end of the basement. Murdock suggested that perhaps his gas tube X-ray generator was the culprit. But it pretty obviously was not, and tests quickly showed that to be the case. Parratt spoke to Livingston about possible interference from his operations--could they try a test: cyclotron on and off at five minute intervals while Parratt observed the light spot on his galvanometer scale.

Livingston was uncooperative. He saw no point in a test; there was nothing to be done even if it did show there was interaction between the cyclotron and Parratt's apparatus. Parratt says he was "mad as hops" at the rebuff and so went to see Gibbs about it. Gibbs listened in his deliberate way and was not very sympathetic; "too bad," wondered what Parratt could do about it. Which made Lyman even more angry, sending him to Richtmyer with word of the problem and the chairman's response to it. Richtmyer exploded: "That son of a bitch." Lyman started looking into geiger counter means of detection of his radiation, and eventually moved down to the other end of the basement alongside Murdock's gas tube.

The cyclotron was not the only source of perturbing radiation. The linear accelerator right above Parratt was an even more potent source when it got into testing phase. We had a couple of 25 kilowatt tubes in the output and a pair of five kilowatt drivers. I recall one evening Charlie Shaw stormed up into the research room where we had things fired up, and

gave me a well-deserved tongue lashing. I had neglected to check with him about the status of any long run he may have been into. Low current measurement in those days was not easy. It still requires care. In general I think we were cooperative with those experimenters needing electrical quiet.

In 1980, Jeanette Lurier, a senior in the History Department, wrote a Senior Thesis for Professor L. Pearce Williams on "The Physics Department at Cornell between World Wars." She covers the period when Merritt was chairman and the critical first five years of Gibbs. It is a much more formal, penetrating treatment than what has been given here, although there is obviously much duplication. Still, she has seen material that I have not, and she has some interesting insights and makes points that may not have been made in the preceding. I have used her paper as basis for a summary of the period.

She quotes Merritt, on taking over the helm, in this way: "I feel more strongly now than I can possibly express that now is the time for something definite to be done to improve the position of the college teacher. If the situation requires something radical then let it be radical, and if necessary, contradictory to all orthodox educational notions. Certainly there is no better time than now for making radical changes in educational methods."

It would appear that things did not change all that radically during his tenure. It was only after Gibbs became chairman that things in the department changed markedly. The immediate postwar crowding and staff overloading was somewhat better in a few years, but funding was forever hopelessly inadequate, and the depression at the end of the twenties did not help matters. The revolution in atomic physics came in this period; Americans went abroad to learn theory, and Europeans came over here to do experiment. By the mid-thirties we were catching up. Hitler helped out in this regard; many notables left Germany and showed up in America. Cornell was caught up in the events. It was during this period, but mostly during Gibbs' tenure, that the faculty began to be drawn from outside Cornell. The great emphasis on teaching was lessened and the importance of research strengthened, as we have seen.

Lurier reports an appropriation to the department in 1920-21 of \$87,000, contrasting it to the near \$13 million today. By 1931-32, the

appropriation had climbed to \$121,000, and it was not until after World War II that it got back to that figure. About 20% she says went for research, \$25,000 in 1932. A year later it was disaster; research was down to a mere \$9,000. Salaries were cut 10%; they were already low. The average at Harvard in physics was \$400 higher than our maximum. Merritt's radical changes were likely not to be of the sort he had imagined.

Cornell had the reputation for overloading staff and advancing them slowly. To help professors' research, the use of instructors to relieve professors of some teaching in undergraduate courses was the norm until the mid-thirties. Merritt saw the ideal professor as spending half of his time in research. Competition, however, for good Ph.D.'s as instructors was keen from other universities and increasingly so from industry. To help financially, the use of graduate students as teaching assistants was increased; they were cheaper. (They were generally not paid for their thesis work until after World War II; I think, under Smith, I was an early exception.)

It was nearly a decade before the European revolution in physics made much impact on the Cornell department. The courses were largely in classical physics, the research for the most part in experimental matters. Richtmyer gave his "modern" physics course for physics majors, in which the Bohr atom was the pinnacle, more or less. For the non technically oriented, he gave a similar course from an historical point of view, which probably was the first third of the first edition of his book. Kennard gave most of the theory courses, including relativity, to graduate students. He went to Europe on sabbatical leave in 1926 and at Göttingen and Copenhagen watched and took some part in the fast moving developments. He offered a course in the new mechanics the following year. The necessary mathematics also had to go with the course, and it cannot be said that it greatly affected the trend in the department's endeavors. Interest there was, no doubt. The visitors to the department are testimony to that: Lorentz, Aston, Sommerfeld, Ehrenfest, Kramers and G. P. Thompson; and the ten or so American greats brought here during summers in Gibbs' experiment mentioned earlier. There were the efforts to bring Born, Franck, Debye, and others to the department and Kennard's proposal that funds be provided for extended visits by such as Heisenberg, efforts mostly for naught.

Kennard was not the only member of the department to head for Europe during sabbatical leaves. Most of the others did, for one reason and another. Collins went to Harvard and Bridgeman's high pressure work, Richtmyer to GE and Irving Langmuir. With the "European" physics gradually coming to this country by the mid-thirties, one found Kennard next going to Cal Tech and J. R. Oppenheimer, where he continued to be impressed at the rate of progress in theoretical physics.

Cornell was managing to keep its head above water in the competition. Even in the discouraging times of the mid-twenties, Merritt could say in 1926 that "We are in the heavy weight class and that is where we belong." There was a lot of publishing and reporting at meetings--"scientific output." We made a "good showing at the meeting of the Physical Society," but not what might be expected of "one of the largest" physics departments in the country. Best known of Cornell's work was that of Bedell, antedating the period with which we are concerned at the moment, that of Richtmyer in X-rays, and that of Nichols and Merritt in luminescence. We might add to Lurier's assessment that somewhat later, way in the thirties, the infrared work of Collins, Gibbs' spectroscopy, and the electronics and ionics of Smith were gaining recognition, as well as the precision of Parratt's research.

But with the great advances being made in quantum mechanics and nuclear physics, it was clear in what direction the department should The hiring of Livingston was a first step. Gibbs has to be move. recognized for his part therein, not to mention the initiative and incentive provided by Smith in inviting Bethe to come to the department. And shortly thereafter, there was Bacher's arrival. Besides representing a break from the past practice of taking Cornell trained people as faculty members, a new research direction had been taken, and a new view of the place of research in the university adopted. Lurier dwells at some length on the "struggle" which we have already discussed: "...as research became synonymous with prestige, the university could not help but respond." Instead of seeking faculty strong in teaching and research, the department was more concerned that a professor be "a young theoretical physicist, preferably one who had a good deal of familiarity with experimental work." Today we do give lip service and pay homage to teaching, but it is fair to say that prowess and promise in research is the chief criterion for

hiring and advancement. And we do seek "theorists" or "experimentalists" having "a good deal of familiarity also" with the complementary discipline.

Lurier ends her essay with a summary of what had transpired in the period:

The unfortunate circumstance of Prof. Richtmyer's sudden death in that same year (1939) was perhaps symbolic of the end of an era. The rapid advances of quantum physics redefined the future of Cornell's research program. It also altered its importance in relation to other functions of the Department. The many scientists being called off to help in the war effort signaled a new role for science and professors of physics who participated in it.

With the war's onset people left Cornell Physics, getting involved first in various defense efforts and then, upon the U.S. entry into the struggle, with offense. Bacher left for MIT, radar, and the Radiation Laboratory, going to Los Alamos when that operation got under way. Kennard was to leave for the Navy's David Taylor Model Boat Basin. Bruno Rossi, who was new, was to go to Los Alamos, taking his student Ken Greisen, who joined peers Higinbotham, Holloway, Baker, McDaniel, and others. With Smith's interest in electron and ion beams, he got involved in microwaves at RCA, and in an isotope separation project (with Parkins and Forrester); but he got back at intervals initially. He tells how Bethe felt left out, his citizenship not yet American. At the same time Bethe felt an understandably great responsibility to contribute something useful. Smith could present him with problems, however, on which he could work. For example, there had been some theory done on the transmission of microwaves through apertures in wave guides, theories none too successful. Smith suggested it as needed and very tough. He has recalled how, when he next returned to Ithaca, Bethe showed him some work he had done on the problem in the interim. It was a tour de force, ingenious and beautiful, a real advance on the problem. Bethe didn't know what then to do with it. Smith got it to the right hands and urged its publication after things returned to normal. Bethe did get cleared, of course--a day or so after Pearl Harbor--and made it to the Radiation Laboratory at MIT, where Schwinger got involved with the iris problem. Eventually, we know that

Bethe ended up at Los Alamos with Oppenheimer, becoming acquainted with the aforementioned hydrodynamics, among other things. Parratt went on loan to the Naval Ordnance Laboratory. Cady did likewise but to China Lake; Parratt was in Washington. He worried about his position in the department. President Day indicated that his role would not be jeopardized; he was to come back two days every fortnight to attend to his graduate students left behind, which he did for a while. He too ended up at Los Alamos, touching more bases perhaps than others.

Among his NOL duties, he was much involved with devices for magnetic detection of submarines, which with their high sensitivity contributed greatly, along with radar, to the end of the submarine menace in the Atlantic. He was to spend a good part of a year in Britain on Naval ordnance, working out of one Captain Solberg's office in the Embassy. (As admiral, Solberg would later be in charge of the ONR and approve the funding of the first Cornell synchrotron and help dedicate the laboratory.) Parratt got acquainted with rocketry in Britain, wrote a report on it and was sent to various installations in this country to evaluate our own Even before his completion of the report concerning that tour, efforts. Bush and Conant came to the conclusion that it was too late for us to salvage much from our efforts in that direction, and shipped Lyman to Los Alamos with some of his NOL colleagues, Trevor Cuykendall among them, where they worked on instrumentation serving both the "gun" and "implosion" devices.

Gibbs wrote retired Professor Bedell in Pasadena about the situation: "Bacher and Higinbotham are at Cambridge in the defense program with Professor DuBridge as director. Nehr and Anderson are also there. We are not supposed to know its aim--but it is not nuclear physics. Smith will go to RCA for another term on defense work. Rossi is working out very well."

All this, of course, really disrupted affairs in the department, as in all of physics, not to mention the world in general. Teaching was the big war activity at home. Cornell had a large V-12 training program; physics courses were a necessary part. It was Gibbs' responsibility to see that staff was available for carrying it out. Bruno Rossi had become part of the faculty but left before long for Los Alamos, taking with him Greisen, who had become an instructor, as had Herbert Newhall. But Herb stayed

with the department, carrying on war related development on the side. Rossi never returned, lured away by MIT. Weisskopf at Rochester came down periodically to give lectures in one course or another, until he too went to Los Alamos. Placzek contributed until he went to Montreal to work on the heavy water reactor, developing there the basis for today's theory of neutron diffusion. Others helping to hold the fort were Gibbs, Grantham, and Howe, of course, together with Barnes, Tomboulian, Collins, and Gartlein. It was a hectic period from all reports--all work and little play. It was not much different elsewhere.

But the war did end. The department entered a new era, almost discontinuously, never to be the same place again.

THE MODERN ERA---1945 TO THE PRESENT

Physicists had made a great impact during the war, not only on what physics was to be like henceforth but also on society and the world in general. University physics departments had been drawn on heavily for staffing government and industrial laboratories engaged in war work. While perhaps best known, Los Alamos and the MIT Radiation Laboratory were but two of many such. As noted in the too brief record of the war years, the Cornell department was no exception in staff involvement in the war effort at places remote from Ithaca.

At war's end, physicists returned to the universities in droves. Never again would the journals be as few or as thin (especially as those still publishing during the war); never again would physics departments be as small either in staff or in graduate student body as they were prior to the war. Physics had made its mark and would become Big Science. Budgets would escalate; millions would come to be spent on single experiments. There was a hint of what was to come in the large machines built at Berkeley before the war.

Cornell physics, in common with that at other institutions, took part in the phenomenal postwar growth. Old staff returned and new names appeared on the department roll. Returned to the fold were Professors Bacher, Bethe, Smith, and Parratt. McDaniel and Greisen returned as assistant professors, and Charlie Baker as a research associate in Nuclear Studies, and a very live corpse indeed: the Cornell Alumni News for April 1945 reported his passing: "---'32, '35 AM, '41 Ph.D., Charles Parker Baker, former research associate in Physics, in October 1944; son of the late George R. Baker, '95, etc." He was to leave within three years for Brookhaven National Laboratory, where he assumed management of their cyclotron, retiring after many years still very much of this world. (It is to Charlie and Marshall Holloway that the world is indebted for the unit describing nuclear cross sections--the "barn." Charlie was always describing this or that as being as big as a barn, and when he and Holloway, on a cyclotron experiment they did at Purdue during the war, were discussing a cross section result as being as big as a barn, the unit was born. It was used facetiously for a while, but sanction came when

Fermi himself, sometime later, began referring to cross sections as being so many barns in magnitude.) New people accompanying Bethe and Bacher from Los Alamos were R. P. Feynman, Phillip Morrison, and Dale Corson (a year later) as assistant professors, and John DeWire as research associate in Nuclear Studies. Staff not returning were Kennard, who stayed on at the Taylor Model Boat Basin; Rossi, who went to MIT from Los Alamos; and Cady, who had gone to NOL at China Lake and died before war's end. The graduate student population burgeoned, careers in physics having become attractive. General interest in physics and its war involvement was high; before a year passed, Oppenheimer came and gave the Messenger Lectures.

With the country-wide physics establishment essentially being reset, it was by no means clear what Cornell would have to start off with. Bethe, Smith, and Bacher were clearly keys to the situation. Other institutions and laboratories would like to draw on their abilities. Dean Pegram at Columbia wrote President Day an apologetic letter about their offering Bethe a position down there; he "dislikes having to attempt to draw away a distinguished professor from a neighboring institution," but he feels that it is fair to give Bethe the choice of a decision. There was a similar letter from Rabi. There were already advantages to staying with Cornell, but Gibbs lost no time in urging Day to increase Bethe's salary and to make an appointment of a research associate to work with him. Within a month, Bethe had made the decision to return to Cornell, suggesting to the president some departmental support he hoped might be forthcoming-support for visiting professors, sponsorship of conferences, and the like. Day responded, expressing his pleasure at the decision but, in a very nice letter, not promising the world. One is impressed by the tone of Day's letters everywhere they are encountered. He must have been a fine person. I met him but once; when I was appointed an instructor, he had new appointees over to his house for coffee or some such, the house being the White Mansion, no longer used for presidential housing.

After Bethe's return to the department, he was invited to give the Hitchcock Lectures at Berkeley, more or less the equivalent of our Messenger Lectures, for which Oppenheimer of Berkeley had been our recent guest. Some symmetry there. Corson at the time was acting director of the Nuclear Laboratory, Wilson not yet having arrived to take over Bacher's role, Bacher having gone to take his own place on the first

Atomic Energy Commission. It was in this interim that Corson received an irate call from the dean of the Arts College, Cornelius deKiewit. Bethe had accepted the lectureship and had gone off without seeking permission from the dean. DeKiewit was really heated; who did this fellow think he was that he could willy-nilly take off two weeks during regular university session? It is not clear that Dale mollified him, but it would not be the last of two-week disappearances for Bethe and others of like position. Clearly, however, President Day was pleased with Bethe's recognition. After accepting the Berkeley invitation, Bethe wrote President Day that he felt he should receive no salary for the period of absence. Day would have nothing of it, pointing out the value to Cornell in having him give the lectures. Day was all right.

Smith had earlier been faced with a similar decision which also brought him back to Ithaca. He had not expected to return to Cornell. The RCA Laboratories were very much interested in having him take over the directorship of their fundamental electronics research. Smith apparently talked it over with Bethe, for in mid-summer of 1942, Bethe wrote President Day about the prospect of Smith's leaving and urged Day to use every effort to keep him. Day responded, indicating that Smith could essentially "write his own ticket"--words noted in two different letters--but that he had to make the decision as to whether it was to be industry or the university. He thought it a difficult choice for Smith. There must have been some conferences with Day, for in December of the same year, Smith writes that he has decided to return to Cornell, that he is pleased with the establishment of Engineering Physics; he is "convinced that it is a coming thing so far as a technical training program is concerned." He had clearly written his ticket. A proposed Engineering Physics curriculum was presented to Day in 1946, not long before the admission of the first Day thought highly of it, and Engineering's Dean S. C. student class. Hollister was enthusiastic and facilitated setting up the school.

It is not likely that Bacher was exempt from similar outside enticement. When he took his wartime leave, he clearly intended to return to Cornell. There is, in the Day files, a note the president apparently made to himself at this time concerning a discussion with Bacher on the necessity for Cornell's building a large cyclotron and the requirement of something like \$4,000-5,000 more per year operating funds for the

department to keep the machine in operation. Bacher had suggested a higher salary would be welcome--perhaps \$5,000? Day thought to himself that perhaps \$4,800 would be more in line. Two hundred dollars surely meant more in those days than forty years later. In any event, Bacher replied to Day five months later, thanking him for obtaining the needed funds but the war now surely meant delaying the plans.

Bacher went first to the Radiation Laboratory at MIT before he was called to Los Alamos. A telegram to the president from Santa Fe asks Day's permission to approach Bacher about coming out there on an important national project, nature unspecified. There was no hesitation in Day's acquiescence.

There is no further note on the cyclotron. Rather, a year after war's end, there is a letter from Day to Bacher informing him that the Trustees have authorized \$1.2 million to support the Nuclear Studies project; go ahead and make plans. We will refer later to this authorization. Within a month, he is pleased to learn that the ONR appears ready to support the accelerator, it had been estimated in substantial agreement with people at Berkeley planning a similar machine, McMillan's synchrotron, that it would cost roughly half a million, \$600,000 to play it safe. Then, six weeks later, there is a letter from Bernard Baruch concerning Bacher's resignation from UN atomic energy activities so that he can be placed full time on the U.S. Atomic Energy Commission. Day replies to Baruch expressing his regret at losing Bacher for even a few years, but being honored to have a Cornellian on the commission. Day and Bacher had high regard for each other; many others so regarded Day.

The Atomic Energy Commission was just getting set up, and Bacher would be its first scientist member. It had been mandated by the McMahon Act, passage of which scientists as amateur lobbyists had greatly helped, disposing of the May-Johnson Bill in the process. As previously noted, Willie Higinbotham had been very much involved in organizing this activity. Physicists at the universities and large scientific laboratories, physicists across the land in fact, were much concerned with the possible coming world which they had done so much to shape. In spite of our civilian control of nuclear energy, there was going to be nuclear weaponry, use of which, however, must not come to pass. It was clear other nations would acquire fission weapons and beyond; there were no fundamental

secrets. The Federation of American Scientists came into being, the Bulletin of Atomic Scientists, read as well by non atomic scientists, appeared, its cover clock set at five minutes to midnight. At Cornell there was a "chapter" of the Federation set up. The majority of the younger members of the department took active part. The prospects were then indeed troubling; they still are, but somehow we have become somewhat inured to living with the hazards. In any event, the spate of organized activity lasted but a few years and then paled. The Federation is still extant; some in the department are members, but it does not play the role as effectively as it once did.

The sudden increase in the number of people involved in the department after the war used to be shown dramatically in Moler's chart. There was plenty of space up to 1945. Thereafter, there was hardly space in each year for simply a typewritten list of the personnel, so that the effort was abandoned in the mid-fifties. While it did not list self-supporting graduate students it was otherwise a quick summary of department personages from Professor E. W. Blake (P) in 1867, to R. J. Zollweg, research assistant (RA), in 1956.

New to the faculty, but not to Cornell, having done graduate study in the department, were H. F. Newhall, R. L. Sproull, and myself. Not nuclear physicists, we had been involved with microwaves and radar in industrial laboratories (Newhall here in Ithaca), and so took up non nuclear activities in the department. Besides the newcomers and rejoins, the department was completed by the stalwarts who had manned things during the tough war years: Barnes, Collins, Gartlein, Grantham, Howe, Murdock, and Tomboulian.

If the shortage of research and office space was critical before the war, it was doubly so after. Of course, the construction of Newman Laboratory, a few years yet in the future, was to help the situation, but with the growth in student and staff population, the relief would be far from sufficient. In Rockefeller Hall, rooms were divided for offices; closets were sequestered for both office and research space. Space under stairways was utilized. Out in back of Rockefeller, where the tennis courts were, sport gave way to education; three identical old army barrack type wood structures were moved in, two for the teaching of laboratory in the sophomore physics courses, the third for Astronomy. They remained

there until Clark Hall was abuilding; Space Sciences would soon follow. They were pretty deplorable, but in their way they made things possible.

There was a political problem, about which I know little. It was obvious that there was going to be an active program in nuclear physics under Bacher. He and Bethe had envisaged that. At the same time, there was growing interest on Smith's part in solid state physics; spectroscopy, as well as vacuum ultraviolet and X-ray, would both flourish under Tomboulian and Parratt, respectively. Gibbs was relinguishing the chairmanship; who should succeed him? Both Smith and Bacher were able, ambitious, available, and obvious candidates. Apparently, however, neither somehow felt comfortable having the other as chairman, a circumstance perhaps to be expected in strong leaders of different and competing interests. To resolve the discomfort, it was agreed that a triumvirate would run things: Smith would chair the department generally and shepherd the atomic and solid state physics; Bacher would be in charge of the nuclear program; and Bethe would act as intermediary and arbiter in case of conflict. Debye remarked once at lunch over at "Martha Van" cafeteria that, running the department, we had the Father, the Son, and the Holy Ghost. What feeling of rivalry there may have been between Smith and Bacher was nothing like that earlier between Richtmyer and Gibbs and was certainly not apparent. They remained on very good terms so far as I know. They had in fact shared congenially an office when they were both National Research Council Fellows at Cal Tech, the year before Bacher went to MIT and Smith went to Munich and Hans Bethe. Anyway, by and large the trio arrangement worked, and the format was followed well beyond the time both men left the scene. One supposes there were trying times, but at least I was unaware of any such in these early years of a new departmental arrangement.

The organization was all set by the time I returned a year after the end of the war. I was offered an Assistant Professorship in Physics with joint appointment in the School of Engineering Physics now commencing operations and also under Smith's direction in the Engineering College. With Bell Labs moving from New York City out to the country in New Jersey, we were on the point of moving out with them when the opportunity was offered to come upstate. So we came. Beside favoring the climate (not necessarily meteorologically speaking) around a

university, I had the feeling, as I told Ralph Bown, director of research at Bell, when he asked why I was leaving, that the telephone had gone about far enough.

We spent the first week or ten days here with the Parratts in an upstairs apartment of their big house on Wyckoff Avenue. With the great influx of people to university positions after the war, there was a severe shortage of housing in Ithaca. The university undertook the development on South Hill of a housing project for people new to the community. When we arrived to take up our abode, the house was hardly closed in. Water had to be connected, heating plant installed, and other necessities provided. After ten days of Parratt hospitality, during which we became very close friends, we were able to move in and "make do" as less essential details in the house got finished up.

More should be said of Engineering Physics. The war had shown the role physics was to play henceforth in technology and had shown the inability of, yet the necessity for, the usual engineer to cope with and apply new physical ideas. Smith, with engineering background, saw the need for training engineers in advanced physics and mathematics. He was involved at RCA with radar and at Berkeley in an isotope separation project. He has said that, following the war, he had never intended to return to Cornell; he had discussed it with Bethe. Alerted by the latter, President Day was very persuasive in getting Smith to consider coming back. Like others, Smith recalls Day with fondness and great admiration. He agreed to come back if Day in turn would agree to push the establishment of a School of Engineering Physics. With the cooperation of Dean Hollister drawn on, the School came into being and Smith was its first director, holding the position for ten years, in parallel with his chairmanship of the Physics Department; it was a heavy load, chaperoning two new organizations, not to mention research and consulting activity. The new school was the first substantial such program in the country, although Lehigh had a small scale venture of like intent somewhat earlier. Other institutions have put similar programs in place, but Cornell's is still perhaps the best known.

Actually, there was conjecture about something similar back in 1930. In a letter to Professor Merritt, Professor Diedrichs of Mechanical Engineering asks about the possibility of more physics in their curriculum:

would it be just more tucked into the regular four-year program, or like Chemical Engineering, could we go to a five-year program?--"but if the latter then it would have a different slant." The difference between the Chemical Engineering and the supposed "five year <u>Engineering Physics</u> lies in that the former trains chemists with an engineering background and the latter trains engineers with a physics background." More or less right on the mark. Incidentally, after the war, all engineering curricula required five years at Cornell and did so for about twenty years.

Two names which must also be associated with the creation of Engineering Physics are those of Henri Sack and Trevor Cuykendall, student of Richtmyer's. They were heavily involved with Smith in devising the curriculum and getting things under way. For a number of years, teaching physics courses, they were listed on the Physics faculty roll. Another name closely linked with early Engineering Physics is that of Mark Kac. Strictly speaking, he was then a member of the Mathematics Department, but he is widely known for developments in statistical mechanics. He frequently took part in Physics doings and was often consulted in matters of theoretical and mathematical physics, and seemed an integral part of the Physics community. It was a distinct loss to Cornell when he left some years later for a position at Rockefeller University.

Of those starting the department off in the postwar years, the shortest periods of service were given by Collins and Bacher, Gibbs not being included, having just retired. His crucial role in setting the stage for future development of the department by pushing through some of the early nuclear appointments must be recognized. Collins died suddenly in 1947, and Bacher left in 1946 for the newly established Atomic Energy Commission as its first scientific member.

Collins was a cheerful sort, perhaps the most versatile member of the older "classical" group in the department. He was an experimentalist who could do theory, teaching in wave motion, optics and heat, and researching in infrared spectroscopy. Following his death, it was not until nearly twenty years had passed that the infrared had a renaissance in the department with A. J. Sievers coming in as an assistant professor.

That Bacher would be called on by other opportunities should have occasioned no surprise. Any surprise lay in how soon it came. He was a
very able physicist and had much to his credit beyond an attractive personality. There was his book of energy level term values put together with Goudsmit and published under Richtmyer's International Series imprimatur; there was his <u>Review</u> article with Bethe on nuclear physics, into which field he had come from spectroscopy, nuclear related; there was his visible experimental program here before the war on Livingston's cyclotron, which likely hastened Livingston's own departure for the greener pastures at MIT; and there was his war record at the MIT Radiation Laboratory with DuBridge and then at Los Alamos as director of the Experimental Physics Division. It has many times been told how, in the final assembly of the first nuclear explosive device, that tested out in the New Mexico desert, a critical part of plutonium would not fit into its appointed place. It was a tense moment, but Bacher coolly managed to set things right. The plutonium part had expanded in the heat and no longer easily fit. One's impression of the man is that he was a calm individual, level-headed, and of great ability. Following his tenure on the Atomic Energy Commission, he went to Cal Tech under by then President DuBridge, becoming head of physics and astronomy, and later provost. When he left Cornell, there was apparently no intention that he would not return. In any event, he took a leave of absence, which implied as much. Two years later something had changed; while he had time still left in his term on the Commission, he wrote President Day that he did not want his leave extended, realizing that he was taking a step which "will sever" his connections with Cornell. He was "pained" at taking the step, and so clearly was President Day. The powers in the department, however, felt that the door was being left open for a possible return. What entreaty was made to him subsequently is not revealed. Whatever there may have been, he nonetheless went to Cal Tech at the end of his term.

The brightest of the postwar additions to the department was undoubtedly Dick Feynman. If he was not the equal to Bethe, and I won't argue the point, he was certainly flashier in style and more colorful in person, more so than any other member of the group. He himself has referred to Bethe at Los Alamos as a dreadnought plowing ahead (in heavy seas?) with other theorists as cover and escort around him. Feynman is, and I presume always has been, a great showman along with his brilliance. His eventual loss was a sad event for the department; the years he was

here he made exciting not only in his loud boisterous manner but, in physics, in the development of modern quantum electrodynamic concepts-post-Dirac, work he did in the dingy Rockefeller Hall office he shared with Phil Morrison.

We have noted earlier that Robley Williams found in his work with Gibbs on the fine structure of deuterium, that there seemed to be a discrepancy between theory and experiment in the fine details of the energy levels involved in the Balmer alpha emission; that later Pasternak hazarded the guess that a slight shift in one of the presumed degenerate levels would fix things up. This was not in the Dirac picture of the Shortly after the war, Lamb and Retherford at Columbia situation. determined in an elegant and very difficult microwave experiment, that indeed things were different than had been presumed and that the $2p^2P_{1/2}$ level was not coincident with the $2s^2S_{1/2}$ level, the difference being about 1056 megacycles, or about 0.03 cm⁻¹. After the Shelter Island Conference on theoretical physics, in 1947, where this important result was reported, Bethe conceived the notion that the self-energy of the radiating electron itself might be responsible for the slight shift. He tells how Kramers, in connection with another matter aired at the conference, had suggested the necessity of renormalizing the mass of the electron to take account of its self energy. Bethe realized a week later that this might also enter into the Lamb shift. On a train ride from New York City back to GE at Schenectady (this is the train ride), where he was at the time, he made a "rear of the envelope" calculation of the shift, taking account of the electron mass renormalization. He could remember a needed formula from Heitler for the quantization of the electro-magnetic field, except for the uncertainty of a factor of two. His calculations could be off by that factor if he had the formula incorrectly in mind. As it stood, his result was of the right order of magnitude. On arrival at GE, he immediately went to the laboratory library to check on Heitler and was pleased to find the factor two was as he had hoped and that the calculation and concept appeared substantially correct. Subsequent calculation has much improved the agreement; today, this central problem in quantum electrodynamics is in total agreement with the best experimental results.

As far as it went the Dirac theory was all right. For the single electron it is correct; but it is incomplete for the case of the electron in

the field of another charge--and that of course fits hydrogen at the One of the difficulties in quantum electrodynamics is the sooutset. called "ultraviolet catastrophe." Zero point oscillations are presumed to pervade the vacuum. At higher frequencies the energy increases so that the total energy diverges. It had not been known how to handle this in the calculations. Feynman took up the problem and developed his own way of looking at things. The famous Feynman diagrams resulted, pointing the way to making calculations to an almost unlimited number of significant Professor Kinoshita has been carrying out calculations on fiaures. hydrogen incorporating corrections involving over 891 (!) different Feynman diagrams. Feynman had the unique ability to see through to the heart of a problem and to present it in a pictorial way. (His three-volume set Lectures in Physics is replete with his characteristic style and approaches.) At Harvard, Schwinger was also working on the problem of quantum electrodynamics in a quite different way, as was Tomanaga in Japan. Together, the three received in 1965 the Nobel Prize for the work, but I believe it is fair to say that the Feynman approach is that which is used in calculation today, save perhaps by Schwinger. Hardly a lecture in high energy physics does not start off with a Feynman diagram.

At the Theoretical Physics Conference, held at Mt. Pocono a year after that at Shelter Island, Schwinger gave hours-long, detailed, and difficult lectures on his theory; two half days were devoted to it. Feynman then outlined in his new language another way of dealing with the problem. He came away quite crushed. Bohr was very unfriendly, did not like it at all: "Positrons moving backward in time?" No one seemed to understand what he was up to, Bethe was an exception and assuaged Feynman's feelings about it all. In the end, however, it has been Feynman over Schwinger in dealing with quantum electrodynamics.

It was during this period that a bright young English mathematician, Freeman Dyson, came to Cornell to study with Bethe. Dyson recounts the circumstances in his entertaining <u>Disturbing the Universe</u>, a somewhat autobiographical and philosophical book (1979). He is remembered well by many in the department; his 1980 Bethe Lectures were particularly well attended. For one of his background and interest, it is not clear how he came into the Advanced Laboratory course. He indicates in his book that he very much enjoyed doing some of the well-known important

experiments in physics that had previously only been read about. (Would that more students felt that way about them.) He says he was persuaded that experimental physics was for experimentalists after he got knocked to the floor by high voltage used in the Millikan oil drop experiment. That was news to me and others, perhaps something of an elaboration of the facts, as I suppose there is in some of what is recounted herein. I was clearly not aware of who it was I was trying to teach when I informed him that his statistical treatment of the data he had taken on the Michelson interferometer was incorrect. He came in the next day with a short proof showing, indeed, that the professor was wrong.

Dyson in his first semester here registered for theory courses, reserving his experience at experiment for the second semester. R. R. Wilson, the Experimental Physics minor member of Dyson's committee, in the end-of-semester evaluation form sent to the committee chairman. theory member Hans Bethe, checked off that Dyson had done no work for his minor and scribbled tersely in the space allowed for comment: "Flunk him!" Anyway, with his mathematical background, Dyson worried how two such different approaches as that of Feynman and Schwinger could each lead to the correct results in quantum electrodynamic calculations. In his book, he entertainingly tells how he badgered both Schwinger and Feynman incessantly until he thoroughly understood both approaches. He made a major contribution in showing that the two were essentially the same thing, somewhat reminiscent of the joining of Heisenberg's matrix approach in guantum mechanics to the wave functions of Schrödinger.

Dyson never did earn a degree; why should he have? He went home and came back for a couple of years as a full professor before taking permanent leave to become a staff member of the Institute for Advanced Study at Princeton. He has broad and, in the opinions of some, rather impure interests in physics, about which he has written. Without doubt, no other of his contributions has been as important as that in QED.

During his time here, he entertained on a winter's day a young Japanese physicist en route home from a Rochester Conference. The physicist was another theorist, T. Kinoshita, also interested (and, as noted, still at it) in problems in quantum electrodynamics. He recalls going out with Dyson to some hill in the vicinity and gathering his first Christmas tree. It was some years later that he joined the department

high energy theorists. And more years later still that he himself recommended from CERN that we appoint a bright young "postdoc" there to our faculty, one Kenneth Wilson. And we did.

But back to Feynman. He was a most stimulating person to have around; this was also true of his officemate, Phil Morrison, but Feynman was always a performer in almost anything that he did. He has told of his arrival in Ithaca, spending the first night sleeping on a bench in the Willard Straight Hall lobby. And the next day, unkempt and very young looking, if a touch drowsy, inquiring of housing at the Straight desk where they usually had listed rooms for rent around the territory, and being told that student housing was unbelievably tight. "Why last night we even had a new Assistant Professor bedded down on a lobby bench!" One has a nice picture in mind of Feynman at a physics picnic, surrounded by enthralled youngsters, bewitching them with the wildest fairy stories his fertile imagination could conjure up, complete with gestures, of Feynman at a party with a comely miss, filling her in on details of his exploits hither and yon. Unbelieving, she challenges him: "The next thing I know, you'll be telling me you made the atomic bomb!" He: "As a matter of fact, I did." She disbelieved him to the end; and as for his being an assistant professor, why. ...!

He came back once from a trip to Buffalo, I believe it was, with a beautiful black eye. He had been in a bar up there where some wise character was fleecing the other customers with some card tricks and other flim-flam. Feynman apparently observed this for a while and then allowed as how the gentleman was cheating and he could show him the way. Which he did, with the result to his eye that he brought back with him to Ithaca. He came into his first class, stood there familiarly twirling a chalk in his hand and asked, "Any questions?" Earlier he was known at Los Alamos, not only for his theoretical brilliance but also for his ability to pick locks and open safes, on one occasion leaving a note to be discovered in a very tightly secured area, saying "guess who?"

He could tell of one hilarious experience with the Selective Service. It can hardly be told with the gusto of Feynman relating the story. It seems that after he came to Ithaca, the Selective Service wished to know why he should not be classified 1-A for the armed forces, would he please come to Albany for his physical examination. There is a memorandum in

the department files on Feynman attesting to the need to keep him out of He was at GE in Schenectady for the summer, warrior classification. along with Bethe, so Albany was an appropriate place to which to be summoned. Well, he went over and seemingly passed everything with flying colors up to the last examination by the psychiatric people. He sat in the room preparatory to the examination watching some of the other examinees being put through their questioning by the staff psychiatrist. It came Feynman's turn. He was asked to put his hands out. He did. But not like the normal person with both palms up, he had one palm up and other down. Turn them over. Instead of turning them as would an ordinary recruit, one clockwise and the other counter clockwise, he turned them both the same way. Naturally the examiner was taken a bit aback and probably raised an eyebrow, but he continued. One thing led to another, all of which can hardly be related properly by anyone but Feynman. He was asked if he ever imagined that people talked about him. Naturally they talked about him he said, which of course was certain. Did he ever imagine that people were looking at him. He wheeled around in his chair and pointed to one after the other of those awaiting their turn for examination: "You, you, you, and you, you're all staring at me!" It wasn't long before he was asking questions of the examiner rather than the other way around. Did the examiner think he was earning his salary? Was he not ready for the psychiatrist himself? Feynman wound up with a 4-F classification--feeble-minded. Feeling guilty about it, he later wrote the service about the incompetent who had examined him, suggesting that he really wasn't of 4-F caliber. But it was to no avail; he remained a weak mind and was never called up.

As is known from his photograph introducing his <u>Lectures</u>, he is fond of bongo drums. It perhaps explains the eagerness with which he accepted an invitation from the University of Sao Paulo to come give a series of lectures in that Brazilian center of learning. He diligently took on learning of Spanish better to facilitate communication when he got there, only to discover on his arrival that it should have been Portuguese.

He had a pretty heavy teaching load; the 1946-47 Arts College announcement has him doing the graduate one-semester course in electrodynamics (of the ordinary variety), one in problems in Theoretical Physics, and the year-long course in Mathematical Methods of Physics.

This last course was one that Smith had developed before the War (a text resulted, Mark Kac doing the chapter in Statistics and Probability) and which was taught for several years subsequently to almost every physics graduate student. It finally went over to the Mathematics Department, with Mark Kac initially, and it has remained there ever since, albeit sometimes in questionable care. Maybe we overworked Feynman; Cal Tech was in 1950 to lure him to sunnier climes. He returned here on occasion, most notably for a couple of weeks in 1964 when he gave the Messenger Lectures on the "Nature of Physical Law." The lectures were popular and very well attended, still to be heard and seen, since the BBC, with a big monitoring and control van outside of Statler Hall, recorded them.

If Feynman was the most brilliant of the new members of the department, his officemate, Phil Morrison, was probably the most widely read, the most erudite, the person with the most diverse interests and knowledge. He wrote well and he lectured well. The erudition and quality of his writing and expression are amply shown by the book review section of the <u>Scientific American</u> that he has written monthly for many years. He simply devoured reading material and seemingly retained what he read--a prodigious memory. His theoretical work ranged more widely than that of Feynman but tended toward the astrophysical and cosmological. (However, in making that comparison, one must not forget the significant theoretical contribution of Feynman in his later career to Landau's rotons, and thus to liquid helium science.) Probably Morrison's best remembered work, although not yet fruitful, is the discussion he and Giuseppi Cocconi submitted to <u>Nature</u> on the possibility of communication with civilizations elsewhere in the universe. How would a message best be coded for our communicant's understanding? at what frequency had it best be sent? and so forth. It is the paper referred to when later authors consider such communication. The ideas were put to a test by F. Drake, a Cornell EP graduate and later director of NAIC, in his project OZMA at the Greenbank radio observatory. Drake, and others subsequently, have listened for intelligence from space. For a brief time, it was thought they had it; but that notion was quickly dispelled when the signals were identified with what came to be known as pulsars. Thus, to date there has only been noise modulated by silence. Not that it seems such a big deal to the writer; so what if a count signal comes in on, say, the duo-decimal

system, or better, the binary code? Aren't we already confident that they are out there somewhere? Admittedly, not a very scientific attitude, but surely observation time must better be spent on other matters.

Space has had an attraction for Cornellians, one way or another. Dyson in some of his "impure" physics has, since he left Cornell, spent considerable time in work on rocket propulsion by atomic explosions (a concept fathered by bomb expert Ted Taylor, a one time graduate student of Bethe's from Los Alamos) and more recently on some wild colonization conjectures. G. K. O'Neil, McDaniel's first graduate student, who first suggested and worked on the storage ring concept and colliding beams, has gone heavily into promoting his notion of space vehicles as space cities. McDaniel disclaims any responsibility for O'Neil's latter activity.

Morrison was also the most vocal of the Cornell physicists in political matters and was at the same time probably furthest to the left, espousing rather publicly various leftist causes. He became fairly well known on the campus and elsewhere for his support of such. The McCarthy era was shortly to be upon the nation. The senator's activities were deplored by many others in the university besides Morrison. But I think it is fair to say that Morrison (and perhaps one or two others) was probably the target of those on the university faculty who wished that he would not so publicly state his position on this or that issue. There was a memorable faculty meeting, which strangely enough, Morris Bishop in his Cornell History does not mention; yet it was he who put away the matter to be discussed, namely, could a Cornell faculty member also be a member of the Communist party--a card-carrying, party-line Communist? To put it bluntly: should not Communists be disbarred from faculty position? lt was not known that there were such at Cornell, least of all, Phil. It later turned out that he had at one time been one, perhaps at Berkeley, but he had not been of the party for years. Faculty meetings convened then in Boardman Hall, where Olin Library now stands at the end of the Arts Quadrangle. For this particular meeting, the old meeting place would not begin to hold the crowd, so they all moved over to Goldwin Smith Hall. The meeting opened, the resolution was presented, and Morris Bishop was recognized by Acting President deKiewit. In his clarion voice, Bishop told us in essence that we would be damned if we took action and passed the resolution, but at the same time we would be damned if we failed to take

action. He moved to table it. There was a shout of "No!"; nutritionist and Nobel Laureate Professor Sumner thereby got in his opinion, for there is no debate on such a motion. Strictly speaking, Bishop was out of order, for he had debated it and made his points before moving to table. In the vote, his position was solidly upheld and the meeting adjourned. "Bishop's a killjoy," Professor Briggs of Government was heard to remark to a colleague as we left the hall.

Some time later, when Phil was being promoted to full professor (or to tenure as associate professor?), there was considerable worry on the part of some on the Board of Trustees. During the board meeting in which board approval of the promotion was being considered, a session near the end of Lloyd Smith's tenure as faculty representative on the board, Lloyd made a number of calls over to Newman Laboratory to get this or that cleared up by people who knew Phil well. The promotion was approved but there was a rider: There would be an investigation of Phil by the board. A subcommittee was set up and at the appointed time Phil showed up at the library of the A. D. White house, with his own counsel, Dale Corson. While most of the committee was fairly reasonable at the inquiry, one member, the chairman, was particularly nasty. It was not a pleasant experience for others on the committee and obviously not for Phil and Dale. Phil was required to testify under oath. It was a long affair and a lengthy public report came out of it. President Malott refused to read the report; he had already made up his mind on Phil's obvious suitability.

Teachers have always been a politically suspect lot; at Cornell probably more so than at some other institutions. Memory does not serve to indicate when it was first implemented, but there was (and still may be) on the books a New York State law requiring teachers at state supported places of education to take an oath swearing support of the New York State constitution as well as that of the country. I well recall going over to Day Hall for a copy of the state document in order to look it over before I would sign on. I don't recall what it was that I found wrong with it, but the University Counsel recalls that I would not sign until a corrected copy was obtained. Others also resented the insinuation that we were less patriotic than our brethren on the outside. But we sign. Merritt has a letter in the archives opposing the requirement and devotes four pages to a defense of his view. To no avail.

The Communist "menace" was with us for a number of years, and the department was not immune from the effects. We had one open, avowed, but rather tame Communist join the department as a graduate student in theory. He made the newspapers now and then for his activity. The last I remember of him was reading that the FBI or some other constabulary had him "treed" up a water tower somewhere. For all I know he is still there. And there was Bruno Pontecorvo, one of Fermi's Rome group. He paid us a visit, gave a colloquium, and was interviewed for a position. He was not offered anything here; he wound up instead in Russian weapons development.

There was another character, a devious, local, politically-minded fellow, who after unsuccessfully attempting to gain admission as a graduate student in nuclear theory, became the circuit and controls engineer for the synchrotron in the Nuclear Studies electronics shop where, it has since been surmised with fair certainty, he could still be in close contact with people who had been at Los Alamos and thus well informed on nuclear weapons technology. Little would probably have been thought of him except that he suddenly disappeared with the wife of a physics graduate student, leaving her very gentle husband with two small children to bring up. That incident was a source of much pain to various persons who were department connected. An article in <u>Physics Today</u> (September 1985) dealt with the activities of the technician.

Quite a few years later, John Howe, director of Materials Science and Engineering Physics during the period of their brief marriage, called wondering if there was some optical equipment over in Clark Hall which could be used by a visiting Russian he was about to sponsor. The man was interested in the optical properties of metal surfaces. Rhodin in Engineering Physics had a fancy ellipsometer which was not being used, so I allowed as how I thought that it could be commandeered for the Russian's use. Came fall and he appeared on schedule. But Howe had disappeared, leaving Cornell for points west from whence he had come (followed by separation of EP and MS&E). So I was stuck with the Russian, a circumstance of some slight irritation to me. I got him set up and helped him get started; but nothing ever came of it. He was around more or less the full academic year, but there was very little experimental work done. He disappeared on numerous occasions to visit the Russian

mission in New York. After a couple of such visits I was presented with a bottle of Stolichnaya vodka; a nice volume of scenes in Moscow graces our coffee table, a small Russian vase the shelf. A very pleasant mild character; he had left his wife and child home in the old country. (Return insurance?) Some time after his departure, a CIA operative out of Syracuse visited me and wondered about him; what had he been interested in; what had he done and where had he gone; who had he seen. There was suspicion that he was a spy. I could only say that he exercised the same curiosity I would have wanted to exercise if I ever got to Moscow; he seemed pretty innocuous to me. But I had to admit that he surely never got any physics done during his year with us.

There was an interesting sequel to this visit of "Slavo" (as he was nicknamed) fifteen years later. Fred Goldstein, a graduate student of mine during the time the Russian was resident here, went to England on a postdoctoral appointment. Told by a colleague, a fellow student of the time, that "Slavo" was in London, Goldstein called the phone number he was given to get in touch with the man. There were a lot of delays, many "nyets" before he was put on the line. It would not be convenient for Fred to meet him at his office; yes, they could get together for lunch, which they did pleasantly with much reminiscence. The Russian's wife was not left in Russia this time around, he had a car, was drawing two salaries somehow, and things were pretty rosy. He couldn't very well describe the work he was doing but things were going well. Many years later, after Goldstein had set up his own computer consulting business and was doing something for Lawrence Livermore Laboratory, he was invited to come to Oakland to talk to Intelligence. He was a bit concerned as to why they wanted him, but he went. After a few pleasantries and preliminary skirmishing, he was asked what he was up to in dealing with Russians. He was guite taken aback, and asked what they were talking about. Well, hadn't he called the Russian Embassy in London on such and such a date some years ago? And then it came to him; the phone number he had been given to get hold of "Slavo" must have been that of the Russian Embassy; our own Intelligence had been on the job. He explained all and his clearance remained intact. It would be interesting to know more about our Russian friend.

We used to get frequent visits from FBI agents or other civil servants making security clearance checks, to ask this or that professor what about so and so, a former student he may have known, one who had taken a sensitive job in government. In general, I think we cooperated, albeit somewhat uncomfortably. At least that was my situation, to serve as background for my own run-in with the investigative bureau.

One summer I was asked by Bell Laboratories if I would visit a number of Air Force bases to look into the failure rate they were experiencing with a particular microwave magnetron, about which I was supposed to know something. I agreed on a Friday to do so. Could I be in Texas on Monday to start? I could. Would I be sure to have my birth certificate with me? Unfortunately, it was in the bank which was then closed for the weekend. OK; what we will do is the following: I will be in Texas at the Fort Worth base on Monday morning; my wife in Ithaca will go to the safe deposit box, get the thing out, take it over to the local FBI office, and they will wire the clearance people that I have been born. I did; she did; the FBI did not. They could not take up a matter like that for any Tom, Dick, or Harry. Nothing doing. Well, I don't know what strings Bell pulled, but I did get in with only a few hours waiting around and no birth certificate that I know about. But I was pretty mad. If we could cooperate in their miserable doings, why could they not reciprocate in what was thought to be an urgent national matter on our part? So I spent the summer flying about, from this base to that, seeing a lot of jet airplanes (it was guite something to see a surveyor triangulating with his transit atop the wing of a B-36) and testing a lot of magnetrons, and reporting.

Came fall, a hapless FBI agent showed up for a recommendation on some former student. I gave him a pretty hard time, and I think he was rather upset at being the innocent man in the middle. I suppose, weak sister that I am, I finally gave him what he wanted and he left. My tirade must have borne fruit; a week or so later I got a call. Could an agent from Albany come by and speak to me? Why not? Some days later, Corson and I were in our shared office when the man showed up. He argued about why they could not have done what I wanted, and I argued that they should have. Neither of us gave much way. And then Corson joined in, piqued especially over the FBI harassment of his student whose wife had run off with the

above nuclear electronics "technician." Together we gave this man a hard time and he left. Shortly after that, as I recall, there were two items in the Times that gave the lie to the man from Albany. The Times reported that an ordinary, common, everyday burglar rifling an apartment in Washington had stumbled on a document labeled RESTRICTED. After finishing his operation, he patriotically called the FBI, telling them that they had better hot foot it over and impound the document. Which they did! Tom, Dick, or Harry? Another item reported that the FBI, in going over credentials of a list of workers in the New York City campaign headquarters of the Republican presidential nominee, had recommended that a certain number be dropped as having unsavory records. Which was done. I fired a letter off with this information and asked: How come all this? I got a reply from J. Edgar himself. He denied the story about the FBI recommending one way or another to the Republicans, although tacitly admitting the investigation. He never mentioned the burglar. I only recently ran across Hoover's letter again.

But in the end we lost Morrison like Feynman, this time, in 1964, to another Institute of Technology, that on the banks of the Charles. His loss was also deeply felt. He gave the department a diversity that it has not since regained I would say. It is good to have him return on occasion to give lectures and colloquia, reminding of the "old" days.

Dale Corson was another of the new members joining the department after the war. He had spent the war years for the most part in radar, but came to Los Alamos toward the end of the War. He was in large measure responsible for the establishment of the Sandia Weapons Laboratory in Albuquerque. He was one of the Berkeley cyclotron group under Lawrence prior to his appointment as assistant professor at Missouri, from which he took leave for his war activity, later resigning. At Missouri he came to know 0. M. Stewart, one of our early Stewart brothers. After the war, Corson was uncertain as to whether to come to Cornell or go to Michigan. It was perhaps the greatest contribution to Cornell of Robley Williams, then at Michigan, that he favored Dale's coming to Cornell when he was asked for his opinion.

Dale tells of his introduction to Lyman Parratt. It was during the war when there was a conference arranged to discuss technical performance of sensitive, magnetic, antisubmarine equipment, that of the

Air Force and that of the Navy. Corson, a radar expert not knowing much about the magnetic devices, nonetheless was representing the Air Force equipment against some Navy representation for their equipment. Both sets of equipment had possible application as airborne detectors. Corson recalls some degree of heat between him and the Naval promoter. There were good points argued on both sides, but the Air Force view of things was unchanged by the meeting. It was not until some years later, when considering the Cornell appointment, that Bacher wanted him to meet another member of the department still at Los Alamos: Parratt. It was not until Corson walked into where Lyman was hanging out that he recognized his old adversary at the wartime conference. This time around it was harmonious and all went well. Lyman does not recall any particular contention, probably because he knew the subject full well; Dale was clearly on the defensive; he feels in retrospect that the Navy gear was probably better than that of the Air Force--it usually was.

I recall well meeting Corson for the first time. I found him in a Rockefeller basement room near the cyclotron discussing some aspect of the planned synchrotron magnet with McDaniel and others. I introduced myself to him and informed him that Smith had told me I was to share an office with him, which we did for many years in Rockefeller 123. A fine office, but I considered it somewhat unkind that he did not take me along with him when he moved into the department chairman's office and other sumptuous quarters that came along to him some later. In the end we have almost made it, however. I am just across the corridor from his retirement headquarters in Clark Hall. Only he has windows, a view, and carpet.

In science, Corson is probably best known for his discovery at Berkeley of astatine, and for his widely used text in Electricity and Magnetism, now in its second edition, written with Paul Lorrain, who had been a postwar spectroscopic research associate in Rockefeller and later a professor at the University of Montreal. Corson notably also measured the radiation loss of accelerated electrons in the synchrotron, which was of more than passing interest. Today that loss represents a major portion of the power input to the Cornell storage ring and places a practical limit on the energy attainable in a machine of given size and circulating beam current. In this case no trickery is in sight to avoid it--yet.

Of course, Corson went on to bigger and better things in the university. In spite of his coming to be chairman in ten years--he did not hold the office all that long before he was tapped to be Engineering's dean--his best contribution to the department probably came before his chairmanship, in the role he played in persuading R. R. Wilson to leave Harvard and come to take over the direction of the Nuclear Studies Laboratory, which had been vacated when Bacher left for Washington and the AEC. (The word best there is arguable; the impetus Corson gave in our acquisition of the Materials Science Center is not an insignificant contribution.) The imprint left by Wilson on the department will be long in disappearing. The way Cornell builds high energy machines is widely recognized and applauded; it is really the Wilson approach that was carried over later in his construction of the great Fermi National Laboratory accelerator, which he headed during a ten year leave and subsequent resignation from Cornell. Upon his own retirement there, it would be good if he "came home again." On the recommendation of the department, the Board of Trustees made him emeritus professor in 1979. More will be said of Bob Wilson.

Corson taught a term or so of heat and mechanics and put in a stint in the third floor Advanced Laboratory. After Murdock became dean of the faculty, Corson fell heir to the former's intermediate course in electricity and magnetism; it became Corson's course. He recalls a "red-haired kid who sat in the back of the room and never said anything-but he did everything I was capable of assigning him." The "red-haired kid", Steven Weinberg, with his 1954 classmate, Sheldon Glashow, together with Abdus Salaam, received the 1979 Nobel Prize in physics for a unification of the electromagnetic interaction with the weak force of nuclear physics, which plays a key role in beta decay. Corson was a teacher of Glashow also in at least three courses, marking both students as exceptional.

He gave the E & M course in Lecture Room C of Rockefeller. At one point, the manually raised blackboards were converted to motor drive. This action resulted from a solid state seminar given by a young lady from industry. Her seminar is remembered only for her inability to raise the boards; it wasn't all that difficult, but she refused to exert herself and some male in the audience had to manipulate them for her. A motor spared future embarrassment. As soon as students were aware of the changeover, a challenge was presented. Corson one day found that when the switch was in the raised position, the board went down, and vice versa. That sort of thing he quickly mastered. But some clever electrical engineer outdid himself. When Corson called on the board to descend, it did that and then went back up to the ceiling, or it would go clear to the floor on being called down. There was no way it was useful. He was forced to surrender and ask that it be switched back to orderly state.

Another member of the young faculty group starting with Smith's tenure, one who also went on to bigger things, was R. L. Sproull. He had been a Cornell undergraduate supported by Telluride, going on to graduate study with Lloyd Smith in physical electronics. I met him first as a summer employee at Bell. During the War he was at the RCA Laboratories and came back to Cornell with his interests solidly in solid state physics. He also taught in the Advanced Laboratory, gave a course in mechanics, and then developed one in modern physics for engineers, which led to a popular text, now in its third edition. Sadly, this most viable course has disappeared from our catalogue. He and his graduate students made a significant contribution to the physics of electron emission of oxide coated cathodes (an important element of vacuum tubes, for those not knowing that device), by studying the fundamental processes of the oxides in single crystals, which they learned to grow. His approach at getting to the basics, thermal and electrical conductivities, ion and electron mobilities, optical absorption, and the like, marked him well. When it came, we made him our first Director of the Laboratory of Atomic and Solid State Physics, later that of the Materials Science Center which followed. His outside activities and contacts (he served as editor of the Journal of Applied Physics for a number of years) led to his being offered in 1963 the directorship of the Advanced Research Projects Agency during the disastrous Vietnam years. ARPA was to have a major impact in the acquisition of Clark Hall. Sproull returned, after two years in that post, to become the Vice President for Academic Affairs in the university. This seemed but a stepping stone to his assuming the position a year later of provost at the University of Rochester and then its presidency. He served for a number of years as a member of the Cornell Board of Trustees.

Besides Corson, there were other young experimentalists of nuclear persuasion starting their postwar departmental careers as Smith took

over. We had Boyce McDaniel and Kenneth Greisen, assistant professors, and John DeWire and Charlie Baker, research associates. A year later there was Bob Wilson, professor, and a year after that, William Woodward, assistant professor, from Los Alamos like the others, but by way of two years at MIT. He and DeWire had worked with Wilson for a period at Princeton on a uranium isotope separation project before they all went out to bigger things at Los Alamos. The first three of these men are still active in the department after thirty five years. Following years of ill health, and innumerable operations, at least in part he felt by the careless things they did in the war neutron research, Woodward would, in 1976, take long term disability leave. (He became professor emeritus in 1982, a vear before his death. In his final illness, he found it ironic that he was undergoing radiation treatment for a condition brought on in part perhaps by radiation.) Wilson left in the mid-sixties for the flats of Illinois. Baker, until he left a few years after his return, more or less took over operation of the cyclotron, with which he was well acquainted from prewar days. For its size, this old friend of his probably did more useful physics, perhaps reluctantly, than any other cyclotron ever built, winding up its days here after the synchrotron "came on line" by being dismantled and shipped to Israel to continue making physics.

Of these experimentalists in the nuclear laboratory, Greisen was somewhat off the beaten track, working in cosmic rays as he had in the days of Rossi's presence. He continued this line of research throughout his scientific career, being joined eventually by Giuseppe and Vana Cocconi and a succession of research associates, Giuseppe being made a professor before he resigned in favor of CERN, the European Fermi Lab. Notable in the research was an experiment, which embraced practically the entire Cornell campus, for the detection of large scale showers. Large scintillation counters were located at widely spaced places around the campus. Coincidences in events seen in the detectors were indicative of And they were not infrequent. truly large showers. Nighttime fluorescence caused in the upper atmosphere was sought, but not very successfully, as another handle to shower detection, a technique now being exploited in the "fly's eye" detector by a group at Utah under Loh and Cassidy, two former Cornell high energy physicists. This was of interest to the writer, who had made measurements on the efficiency of light

production in air fluorescence in a couple of sabbatical years spent at Los Alamos. Another of Greisen's achievements was the promotion of a rewarding high altitude balloon flight for cosmic ray study in the 1960's. He became dean of the faculty in 1982.

From the beginning of Cornell's synchrotron developments, McDaniel and DeWire have been very much in the thick of both the building and the utilization of the succession of machines we have had. McDaniel continued for a while in the use of the cyclotron, which he, like Baker, also well knew; under Bacher he had made those good time of flight measurements of slow neutrons, Baker's ion source modulated to good advantage.

John DeWire, who had worked under Wilson at Los Alamos and earlier, at Princeton, on an isotope separation project as had Woodward, was made an assistant professor after two years. He did his graduate work at Ohio State, his undergraduate at Ursinus. Anyone knowing DeWire very guickly becomes aware of where Ursinus is--Pennsylvania, for the unknowing. That state has no more loyal son than DeWire; and Milton, a town in the province, no prouder booster. After physics, railroads, steam in particular, are his next love. He is the proud possessor of a picture clipped from a German newspaper showing him at the throttle of the German locomotive of a crack train he chaperoned on its run along the Rhine. German high energy friends, who knew of his not-so-secret longing, arranged the trip during a high energy conference over there. Presumably there was a more experienced hand looking carefully over his shoulder during DeWire's shift. Any time one wants to know anything about Pennsylvania or the railroad of the same name, he need only ask DeWire. His Alma Mater honored him in 1979 with an honorary degree. Well deserved. It is not clear whether on the occasion he reminded them of an interesting point which came to light after he had been at Ithaca for The Cornell Mathematics Department one year had an some years. application for admission to graduate school from one Jones at Ursinus. One of the letters of recommendation supporting the candidacy of the applicant was one from a former professor of DeWire's. The professor compared Jones to DeWire, the latter coming off rather better than the "However," the professor ended his recommendation, "Jones candidate. does not drink." John must surely have mended his ways since those days to warrant that comparison; he is a very moderate person. But his

circumstances back then were hardly such as to permit much excess of any sort in student life.

Woodward, I believe, was the last Los Alamos participant to come to the department faculty. We did have a graduate student come later who had worked at Los Alamos, Ted Taylor, a bomb designer of considerable repute featured in one of those <u>New Yorker</u> profiles, who came to work with Bethe. There were others before him, notably, Bob Walker, who went to the faculty at Cal Tech. The first faculty member to join the experimental nuclear group here who had <u>not</u> been through the Los Alamos experience was Albert Silverman, coming from Berkeley in 1950 as a research associate. He became an assistant professor in due course, an important addition to the Wilson team. He has been here ever since, outspoken and valued, something of a department conscience.

Of the "old guard," Barnes, Grantham, and Howe were not connected to any research. Grantham had the freshmen engineering courses, Howe the other introductory physics courses, and Barnes taught a physics course for premedics and became the premed faculty advisor. Murdock's activity in crystallography was soon to end when he became dean of the faculty. Tomboulian, in charge of the sophomore engineering courses, was very active in his research--soft X-rays--until his untimely death in 1963. Herb Newhall, although with Cornell experience, was also a new assistant professor who taught the freshman engineers with Grantham. His research was in mass spectroscopy, in particular on the products evolved in the processing and subsequent use of the oxide coated cathode. The work was not very fruitful in his view. He has said that "progress in the field was not noticeably hampered by what we had done." Over the years his interest turned more and more to computers, both as teaching aids and as entertainment. At his retirement in June of 1981, he was presented with a personal computer to facilitate his interests along that line.

In 1953, R. C. Bradley from Berkeley joined in the mass spectroscopic research, became an assistant professor, and went on to Colorado College in 1961. Parratt kept on with his high resolution X-ray spectroscopy, developing highly sophisticated, precision instrumentation for that work, much of which is still referred to. He and Collins looked after the Advanced Laboratory course, in which others of us also served. Unhappily, Collins died in 1947. Carl Gartlein, who held that somewhat

odd position in the department of so-called curator, did little or no formal teaching after the war years but was almost indispensable to the department, having great knowledge of general instrumentation, ways to do things, where things were, and supervision of some of the service people. He continued to receive support from the Geographic Society for his research program on the aurora, mostly borealis, although he did get to Antarctica for some <u>australis</u>. This was a program he had built up with the aid of his wife, Helen, and a research associate, Gale Sprague. The effort became an important part of the IGY program with the extensive network of observers he had managed to enlist across the country and Canada. Between Ithaca, Hamilton, and Geneva, he and his cohorts obtained simultaneous photographs for parallax measurement and height His observatory, housing a number of cameras and determination. instruments he had developed, was housed in a barn at the back of his house, then out in the country north of Ithaca, about were McDonald's eatery (hard by Pyramid Mall) now graces the scene. He too died an untimely death in the 1960's; his program was going strong but it then came to an end.

As for myself, I became part of the faculty as another of the new assistant professors and was assigned teaching in the Advanced Laboratory course with Collins and Parratt. For research I engaged in an experimental approach to a proposal which Smith had suggested. He had worried about the quantum aspects of microwave radiation and wondered whether an ammonia oscillator could not be made. There was no clear idea as to how it might be accomplished, but measurements of absorption and the building up of some equipment could go ahead. A couple of theory research assistants concerned themselves with the physics of the concept. Townes at Columbia was guite interested in what we were up to; for good reason, although he need not have worried too much. The two theorists decided it was an impossibility; the Einstein B coefficient for absorption and emission were one and the same, and population inversion was not known. And then Townes reported his ammonia maser. He had managed to invert the population, separating his excited molecules from those unexcited through a difference in the two guadrupole moments in a molecular beam apparatus at Columbia, a place most appropriate for such

technique. I then went into vacuum ultraviolet spectroscopy and the optical properties of some alkali halides.

The assignment to teach in the laboratory course was much to my liking. There was opportunity for improvement and modernization; and that I could enjoy. A year later I was also doing the optics course that Collins' death necessitated someone's taking on. That also was a subject I had always enjoyed. So I was lucky. I later gave a mechanics course for sophomore EP students; this meant a lot of homework problem solving. Back in those days, we did not often have teaching assistants reading our problem sets and prelims for us. At least I have never basked in that luxury.

The laboratory course was a long and established feature of the Cornell Physics Department, antedating World War I, deriving indeed from Blaker's course in Franklin Hall. Cards are on file, presumably for every student who ever took it-through 1979 at any rate. In that year I was retired from supervision, which I had wound up with, succeeding Parratt, who in turn had taken over at Collins' death. It turns out that before semi-retirement, I had taught in the laboratory every semester (and some summers) since becoming a faculty member, barring leaves of absence. Leaving it should have come sooner. A program of modernization was again undertaken in 1979; not that it was stagnant during the years after 1945, but the instrumentation and the emphases were clearly dated by 1979. Probably the most noted student who ever took the course was I.I. Rabi in 1922. He got a B (lab C; oral A; written C). Whether that had anything to do with his not coming to Cornell for his graduate study is not known. He had been an undergraduate in Chemistry and applied for graduate study in Physics. But he was not admitted; we don't know why. He went to Columbia. Another noted student was L. H. Germer in 1916; he too got a B and went to Columbia. It must have been a tough course in those days. But at least he came back to Cornell to live and work out his retirement years. Columbia must have recognized our worth.

Even with the somewhat dated appearance at the time of its present modernization program, the laboratory bore little resemblance to what it was at the end of World War II when the staff under Parratt set about updating it, a process which continued more or less slowly over the intervening years. The Compton electrometer and contact potential

measurement disappeared; vacuum tube electrometers took over, first home built and then the commercial instruments. The World War I radio frequency bridge--housed in a giant, coffin-like, shielded box--was retired. But some of the optics and spectroscopy experiments, X-ray experiments, and heat experiments were retained and are viable even today; some good physics remains good physics. Techniques and instrumentation change, and this is reflected in even ancient experiments. Needless to say, the quarters in Clark Hall are vastly superior to the third floor of Rockefeller.

In its heyday, under Parratt, two semesters of the laboratory were required of Physics graduate students and EP seniors, one semester of the undergraduate Physics majors. During one semester, there were over one hundred students taking the course; two sections, each meeting two days a week with ten professors teaching was the usual. Experiments were spread all over most of the top floor of Rockefeller. It was pretty lively. It was up there that Dyson supposedly got laid out. We had floods occasionally, power outages more than occasionally, one fire, and other smaller crises.

One occurred at night when two students were working on an X-ray apparatus. They were making a long exposure, meanwhile doing other school work. One noted that a beam port had been left open toward a blank wall, and X-rays had been shining out unused, and he made mention of the The other student commented on this to his wife later that night, fact. and she almost had a fit. He was sterile; they would never have kids! Oh, the pity. Sue the university. Well, it took a fair amount of doing to settle things down. The medical office was called in, the radiation people came over, and some others. The upshot was that the two students had received an exposure equivalent to sitting for several hours in front of the television. Not that leaving beam ports open was at all condoned, however. On another occasion, and this in Clark Hall years later, another student was carrying a storage battery up the stairs from the third floor to the fourth floor. (Storage batteries serve as awfully guiet and stable DC supplies.) Anyway, this EP Senior (as were the men above) did not know (!) that storage batteries have a liquid in them which will eat up a necktie, given the opportunity. Which he gave it. He spilled acid down the front of himself and came in fright down to Nick Szabo, our technician, for

aid. Nick got alarmed and pushed the fellow under the emergency shower and gave him a bath. Safety people rushed over in response to an alert, and all must have been pandemonium there for a few minutes. I believe he did spoil a shirt and tie. I was on leave at the time and heard about it later.

There was another incident, possibly serious, associated however, with laboratory research in the Rockefeller basement. Although no accident, there was for a time a threat to the university. A female graduate student working with Sproull was doing an experiment involving radioactive tracers or some such. As her work progressed--experiments were not done in a mere week or so--she noticed her well-being She visited her doctor, told him her situation, and he deteriorating. agreed with her that she had gamma ray problems. She saw a lawyer. He advised suing the university. The University Counsel was alerted; he told the department to prepare the evidence for a defense. There was considerable concern in the department over it, although monitors at the experiment indicated that only a very low count rate would accrue to a careful experimenter. The young woman's condition did not improve. Rather abruptly but quietly, however, the case against the department disappeared. At a subsequent visit to the physician who was following the course of her radiation poisoning, he determined that she was some months pregnant.

Something like that can induce others to have similar symptoms of indisposition. In late 1980, after a couple of dozen people had complained of respiratory problems in Clark Hall, an investigation was undertaken. Outside consultants were called in to determine if there was indeed some contaminant and if so what it might be. Indications were that any such was well below tolerance level. There was no clear resolution of the matter. Some hall inhabitants sensed psychosomatics at work.

I think it was when I returned from my first sabbatical leave at Los Alamos that I was asked by Sproull, Director of the Materials Science Center, to lay out plans for Advanced Laboratory space on the third and fourth floors of Clark Hall, then in the planning stages, a project with which Sproull was much concerned. With the help of others who had taught in the course, we did so and Cornell as a result probably has the best advanced and intermediate laboratory space in the country. As the emphasis on advanced laboratory work diminished, no more than a single

semester being required of anyone, other laboratory courses have moved in to share the space: the new optics course of Mahr's, the intermediate sophomore course once taught by McDaniel and Barnes, and an electronics course. The space proved to be flexible enough to accommodate this variety of intermediate level laboratory courses offered by the Department.

The electronics course has been a notable success. For many years the need was felt for "hands on" work in electronics. One would have assumed that this would have been available somewhere in Electrical Engineering, but what there was was not available without one's having passed <u>n</u> prerequisites. There were on the campus many places where experience with the knowledge of electronic circuitry would further research objectives. Even as late as the early seventies, when Engineering Physics was to make electronics a required course, there were no courses in engineering as suitable as that which Physics had established for its students and others across the campus who required some facility in the art. The course was an outgrowth of the Advanced Laboratory. In the late fifties, what seemed an ideal approach was developed. Malmstadt and Encke at Illinois, in cooperation with Heathkit, Inc., brought out a laboratory text complete with suitable instrumentation as a package from which to learn and do electronics. Parrate, then in charge of the laboratory, agreed with me that it might be a good way to get started in the area. We could try it and see how it would go. We bought two set-ups and installed them in a back room of Rockefeller's third floor. They were popular enough that more set-ups were purchased and a separate electronics course scheduled. Professor Hywel White was the first appointed teacher in the new course; a text written by him resulted, one not widely adopted. It was then largely a matter of vacuum tubes; but transistors did make their appearance. It was not long, however, before the vacuum tube essentially disappeared. Too bad; there is some nice physics connected with vacuum tubes, but one can't have everything. In time, the single transistor as a device took a back seat, and the course essentially one in integrated circuits. Recently, the became microprocessor has put in its appearance. So rapid is the advance of electronics, one cannot predict where it will be a few years down the road. Engineering Physics made the course one of its required courses in

the four-year curriculum for its students and made arrangements with Physics to share in its operation, Physics being in charge during the fall and Engineering Physics in the spring. In some semesters, three sections have been necessary, one given in evenings. Students work in pairs, unlike the Advanced Laboratory, and there are fourteen completely instrumented bench set-ups, now quite beyond the original Heath instrumentation. It is serving an important function for experimentalists, whether physicists or not. The emphasis has always been on the applications and logic in device use, and not on the physics. One hopes that things will not become so sophisticated that those without previous knowledge of circuit elements cannot be included in the course to advantage.

For advanced graduate experimentalists, such as those who work in pulsed and counting electronics in high energy physics, Raphael Littauer developed, and wrote a rather successful text for, another electronics course, of very much higher level than that above. He also gave his talents in the development of the lower course, as did Arthur Kuckes of Engineering Physics.

Electronics was not the first Physics course in which Engineering Physics took part. Henri Sack introduced and taught for years the introductory course in solid state physics--old Physics 254--taken both by EP students and undergraduate physics majors and still a staple in Department offerings. Henri was a protégé of Peter Debye, coming to this country at about the same time that Debye did; he had worked with Debye previously in Europe. During the war, he was associated with the Physics Department. He taught in the V-12 program, and conducted research locally which was important to the war effort. He and Cuykendall worked closely with Smith in formulating plans for the new EP School, and the pair later collaborated in an interesting application of nuclear physics. They invented a logging system for water and oil (at least hydrogen rich deposits) by sensing the slow neutron scattering from soils.

There is an amusing incident in connection with a trip that Henri made for the Materials Science Center, of which he was to become director; or perhaps it was in connection with his own research. He had to go to New York. This must have been before the MSC, for he was to travel by train. It was a nice arrangement the Lehigh Valley had. A couple of sleepers were in the station and a person went down at bedtime, got on

and into his berth, upper or lower. The cars always had a nice Pullman smell about them and a fine name on the door--Scenic Ravine, Scenic Glade or some such. Around midnight, the train from Buffalo rolled in, a switch engine pulled the sleeper off the siding and backed it on to the end of the train. In due course, the train pulled out for points southeast. There was usually some commotion down around Bethlehem when the Philadelphia bound cars were detached and routed on down that way. But on this particular night, Henri was being shunted backwards and forwards, generally being bumped around for what seemed a good part of the night, a situation he found less than restful. At best it was none too good, but on this occasion it was well nigh impossible. Along about dawn, things seemed to settle down, and the train rolled steadily, well enough for a while. But along about sun-up, he was much taken aback to find, in raising the curtain at his berth, that he was rolling down West Hill into Ithaca! There had been a train wreck down south of Sayre somewhere, and they had spent the whole night routing themselves around in an attempt to circumvent the track blockage. To no avail; Henri spent a sleepy day in his Rockefeller Hall office, down near where Bedell used to hang out.

One of the early graduate students in Applied Physics was Robert Moog, a student of Sack. In the laboratory, Moog was frequently to be found electrically generating sounds in strange ways--not exactly the topic of his thesis; he later became well known for his synthesizer. He tells of the time he was delivering the first draft of his thesis to Henri; he had been given twelve hours to get it in--or else. En route to Henri's MSC office on the sixth floor of Clark Hall, Moog noted a tremor in the elevator ascent and, as was his wont, set to testing it by setting his own body into up and down motion in synchronism. Resonance worked as usual. The oscillatory component of travel increased surprisingly in amplitude and the conveyance came to an abrupt halt somewhere between one floor and another. And there Moog remained for about three hours until he was rescued. Sack thought that was pretty funny.

As alluded to at the beginning of this postwar story, the magnitude of physics, the shape and structure of the Department, its management and financing were all drastically altered. Before the war, there was the Department of Physics, small and fairly coherent, in spite of a diversity of research interests which were financed very much from university

funds supplemented by small grants from private sources. After the war, there is the Department of Physics and the Laboratory of Nuclear Studies, LNS, as a strong, expensive, Navy funded, semi-independent component tied firmly to the department. At the end of Corson's regime, to come years later, a second strong component will appear--the Laboratory of Atomic and Solid State Physics, LASSP, again semi-independent, funded by many individual grants, but tied as firmly to the department as LNS. There is danger in such bifurcation; will the components become so strong in themselves that they stand outside the department? Will the components have an interaction with one another? The danger was recognized at the outset when the first change in organization was affected, and again later. But happily the entire Cornell Physics community has remained a rather close-knit whole. It has been otherwise at some institutions. Here, while "solid staters" may not know the details of a complicated high energy experiment, a cheer nevertheless goes up when the first stored beam is achieved in the storage ring. Likewise, while a high energy physicist may not know the details of a super low energy experiment with discovery in liquid helium, a feeling of pride is felt throughout when Lee, Richardson, and Osheroff receive a prize for finding a new phase in He³. With little exception, it has been a harmonious arrangement with little of the internal squabbling which seems to be proverbial--in novels at least--in university affairs. A distinguishing feature of the department in my experience has been the overall harmony with which it has run now for forty years, with but few exceptions. That does not mean there may not have been pushing and pulling at one time or another; there undoubtedly has been that, but we are still pretty much one happy group of people.

The plethora of relatively young people coming into the department after the war, all near the same age, was not without problems thirty and forty years later. The age distribution of faculty members had this large bulge in it, moving linearly with time toward retirement age. At that point, one after another of a considerable number of staff all in a space of a relatively small number of years would be dropping out. Simply replacing them with another lot of young physicists only repeats the unfavorable distribution. Considerable attention would be paid to this aspect of department fortunes, particularly during the regimes of Littauer and Fitchen, far into the future. It was, however, long recognized that we

should try to smooth things out so that a steady state could be attained with a reasonably constant input of new, young, fired-up people added to the staff, concomitant with a reasonably constant departure of the elders.

And so the postwar Physics Department got off in its dual configuration--high energy physics on the one hand under Bacher, and the rest of physics, with teaching, under Smith as department chairman, Bethe serving as moderator. In support of the nuclear physics, President Day had made a large gamble and was prepared to resign if the trustees refused him his request, according to Morris Bishop. "The problem was not nuclear forces," Bishop in his <u>History</u> quotes him as saying, "the trouble is nuclear physicists." He committed the university to putting over one million dollars into nuclear physics, part for a new building and part for the first synchrotron.

Day was apparently really concerned about what the nuclear physicists were up to and whether indeed he had been wise in his support. Wilson relates how, after the synchrotron had been put to use, he was alone over in the Newman experimental hall one Sunday morning making some adjustments, or whatever, when he became aware of a presence looking over his shoulder. Turning around, he found President Day checking up and watching what he was doing; the president had wandered in for a look around during a morning stroll (in his era, the President still lived in the White Mansion, south of Rockefeller). It would be well if more presidents took to wandering around to see what their charges were up to.

There was a fair degree of grumbling on the part of the trustees about Day's large financial commitment, but his tactic paid off. The Navy came through with funds for the synchrotron, and alumnus Floyd Newman, big in Ashland Oil, came through with the building after plans of an earlier "donor" had fallen through. Experimental high energy physics and machine development have prospered here as they have in no other physics department. While this has come about through the dedication and hard work of the people working in the program, the driving force over many of the early years came through the man taking Bacher's place after Bacher was called to the AEC--namely, Robert Wilson. Starting work with Lawrence, he has been involved with machines all through his career, culminating of course in his directorship of Fermi Laboratory and the construction out there. He first made a name for himself in his study at

Berkeley of the focusing properties of the cyclotron, which comes about for the most part through the falling off of the magnetic field as a function of radius, the field configuration providing a force toward the median plane for the circulating ions.

An interesting sequence of events is attendant on this property of conventional cyclotrons, that the field falls off with radius. In the late thirties, Bethe and M. E. Rose, at that time a research associate working with Bethe, determined theoretically the limit to which cyclotrons could be pushed in terms of energy. As the particle gains energy, so also does its mass increase, with the result that the particle falls out of synchronism with the driving RF voltage. A limit was deduced which made the Berkeley people, including Wilson, rather unhappy and was the cause of some embarrassment for their future plans then in the making. A way out was later proposed by Thomas at Ohio State. One allows the magnet field to increase with radius; forget Wilson's focusing. But focusing there must be. Thomas proposed ridges spiraling out in the pole pieces. The average field increases to take care of the synchronism and the alternating field gradients provide beam focusing. This is of some interest historically. In the fifties Ernest Courant at Brookhaven, for a couple of years a research associate in our department, conceived the notion of "strong focusing." A beam of particles passing through a series of converging and diverging lenses can be held together. In fact, it was Cornell's second machine which first put the concept to test in an actual accelerator. It came to be realized that that is exactly what Thomas' focusing ridges amounted to.

Other means for beating the limit were also proposed. So far as Cornell is concerned, the most important suggestion was that made independently by Veksler and McMillan--the synchrotron--in which the magnetic field is ramped upward to keep pace with the energy and in which phase focusing plays a major role. This was to be the nature of future Cornell machines.

It was natural that the nuclear people here would consider Bob Wilson as the replacement for Bacher. He was known to nearly all of the staff, most of whom had worked with him at Berkeley, Princeton, or Los Alamos. He was thus approached for the position. Reluctant to leave Harvard, where he had been for less than two years, he turned us down. But his old associates here were not willing to take that as a proper

response. So Corson, an old friend from Wilson's Berkeley days, was dispatched to Cambridge to convince Wilson that he would fare better in Ithaca; Cornell was committed to a building and to the machine, things were hardly under way and he would have a major role in the developments which were to take place. It was a productive weekend for Corson, Cornell, and, it is hoped, Wilson.

Corson tells how, when he got over to Jefferson Laboratory, he found Wilson on his way to the stockroom; he would like to complete the errand before they sat down to talk. So the two of them went together to draw the stock that Wilson was after. To Wilson's chagrin and embarrassment, the stock keeper would not let him have a thing; only full-fledged staff members were allowed to draw things out. In spite of Wilson's protestation, he was in no way seen by the attendant as a full Harvard While Corson assured Wilson that such a thing would never professor. happen at Cornell, he clearly had more persuasive arguments. Wilson changed his mind and came. His influence in the high energy program, not only here but in high energy physics and machines generally, is well known and pervasive. With Wilson present, Smith still had a strong person with whom to contend, Bethe undoubtedly some moderating yet to perform.

A picture taken by Corson after Wilson had arrived, shows President Day, Wilson, Bethe, Smith, Long, and the University Secretary, all wrapped up against a chill wind, Bethe turning the first spade of earth for Newman Laboratory. The appearance of Frank Long, then chairman of Chemistry, is of interest. For a number of years, there was on the faculty a nuclear chemist as part of the overall nuclear program development. He held a joint appointment between Chemistry and Physics; hence Long's presence at the ground breaking. The presence of the University Secretary, representing the Board of Trustees, one assumes, is also of interest. As revealed in the files of the Federal Bureau of Investigation, made public in the late seventies, he had carefully kept the bureau apprised of the doings of J. R. Oppenheimer during his visit here for the Messenger Lectures he delivered in the spring of 1946.

The building was dedicated on October 7, 1948. The remarks made then by President Day are of interest. He told how at three o'clock on September 24, 1945, Professor Gibbs and four young physicists had presented him with a plan necessitating \$2.5-\$3 million and an operating

budget of \$250K. There was no ultimatum he pointed out; they just wanted to know what Cornell's intention was to be. They wanted to do nuclear physics and would go where it could be found to be in good health. They gave him thirty days to think it over, he said. Two weeks later, the trustees in meeting broached the problem; their considerations were serious and protracted; the funds were just not in hand. But they voted unanimously to go ahead. "It is my considered opinion," Day said in his dedicatory remarks, "that no decision of the Board of Trustees during my term of office has been of greater importance both to this institution and to the prospects of scientific research in this country." The fruits were by then in evidence: plans had been developed, an organization created; contribution from the Office of Naval Research was secured; as able a staff as anywhere, held and amplified; and a program of basic research actually under way. He ended by saluting those in charge.

Newman Laboratory I believe was the first building on campus to be air-conditioned. A letter from Wilson, supported by a memo of Charlie Baker's, defended the need, citing the ill effects of humidity on electronic apparatus. So the necessity was taken care of. In other ways, nuclear physicists must have been a trial to university officials. This must have been particularly true of the university auditor, Mr. Trousdale, as evidenced by a letter of complaint from him in the Day files. The nuclear people had run through their stock budget already early in the spring, a few months after the building dedication. And they wanted more at once. "They claim they need \$35K in stock," Trousdale wrote. He doubted that it was a necessity, thought "they could get along" on less than \$15K between April 15 and June 30. After all, they had received an appropriation of \$60K plus an additional \$8.6K. "They spent the entire \$68K by the middle of March," he fumed. "The rapidity of their expenditures, approximately \$8K per month, indicates that they considered the original appropriation merely a sum to use until exhausted, at which time they would ask for more." Like Oliver Twist, apparently. Vice-President Ted Wright got into the act. In a letter he details the points that Roger Knox and Paul Loveless ("manager" and stockman, respectively) must have made in defense of the request. Wright recommended giving them \$5K to replenish their stores to see them to the end of the fiscal year; they would have to be used sparingly. An inventory would be taken at the end of June, and then the

laboratory stores would be transferred to the University Electronic Stores but would still be located in Newman Laboratory as a substation of the general university electronic stores. Whether that was actually implemented is not indicated. The recommendation sounds a little rough on their progress.

But it was not only stockkeepers who kept administration off balance; so also did the Director. Early in Wilson's career here, the University acting president, Cornelius deKiewit, called a meeting of various department heads; all were ordered to bring their organization charts. Wilson said he would attend but no way would he bring in a chart; there was none. But deKiewit persisted and Bob finally agreed; OK, he The meeting came; various people went through their department would. operations with the aid of their charts. Came Wilson's turn he unveiled an object on the table which was his organization chart. Mounted on a neat pedestal, rising nicely from it, was a very heavy bare and braided copper cable (probably the synchrotron magnet conductor), one or two turns converging to a smaller radius as it went up, a pretty shape. Pivoted and hung from the top extremity was a balanced, shiny, steel rod, one side quite longer than the other. From the long side was hung a small vacuum tube representing the electronics shop, a milling cutter representing the main ship, and perhaps a typewriter ribbon for the secretarial staff. On the short side of the balanced arm was a hunk of lead, representing "the dead weight of the staff," as Wilson put it. The staff on the other hand, always presumed the lead represented the dead weight of the administration! The piece is preserved somewhere and was pictured on the front cover of the old "Circuit Manual" of the laboratory.

Wilson and others associated in the first machine construction assembled an organization of people necessary in such an endeavor, a minuscule body of workers compared to what was necessary in the construction three decades later of the 10 Gev machine and colliding beam storage ring. One of the quite remarkable people was the supervisor of their machine shop. He was a big, burly, rough-cut, non aristocratic type in spite of a name like Van Amber. But he had a way with him for scrounging machinery to get a shop established quickly, and for turning up odd bits and pieces of this and that which were frequently needed in construction of the machine and its auxiliaries. He was sorely missed

when he died suddenly, too long before his time. He ate too well. Besides being a great expediter, he was quite a storyteller. One which is remembered concerns his six year old son, Johnnie. Johnnie was starting school, out in Danby if I have it correctly. On his first day, Johnnie got hungry about ten-thirty and started eating his lunch. The teacher observed this early eater. Teacher: "Johnnie, we don't eat our lunch until noontime." Johnnie: "I don't give a God damn when you eat; I'm hungry." No little credit for help in the building of the first synchrotron goes to Van Amber.

(At this time, the department shop was under the supervision of another entrepreneurial character, also big and burly, a robust Irishman with curly red hair: jovial John Fitzgerald. He always seemed to be promoting some more or less wild enterprise; an example, the trout "farm" he had in his basement dug well out on West Hill somewhere. Most notable of his ventures, perhaps, was his activity in starting the Cornell Federal Credit Union. He was its first president and is said to have borrowed \$750 from his wife to make the first deposit in the union to get it on its way. Fitzgerald subsequently moved on, but his now very solid and much used financial establishment remains with us.)

The magnet coils for the synchrotron were wound locally under the supervision of an Electrical Engineering professor. After a long period of waiting, upon delivery it was found that one coil had one turn in excess, which was easy enough to remedy, but more seriously the thing was incorrectly wound, the turns not intertwined properly. Essentially useless. But McDaniel saved the day. Using a cylindrical oatmeal box and some colored yarns wound and woven around its outside, he saw how one could cut various turns of the delivered coil and braze them to other cut turns to give the desired configuration. And so it was done. The coil was cut at various places, the cut ends rearranged and brazed together successfully, and the finished coil installed and behaving as intended.

Great difficulty was experienced with injection of electrons in this machine. The injected beam was almost at zero energy, and so the magnetic field in its cycle had to be crossing zero, a match not easy to achieve, certainly in all the C-sections of magnets around the one meter diameter ring. In subsequent machines, actually in this first one later, the difficulty was appreciated and electrons were injected at

considerable energy, the field was DC biased, and so the match at injection became less critical. The high energy injected beam was also considerably stiffer, which is a great help. This machine, one of the first to be put into operation, was used primarily for gamma ray production which was then utilized in photo-production, high energy experiments. The pi meson-nucleon interaction was elucidated. As a side experiment, McDaniel and his student, John Weil (now vice-president for research at Bendix), measured the velocity of propagation of gamma rays of high energy. The expected value resulted. Another side experiment, mentioned earlier, was the collaboration with the other "half" of the Physics Department in the matter of synchrotron radiation.

Much was learned with this first machine, both in physics and in synchrotron technology. But there was all that physics lying out there beyond the reach of 300 MeV gamma rays. Wilson saw in the experimental hall how a much larger machine could be run around the walls, enabling attainment of an energy in the GeV range. A new style of magnet, of vacuum chamber, and of injection were envisagedand used: strong focusing could be tried, the first research machine to do so. This was a scheme conceived and analyzed by Courant and Snyder and our first accelerator physicist, Stan Livingston, whereby the magnets around the ring of the machine alternate in the gradient of the field, a strong focus in the horizontal alternating with a strong focus in the vertical, resulting in a much tighter beam with consequent decrease in the cross section of the vacuum chamber and thus of the magnet gaps and iron volume. It was a real success and is universally used today. In remarkably short order funds were obtained, again from the Navy, construction was started and completed in 1952, electrons of 1.3 Gev produced. The old synchrotron's days were not over, however; the magnet was used as the choke for charging the new magnet system. More physics was uncovered; higher order resonances in the pi meson-nucleon interaction were discovered. The machine was a great improvement, so much so that a decade later something larger was a natural to attempt. Openings could be cut in walls, adjoining rooms made use of, and, with the addition of a few more rooms on the periphery of the building as it then existed, an accelerator could be run around and through the old experimental hall which would make possible an energy of 2.3 GeV, approximately double that of the second machine. Again, the project was funded, and in 1962 the building was modified and construction completed successfully. With each step forward, both energy and reliability went up. I believe it was the 2.3 Gev machine that, when completed, simply refused to work. While it had been very carefully surveyed in, it was none-the-less resurveyed by DeWire and Wilson; the numbers all checked out. Staff members were all fighting the problem one afternoon when a student from Wilson's course wandered in to ask Bob about a bad grade, a point of physics, or whatever. Wilson told him to beat it--they were in trouble and busy. To Bob's annoyance, the student hung around however and finally hesitatingly asked why the space between magnets was about seven feet on one end of a particular magnet and only about three feet on the other end. Something like that. All the others were as they were intended at five feet! Wilson was delighted with the question but felt like crawling in a hole.

Throughout the period of this high energy experimental program, theory was not forgotten. Between his national, public assignments, Bethe continued working away, attracting a succession of visitors, research associates, and outstanding graduate students. Feynman made his great contribution, Morrison showed his talents. Before they left, E. E. Salpeter in 1949 joined the department in Nuclear Studies. While that was his formal connection, he was widely diverse in his interests, and high energy physics was probably not at the top of his priorities. With Bethe, but largely on his own, he rewrote the Bethe Handbuch article on the "Quantum Mechanics of One and Two Electron Atoms." He had great interest in astrophysical problems and would become an important associate of the new and enlarged Department of Astronomy after Thomas Gold took over and it began receiving support orders of magnitude greater than hitherto had been the case. The contributions of Salpeter throughout the realm of stellar astrophysics have been numerous and significant; One can hardly without fanfare, he has done an enormous amount. overestimate the importance of his work therein.

The research interests of those outside Nuclear Studies were more diverse than those of people associated with Newman Laboratory. It is true that some of the "nuclear" work was not directly related to the machine, i.e., the Q.E.D. of Feynman, the astrophysics of Salpeter and Morrison, the cosmic rays of Greisen, but the high energy machines have

provided a focus for much of the work in that side of the department. At the same time, while diverse, the work on the other side of the department could generally be categorized as solid state physics. Even the spectroscopy--X-ray, soft X-ray, vacuum-UV, and infrared--had ramifications directly related to solid state physics. Thermal conductivity, mass spectrometry, and, later, liquid helium were all connected with condensed matter, if not solids. Gartlein's auroral program was practically the only research in Rockefeller Hall that was in Smith's interest in the ammonia oscillator was another no way related. But the fact remains that the experimental techniques clear exception. and approaches were quite diverse. Theory was considerably less so. In the first years following the war, departmental theoretical interest in solid state physics was weak; that it must be strengthened was recognized. Bethe could be induced to consider a theoretical question in the area but clearly his interests were across the way; Smith was practically the only one with theoretical aptitude who was interested in solid state physics. This situation changed markedly when J. A. Krumhansl joined the faculty. He was a Cornell product, as was the case of so many in the department. Krumhansl did his graduate work under Smith during the war years, becoming part of a Cornell microwave project with Stromberg-Carlson in Cleveland. After a couple of years in the department at Brown, he came here in 1948. While his interests were broad, his theoretical inclinations were mostly in the direction of solid state physics. He represented a significant step up in our solid state theoretical capabilities; graduate students began working the field. Another big boost came soon after in 1953, when A. W. Overhauser from Illinois joined the faculty. In the same year came experimentalist Bradley The appointment of these "foreigners" (coincidentally, from Berkeley. Overhauser had been a war time shipmate of Al Silverman) was at last a break from the tradition; no longer would solid state appointments of Cornellians be made to the same extent as earlier. The nuclear people had already diverged from the path. With few exceptions since (Berkelman, Siemann, Tigner) appointments throughout the department have gone to people trained elsewhere.

Overhauser has had a distinguished career. He predicted the Overhauser effect before he came to Cornell--the polarization of nuclei by
the extremely high fields generated by the polarization of the extranuclear electrons with laboratory fields. He was a long time proponent of spin and charge density waves before their acceptance was universal. He left the department in 1959 to take a position at Ford Motor Company, at a much higher salary, which was a welcome prospect for him what with seven children to support and educate. He eventually wound up in the Physics Department at Purdue, where he has done some nice work, including a beautiful experiment mixing gravity with quantum mechanics, observing neutron interference by introducing a wave function phase shift through a change in the neutron gravitational potential energy. His departure was another grave loss to the whole department, only gradually overcome as other theorists came into the solid state activity.

With growing strength in theory, it is of course understandable that there came a steady stream of visitors, research associates, and visiting professors to the department, no less in theoretical solid state physics than in nuclear and high energy physics. That was already the situation in the experimental programs on both sides of the department.

For a number of years, experimental solid state physics was very much oriented toward ionic crystals, largely alkali halides. In some quarters we were facetiously known as the Institute of Alkali Halides. Electronic properties, optical properties, thermal conductivities, photoemission and photoconductivity, defect properties, all were studied by different investigators utilizing the techniques unique to the individual investigator, from Hall effect measurements to electron spin and nuclear magnetic resonance.

The power of the last named technique was brought home to some in the Department in a colloquium given by Felix Bloch on nuclear magnetic resonance. Near the end of his presentation, he showed in a slide the resonance record of ethanol in a high resolution, very uniform field spectrometer. There on the screen was the trace of three closely spaced peaks of amplitudes in the ratio of 3:2:1, characteristic of the hydrogens in the three alcohol components, CH₃, CH₂, OH, the H in each group being in slightly different molecular and magnetic environment than the others. This was in the spring of 1951; Bloch was on his way to the Washington meetings. Unlike circumstance in most years at this springtime event, Ithaca weather was apparently good; Corson's notebook indicates Bloch

left his overcoat at their house and it was returned to him in Washington. It was three years before the department took on its first nuclear magnetic resonance experimenter. In 1954, Donald F. Holcomb, fresh out of Slicter's laboratory at Illinois, came to Ithaca. It was obvious that the technique was to have a major impact in solid state physics and structural chemistry. In succession Holcomb was followed by R. H. Silsbee (1958) from Harvard by way of Oak Ridge, and R. M. Cotts (1958) from Berkeley by way of Stanford, experimentalists also in the same area and also from institutions leading in that sort of research. Primitive electromagnets had been used here previously in mass spectrometry, the Hall effect, and magnetoresistance measurements, but with the new tool, large, highly uniform field magnets became the norm and are today all over the laboratory. In time workers would feel the limitations of their 0-15 kilogauss fields provided by such magnets and would begin using the superconducting magnets developing as liquid helium and niobium wire became more common commodities, such magnets providing fields upwards of 100 kG.

The departmental teaching activities have been pretty evenly shared between the people in Nuclear Studies and those in solid state. The general physics courses were taught by members all across the department. Of course, specialized courses, such as nuclear physics, high energy phenomena, and the like, would be taught by people from Nuclear Studies, and such courses as solid state physics, optics, and physical electronics would be given by others. At some point in time, the physics for engineers came to be a four-semester sequence (now, three); Grantham handled the freshmen, and Tomboulian took care of the sophomores, Newhall participating with Grantham and finally taking that over when Grantham retired. Howe continued his involvement with the other non science majors until his own retirement, when the courses for those students went to assorted faculty members. The identification of particular courses with specific faculty gradually melted away as various members took their turns with them. The introductory engineering service courses of Grantham, Newhall, and Tomboulian came to involve professors across the department and, indeed, from Engineering Physics. In some instances, faculty were even drawn from other parts of engineering for participation in these courses as its own faculty became more science

oriented and trained. In recent years, this last practice has diminished to the point where it no longer occurs, except for EP faculty. Of course, a large troop of teaching assistants has been necessary to keep the service operations afloat. The broadening of the faculty involvement in these courses came for the most part during Parratt's chairmanship. But earlier, even Corson had Professor Bethe teaching in one or two freshman recitation sections. It is hoped those few lucky individuals were duly appreciative of their instruction. In the Advanced Laboratory course, the instruction has almost universally (some summer exceptions) been provided by experimentalists drawn from all across the board, including some research associates and members, again, of Applied and Engineering Physics.

By 1956, Smith had served two terms as department chairman. When the position and mode of selection (chosen by departments, approved by the dean) were invented by the university, the Arts College decreed that its chairmen should serve no more that two five-year terms. And there was no further position higher in the university open to Smith, this in spite of a new provost appointment having to be made. Thereupon he left Cornell. However it was, a new chairman had to be named. In almost everyone's mind, the man to take it on was Dale Corson; he had drive, a good outside reputation, had done good science, was easy going, and yet could make tough decisions. It was not a hard choice for the department to make, nor for Corson to accept. One feels he looked forward to it, in view of his subsequent moves, it is clear he was not averse to administration.

As far as this writer knows and experienced, it was a happy, if short tenure. Corson had not been in the job for a full three years before he was tapped for the deanship of the Engineering College, to succeed S. C. Hollister. After he took on that position, his connection with the department grew less and less. On the other hand, maybe it became otherwise; when he was made provost and later president, he perhaps had more to say about the growth of the department than he had as chairman. He has said that he did not know how well off he was in Physics, referring to the relative peace and harmony enjoyed in the Department. Be that as it may, his regime as chairman was a tranquil one. There was continued growth in both sides of the department, in graduate student enrollment

and in staff positions. University enrollment as a whole was increasing, resulting in the need for an increase in graduate assistants.

As it had at previous times during its history, the department again, about this time, looked at where it was going. There was, of course, nuclear and high energy work, from which there would be no departure; it was still a growing, exciting, and productive area. But on the other side of the department, there was growing feeling that preoccupation with alkali halides was too narrow an interest, important and fundamental though it was. Lengthy discussions were held by the "solid staters" on whether the research activities of the group should not be broadened. Plasma physics, biophysics, and low temperature physics were areas discussed. The result of the considerations was that a low temperature physicist from Yale, David Lee, was hired as an assistant professor to initiate a program in the liquid helium temperature domain. He recalls on his interview trip up here being asked by Overhauser just what was a liquid, and not handling it very well.

Liquid helium had for quite some time been used for refrigeration in solid state experimentation, the coolant being obtained from our own Collins helium liquefier. Researchers, right and left, had been building (and leak hunting) their own cryostats to handle liquid helium well before the decision was made to embark on a program in low temperature physics per se. Parenthetically, it may be noted that the high energy group had also been in the low temperature regime with their liquid hydrogen targets.

(Concurrent with Lee's arrival was that cf a visiting professor from England, James Cassels, come from across the water to work with our nuclear experimentalists. It was hoped that a tenured appointment would be made. But Ithaca had its drawbacks for him and there was opportunity in Britain, where he could play a major role in the to-be Daresbury machine; he went back after the one year.)

As Lee's program got under way, a helium shortage in the country developed, and an extensive recovery system was installed throughout the basement of Rockefeller Hall as a conservation measure. The supply of helium for science might dry up; it was being dissipated in aircraft heliarc welding operations. Today, somehow, there is no longer famine--it is all feast and there is not much recovery. And, like the old fashioned

milkman, we have today the helium man leaving his delivery of several large dewars at the loading dock once or twice a week. The cost is not prohibitive, liquid helium has even made its appearance in the Advanced Laboratory, but plans were being made in the early eighties to once again go into our own production of the important fluid and to employ recovery means.

So Dave Lee came to Cornell and started the continuing and very successful program in low temperature physics. A year or so later, he was joined by his Yale colleague, John Reppy, and subsequently they were joined by Robert Richardson from Duke, where he had done some nice work in nuclear magnetic resonance on the lighter helium isotope, He3. The laboratory in the south Clark basement burgeoned, appearing to an outsider as something of a nightmare of pipes, pumps, gauges, valves, platforms, and electronic apparatus. Richardson today claims the lowest temperature ever recorded in Tompkins County, in the tenths of a millidegree above the absolute zero. Work has to be carried on in rooms shielded against outside electrical interference from such as Ithaca's miserable radio stations, which could otherwise heat things up sufficiently to defeat laboratory purpose.

Notwithstanding this record, Richardson was not satisfied. As with the high energy physicists, there was all that unexplored very low energy region beyond where research had been. He managed in 1984 to get funded (NSF principally, various other contributors) a whole new laboratory. On the last day of November of that year, ground was broken (literally) alongside of Clark Hall for the facility--one to reach degrees microkelvin (a few such). The ceremony was preceded by Bob's taking a generous swig of liquid nitrogen and spitting it out onto the ground, followed by more generous application of the cold fluid, making the ground pretty impervious to attack by mere shovel. Provost Barker went at it with a sledge hammer with much flying of ice chips. By this means it was considered that construction could begin. The building, underground, represents the first real addition to Clark Hall.

Within a year and a half, the addition was done and cryostats and plumbing were largely in place. On a sunny morning of late Spring in 1986, Richardson was observed mowing the grass planted atop the ground level roof of the laboratory--another instance of a professor trimming

University turf around Clark Hall. But his activity had a little more meaning than the earlier cutting; a few hours later a small tent appeared on his greensward and that afternoon, with assembled guests and champagne, the laboratory was officially dedicated and opened for business.

Prior to this development, Richardson, Lee and graduate student Douglas Osheroff discovered discovered a superfluid phase in liquid helium three (flow of which has interesting magnetic anisotropy), for which they were honored with the Simon Medal award in 1978, and the Buckley Solid State Physics Prize in 1980. In May of 1981, Osheroff, then gone to Bell Labs, was listed as one of the twenty-one American geniuses to be awarded from \$24-60K annually for five years with no strings attached, a aift of recognition from the MacArthur Foundation of Chicago. (Another recipient the same year was a Cornell poet.) Very nice. Osheroff did his first low temperature work in the Advanced Laboratory course, doing a superconductivity experiment. For his part, Reppy has done some very beautiful work on the superfluidity of ordinary liquid helium below the lambda point and on quantized rotons, the entity conceived by Landau and worked over by Feynman. In 1981 Reppy received the London prize for his work. For the department to have gone into such research was a decision well taken and justified.

Lee tells of one of his low temperature graduate students of advanced tenure who took off for a Christmas skiing vacation in the middle of an exciting run they were making together, winding up the thesis work of the student. Not only did the fellow leave to let others finish up the experimentation, but he had the effrontery to ask Lee to take his dirty clothing and linen down to the laundry so that it would be ready for him on his return. Being excited about the experimental work, Lee was willing to labor on the man's thesis for him, but he drew the line at being his butler. The name of the character is known but to Lee and to God.

Lee is also an amusing colleague. He has an offbeat sense of humor, entertaining really more for the style than for the content. He seems always to be having some scrape or escapade, whether it be in his collapsing laboratory in the old Annex, getting knocked off his bicycle at night down on Thurston Avenue, being asked to be someone's valet, or simply spraining his ankle in some crazy fashion. Remembered the afternoon of the last mentioned mishap was Dave's late entry at colloquium, surprisingly on crutches. The speaker was well under way, the door to the auditorium ineptly slowly opened drawing everyone's attention, and a crutch appeared followed by Dave awkwardly managing to get through the obstruction. Knowing the cripple, it was both funny and not funny.

A discussion similar to the one above on future directions was also undertaken at a somewhat later time, perhaps 1968, among a smaller group of solid state professors, over whether an effort should not be mounted toward making use of the synchrotron radiation thrown away by the high energy people in their accelerator. Tomboulian had by that time died; otherwise things might have been different. He was consultant in making plans to work in conjunction with the Cambridge machine; he had spent a sabbatical semester up there, in fact, working with them on devising plans for such. For some reason he was apparently not thinking in terms of a Cornell operation. A group including Parratt, Chester, Ashcroft, Krumhansl, Wilkins, Batterman (of Applied Physics), and myself discussed a possible Cornell effort in the X-ray region of the spectrum. The considered judgment at the time was that it appeared not fruitful to The possibility of doing fluorescent scattering of X-rays was undertake. high on the considered agenda. Calculations by Batterman and Parratt indicated that the intensity was not there to make the effort worth the some hundreds of kilo-dollars that would be involved. I have always felt some guilt in not pushing some such project myself, but have too fully realized that I had neither the disposition nor the managerial ability to run an operation of that sort; it could clearly be no one-man, "sealing wax and string" operation. With the later storage ring, however, and its nearly constant, high density, and high energy bunch of electrons circulating around it, providing orders of magnitude higher radiation intensity, the prospect became much more attractive. That, and the fact that there was a person anxious to take on the responsibility, together with NSF money available on the basis of a proposal made in 1978, has resulted in the three-beam facility, CHESS, made part of CESR, of which more later. As of this writing, it is the highest energy terrestrial synchrotron X-ray (The use of "wigglers" muddles that simple statement.) It remains source. to be seen whether the research to be done on it won't prove to be as

exciting as that of the high energy people; it is surely more modest in scope, but elaborate enough for all of that.

But back to the 1950's. Corson was in the chair; science was still on the march; physics was looked on very favorably in funding. The department fell in step, but not to the extent of physics departments in With funding pretty easy to come by, it was some other institutions. natural that an institution should consider taking on staff and paying them on so called "soft money," that from government sources and not from the The proposition was debated in the Physics faculty and university till. was soundly rejected. Were funding to disappear, faculty could not be retained. Either that or the university would be over committed in keeping them. Where went tenure, among the other problems? There was worry enough about the government supporting the research; would it not be a sword that could be held over the head of the university in demanding that it follow this or that course of government action?

In a restricted and indirect way we have edged toward this dilemma. We have come to depend heavily--almost entirely today--on government support for our research. At the same time, increasingly, we have government regulations, on safety, on affirmative action, on aids for the handicapped, and the like, in degree immeasurably greater than in the days before the war. The regulations thus far are very well intentioned, and they have our support, however inconvenient they may be. The university tries to meet them, but what happens if a regulation comes along which is beyond our means to implement or, maliciously, is completely contrary to the university spirit and notion of free inquiry? We shall fight it we trust, but one hopes the day will not come when we cannot accept some strong government regulation.

Parratt indicates that a similar soul searching took place in the department at the time the first government sponsored research project was brought to the campus, back in 1941. He should know about it, for he was on leave from the department, and at NOL; NOL was seeking research and development assistance from the university. Should a university prostitute itself by taking on specific assignments that the government suggests? It did; a magnetic amplifier development was undertaken by Cornell with Henri Sack and Jim Krumhansl as the investigators. This and one in Electrical Engineering (Professor Burckmyer) were possibly the

very first such government-funded research contracts in academia. (Parratt says there were intense problems in the area of possible patents and in "no tangible guid pro guo.") Others obviously followed in profusion. The specificity of such contracts undertaken during this time could certainly be justified by the exigencies of war. Following the war. government funding continued, largely through the Office of Naval Research. One again worried about taking funds under such auspices, but it can be said that the Navy was very broad and liberal in what it did, a memorable national service guite out of line with its usual function. lt was basic research that was funded, research conceived by the investigator; witness our first synchrotron, to mention the largest item at Cornell. It was not until later, when ARPA's Materials Science Center was established here, that the mandate became somewhat restrictive. ARPA had interest in materials; research and development in the area was much to be desired. So that is restrictive. But since practically all of solid state research is related to materials, nearly everything we were doing in Rockefeller Hall passed as such. In spite of the National Science Foundation's tightening up when they took over funding of the MSC from ARPA, to the extent that they wanted the research to be "relevant," the Cornell research funded in part by the center is still very free. And the Cornell MSC is regarded as perhaps the most successful of the several centers that were set up. It does not appear that we have lost our soul. But it would be a very difficult situation were government support to drop away. Of course, the same may be said for almost any (except perhaps industrial) research oriented institution in the country, so we are not The subject was discussed at the time that the National alone in that. Science Foundation itself was established. Its first budget was a miserly few tens of millions, but the cloud on the horizon was there. Today, the budget is in the many hundreds of millions; but the cloud has not yet become a storm.

Near the end of Corson's regime the concern came much to the fore with the possibility of getting one of ARPA'S Materials Science Centers here at Cornell. Corson had learned of the interest that the Advanced Research Projects Agency had in the establishment around the country of some centers for materials research. He recalls taking Henri Sack and Bob Silsbee over to Vice-President for Research Ted Wright's office in Day

Hall to explain what was in the wind and to explore what Cornell might be able to offer the agency. Should not Cornell have one of them here? Much of the research in the basement of Rockefeller Hall would easily qualify as materials research. There was materials research in other places in the university--in Engineering, in Chemistry. The prospect of a new building was attractive; research space in Rockefeller had reached intolerable proportions. The aid such a center would provide our research was clear enough. It was decided that the university should draw up a proposal, citing Cornell's strengths and diversity, and incorporating protection of our various concerns. This was done very much under Henri Sack with the assistance of others from Physics, Chemistry and Engineering.

The lack of space in Rockefeller Hall was a problem that Corson sought to remedy beyond the prospect of a new MSC building. He arranged an evening tour of the facilities for some of the trustees. Various research groups put their wares on display, and the powers that be were provided maximum exposure to the least attractive aspects of the housing. A study had been instituted and some plans drawn up (again!) in a move to alleviate the problems. About the only thing accomplished by the effort was the refurbishment of Lecture Room B on the second floor. Its orientation was turned 90°, and it was considerably dressed up. The cost came to \$22,000.

Somewhere in time here, a visit was arranged for David Rockefeller in the hope of attracting a new gift from the family fortune. Professor Bowers, a relatively new faculty member but appropriately from the University of Chicago (via Westinghouse), entertained the visitor and showed him around his grandfather's now crowded gift. Nothing came of the effort.

The possibility of there being a Materials Science Center at Cornell was thus in the background when a successor for Corson had to be found. In the process, the department set up its second arm, which exists today alongside of Nuclear Studies. This was the creation in 1959 of the Laboratory of Atomic and Solid State Physics, LASSP. Unlike the situation when Corson had been named, there was no one in the department willing to assume the chairmanship burden, at least at that juncture and with the department organization as it then stood. Bob Wilson suggested that we

form a second research side to the department so that the chairman did not have that on his shoulders as well as the routine department matters, which were sufficient in themselves. The "Rockefeller group" held a number of meetings to thrash this around and decided that it would be a good idea, independent of any easement it might provide in finding a chairman, and independent of any possible materials center. A rather toodetailed story for us here of what transpired and what was thought about, was informally put down at the time by me, simply for the record. Copies are somewhere in the dean's office file and in our own department and LASSP files. (In the May 1985 symposium held at McDaniel's retirement from the LNS Directorship, Bacher gave a talk on the origins of that side of the Department--a much better and more interesting record than that put down for the origins of LASSP. It too is in the files somewhere.)

And so was born the Laboratory of Atomic and Solid State Physics, with a director, parallel to the organization in Nuclear Studies. The department chairman would look after the teaching end of things and promote new or old research falling not quite in the category of atomic or solid state physics (i.e., Gartlein's auroral work). The "Atomic" was in there because some spectroscopic research such as that of Tomboulian in the vacuum-UV and Parratt on X-rays, while it spilled over into condensed matter, was much concerned with the energy levels in atoms, and new research in the area was not to be discouraged. There would now be a quartet instead of a trio running things, Bethe still serving the function of Debye's "Holy Ghost," mediating if things got too tough between the directors and the chairman. After Bethe's retirement, in the seventies, this "post" has gone unfilled. Over the years, particularly during Parratt's first five, the bugs have gotten worked out, and the department has run smoothly, without hassle, without need of mediation. Much credit and sympathy goes to him for his patience in seeing us on the new course.

With this new arrangement, Lyman was willing to see what he could do in the new chairmanship. Professor Sproull, who was in Belgium on sabbatical leave at the time, was the choice for directorship of the "new" laboratory. It is to Sproull that the concept of central facilities in the MSC and much innovative design in Clark Hall is due. He had been consulted by phone a number of times, was somewhat worried about his own ability to maintain balance, whether he might not push research to

the detriment of the teaching function of the department. The importance of the latter is stressed more by some than by others. But the importance of research has been emphasized from the earliest days, certainly from Nichols' time on. There have been instances when the research accomplishments of a person and his interests in research seem to have far outweighed any interest or accomplishment in teaching. At some point in the university affairs, the concept of the <u>ad hoc</u> committee came into being to evaluate department recommendations for promotion and hiring at the associate professor (thus, tenure) level, so that in general, the teaching of the candidate is adequately weighed these days. Some balance is the goal. Overall, we have done all right in steering a middle course, and Sproull's own concern was groundless.

In the matter of promotions, the last word of course is given by the president, who signs all promotion tickets. There was one instance of a Physics Department promotion to an associate professorship that was almost withdrawn at the presidential level, in fact. Peter Stein was hired from MIT as part of the Nuclear Studies group of experimentalists. He was good at experiment and planning--and he was a good teacher. In due time he was up for consideration for tenure. He sailed on through the department's considerations, on through the ad hoc committee, and was approved by the Arts College dean. The president at this time was Deane Malott. He made it a practice to talk to those being advanced to tenure. So Peter went over to visit with the president. Malott got a bit of a shock. Peter had worked all night on the synchrotron, came over in red socks and old clothes, considerably disheveled. After preliminaries, Malott asked him to tell him what he was doing in high energy physics and why he was doing it. What's the purpose? Now Peter is if anything a frank person. He simply told the president that he wouldn't understand it. The president was clearly upset and vouchsafed the opinion that Peter should perhaps not be advanced to associate professorship. After the interview, Corson got a call from the president asking about Peter. Was he for real? Dale assured him of Peter's reality and Malott approved the appointment, and the professorship has worked out very well for both sides. Stein has taken an active role in the Nuclear Laboratory ever since and has been an exemplary teacher in his classes. At the same time he has been active in university faculty affairs, among other things serving as speaker of the

Council of Representatives, and a very able consultant to the faculty Committee on the Budget, which he also chaired for a year or so. It was during the campus revolution, when he became very exercised, that he made the decision to get involved with policy, to head toward administration. He is apparently making it, in 1981 being made a viceprovost. And he has about the widest range of wild anecdotes of any member of the department, with a repertoire and style of reporting which is reminiscent of Feynman.

While this is not to be a history of Engineering Physics, it is appropriate enough that some attention be paid it, closely allied as the school and the department have been. When Smith left Cornell, Cuykendall took on the EP directorship. Smith had served both as Physics chairman and school director from the time of inception of the EP, and he had outside consulting in addition. Nevertheless, in spite of the extent of his activity, the school, like the department, also prospered, and became an important adjunct to Physics, and a component in the Engineering College important to developments there. I think it can be fairly said that the EP School was instrumental in the upgrading experienced throughout the college. It would have perhaps happened in spite of the example of EP, but there is no gainsaying that curricula and emphasis in the College changed significantly over the two decades after EP came into being. It is not to be discounted, however, that Corson, who like myself had held a joint appointment in EP and in Physics, was Engineering dean concurrent with the development away from handbook practice. There was criticism of Corson from some of the older Engineering faculty for his stance in favor of science and fundamentals in Engineering education.

During the Engineering deanship of Corson, the Schools of Engineering Physics and of Materials Science and Engineering were combined under John Howe, from North American Aviation, as director. Research interests of the two groups were somewhat similar; what else was involved I don't know. In any event, the marriage was not good. After a few years, Howe went back to California, and the former EP School was reestablished as the School of Applied Physics. The undergraduate program remained under Trevor Cuykendall, but he now served under a new director, Norman Rostoker, who looked after the graduate side of the school and served as director of the new Plasma Laboratory. In due course

he too resigned and, like Howe, returned to the west coast from whence he had come. When I was asked to assume Cuykendall's function on the latter's retirement, I did so with the proviso that the Engineering Physics appear back in the school's name, believing that the undergraduate program was one in engineering <u>physics</u> rather than in <u>applied</u> physics--a difference there. Thus we have associated with Physics, the School of Applied and Engineering Physics. It has been a harmonious relationship. Graduate students in Applied Physics frequently do their theses under someone in Physics and vice versa; and there is a fair degree of student admixture at the undergraduate level.

In the course of the move of the Engineering College away from the north end of the Arts quadrangle, largely the work of Dean Hollister, it was urged by Hollister that Engineering Physics move its activities down to where the rest of Engineering was setting up shop at the south end of the campus. The EP faculty strongly opposed this, viewing the close association with Physics as necessary. While Physics may have seen EP as something of an interloper, the department has never suggested that the school be on its way to new quarters where the rest of the college is located around the Engineering quadrangle (now known as the Pew Quadrangle, the Corson designed sun dial at its focus). Today, the interaction of EP with the department, particularly with LASSP, continues close.

The interaction of the Physics Department with other science departments has not been negligible. From the earliest times, Chemistry has been close to Physics, which is not surprising given that the former essentially follows from the latter. Especially remembered in my time, with their interests and methods almost closer to Physics that to Chemistry, are Kirkwood, Debye, Hoard, Sienko, Bauer, Flory, Porter, Morrison, Widom, and Fisher, in near chronological order of my acquaintanceship. Fisher holds joint appointment with Physics and also with Mathematics. In Mathematics, there were Wally Hurwitz, Ralph Agnew, Paul Olum, and Mark Kac. They were frequently to be seen in Rockefeller Hall. The first three were not so much involved in Physics, but they alternated in giving a graduate course on the Differential Equations of Physics--old Math 80-81; they worked "simple" harmonic motion, the wave equation, the heat flow equation, and others up and down

until there was nothing more to be said of them. Kac, besides being a formidable mathematician, was a real physicist as well; he held a joint appointment with Engineering Physics until he left for Rockefeller University. These three were well known to a host of physics graduate students. In my time and until the sixties, a graduate student had a major subject and two minor subjects. The usual in physics was to major or minor in both theoretical or experimental physics and take the second minor in mathematics. Kac at one time was on twenty or thirty student special committees! A few students minored rather in astronomy as I had. That department has also been fairly close to physics from the time that Boothroyd moved his guarters over into Rockefeller from Lincoln Hall, where Civil Engineering lived in pre-World War II days. Shaw, who was essentially the Astronomy Department before the big expansion, had come from Physics. And today of course, astronomy is almost entirely just physics on a grand scale. Salpeter and Teukolsky both are closely allied with the two departments.

Corson had spent only three years as Physics Department chairman when others recognized his abilities and he went off to Engineering and thence on to the highest positions in the university. We had a new chairman in Lyman Parratt. Cornell's bid to the Advanced Research Projects Agency met with success; a Materials Science Center would be located on the campus to play a vital role in the fortunes of the department, particularly of the Laboratory of Atomic and Solid State Physics. The four-year first contract at \$6.1 million was signed in June An interdisciplinary character was manifest in the proposal of 1960. submitted; Chemistry and Engineering would also benefit. Bob Sproull was named the first director of the new center, relinquishing his LASSP directorship to Jim Krumhansl, recently back to Cornell after his years away at Carbide. With Sproull came Paul Leurgans from Oak Ridge as associate director of MSC, and with Krumhansl, Jack Rogers as administrative assistant in LASSP. (At one point in his career here, approaching Christmas it was, Leurgans sent a memo down to Bill Bement, proprietor of our Technology Operations Laboratory, then part of MSC's Central Facilities. The memo inquired of Bement how alcohol reacted with Bement hand-wrote a reply on his investigation. styrofoam cups. He assumed it was for an MSC party or so, and it would therefore not be

methanol in which the interest lay but rather, ethanol. Ethanol was something they had in stock and they'd been testing. And there followed technical details. Down the course of the response memo the writing got slimpsier and slimpsier, gradually deteriorating to an illegible scrawl, winding up in a pen track that wandered drunkenly off the page. And then, mustering full effort in large labored script: "Aw, hell, it's OK.")

In due course, during our Vietnam adventure, Sproull went on leave to ARPA in Washington, to come back a couple of years later as vice president for academic affairs, from which he was called a short time later to the University of Rochester, where he still holds the highest position. When Sproull left for Washington, Sack became the Director of MSC; a few months before his sudden death, he was succeeded by Chemistry's Bob Hughes, who was in turn succeeded by MS&E's Herb Johnson when the former went to the NSF to head a section down there. To fill out the chronology, in LASSP then we had a succession of directors, Holcomb, Chester, Silsbee, and Ashcroft.

With the new department arrangement getting off the ground, there was a fair amount of controversy among the chairman, Parratt, the LASSP directors, Sproull and Krumhansl, and the LNS director, Wilson. There was misunderstanding over roles and range of authority, lack of clarity in the laying down of the ground rules. Bethe had some moderating to do, we presume. They were strong individuals who were involved; else why would they be in the positions? Poor Parratt; unhappy at times was he with his position and with what he felt he had to contend, that it appeared foregone to the writer and others that he would be very much relieved when the first term was finished. It was a great surprise when the department learned that he would do it again, but only under condition. He has told how, in the spring of 1964, as the end of his first term was approaching, he expressed to the other three members of the Policy Committee (our "guartet" it had become) his wish not to be considered for a second term as chairman. His own research program had degenerated into a shambles during the five years just passed; with eight more years until his retirement, the program might be resuscitated; not so, however, if he were saddled for another five years with the chairmanship. Further, he was indeed weary of trying to diagnose and solve administrative problems before they became acute; he felt he had not done very well in this during

the term about to expire. He had accepted that five-year term only under pressure of near unanimous support of the faculty. He insisted that he would not be pressured away from his desire now without a mutual understanding, among the entire faculty and the dean of the college, of explicit responsibilities incident to the chairmanship and without essentially a unanimous supporting vote of the faculty, a vote he considered quite unlikely this time around. There he stood.

The department and the dean were more than happy with the conditional development. There was agreement on procedures which would be followed in administering department-laboratories affairs, and a convincing vote of support was taken to persuade Parratt to take on the second term. So Parratt was relegated to another five years in the front office. For all concerned it was a far more comfortable five years than the difficult first five had been, during which the "bugs" were being worked out of the system. The firm statement of responsibilities undoubtedly helped. Smith probably also experienced similar frustration in his first five years as chairman, following the first department rearrangement.

Many changes occurred during Parratt's regime. Not only was there a new organization in the department, there was a new building with the MSC. There was the student revolution, in which graduate students as well as undergraduates were not inactive or professors passive. The role of chairman was not noticeably lessened with the new department format. This can probably be said of every chairman in turn. Even by the end of Smith's tenure, it had been necessary to appoint an administrative assistant, and more secretaries appeared.

The first department administrative assistant was an interesting character, one AI Johnson. He had become independently wealthy in a Nebraska grain milling business, and decided somewhere in his early forties to come with his family to Cornell and learn physics. He came to Cornell; it is not clear he learned physics. In fact, he found it too much, certainly at his age. He reported at one time a great difference between what he had done in Nebraska and what he found in Rockefeller Hall. "In the old days," he said, "when things got rough, I merely put additional men on the job." He gave up on graduate study and was seen as an admirable administrative assistant; he knew some physics and the demands it made;

he had made his fortune and thus knew how to run a show, at least in milling; he had a jovial, good nature and could get along with people. After Corson left, Al's services were shared by Parratt and Sproull; this did not work out so well. He eventually left to oversee construction of the great Cornell radio telescope in Puerto Rico. He was quite an entrepreneur; he established here, at Mt. Holyoke, and Rochester, stores featuring elegant furniture and housewares. Contemporary Trends in Ithaca is still a fine store operated by his son-in-law. Al himself died some years ago. When he left the department, his value was seen; he was followed by others, finally by Ben Tipi. Ben retired at the beginning of Fitchen's tenure, and the position remained vacant for an extended period., But the need was there; a woman, Linda Hooper, was hired to fill it, but she too resigned after three years, succeeded by Jane Pedersen. Permanence seems not to be an attribute which goes with the position. It has been otherwise in LASSP and LNS; Jack Rogers and Roger Knox each held the corresponding position in their respective laboratories for many years.

Concomitant with a Materials Science Center was the acquisition of a new building. Clark Hall was the result, something of an improvement over Rockefeller Hall, and as great a boon to the department in 1965, when we moved in, as Rockefeller Hall had been sixty years earlier. Incidentally, it is the first building on the campus to have its upkeep endowed. It is very practical in plan, rather antiseptic but very functional, esthetically pleasing on the exterior up to the third floor, in the writer's opinion. Above that, the box like appearance is a little less attractive. Connecting two such dissimilar structures as Baker Laboratory and Rockefeller Hall, it could perhaps not be otherwise.

The building is named for the van Allen Clarks, who came into their wealth via Avon cosmetics. Clark and his wife have been strong supporters of Cornell; the Science Library in Clark Hall is named for her. The breezeway inscription informs:

> CLARK HALL OF SCIENCE Named in honor of Edna McConnell Clark and W. van Allen Clark '09 whose generosity advances

man's understanding of the physical world

A clue to the other financial means by which the project was put together is conveyed by two stainless steel plaques beneath, one telling of the role in 1965 of the Dormitory Authority of the State of New York. The other, alongside, removed and stolen (but recently replaced) in a noontime march through the breezeway during the turbulent student revolution, had this:

> The Advanced Research Projects Agency of the Department of Defense was essential in the genesis of this building, has provided financial assistance for its development and has strongly supported the research being undertaken in it. This generous assistance is gratefully acknowledged.

And so we came into modern, satisfactory housing for a large portion of the Physics community.

Plans for construction went out to bid early in October of 1962: 376 sheets, each two by three feet in a pile two feet four inches in height. Three years later we were in and the building was dedicated. It was originally envisioned as an eight-story building, but in the planning, sights were lowered. In fact, the seventh floor was an alternate design on the building bid plans; if the bids came in too high we would settle for six stories and no fancy business on the top story. Fortunately the bids came in with seventh story feasibility, and we have our fine facilities there which have proven to be most valuable assets. (Problems in the design of the building have been dealt with extensively in the December 1965 <u>Cornell Engineer</u> by Jack Rogers, long time manager of operations in LASSP. Copies of this and a record he put together of the financing details and a short history of LASSP formation are to be found in the LASSP files.)

During the Clark construction, it was "business as usual," more or less. The old machine shop had to be torn down along with Bedell's old dynamo lab. To accommodate the former (no attempt was made to save the latter), the courtyard back of Rockefeller, used for parking and deliveries, was covered over with a roof, a concrete floor laid down, and

the machine tools and personnel moved out there, where they stayed for a couple of years, until it could all be moved into the new quarters in Clark. Parking during the period was something of a problem, but at least there were no cars stove in by avalanches off the roof above.

There was a near disaster during the period of preparation for construction. Excavating was going on out near the ancient, stonewalled Annex in back, when one of the walls started giving way. The low temperature, helium work of Dave Lee had been moved into the old structure when that program was initiated and there stood his facility, ready now to be demolished. It was an anxious day or so until the equipment was all moved out. Lee set up shop down in Bard Hall and remained there until he could move back into his new laboratory in Clark. The overall excavation and drilling operations for 235 caissons were impressive and noisy indeed. Each of these, three feet in diameter, was drilled one foot into bedrock and filled with concrete and structural steel to provide a firm structure with minimum vibration problems.

It seems proverbial that experimenters wish to have their apparatus on solid footing and as close to ground as feasible. It was so in the planning of Rockefeller and so it was in Clark. Thus, nearly all the research areas are in the basement, with its perimeter corridor nearly a one quarter mile run, a fact utilized on occasion by athletically inclined experimenters. While the stability and vibration characteristics are perfectly adequate for athletics, it is not necessarily so for sensitive physics apparatus; the basement floor is still one floor above the subbasement at ground level. There is no research down there, however, and the basement has proven more than adequate up to now, thanks to a rigid frame and the extensive underpinning to bedrock. Some research areas are on the first floor, north of the breezeway passage through the building, where old Reservoir Avenue used to wind up the slope toward and around Bailey Hall. On either side of the old road were those two magnificent oak trees, widespread, hugely trunked. Construction of the building necessitated cutting them down, which occasioned some caustic comment in letters to the Sun. There was a third fine oak further east which was saved and enclosed around with a low stone wall, an imposing feature of the east side of the breezeway. Unfortunately, all the paving, fill, heating pipes, and general disturbance of the building brought about its demise a

few years after our occupancy. It has since been replaced with a fine young oak, "Dedicated to Dale R. Corson." It thrives, in two hundred years it too may be magnificent.

And while on matters arboreal, mention might be made of the lovely horse chestnut tree in front of Rockefeller Hall. One late summer day about a decade ago, some bacteriologists passing by the tree picked up, as is their wont, some of the polished fruit lying about on the ground, put them in a blender and found that bacteria just loved the resulting soup, a really superior brew. For some years thereafter the fruit was harvested with a "cherry picker" crane each fall. It seems that the nearest tree of that particular species lies some 300 miles to the west; our juice made the best culture medium around here.

On the south side of the Clark Hall breezeway are the Physics Department offices, a class room or two for Astronomy, a lounge, and small conference rooms. Engineering Physics offices are ensconced above on the second floor, and a very attractive and still very adequate Science Library is on the north side of the floor. On the third and fourth floors are housed student instructional laboratories. Offices of professors and graduate theorists in LASSP are on the fifth floor, more on the sixth where also are located the offices of the Materials Science Center and of the Center for the Study of Science, Technology, and Society, STS. The seventh floor is made up of a good (except for acoustics) auditorium, now named for Hans Bethe, and two nice seminar rooms, expandable and usually used as one, from which one of the finest views on the campus is to be had. Here are held the regular department Monday lunches.

From the second floor corridor across the front of the building, a continuous stretch of glass window as seen from the outside, one looks down on an open court space connected with the plaza below with a set of stairs and some planting. In the open space itself is a very large planter with lawn and three flowering crab apple trees--a pretty vista, especially in the spring. The planter was the scene of a small agricultural experiment, only marginally successful. During the early summer before dedication, the planter had been filled in with top soil and there it sat. Returning from lunch one day, DeWire suggested it would be good for a planting of corn. An admirable idea. That evening my wife and I returned with some seed and proceeded forthwith to test the proposition. It was

not sown in very orderly rows; in fact, it was more or less a single row just inside the low stone wall around the area. We even returned on a few evenings during the summer drought to pour a little water on each struggling plant. But they came through, and there is a picture or two around showing a rather nice, green stand of corn. The harvest was not prolific; I seem to recall taking only a few small ears home. The soil must not have been all that great; either that or passersby more anxious than myself took the crop. There were those, I am told, who were not amused by the experiment. As some recompense to those spoilsports, I may add the footnote that after lawn was properly sown in the space, I came over with my own lawn mower and cut the first grass myself when it was let grow to hayfield length. I also prefer looking at well manicured greensward, but there is something to be said for a cash crop.

Above Clark Hall's seventh floor there are machinery rooms, north and south, and there was almost an observatory. Between the two machinery enclosures, to provide for an attractive exterior skyline, there is a thin facing on east and west sides. Thus there is a small enclosed court resting on the ceiling of the auditorium below. When some funds became available from a Sloan grant to Lou Hand, the surplus of which could be used for teaching purposes, a Celestron 8" folded telescope was purchased for the Advanced Laboratory. It was thought to place it in the middle of the little rooftop court. The walls around served nicely to shield the site from campus lights and wind. Ordered with that telescope was also a Schmidt attachment to make possible some photography. It was hoped, perhaps optimistically, that an astronomical experiment or two might be made a part of the laboratory course. The telescope came with both a portable tripod mount and with a permanent pier to be tied to the roof structure. Some viewing was done with it on the tripod, weather being kept out with a canvas cover. The seeing was not all that great, what with ventilator drafts stirring the air; and the floor was not all that Preparatory to mounting it permanently, the instrument was rigid. brought down to the third-floor laboratory, near the end of the spring semester so that something could be constructed on the roof during the summer, a roll-away shed or something similar. Unfortunately, right at the time of the student exodus from campus, some parties gained access to the laboratory at night and removed the whole works, Schmidt attachment included, which was in a separate box off in the corner of the laboratory--clearly the work of someone knowledgeable of what was up there. That was not the only astronomical theft from the laboratory. A 12" mirror which I made and loaned for use in a student optics experiment on the Foucault test, was removed during a sabbatical leave. My insurance covered it and it was replaced, but nothing covered the telescope and no word has yet been heard of it.

Clark Hall is fireproof. This may be determined from the following personal experience. It was a nice late spring afternoon, and I had about decided to go home when duty overcame pleasure and it seemed wiser to go up to my fifth-floor office, catch the late sun streaming in, and read a student notebook or two turned in from the laboratory course. But first a cup of coffee. I filled the old electric pot I had found in cleaning out the debris of Tomboulian's loaded storage room after his death. So I plugged it in and started on a notebook. As I sat there before dozing off, Silsbee passed the open door and asked if I wasn't going to the university faculty meeting; in fact, I had wanted to do just that, since Trevor Cuykendall was to be recognized in retirement. So off we went. It was a short and pleasant meeting, hardly disturbed by the sound of sirens heading for a blaze somewhere. Meeting adjourned and we wandered back toward Clark. A crowd outside the building was observed and my first reaction was that Newman Lab had done it again; they were always having false alarms over there. But as we drew closer it was clear that everyone was standing around outside of Clark. So we joined the crowd wondering with the others what was afoot. It was only after a minute or so that I noticed the only window open on the entire south side was one on the fifth floor. Oh, oh. I decided I had better dash into the conflagration to assist and explain. The ceiling sensor had worked. My coffee pot had boiled dry, went on to excited state and set afire a stacked pile of papers--a complete set of the Cornell Chronicle--that was too close to the coffee pot, or vice versa. There was ash all over the room; it was a real blaze. Fire officials were satisfied that it was unintentional and let me go; I came back that night to clean up the mess. Books and papers still show a flake of ash now and then to testify that it really happened. That I also seem to have sponsored the one flood in the Advanced Laboratory would seem to indicate that I have it in for the building. I really don't; I am quite happy in it.

Rockefeller Hall was the structure that could be threatened by fire. The protective sprinkler system throughout the building was not added until the early thirties. There were electrically operated indicators on the first floor showing on what floor the presumed blaze was taking place when the alarm sounded. It was mostly malfunction, occasionally not. There was the night of the lightning strike previously mentioned. And there was a real fire in the third floor Advanced Laboratory. At least it wasn't a fire of my doing; I had left for New York and Bell Labs. May 19, That's near term's end; some student must have been working 1941. overtime and have fallen asleep at the switch. It was in the spectroscopy area I believe. Yes, a Schuler lamp was broken (glass blowing repair: 5 hours @ \$1.50, \$7.50), and a Hyvac pump got filled with water (not from the sprinkler system certainly). We still have the experiment, on the Rydberg constant variation with nuclear mass. 4 doz. photographic plates spoiled--\$3.75. They are almost that much apiece these days. And in the basement (!) Room 14, water damage, 2 gallons transformer oil "rendered useless by admixture of water" @40 cents. Total cost came to \$31.45 in the bill sent by Gibbs to Treasurer Rogalsky asking for credit. The postscript to his letter indicates that Buildings and Properties must have been a trial in normal circumstances but that they had come through handsomely this time: "P.S. I wanted to say that all repairs to the building (carpenter work, painting, etc.) made necessary by this fire have been very promptly and satisfactorily executed. Please hand on any compliment that may be in order in this connection to whoever was responsible for attending to this, but please don't run the risk of shocking anybody. RCG"

The occasion of the Clark Hall dedication was festive. On the previous day, Buildings and Properties had rolled out (literally) a carpet of lawn and had planted shrubs and trees in the area bordering the front plaza, to give it all a quite settled look. It was a glorious 1965 fall day, in some contrast to the days preceding and following. The exercises were held out of doors. There were brief remarks by Mr. Clark, President Perkins, Professors Sack and Sproull; Professor Bethe made the address of the day--essentially a brief history of the department. His text is still in department files.

I remember there was a bright daytime comet in the sky at the time. While there were a number in the department who kept track of what was going on up there, not many saw this one, a so-called sungrazer, Comet Ikeya-Seki, it was too close to the sun. With my backyard telescope on the morning of our dedication, I managed it, in consequence being almost late for the proceedings. There was another one earlier, more noteworthy, at least to Ithaca observers. We were singularly unsuccessful at finding it until one morning Bob Cotts reported seeing it from his bedroom window when he had arisen to attend an ill child. So it was the next morning we were out before daybreak. Spectacular and beautiful. For a few mornings thereafter it presented a beautiful sight. Comet Seyki-Lines, I believe, 1962.

Solar eclipses have always drawn a crowd. That of 1925, for which the path of totality included Ithaca, has been cited: the Department must have shut down that day. There was the noteworthy meeting of the American Physical Society in June of 1954 at Minneapolis, which featured a total solar eclipse one morning just after sunrise. A lot of physicists, including theorists, got up early that day. My first experience with one came in 1963 in Maine. Corson took it in with me. We barely made it; five minutes after totality was over, it was pouring down rain. But it was beautiful. The best, however, was one in North Carolina seven years later in early March. A number of Cornellians headed south for it. It was winter in Ithaca but a fine Spring day in a country Baptist church yard in North Carolina, where Corson, DeWire, I, and other visitors set up cameras. Midway toward totality, it appeared we were to be wiped out. Three or four car loads of vigilantes roared up to protect the property. But they were mollified when they saw what we were up to and what was going on above them.

Of other things in the sky, aurora drew the attention of several in the department besides Gartlein. We had an informal network set up; if someone saw something interesting shaping up, he alerted others, so that among us all, a number saw many fine displays.

There was one other semicelestial event of more significance to physics and more worthy of mention. It resulted in a surge forward in science education. Sputnik. The startling surprise announcement from Russia that it had orbited a satellite was something like a thunderclap out

of the blue. It came on a Friday. Friday night was cloudy in the northeast, and it was not observed in the region. Hagstrum, Bell Labs friend and collaborator, and his wife were up to spend the weekend with The Hartmans. I told him that I hoped I would not disturb him but, in spite of my previous skepticism about rocket expenditures, I was going to get up before dawn on Sunday morning to see if I could observe it going over at the announced time. He wasn't about to let me go alone. So we were up and went out into the predawn chill for the search. It was still a beautiful night, the stars in their appointed places. All but one. We hadn't been out there two minutes before I noticed this brighter star in the north sky, not fixed, moving southeast. "There she comes!" It was a pretty exciting five or ten minutes until it disappeared off toward New York and beyond, beep-beep-beeping away, if one had the right radio receiver. Later that week there was a group from physics assembled up at Corson's house on South Hill at a similar hour before night had gone. There were cameras on tripods mounted all over the place in hopes of capturing the trail on film. Besides the Ithaca Journal photographer, I think Corson was the only one who got something of interest, but I don't believe the orbit was ever worked out. It was pretty well known where it was anyway.

It was during Parratt's second term that Cornell had its student revolution; no sector of the university was untouched. On the drizzly Saturday morning of Parents' Weekend in the spring of 1969, black students, in protest over reprimands issued to three of their number, took over and occupied Willard Straight Hall, putting out employees and shepherding weekending parents from their rooms on the upper floors. Tension ran high; threat of invasion by white students was rumored: arms were smuggled into the building. By the next afternoon, a sunny occasion otherwise, a truce had been arranged and the blacks marched from the building carrying their rifles, creating a picture carried in newspapers across the country. There had been racial incidents, a cross burning, a fire in the Black Center on Wait Avenue, another in Annabel Taylor Chapel, Martin Luther King, Jr., had been assassinated. In consequence of the last event, there was a memorial service in a filled Bailey Hall at which for the first time I heard four letter obscenities shouted angrily from a public There were large faculty meetings, one seemingly after platform. another, large enough that Bailey Hall was required. The faculty

disavowed the treaty by which Willard Straight had been liberated. The dean of the faculty resigned. The faculty met to urge recision of its action two days earlier. The university was "to burn" if it did not. It is all spelled out in Professors Strout and Grossvogel's <u>Divided We Stand</u>. And then in a few years came the Vietnam escalation and more unrest. It seemed there was new protest every spring.

It was not a pleasant time. Clark Hall became a target. Missing for years from the wall in the breezeway was the stainless steel plaque testifying that the building owed its existence in part to ARPA. A student march one noon came through and ripped it off. There were one or two nights when faculty members patrolled both the building and Rockefeller Hall to prevent possible arson (at least in Rockefeller) or other acts of sabotage. One or two incidents on campus were of that nature and more were threatened.

Students and staff alike were caught up in the strife. There were almost nightly meetings of student groups, particularly one known as Students for Democratic Society, SDS; it had "chapters" on many campuses. Sprinkled through the largely student audience would generally be a few Physics professors attempting to follow what was going on. It was not the easiest discourse. Speeches were mostly tirades, every other sentence punctuated by "ya know." There were three or four geheimrats running things and doing most of the talking. And at other times there was confrontation: a street barricade down by the Law School; the great occupation of Barton Hall, which brought the university to a standstill; meetings in Bailey Hall of "concerned faculty, of students with trustees; buildings occupied, Day Hall then and during the Vietnam period, Carpenter during the latter. Physics faculty were hardly immune from the events, but were themselves somewhat divergent in views and participation.

Physics graduate students shared in the protests. Out of it all, some good things accrued to the department (and to the university). Graduate students were invited to become involved in our affairs to an extent greater than previously. Students were appointed to some department committees: colloquium, admissions, lectureships. Advice from them was sought on promotions, their reactions changing the department course in one such. There was some trepidation at instituting student participation

in such activities, but it may be said to have been a very beneficial venture.

Probably most new interaction came in the weekly lunch which Smith had instituted and Parratt carried on. Originally, the lunches, for faculty, were over in a Willard Straight dining room, formally served up in style. This mode was continued after we left the Straight and went to the lower level of what is now the Pancake House overlooking Beebe Lake. The view was better, but the dining time was as lengthy as it had been at the Straight. With the advent of Clark Hall and its kitchen on the seventh floor adjoining the connected seminar rooms overlooking the town, lake and inlet valley, an ideal solution was at hand. And now with the revolution, graduate students were invited and encouraged to join the faculty in these weekly lunches -- a put-it-together-yourself sandwich and apple affair, which it still is. For serious matters we still have irregularly scheduled faculty meetings, any minutes of previous meetings no longer read or approved. But the Monday lunch has taken over the trivia the earlier faculty meetings handled, i.e., the vending-machine-in-thebasement sort. Announcements are made, visitors introduced, and things of general interest considered. Votes are never taken (opinion polling on this or that excepted); anyone can bring up any subject or make what comments one wants; nothing is "on the record." In his introductory remarks each week, presumably to stimulate discussion, Parratt took the trivia a bit far; McDaniel told him once that he wished he'd stop reading to us his third class mail. The lunch remains a worthwhile get-together for both students and faculty, and you can't beat the price tag, although it is not what it was in Parratt's time. The lunch is by now a tradition that should be maintained.

Following the "revolution," there was set up a university "Constituent Assembly" to draft a charter for establishing a joint Student-Faculty-Employee Senate. There were a number of Physics faculty who spent long hours in that ordeal: Wilkins, Chester, Stein, Mermin, perhaps others. A Senate Charter was ratified, senators elected and installed. Physics faculty was always represented in that body by two or three senators. Bethe was a charter member of the Senate, served on a committee studying prison reform(!), chaired and worked very diligently on one dealing with financial aid. That was a good committee; he gave a

report on its work which was well received by the entire community. Well remembered was one Senate meeting lasting until about two in the morning; Bethe's wife, Rose, had called mine sometime after midnight to inquire as to our fates. It was a Physics graduate student who sponsored a piece of legislation which passed but did much to discredit the organization, provoking the ire of many people: Cornell would serve only union lettuce on campus in support of the Farm Workers' Union.

The Senate finally died for lack of interest and of responsibility. All those long hours for naught. Or were they? The body did provide a safety valve of sorts and it was not bad for some faculty to have interaction with each other and with students in such activity. I got acquainted with a number of faculty members and students that I never would have known otherwise.

Parratt took his chairman job perhaps more seriously than any of his predecessors. What with the new department arrangement, it is little wonder that he found it at times frustrating and perplexing. He is a man who has never done things by halves, be it handball, tennis, physics, finance, or whatever. Days and nights found him in his office. His research passed on to research associates and eventually died. While his interest remained, his laboratory days were ended. But he is still called on for invited papers, for consultation as an authority in X-ray physics (Cornell research in which is now done by others, at CHESS with synchrotron "light"). In tribute and recognition of his contributions, he was called on for the toast to the emperor at the formal banquet in Japan of the 1978 International Conference on X-Ray and Atomic Inner-Shell Physics, on the honorary six member 1982 Conference Board of which, he and his old student, Leonard Jossem serve. The toast, not exactly according to Hoyle, was given in English, ending, however in the traditional, loudly shouted huzzah--"COMPAI!!!"

Unlike the days of his National Research Fellowship, when Parratt had little time for students, the time of his tenure as a member of the faculty revealed a deep interest and commitment to both undergraduate and graduate teaching. On undergraduate physics education, he came over the years to the view that the design and teaching of courses had become too analytical and not enough connected to our culture. He lobbied for years, encouraging prestigious departments elsewhere to offer courses

not oriented solely for potential research physicists. But because the research mission was so strong in such pace-setting departments, because "in-bred" faculty in those places were busy doing what they were doing, these efforts were not very successful. Shortly after stepping down from his chairmanship, he recommended to the Cornell department that our own undergraduate major program be modified to include a second track, one including more "culture," by replacing some of the advanced analytical courses with appropriately selected and approved courses offered in other departments. The faculty discussed it, bought it, and implemented it. We still have and favor our two options, A and B, for undergraduate physicists.

At a different level of physics education, he invented a summer program for gifted high school students--Adventures in Physics--which ran for several years here and is now elsewhere under NSF funding. He was a firm believer in laboratory work as part of the physicist's education; he had run the Advanced Laboratory course before he took on the Chairmanship. It was not run better thereafter. He had written a book on probability; for a physicist, it is a useful and comprehensive text; the mathematicians found some fault with it because he made a distinction between classical (mathematical) probability and scientific probability.

The department continued its growth throughout the period. In 1960, Corson having deserted me, a visiting assistant professor, on leave from Westinghouse Research Laboratories, came to the department sharing my British, with a capital B, he got his degree at Clarendon office. Laboratory, and had been a postdoctoral fellow at Chicago. Ray Bowers had done his work in metal physics at low temperatures. He spoke elegantly, frequently with caustic turn of phrase. His first general colloquium was a beautiful performance, in organization, scope and delivery; the Department decided almost forthwith to offer him a permanent position, which he accepted. He established here a laboratory in metal physics from which came a lot of significant work. With one of his graduate students, Frank Rose, he discovered an important metals phenomenon they identified as a helicon wave manifestation, counterpart of the "whistler" in ionospheric physics. Bowers cut a fairly large figure in campus affairs, serving on several important committees, the best known of which was one he co-chaired with Dean-to-be Fred Kahn (later

CAB Administrator and President Carter's "inflation fighter") on undergraduate education. From that study came a lengthy report, widely distributed, here and elsewhere. He gradually became more interested in the implications of science to society, serving during 1966-67 in the Executive Office of the U.S. President on the staff of the Office of Science and Technology. He helped to establish the Cornell Program in Science, Technology, and Society, becoming its director in 1973, serving until 1978. He was killed in a fall from the cliff at Robert Treman State Park during a sunny, spring, Sunday morning walk with his wife in 1979.

About this time also, Eugene Dresselhaus from the University of Chicago was appointed as assistant professor in theoretical solid state physics. He was followed a year later by Mildred Spiewack, with whom he had become acquainted at Chicago, where he had suggested to her that she apply for a postdoctoral position at Cornell. There was a growing group back here and, incidentally but not quite peripherally, that's where he would be by then. There was correspondence, the appointment arranged, and she came, under Sproull's sponsorship, making a happy addition to the Rockefeller Hall basement research. Millie was a very bright and good experimentalist, a pleasant individual with lots of drive. She worked in magneto-resistance and Gene, an able metals theorist, was clearly her theoretical consultant in the area; geographically he had his office up on the third floor near the elevator, some hike from the basement at the other end of the building. Gene himself was a very mild, somewhat retiring person, quiet and unobtrusive, a booster for the apples of Washington State. There was no indication that their relationship was to be anything but professional. We always thought that it was the arrival of spring, the coming out of winter garb into light flowery dresses, perhaps somewhat frothy for a physics laboratory, which brought about Gene's interest and their marriage. In actuality, it must have been in the cards They went to Boston where she is now a real power at MIT, all along. holding a distinguished chair as professor in Electrical Engineering, and the directorship of their Center for Materials Science and Engineering; he is at Lincoln Laboratory. He was never very enthusiastic about having students bother him or about the teaching. Together they were a warm and delightful couple. Late in 1981, Millie was elected vice-president of the American Physical Society and will become the society's first woman president. Fitting that it should be she.

Other romances have also bloomed in the department. One, both parties of which are still with us, was that of Ed and Mika Salpeter. Mika, a scientist in her own right, now chairs the Section of Neurobiology and Behavior. One heard that the wedding and party following was a great occasion, long remembered by those who were there.

About the time of the arrival in the department of Dresselhaus and his collaborator, Krumhansl returned to the fold. It hasn't been mentioned that he had gone. Near the time of Smith's departure, Krumhansl also left the department for a position with National Carbon Company in Cleveland. Fortunately, the grass was not quite as green on that side of the fence as it had first appeared, and we were able to attract him back three years later. Two more after that he was chosen to be director of LASSP when Sproull moved into the directorship of the Materials Science Center. We were to lose Krumhansl for another period when, in the late seventies, he was appointed by President Carter to be assistant director of the National Science Foundation in charge of the Physical Sciences, Mathematics, and Engineering, which high post he held for two years before returning to Cornell. Prior to that, he had been editor of <u>Physical Review Letters</u>.

The name Krumhansl is a tough one to spell the first time met. He had once in his office a compilation of about twenty different versions which had come to him in the mail, ranging from Crumbhands to And there is the amusing tale told of him in a New York Grubhandle. subway escapade. It seems that during a Physical Society meeting, or some such which was being held in New York, he had an appointment to make and was dashing through rain to make a subway. He raced down the stairs and through the turnstile quickly; there was a train ready to depart. Doors were closing but Jim squeezed through--all but his foot made it as the train started forward for points uptown. In a bit of panic, he pulled hard and to his relief the foot came inside but without any shoe and There he stood, half shod, and seemingly all of New York there to rubber. observe his predicament, with which he was at some loss to know how to cope. There was one dour fellow sitting next to the door, deep in his daily newspaper. Barely giving notice that he had seen what had happened, he looked up over his glasses and said merely, "Put the rubber on the other

foot," and went on with his perusal of the day's news. It was a good suggestion, Jim took it and got off at the next stop, caught the next train back and indeed managed to retrieve the rubber-clad shoe, which had fallen down on to the tracks.

The fluctuations in the growth of solid state physics have been rather more marked than in the nuclear side of the department. Experimental work there was centered on a synchrotron--the three we have mentioned, culminating in that at 2.3 GeV. While the Cornell group is known for its ability to put machines together with skill, rapidity, and at reasonable cost, it must not be presumed that it was only machine construction that occupied them. Good and important work was done on each generation of machine; indicative of the nature of the operations, the multiplicity in authorship of publications at the same time was gradually expanding. Of course, even with 2.3 GeV in hand, there was the entire energy domain still beyond, enticing to a high energy physicist. But where could anything larger be built at Cornell? Wilson saw the ideal place-under the Upper Alumni playing fields. Laboratory facilities could be built into the side of the south slope, more or less at Cascadilla Creek level, and the accelerator could burrow its way into the hill and around the periphery of the field thirty or forty feet below the playing turf.

But there were difficulties. The administration favored a site out at the airport. The creek channel would have to be straightened out. There were some valuable cattails which would be inundated somehow at Dwyer Dam at the head of the gorge. And there was fear that with a repetition of the 1935 flood, the laboratory would be washed away. Wilson was prepared to resign if reason did not prevail. But it did. The project got funded (NSF--\$11.6x10⁶) and construction of the 10 GeV synchrotron got under way. A tunnel digging machine started into the hillside, disappeared and reappeared a few months later at some remove to the west; that portion of the eventual orbit track not in the circular tunnel itself, would be covered by the building and experimental hall. The tunnel was concreted and assembly and positioning of magnets begun even before the building was completed. Part of the ring was in the open air before the building provided cover for it. Such was the way things have always progressed with the "nuclear" people.

Dipole and quadrupole magnets and mountings were all constructed across the creek and highway in the Annex building. Injection of electrons would be at 100 MeV provided by a linear accelerator, later added to by parts of a similar injector "scavenged" by way of Fermi Lab from the dismantled program up at Cambridge. The injection for the 2.3 GeV machine had also been done with a LINAC, at energy of 20 MeV. It was on the installation of that earlier injection accelerator that a fatality occurred in the laboratory. An engineer from the Walnut Creek firm supplying the equipment, working alone one Sunday afternoon during installation, was electrocuted. Inside the cramped quarters of a cabinet he had gotten across the AC line. Gloom pervaded the laboratory; safety regulations, already stringent, were further strengthened. Today safety is a major factor in the machine and facility design. The large assembly of components makes for a lethal instrument at many points, in more ways than one. There had been in the construction of Newman Laboratory the case of a worker smothered in the cave-in of a utilities ditch in which he was working, but that was quite apart from any laboratory operations.

About the time the 10 GeV building construction was moving along, well before the machine was completed, authorization and funding for the National Laboratory later to be named for Fermi, was obtained and Wilson was called to its directorship. To be built near Chicago, it was to be the machine to end all machines for years to come, at least in the U.S. There had been considerable bidding for possible sites. One was proposed for the Ithaca vicinity, up near the north end of the lake. But we lost that one.

With Wilson departing, a new director was named. There does not seem to have been any question but that it should be McDaniel. He had been closely involved in all of the Cornell machines and had the energy, drive, and enthusiasm for it. DeWire was chosen as Associate Director. And so, under Mac, and John, the new machine was completed and enjoyed a long period of very successful operation. The preceding 2.3 GeV machine was dismantled and sent to the Argonne National Laboratory. Appropriately, the new Cornell laboratory was designated the Wilson Synchrotron Laboratory. In November of 1984, Wilson was recipient of the Fermi award--it was certainly long overdue, but later is better than never. He split the \$200K award with a Frenchman. At the champagne party over in

Newman Lab on the afternoon of the announcement, Bob was properly feted and he responded with some delightful reminiscence; very nice.

McDaniel would hold the Directorship until 1985; in May of that year there would be a big two or three day symposium held in his honor, Bacher returning to speak on the origins of nuclear physics at Cornell, many high energy figures, Cornellians and otherwise, joining the celebration. Karl Berkelman and Dave Cassel would take over direction of the operations as Director and Associate Director, respectively.

In seeking a new chairman at the end of Parratt's second term, we discussed whether the position should not be taken by someone from the other wing of the department, namely LNS. It was not a matter of whether favoritism would be, or had been, shown toward one side or the other; rather, the job was something of a burden and should that not be shared? There was enthusiasm on the part of the department members for two candidates, Holcomb and DeWire, but less enthusiasm on the part of the two candidates, both eminently qualified. LNS won out; there was opinion that they could ill afford to lose an important person from their program and the 10 GeV machine installation. Discussion was harmonious, and Holcomb was named to the chairmanship.

The five-year period was relatively uneventful. The department course had been set, and it was followed. There was still growth, both in staff and graduate student population, the latter reaching into the neighborhood of 225 students. This represented the peak. There came a period when there were too many physicists; jobs were not easy to come by and choices were somewhat limited. There was even encouragement given to students to take courses in Engineering. The student showing some willingness and inclination to engage in engineering fields had an edge with industry over the person disdaining such occupation. A retrenchment set in and the graduate student population declined more or less to its present state of about 150 students.

Department procedures regarding graduate students underwent some changes after Parratt; these were brought about by changes in Graduate School rules. Previously, the incoming student selected a committee chairman and two minor members as required by the Graduate School. The selection was made rather by random choice; he was encouraged to change to a more appropriate chairman and committee after his first or second

year when his interests had crystallized to some extent. The freedom which a student could exercise in his choice of major and minor subjects occasionally brought some weird combinations. Sometime after I became a faculty member, Professor Murdock found himself a minor member of an Oriental graduate student's Special Committee whose major member was in French Literature! The other minor member was in English prose or a The time came for the student's final oral coursework similar subject. examination before the thesis. Murdock was in the hospital for correction of some disability; could I serve in his place at the examination. I guessed it could be handled, and so went over to Goldwin Smith for the student workout. I walked into the room where it was held, a small office, that of the major professor, and found it without even a blackboard! How does one examine a student in physics without a blackboard? I don't know what we did for it; he had taken a semester of laboratory, where I had met him, and so I suppose I got through some queries on that. A man like Morrison could have gone at him on the philosophy of his science or some such. As to what Madame Bovary's reaction was to this or that situation, or as to Flaubert's style, I had no inkling. The student passed, it may be noted.

The new Graduate School rules reduced the minor requirements to a single minor. Our incoming student is now assigned a committee chairman when he or she arrives, one of four "wisemen," who supervises and advises the student generally through the first and possibly second year of coursework, with the consultation of a minor member, also assigned. The major and the minor are both in physics, theoretical and experimental, not necessarily respectively. In the earlier period, the examination sequence was that of an oral qualifying examination with three committee members, followed after completion of course work by written examinations in the major field and minor fields administered by the committee, who then listened to an oral examination covering the subject After the thesis was completed (typed, bound, etc.; none of this matter. system allowing all that to be done after the examination), there was then a final oral thesis defense with the committee and others who chose to come. Others did not (and do not) often so chose. Somewhere along the way, language examinations had to be passed. This too the Graduate School made optional; Physics eliminated them. When our "wisemen" concept was introduced, a comprehensive written examination common to
all second year students was instituted. After a few years trial, the common written qualifying test was replaced by an oral examination given by the student's "wisemen" and his assigned minor member. The subsequent "real" chairman of the committee, who is usually (although frequently not) the person to direct the student's research, thus has relatively little to say about whether the student is qualified or not.

In the first two years, the student is supported as a teaching assistant on department (university) funds; thereafter he is likely to hold a research assistantship and be supported by government funds carrying the research. Long gone are the prewar days when the research was done on the side, student support coming through his teaching duties. Today, instead we pay the student to get his Ph.D. It may yet come back to "olden times" if some of our short-sighted national legislators have their way with basic research.

Things went reasonably smoothly during Holcomb's regime. But the campus itself continued in a state of some turmoil, particularly each spring after the long winter. There was always some worthy cause over which to demonstrate: civil rights for minorities, investments in South Africa, Vietnam, nuclear power, the latter becoming especially heated up in the late seventies. At the height of the Vietnam conflict, Littauer received a private grant and took time off to organize a study of the magnitude of that disaster and the enormity of U.S. steps taken to achieve domination in Southeast Asia. The chairman had need to write frequent letters to Selective Service boards defending the deferral of this or that student.

At the end of Holcomb's five year term, the department turned to LNS for its next chairman. It was a happy choice and something of a surprise. Raphael Littauer had not previously been close to administration; he was heavily involved and clever in the electronics instrumentation connected with the synchrotron; he had been innovative in his teaching, having installed in Lecture Room B a lecture room electronics answering apparatus. In its use, the lecturer poses a question, each student has buttons with which to respond--right or wrong. The lecturer sees quickly whether he has gotten the message across to his audience and can back up or proceed with some confidence. And the student stays more alert.

Littauer thought being chairman would be an interesting experience. And presumably it was. He obviously had to curtail much of his activity at But it was his involvement over there that led to his Wilson Laboratory. resigning the chairmanship after only three years. It was during this period that the notion of doing experiments with colliding stored beams of high energy particles and their antis, particularly electrons and positrons, became popular, the soundness of the concept having first been proved in the ring at Stanford under Burt Richter's direction. The idea had originated at Princeton with G. K. O'Neil, McDaniel's first student. (This contribution of O'Neil's would seem to be much more meritorious than some more recent relating to space colonization.) Our high energy people initiated a study of the feasibility of converting the 10 GeV machine into such a facility. Besides the talent already shown in the construction of the synchrotron, Bernie Gittelman, important to Richter's success at Stanford, was also now here. And so the Cornell contingent came to the decision to try and get funds for a storage, colliding beam machine, CESR, to be built in the same tunnel alongside of the synchrotron; the latter would be used to bring the particles up to energy and to fill the former. Funding was questionable; there were many proposals abroad the land seeking funds. But with the modesty of the Cornell proposal, \$15x10⁶, and with the Cornell record behind it, the National Science Foundation found the funds.

With Littauer's ingenuity in electronics, his insight into machine dynamics, and the complexity envisioned for the new machine, it was seen as necessary that he participate closely in the design and construction of the new development. He felt a deep obligation to help in that effort and so stepped down from the Physics front office. While the department was not delighted over the circumstance, it saw the probable wisdom in his decision. He had played a major role in the new development, which was shepherded by Maury Tigner. In October of 1977, the synchrotron was shut down and those portions in the experimental hall were dismantled. A schedule calling for an injected beam by September 1979 was advanced five months to March 31, and a very strenuous program of construction was begun. The first electrons were injected a bit behind schedule--two hours into April 1. Two weeks later--Friday the 13th--the first electrons were stored for two minutes. It thus came "on line" with colliding beams

well ahead of schedule; the "upsilon" meson was rather quickly seen; the "prime" and "double prime" have since followed. All evidence points to a very successful operation. The department loss of Littauer as chairman has been a real boon to the high energy program, and so there cannot be too much moaning.

With the new machine, a large excavation had to be dug in the experimental hall for the immense and extremely complicated detector system to enclose the interaction region of the colliding beams. (The other interaction region on the opposite side of the ring, where a Columbia-Stony Brook group has an experiment set up, incorporates a detector far less formidable.) The excavation had guite a pool of water in it before concrete was poured; some wag painted a big sign on the wall labeling it Lake Tigner, Elevation 980 ft., or whatever it was. The dried and concreted "lake" basin was a year and a half later a sunken room filled with the giant detector components and thousands of electronic units which would enable experimenters to determine the trajectories, energies, momenta, etc., of interesting events. Tigner is therefore not honored with a lake bearing his name; rather, the devising of the clever means for taking sixty bunches of positrons circulating in the synchrotron, where they are brought up to energy, transferring and coalescing them into a single bunch in the storage ring, will be honor enough.

Tigner has been in charge of the machine construction in this latest of Cornell high energy enterprises. Presumably latest does not mean last; already, even before the "upsilon" states had been definitely observed, one heard undertones of a next machine. By now it has come to more than that; workshops have been held on the problems connected with a 50 on 50 GeV colliding beam monster.⁴ If it comes about, it will not likely be on the Cornell campus, which will be too bad. A great plus in the presence of our machine on the campus, has been the presence also on the campus of the staff needed to run it. It will be a sad day if a large fraction of the department faculty is fifty or more miles off in the boondocks most of the time.

⁴ In the fall of 1982 it was announced the project was dead for lack of NSF money in the amount required. What this portends for Cornell Physics is far from clear.

That loss happens now in the case of certain few of the faculty. With the great accelerators at Brookhaven and at Fermi Laboratory, at least three in the department find themselves at one of these places for extended periods of time, where their graduate students may also be found. Jay Orear's research has been almost entirely done at Fermi Laboratory, and at Brookhaven earlier. Lou Hand had a program at Fermi Lab for a number of years, spending a part of each week out there for months on end--an arduous schedule. These two men have not much associated themselves with the synchrotron physics at Cornell. Hywel White had strong connections with Brookhaven and, with other Cornellians, carried on a strong collaboration with that laboratory on the "Cosmotron." While he later was quite involved with the program on the Cornell machine, he took leave in 1978 to work at Brookhaven, resigning two years later to remain there.

Tigner has been a mainstay in the construction of at least the last two machines here. He appeared on the scene first as a graduate student fresh out of Rensselaer Polytechnic Institute over at Troy. My first contact with him that I recall was in my office in Rockefeller when he came wandering in wondering if I would not supervise him in the construction of an accelerator, just any accelerator he could work on by himself. I don't know why he came to me; my machine experience was pretty meager and lackluster. I had the good sense not to convince him that there were other things in life beyond accelerators. Rather. I told him to go see Bob Wilson, that the latter might spare time from his synchrotron cares to see Tigner through some small scale kind of machine. They did get together, and Maury built a microtron and wrote his thesis on One thing led to another, and today Tigner is one of the worldit. recognized authorities in machine construction and operation. In 1985 he would be put in charge of the research and design in Berkeley for a proposed \$3 billion, 20 on 20 Tev, super-conducting proton collider, an accelerator-storage ring, 50 or 60 miles around, to be built somewhere (Texas, it turned out) a national and perhaps international facility, to be funded largely by the Department of Energy. McDaniel would head the organizing consortium for the enormous project.

The complexities of the present operation "boggle the mind," in current vernacular. Not only is the machine largely computer controlled,

but the entire detector system beyond the actual scintillators and 5500 proportional counters for the drift chamber is a nightmare of electronics, although obviously with a high degree of order. Literally thousands of amplifiers interface with computers to present information leading to a diagnosis of events, almost in "real time." Much of this sort of thing is Don Hartill's doing; he came to us from Cal Tech by way of CERN and spent a sabbatical year at Stanford in the design of their drift chamber prior to Design of the computer control of the machine was his work on ours. largely the work of Littauer and Bob Siemann. The latter joined the high energy group after some years at Brookhaven on the large proton accelerator there. He, Tigner, and Karl Berkelman are the exceptions that prove the rule that no Cornell-trained people have come into the department after the large post-World War II influx. Berkelman, Dave Cassel, and Gittelman are more heavily involved than the others in the data analysis and the physics of what the detector presents.

The detector, dubbed CLEO, is an impressively large and complicated assembly of parts. Given any event in a positron-electron collision, the detector is supposed to determine what happened. Perhaps the most impressive part of it is the internal drift chamber. This is immersed in a magnetic field provided by a solenoidal winding around it, presently to be replaced by a superconducting coil, which will set up a field of about 10⁴ gauss over the cylindrical volume of roughly 10 cubic meters; a fair amount of energy stored therein. The drift chamber itself consists of two massive aluminum plates about 5' in diameter and a 2" thick, each drilled in an array, and to a tolerance of a mil or so, with 22,000 holes 1/8" or so in diameter. The array makes up a lattice of units four holes each ("per unit cell"), with wires strung across between corresponding holes in the opposite carefully aligned end plates, each unit of four constituting a little proportional counter of length the distance between end plates, six to eight feet--5500 proportional counters surrounding the interaction region, each with its own amplifier to provide adequate signal for processing! Depending on which counters go off at an event, the product particle trajectory is determined. Other counters on the outside, scintillators, help identify the particle. The drift chamber weaving job took about three months of around-the-clock work. After stringing and anchoring the wires, each insulated from the end plates in which they

were clamped, they were all stretched a bit by pulling the end plates further apart about an eighth of an inch; only a few of the thousands gave up and broke. As is customary in describing wires, they were finer than a human hair, one of each four in a unit being of gold-plated molybdenum 0.8 mil in diameter; that is fine wire to "weave." The other components surrounding the drift chamber, again with associated electronics, will not be gone into here. It is thoroughly outlined in the official proposal to NSF, which has funded the whole operation. Quite a bargain; it is surprising how much \$15 million will provide.

The contrast between this and the early machines is striking. Where the control desk of the first primitive machine at Newman Laboratory was a simple affair with relatively few control knobs to turn and a few oscilloscope faces to look at, the control panel of the latest is something from science fiction or a space age movie, fairly bristling with knobs, panel lights, scopes with computer read out or signal traces, and TV pictures staring out at one seemingly from every direction; it is dazzling.

NSF also funded as an adjunct to the machine at about \$1 million over a three-year period, three X-ray beam lines taking off synchrotron radiation from the circulating electrons in the storage ring. This project, CHESS, is a national facility under the direction of B. W. Batterman, of Applied and Engineering Physics, with Neil Ashcroft of Physics as associate director. The intensity of the X-radiation thrown off the orbiting charges is truly prodigious; what was next to impossible to accomplish in the use of an ordinary high intensity X-ray tube source, is now readily done; work taking hours and hours previously is now done in a matter of minutes; all a matter of signal to noise. Various groups, in and out of the university, are making good use of this facility.

It must not be overlooked that CESR itself is something of a cooperative effort involving groups from outside the university. High energy groups at Harvard, Columbia, Syracuse, Rochester, Rutgers, and Vanderbilt have been active, principally in the various components associated with CLEO.

By late 1979, the entire enterprise was operational at a level such that real physics was being done. By mid-December, confirmation that the upsilon and upsilon "prime" mesons had been seen was well in hand. By way of announcement, the laboratory prepared a Christmas card to be sent

to various interested laboratories and high energy physicists. "Greetings," or some such, it would say on the outside. On the inside, in place of the usual fir tree or nativity scene, would be the double peaked plot of the data. Before the card got printed, however, existence of another and new upsilon particle was shown from the continuing work. Consequently, the card design was added to: On the opposite side of the fold, to accompany the first plot, there was "Note added in proof" and a second resonance plot, complete with its own error bars like the first, showing the existence of this new "double prime" particle.

Throughout the period of later machine development, theory was not forgotten--in both sides of the Department. Our specialist in quantum electrodynamics, Toichiro ("Tom") Kinoshita, was here by then, becoming more knowledgeable about details of the electron than anyone else, anywhere. The first department theorists for whom high energy physics and fundamental particles were bread and butter, were Peter Carruthers, Don Yennie, and Kurt Gottfried. Carruthers had done his graduate work under Bethe, stayed on to professorship but eventually left to take on headship of the theory group at Los Alamos. He claimed his most important discovery to be Mitchell Feigenbaum, who would leave his Los Alamos theory group to join LASSP here in 1981. In 1986 Feigenbaum would be awarded the prestigious Wolf Prize for his important discoveries in "order to chaos" theory. Carruthers made his claim in a NY Times Sunday Magazine profile, which preceded one later on Feigenbaum. Yennie came from Stanford after graduate work at Minnesota. He has become the authority on the hydrogen atom. Gottfried came to us from a position at Harvard, where he had built a solid reputation both as theoretical physicist and as teacher. The loss at Cambridge was our gain. He had authored a book on guantum mechanics and is currently working with Viki Weisskopf on one in high energy physics. Tung-Mow Yan joined this group in 1970. Gottfried and Yan, together with Kinoshita, made up a formidable team in formulating a theory of the newly discovered psi meson, which discovery had occurred in 1974 at Stanford and at Brookhaven. On the basis of the new quantum number "charm," the resonance could be understood as the lowest bound state of the charmed and the anticharmed quarks. Indeed, the theorists made prediction of a higher state of the bound pair, which resonance was then discovered at Cornell. They

predicted, in addition, that there should be bound states of the "bottom" and "antibottom" (top) quarks, the lowest of which was also discovered as the upsilon meson with the Cornell machine. After all three resonances were identified at Wilson Laboratory, the detailed measurements of their energies and widths were found also to be in good agreement with the theorists' predictions. There is growing confidence that the psi and upsilon mesons are indeed bound states of the various quarks. Order is gradually emerging from the confusion.

Another theorist flying the high energy colors was Ken Wilson. He had worked with Murray Gell-Mann at Cal Tech and came here from CERN after two years at Harvard. It was at CERN that he became acquainted with Kinoshita, on a sabbatic leave, who urged us from there to appoint Wilson to the faculty. His interest is in gauge theory and group His approach has had an important diversion in renormalization. condensed matter physics, particularly in phase transformations--critical phenomena. Working with Michael Fisher he has proved an important theorem concerning transformations in systems of dimensionality epsilon less than four. To a plumber that may not mean much, but to those in the know, it was an important result worthy of a handsome prize or so. In 1980, he and Fisher shared with Kadanoff at Chicago the Wolf Prize, worth a mere \$100K. Together with Bethe, Salpeter, Greisen, and McDaniel he is also a member of the National Academy of Sciences.

It was my intention to terminate this "record" of department fortunes at the end of year 1981. Thus no mention was made of events transpiring since that time (continuing progress on Rockefeller Hall; additions to the professorial staff of Mitchell Feigenbaum, working in turbulence and "chaotic" theory, of Carl Franck in solid state experiment, of R. Galik in high energy experiment). But an event occurred in October 1982 which cannot be ignored as long as the writing is still in manuscript. The event was the award to Ken Wilson of the 1982 Nobel Prize in physics for the work on critical phenomena. The eventual attainment of the award by Wilson was not unexpected but it was a bit of a surprise that it was not shared with others with whom he had strongly interacted and on whose work he strongly leaned. After the award was announced, Wilson graciously said he would have been honored to have shared it with Fisher

and Kadanoff. It is not often in this world that advances are made without "standing on the shoulders of giants."

Fittingly, it was a glorious day in Ithaca. There was a press conference over in Newman with full television coverage. A great congratulatory banner hung down across the front of Clark Hall from the seventh floor balcony. A department party at noon and a later reception for a broader representation; champagne seemingly flowed like water a good part of the day. It was the second such occasion in two years for Cornell, Roald Hoffmann of Chemistry having been awarded the chemistry prize a year earlier. When Sweden tried to get through to notify Wilson at his home early on the day the phone was tied up--with Ken's usual connection to the Cornell computer terminal. Somehow the operator broke in with word of an emergency call, thereby starting a joyous if hectic day for the award winner, the joy shared by all his colleagues and the Cornell community as a whole.

Like Kinoshita, Salpeter also urged an appointment on the department. He argued persuasively that relativity was a field that we should not be ignoring, it was also of concern to Astronomy. The result was the addition to the department roster in the mid-seventies of Saul Teukolsky, an outstanding theoretical physicist in general relativity from the Cal Tech group. His lectures on the subject have been extremely popular among theorists and experimentalists alike, students from both sides of the two departments.

Concurrent with high energy machine developments, a new position was created in the Department. Raphael Littauer, who came to Cornell by way of Cambridge (England) and General Electric (U.S.) was the first to hold the position of Senior Research Professor. Tigner later was of the rank before he became permanent in the department. The rank was initially created largely at the behest of the nuclear staff as a means of retaining valued personnel for whom a tenured professorial position was not available. Essentially all the advantages pertaining to a full professorship were there except tenure: sabbatical leaves, children's tuition, retirement benefits, and the like, not to mention salary. Position permanence, however, was at the whim of the funding agency as it were. Of course, the director would have something to say about it. Occasional teaching is even indulged, particularly in the advanced and intermediate

laboratory courses, but the obligations are principally to the funded research program.

Meanwhile, during the growth in LNS theory, things were not standing exactly still in LASSP, if perhaps they weren't going very rapidly forward. Krumhansl had gone to Carbide in 1954, a year before Smith was to leave. Fortunately, Al Overhauser was by then on the scene, having arrived in 1953, a year before Krumhansl departed. Three years later, in 1956, Gene Dresselhaus came (Millie a couple of years later). Robert Brout, native of New York city and a statistical mechanician, came the same year and was for two years an Instructor. His wife was from Europe, where he met her; she was a cousin of Rose Bethe. He advanced to associate professor, as did Overhauser, but he took a leave in 1961 to go to Brussels. He subsequently resigned and never returned. The Dresselhauses left a year before Brout did. But Krumhansl had come back in 1957 after three years away and a year before the shock of Overhauser's leaving hit us. The latter's leaving was a major loss. Overall, there was some continuity, however fragile. Thereafter, things went steadily ahead. In the four years, 1962-66, we had coming Vinay Ambegaokar, Geoffrey Chester, David Mermin, John Wilkins, Neil Ashcroft, and more recently, Eric Siggia--all still with us--and in their company over the years an innumerable number of postdoctoral theorists. A very strong group indeed, possibly second to none in the country. But the beginnings of LASSP theory seem rather faltering in retrospect. The joint appointment of Michael Fisher, Chemistry, added further luster; he worked hand in glove with Ken Wilson in that interdisciplinary phase transformation business mentioned earlier, sharing in that handsome Wolf prize, which Mitchell Feigenbaum would also win six years later.

Chester had a term as LASSP director and has become associate dean in the Arts College. Wilkins spends extended periods at NORDITA in Copenhagen. If he perhaps is the loudest and most visible member of the LASSP theory group, Mermin is the most whimsical. Besides writing for various dramatic productions presented at department parties, he has written a fine introductory text on relativity and one, more high powered, on solid state physics, written with Ashcroft and very well received. A few years ago he gave a charming colloquium (later published in <u>Physics</u> <u>Today</u>) on his trials, tribulations, and frustrations at getting accepted a

term he wished to have applied to a singularity arising in liquid helium vortex flow: the boojum. Very amusing. It was even more amusing to inform him, a week after, of an article noting Sir Fred Hoyle's term for a peculiar galaxy: the boojum. At this writing, it is not clear how it is all to come out.

Like Parratt, Mermin has an amusing automotive experience behind him--not with a large Oldsmobile but with a VW. He and his wife had two of the cars, a "beetle" and a "squareback." One of the two was up for yearly inspection so Dave took it down to Ripley Motors, referred to by some graduate students as "Rip-offs." Dave left the car in an obvious spot in front, went in with the registration and said he'd call later about progress and report of any deficiencies needing correction. Some time later, he did call, gave his name and asked how things were. There was a very long silence on the line as the person at Ripley went to check it out. Finally, Dave was asked again for his name or some such trivia, and again there was a very, very long interlude of quiet with only some background noises. Finally, Ripley came back on the phone and reluctantly told Dave that his car had been stolen from the lot, but that a statewide police alert had been sent out; it was conceivable that the vehicle might be recovered--he shouldn't start any legal action for a while. Ripley Motors was clearly very upset over the turn of events. So was Dave--you take the car down for a miserable \$3 inspection and then have the thing stolen from right under your nose. He phoned Dorothy the bad news. "David," she asked patiently, "which registration did you give them when you left the car?"

The experimental side of LASSP was also growing. Fitchen came from high pressure work at Illinois; the magnetic resonance people were here; Sievers with his infrared techniques applied to solids study came, perhaps our most prolific experimenter. (Fitchen, one day to be department chairman, had roots in the Ithaca area and knew the scene well. Summers, as a youngster, he had visited his grandparents at their farm home up the lake a bit; his grandfather held down the Presbyterian ministry in Ithaca at the time. Shades of Charlie Baker before him--in that case it was his father and he had been of Baptist persuasion.) Earlier than these came Bobby Pohl and Herbert ("Bib") Mahr. Pohl had come as a research associate to Sproull and went into thermal conductivity work, has continued in phonon physics ever since. Along the way he became very

concerned about effects of nuclear reactors and became an outspoken opponent of the technology. Mahr had been a colleague of Pohl's at Erlangen, Germany, and was suggested by Pohl as a possible research associate to work in our UV study of some alkali halides. Mahr showed up one day in the company of Hermann Haaken, who was also arriving, to be a visiting professor with Overhauser and Brout, if memory serves. Haaken went back to Germany and Bib stayed at Cornell, introducing to the department the first ruby Q-switched laser, mode locking and picosecond pulse techniques, remaining in the area of laser optics research ever after. I believe it was Haaken who, before his return, took a trip west in an old beat-up car with his family. At least it was one of our German visitors. On the trip back to New York and LaGuardia airport for the flight home, they dared not let the automobile engine die, else it would never start again. I don't recall the malady. At any rate, they made it nonstop, reached the airport and in great finality cut the ignition. As far as I know, the machine is still there.

Mahr's first work of note here was on the study of the optical absorptions of mixed alkali halides, KC1-KBr and KC1-KI, measurements involving our vacuum-ultraviolet instrumentation. During the work it occurred to him that other properties would be of interest. Pohl was deep in his thermal conductivity measurements so Mahr suggested to him that he should try the mixed systems, which Pohl did in major contribution. From this, Pohl went on to study the role of crystal defects and disorder on the propagation of phonons, in which area he has since spent productively a good part of his life.

At the end of 1981, Mahr was dying of a brain tumor, operated on three years earlier but recurring in an inoperable way during the spring of his last year--a distressing end to a productive career of an enthusiastic, pleasant, and outspoken colleague.

In their interests and teaching, Mahr and Sievers were quite responsible for the rebirth of optics in the department. In the rush of laser development following Maiman's original ruby laser, Electrical Engineering here was more active in optics than Physics to the extent that one might have supposed the field would remain an engineering discipline. But optics has become of great importance and has enjoyed a renaissance large enough for both fundamental and applied work to be done.

Important to the welfare of LASSP has been the Materials Science Besides a new building of benefit to the whole science Center. community, with it came a number of central facilities, including those for crystal growing, for chemical analysis, for X-ray analysis, for technical operations, and for report preparation. More important has been its role in funding, particularly in helping to get the research of new faculty under way. Purchase of large pieces of equipment not affordable under separate grants or to be used jointly by more than one group, has been greatly facilitated. Unlike the high energy research, which is funded under one large grant, the solid state research is very diverse, each group funded under a separate grant. In this mode, the MSC has played a unifying and covering role, not only for LASSP but for all the participants in the center. With the individual researcher responsible for obtaining his own funds from this or that government agency, the LASSP office has not had the advantage for its members that the LNS has had for researchers over there, where the focus is primarily on one great machine to which most of the rest is subservient. But LASSP has brought a unity that was not present previously and has served a very useful purpose in providing a direction for the overall research effort and course that the laboratory has followed. Nevertheless, the large number and diversity of individual researches remains, and it is probably only the director who is aware of what everyone is doing. It is not like the cozy "olden times" when practically everyone knew what everyone else was up to. This diversity in physics, in purpose, in tools and techniques, and in funding is somewhat reflected in the turnover in the two directorships. In LNS there have been but two directors during its history (not including Bacher's brief, although important, tenure)--Wilson and McDaniel. In LASSP there have been several over a shorter period of time. Following Sproull, our first, we had Krumhansl; then came Holcomb, Chester, Silsbee, Ashcroft, and now Mermin--a reasonable balance between experiment and theory.

In his last year as chairman, Littauer got caught up in an Arts College "revolt." Administration had made some commitment concerning salaries. There has always been dissatisfaction over academic salaries, at Cornell as elsewhere. Cornell Physics lags behind some of our sister institution departments, yet recognized in a seventies survey as one of the ten best physics departments in the country. Indeed, Physics lags

salary-wise behind Chemistry here, a situation somewhat of our own making in the expansion we undertook. The Humanities fare even less well.

The difference in salary scale between Physics and Chemistry was a bitter subject with us but resulted strictly from the department policy of placing permanent positions on a higher priority level than salaries. Chemistry was exactly the opposite, the faculty there willing to withhold tenure track positions and to assume heavier teaching loads in favor of So the two departments went their merry ways; we higher salaries. expanded and salaries did not, while Chemistry essentially held its size and salaries went up. Our policy for growth was carried forward with vigor under Parratt and with the concurrence of the rest of the Maybe we didn't pay attention to the consequences of our department. ways; there was outrage when the discordant salaries in the two departments came to be realized. It must be pointed out that Chemistry, to hold the line, was absolutely ruthless in its promotions, probably the strictest department in the university in that regard.

Shortly after Parratt's second term as chairman had run out, the department Steering (Policy) Committee, then composed of Holcomb, McDaniel, Chester, and Bethe, met with then President Corson, proposing that department growth continue at two new members per year. Corson sent them away with a fiat refusal. On a return visit, the refusal was acceded to with the recognition of what had transpired over the preceding decade or so.

In the spring of 1977, Arts College chairmen in revolt issued a manifesto declining to submit their budgets until some clarification and remedial steps were forthcoming on salaries. Chemistry was in the vanguard of the action, Professor Fisher seeking to have established a salary policy that was a model for the country, placing plant maintenance on a pretty low priority. One admires the lofty objective but has to recognize the necessity for roof repairs. There was ultimately some meeting of minds and budgets were submitted, but they were all late and salaries were not known practically until after the start of the fiscal year. Salary levels were not markedly raised either, and of course there is still complaint on that score.

After Littauer resigned the chairmanship in favor of CESR, the choice of a new chairman was a straightforward affair. Douglas Fitchen came across strongly. Innovation and progress have been made during his term. Most important, the extensive renovation of Rockefeller Hall is under way--three quarters of a century from first occupancy. After many previous proposals, plans, and long hours of discussion, all of which led to Lecture Room B being turned through 90°, the building is finally going to take on a new interior look and be made comfortable. The exterior will be unchanged except for an unobtrusive fire stairway which has been added to the south end. The roof and guttering has had extensive work done on it; the danger from falling ice over the entrance, cited back in Merritt's time, has been much lessened. Stair railings at the front entrances will presumably permit easier scaling of icy steps to gain the front doors.

At this writing, the basement and third floor have been changed almost beyond recognition. An old resident would recognize the long corridors, of course, but not much else. Even they have had the paint laboriously sandblasted away to reveal the patina of terra cotta tile, paint which was as laboriously applied in the late twenties, according to Bill Shaw. The old, rather dingy laboratory research rooms in the basement were ripped out and replaced with comfortable, light and airy, wellheated student laboratory facilities. Upstairs on the third floor, the same thing has occurred, the old spaces now subdivided differently into attractive offices for people in English. Physics has lost that floor. An elevator and enclosed stairwell have replaced on the north end the old open staircase with slate treads hollowed deeply by the feet of past thousands of eager students; the similar one on the south end will likewise be replaced. Work has started on the second floor; as of this writing, everything on the north end has been gutted. As Fitchen described it at the time, with its high ceilings and vast open space it reminded one of "nothing less than a great empty warehouse."

The "charm" and antiquity of Lecture Room A is somehow to be preserved; how this is to be managed in an energy-inefficient barn of a hall which must be made acoustically correct, watertight, and silent in a downpour is not yet determined. Money for much of the renovation has been appropriated, that for A has not. Some gifts from former occupants

of the building have come in unsolicited. Should old acquaintance be forgot?

The community has been treated to more exposure to the department. A public lecture series in the mode of the Christmas lectures of the Royal Society has been inaugurated. Clearly, these must be held in the ancient setting of Lecture Room A. One or two a year are envisioned. For the two so far presented, on low temperature physics (by Richardson) and on high energy physics (by Tigner), people were turned away from the old auditorium, so many for the low temperature show that it had to be repeated. The reception lends encouragement for the continuation of such lectures.

Beyond the public lecture, the department has now and then been host to the public for inspection of facilities. In the mid seventies, Clark Hall held an "open house" which was, however, only marginally successful. Far more so was one at Wilson Laboratory late in 1979 in celebration of the storage ring and CLEO "turn on" and of the detection of the upsilon and upsilon-prime mesons; 2000 ±25 persons visited the laboratory in a twoday showing. With wide publicity, via radio and press, Clark Hall tried again, more successfully. During Halloween weekend of 1981 more than 3000 people milled about the research corridors of Clark Hall during Friday evening and all day Saturday, visiting research displays and simple but striking physics demonstration experiments scattered along the selfguided tour route, winding up on the seventh floor to more demonstrations, cider, cookies, and balloons.

In 1980, following some years of gradually improving relations with mainland China, T. D. Lee at Stony Brook suggested a program that would bring young physicists of China to this country for their graduate education. In cooperation with physicists at other places in this country, a tough examination would be drawn up and submitted to key persons in China for administration to those wishing to participate. After grading the examinations taken, a team from this country would go over there and interview likely candidates and select a certain number for the program. The number would depend on how many institutions would become involved. The plan is being implemented as of this writing. The examination, very tough, was administered; there were obviously a surprising number of brilliant students and budding physicists to be had.

In company of another couple from Columbia, Fitchen and his wife went to China to interview in a short period, in a number of places, some hundred or so applicants. Fitchen and his physicist colleague dwelt on the science aspects, and the two wives, who also interviewed, dwelt on social and more personal questions. Language was no problem; if the candidate could not be understood in English, it was a "no go" situation. From the trip results, we had three or four mainland Chinese graduate students join the incoming class in 1981. Others went to Physics departments of our sister institutions. An interesting experiment.

Fitchen's was a most interesting and significant tenure. It was during the early part of his term that a national law enacted by Congress affected one particular aspect of departmental affairs. The age of mandatory retirement in the country was raised in 1978 from 65 years to 70 years, except for professors and high-priced executives (as though they are equivalent!), who would both come under the act in 1982. This would mean the extension of the bulge in our age distribution brought about by the influx of so many young professors at War's end, which we mentioned earlier. It would mean the delay in our ability to bring in to the faculty young additions full of fire, ambition, and energy. Furthermore, it put present chairmen in an awkward position. Cornell administration decided that the deans, in consultation with their chairmen, could decide whether or not to make it possible for those attaining age 65 after passage of the law and before 1982, to continue their department association in some manner or other. A few deans, flat out, said it was retirement at 65; no bones about it. Others, those in Arts and Engineering for example, might or might not see fit to continue a person on longer. In the year 1982, of course, they have no option, except for malfeasance or misfeasance, the faculty member may go on to age 70. I was the first in the department to get caught up in this situation. In pursuit of my mode of getting through this life by the skin of my teeth, I turned 65 two weeks too late (after July 1) for the university good and so went on toward normal retirement until nearly age 66. During the intervening year, the law took effect and I could ask the appropriate deans, through my two chairmen (I had become paid half by Engineering when I took on looking after the undergraduate Engineering Physics program; I was reluctant to sever all connection with Physics when I was asked to assume that responsibility so I became in

full meaning a joint appointee) what might be the prospects of my staying on in part- time. Of course, that put them on the spot; could I contribute anything? They did see fit to let me stay and here we are. Whether any of the project contained herein makes partial contribution remains to be seen. But the real point is that the whole new situation only prolongs the unfortunate age distribution, and delays getting active members added to the staff.

It was not my intent to carry this "history" into the chairmanship of Fitchen's successor, Don Holcomb, in a second term; indeed not even to the end of Fitchen's term. Here and there only has parenthetical reference been made to events occurring following the writing of the first draft. It seems appropriate, however, to include, now in 1983, some random observations before winding things up. They follow.

SUNDRY NOTES AND COMMENT

Physics has been a white-male-dominated profession; the Cornell department has shown this same characteristic, not because of any outright stated policy, but simply because there have not been in the market enough qualified minority physicists. There have apparently been a few women involved in the physics of the department for many years. Frances G. Wick seems to have been our first woman graduate student to earn the Ph.D. Appearing in 1906, she worked with Nichols and Merritt in fluorescence and went on to Vassar, returning here summers to continue such work. Clara Cheney, who did a Master's thesis with Murdock back in the twenties, had been one of Miss Wick's students at Vassar. (Miss Cheney got tied up with a Cornell physics major, R. H. McCarthy, who went then over to Mechanical Engineering for his Master's degree; they wound up at Nevada as Hartman family friends for a couple of years before he took a position with Western Electric, where fifteen years later, I was to interact with him when Western Electric manufactured one of our Bell McCarthy's neat notes taken in Richtmyer's Modern Labs magnetrons. Physics course are appropriately reminiscent of the first edition of the text. The couple has long been staunch Cornellians and have retired to live in Ithaca. McCarthy's sister was one of the first women to earn the Ph.D. in Chemistry at Cornell.) Dorothy Waugh came earlier; she also worked with Murdock, published with him, and married him. Charlotte Throop, granddaughter of Nichols, coming when I did, was another of Wick's Helen Gilroy, Rebecca Oliver, and Juanita Witters were other students. women of the era. Hildred Blewett came for the Ph.D. a few years later at the end of the thirties, husband John across the state at General Electric. (Connect him with accelerators and the first search for "synchrotron" radiation at G.E.'s betatron.) It was not until 1944 that the first woman has hired as teaching assistant; Jane Faggen entered, teaching first under Howe and then "permitted" (as she puts it) to teach laboratory sessions under Grantham, both jobs "real pleasures." It has not been unusual that one or two women entered each year with new graduate students. The percentage has gradually risen since World War II, particularly since the rise of the "Women's Movement" for equal rights. But by far the great

majority of admissions continues to be male. It is surely not demonstrated that men have a talent for physics that women lack, so that it may be expected that this domination by the male sex may lessen, not only at Cornell but in the profession generally.

One may note the first appointment of a woman to a "tenure track" position in the department. Barbara Cooper, former Cornell physics undergraduate, took up her assistant professorship in the fall of 1983 with LASSP, after two years as a postdoc at Cal Tech, where she earned her doctorate. Professor Vera Kistiakowski of MIT spent a sabbatical year here (1981-82), both teaching and doing research with the storage ring people. It was a fruitful year from all reports. And one recalls the profitable association much earlier with Millie Dresselhaus in metal physics. Too few such have we had; there will be others (e.g., Judy Franz, professor from Indiana, once a Cornell physics undergraduate).

While the department may still be dominated by males, the women are now heard from. In the summer of 1980, after an American Physical Society meeting here on Surface Physics had featured entertainment furnished by a talented company of belly dancers at the conference banquet, a fine affair from all reports, the entertainment committee for a scheduled conference at Cornell on helium three a few months later contracted for the same company to supply entertainment for their own banquet. Somehow, when this became known, the program quietly but abruptly changed to something more decorous, at some cost to the conference committee.

While there used to be a university rule that husband and wife may not be tenured Faculty members in the same department (Ed Salpeter's wife, Mika, got close with her professorial appointment in Neurobiology joint with Applied Physics), that did not mean that they may not both serve the department. We had Giuseppi and Vana Cocconi, he a professor and she a research associate, both in cosmic rays. As university policy this was changed during Corson's presidency. Presently we have Bob and Betty Richardson, Dave and Edith Cassel, husbands professors, wives instructors. Things are changing slowly, are bound to change further, but it is still the male in dominant position.

Entry into physics by blacks has been a very minor factor. The first black student of whom there is definite memory, indeed record, was John

Hunter, who did his work in experimental physics under Smith in the period before World War II. He was older than most of us, having done his undergraduate years at MIT in the early twenties. He came from teaching in a black college--Virginia State University--and seems to have been self-supporting. After earning his Ph.D., he went back to Virginia State as professor and director of Graduate Studies. He was an able person, qualified for a more remunerative position, though perhaps not more meaningful and useful, than that to which he returned. Right after the war, there was a talented theorist, Walter MacAfee, who worked under Bethe. He went on to a good career in the Signal Corps after earning his Ph.D. Since then, blacks have been few and far between, no really significant rise having taken place, even after the civil rights struggle of the sixties and concerted effort in "Affirmative Action" to recruit minority people, both in graduate admissions and in staff appointments. A research associate, black and a woman in addition, was appointed to Nuclear Studies in 1976; after two years she was appointed an instructor in the department, particularly as an aide to minority students in the service courses of the department. But the fact remains that there has been no great influx of blacks or women into the department ranks. Oriental students, and particularly students from India, have been far more common. With some notable exceptions, they have mostly gone into theory. Does it say something about theory being the preference of those from environments less mechanistically oriented than the American? Three department faculty members, Vinay Ambegaokar, Toichiro Kinoshita, and TungMow Yan are all theorists. On the other hand, Nariman Mistry, senior research associate in Nuclear Studies and an important person in the construction of the storage ring, is very much an experimentalist. So was Eugene Loh (of Utah's cosmic ray "fly's eye" fluorescence detector), and Wilson Ho, now assistant professor. And one recalls an exceptional experimentalist, "Venky" Narayanamurti, a student of Pohl's, still working in phonon physics and making a name for himself at Bell.

Over the last half century, which my own recollections nearly encompass, things have obviously changed enormously in experimental physics apparatus, not to mention the experiments themselves. One might imagine that things would not have changed so much in theoretical work, what with pencils and paper still around. But theory is not of much use

unless it can predict the outcome of an experiment, and that involves calculation. At the time of my parents' graduate student days, calculation made use of log tables; we had a seven place Vega table in the house at home, a remnant of my mother's student days in positional astronomy. Machinery for calculation was not yet on hand; slide rules of course were common and an old device; all students had one--probably in the Manheim form--flip over the sliding member and find the trig functions. There was around the department from the old days, and still in the "museum," a cylindrical slide rule, the Thatcher rule, a many segmented affair equivalent to a straight rule of length perhaps ten feet, allowing six-place accuracy at the low end of the scale. I never used one, happy enough with my own circular, spiral fifty-inch "rule." Machine calculators, electric motor driven, were a great advance, even though in the first models the carriage had to be shifted manually for multiplication and division. Then came the automatic shift types. Still, they were basically add and subtract; but what other numerical operations in last analysis are not of such form? So one could do square root, cube root, laboriously numerically integrate, and so on. There is in the attic a mechanical machine of quite different sort, a harmonic analyzer of some sort. Who used it or built it and for what purpose is not known. Later there were mechanical machines of still different type. Vannevar Bush at MIT had a large mechanical machine; it would do numerical integration directly for one. My friend von der Lage finished his metallic sodium calculations for Bethe on it.

Of course after the war, things changed markedly with the large scale electronic computers, incorporating rooms full of racks loaded with vacuum tubes and consuming large amounts of power. Student slide rules, of magnesium as well as the still extant ivory covered wood, got fancier, more versatile, and more expensive. The whole scene changed with the advent of the transistor. Then came the integrated circuit and compact chip technology. The slide rule has folded its tent like the Arab and silently disappeared; the pocket calculator is everywhere, and does practically anything that the room-sized vacuum tube affairs of the late forties could do. Even fancier desk type computers are proliferating. It is natural, with their versatility, their displays, and programming ease, that they make inroads into physics teaching. Not only do they make calculations easier in problem solving, but they can so easily demonstrate

and display the behavior of wave packet propagation, interacting harmonic oscillators, particle trajectories in various fields, and so on. We are just beginning to see the effect of the computer in our science, and indeed the whole of society. It is almost frightening to imagine where the technology will have gone and the place it may hold in another halfcentury. In the department, computers are playing an ever increasing role; theorists use them to model and calculate; experimentalists in data processing; teachers in classroom demonstration and problem solving.

Television too has long since made its appearance in our teaching. Beyond the use of the television camera and video screens distributed about the lecture room for help in demonstration lectures, the use of recording on tape for playback of demonstrations, lectures, and the like is now commonplace. One of the first comprehensive TV roles in physics was the BBC's recording (on film) of Feynman's Messenger Lectures here at Cornell in 1964. They are still to be seen and heard. In the department, this mode is most commonly used in the audio-tutorial Introductory Physics course P101-2. There the student may draw out a tape cassette and sit down with the playback unit and watch Professor Sievers give a beautiful lecture on angular momentum--demonstrations, his inimitable style and all, with the professor coming in and going off the screen on his unicycle. (Unfortunately, because of his display of dexterity, that tape of all in the course is long gone, having been played to extinction as the most popular of all the tapes.)

The same system has also been used to help new assistants (at least one professor could also have benefited) in their teaching techniques. The camera and recording unit are mounted unobtrusively at the back of the room and a tape is made of the assistant's performance. He can then watch himself later, as he was seen by his class. With television, one can wonder whether live teachers will much longer be necessary; let Harvey White or Dick Feynman record their physics courses and distribute them countrywide to all the universities. White did give that early morning TV physics course in "Sunrise University." Not bad either. Perhaps it was recorded.

Over the years, printing the department syllabi, prelims, laboratory notes, and everyday letter typing have gone through a revolution. In the old days duplicating was via the well known mimeograph technique. Miss

Lyons did most of this. The copies were readable, but the print was anything but clean and crisp, the paper anything but smooth. Typing was done on the many "upright" machines around in department offices. Today, the department office operates a small printing plant turning out hundreds of copies of manuals, prelims, etc., on large multilith printing and collating machinery inherited from the Materials Science Center. Duplicating on small scale (and sometimes not so small) is done all over by the ubiquitous Xerography machines of various manufacture, which outmoded the Verifax wet duplicating. Typewriters today are sleek and electric. The word processor, made possible by silicon in the small, solid state computer art, has made its appearance in some offices--LASSP, LNS, MSC, A&EP, and Physics (this text was done on it). In the Physics office, among the electronics, is a relic: a Hammond typewriter of one hundred years ago, the printing characters on a drum being placed by the keyboard sequentially in front of the document being printed, a small hammer then striking the paper from behind up against the inked character. This fine old instrument was found during the recent renovation of Rockefeller Hall, down in a basement storage room where utilities enter the building. It's a true museum piece.

While it was not recognized as such, for years and years the department had a museum--not open to the public, but still a museum. This was embodied in the jumble of stuff in the Rockefeller attic. So much accumulated that it had to be cleared out, and we certainly lost some fine artifacts. Not all was lost, however. With the occupation of Clark Hall, a room became available in Rockefeller in which might be displayed some of the department's more noted pieces. A group of enterprising graduate student wives set out to make a formal museum of It was a nice effort and some things got displayed. But over the sorts. years since, the project was forgotten, and the collection has not made much progress; what changes there are have gone in the negative direction. Still, there are some pieces of interest lodged therein: two coils of Anthony's great galvanometer, some early X-ray tubes, nuclear devices (including the first pulse height analyzer--a mechanical device built by Littauer after a model of Otto Frisch), photographs and the like. It could have been more had we had the foresight and time properly to have supervised attic cleanup. There was another museum piece, active,

which was for a few years installed at the south stairwell of Rockefeller Hall. Stretched from the third floor ceiling to the first floor was a Foucault pendulum. It was not electrically driven and so it decayed in amplitude after starting, but with a satisfactorily long enough decay constant. It showed what Foucault intended and the period of rotation of the plane of vibration came out okay, the co-secant of the latitude times twenty-four hours. It would be good to have it restored in the current refurbishment of the old building. For fire protection, however, the stairwells are to be enclosed.

An imaginative person, however, can make use of even an enclosed stairwell. Clark Hall has two such, extending from the basement to the seventh floor. I went over to Clark one Sunday night late for something or other and there in the south stairwell, piled from the basement to the seventh floor, was a stack of beer cans, one atop the other. They were strung on a wire and made for a spectacular column of beer can lateral dimension. Unfortunately, some spoilsport from safety heard about it and came over and cut it down before morning, the whole thing tumbling with considerable racket, one hopes. It could have at least been left for a few days before disassembly.

Probably because I was one, I have the opinion that in general graduate students worked harder in the depression days than today. At least it seems they populated Rockefeller for longer hours than they do Clark. Times were tough, money was scarce, there was nothing much to do but hang around the building and work. On almost any evening the full complement of students were at work in the laboratory or in their offices or library. Very few were married; when John Cooper showed up as a first-year graduate student with a wife, our wonder was: How could he manage it? She worked, of course, as do many wives today. My view may be jaundiced and wrong; the vantage point has changed, and I am not over there so much at night myself anymore. And there are obvious exceptions, both in Clark and in the Wilson round-the-clock operations. But certainly students are now more commonly married. bringing family responsibilities beyond the laboratory, taking them from it.

On the other hand, it also seems that what a student is expected to know today is far and away more sophisticated and of a higher level than was the case forty years ago. Physics has grown; there is much more now

to know. Student preparation is far more advanced. Quantum mechanics, for example, is now a junior level course, seen already in the sophomore year, soon likely to be taught in high school.

Student attitudes and character would seem to have changed very much. I think there is more awareness these days in what kind of a job, and at what salary, one will end up with; I don't recall any recruiters coming around looking over students about to finish, or our worrying about what we were going to do when we did. I have the feeling, again perhaps unjustified, that one was in physics in the "old" days mostly because it was exciting and fun.

There have always been the few brilliant students around as well as those barely able to make it; those with great personalities and those of more glum disposition. There have been a few who "cracked up" and became mental cases, a few who have threatened professors, and one actual physical attack.

Over many years, some of the early, easy going attitudes in the university have gotten tighter and this has been reflected in the way the department goes about its business. In some way the familial spirit of fraternity has been eroded. Under the action of Congress, a bill sponsored by a New York senator no longer allows us to give out any information on a student--even to his or her family. An assistant professor, perhaps a woman, does not get tenure; sue the university. Several suits are now pending. One almost expects a student to sue the university because he or she has not been properly advised or educated; it has happened elsewhere. Students may exercise their right to see their letters of recommendation held by the department. No bicycles or dogs in the buildings; presumably one might trip over same and sue. We have to be doubly careful of chemicals and fumes, of radiations--from microwaves to gamma rays; no matter how weak, one may claim damage in exposure; sue. Too bad, but society seems to encourage the trend.

On a less serious level, but still a sign of the times, is the virtual disappearance of that early college staple--the Saturday class. It is almost extinct. There is, however, a distressing asymmetry between the M-W-F class and the T-Th class. So the department has hung on to a few of the latter, with symmetry restored with the addition of S. Some professors refuse, however, to teach the Saturday schedule; attendance is

poor, it ruins the weekend trip, it is generally to be avoided at all cost. The day is not far off when Saturday is entirely unused for classes. To achieve a completely civilized schedule, there then remains only the eight o'clock class to eliminate; there has always been reluctance on the part of both student and faculty to engage in scholastic endeavor at that early hour. But if it goes, who then will be around to appreciate the "Jennie McGraw Rag" from the library tower bells?

And while we are bemoaning some modern trends, we may cite the growth in disrespect of others' property. Our buildings these days are commonly defaced by graffiti which is made easily possible with the ubiguitous spray paint can; statuary is painted and otherwise plastered over with announcements of this or that coming event; or stolen if that is Bicycles are not casually parked in racks; they are securely possible. In our own bailiwick, apparatus, calculators, chained thereto. typewriters, and so forth disappear. Professor DeWire has contrasted the appearance outside of Lecture Room A during an examination back in the fifties with what it is today. Back then the corridor at such times would be crowded with coats and books parked all over the floor by students inside taking the examination; it could take some doing to navigate through it all. Today, there is no sign of such. The student knows his belongings are safe only if they be at his side. One wonders if there will ever come a return to the earlier, simpler, and more wholesome ways.

In early 1981 came another sign of the times. Maintenance personnel and those in University food services voted to join the United Auto (!) Workers Union. Credit for this achievement must in large measure be given to a duplicating operator in Newman Laboratory. In itself, this cloud on the horizon may portend no storm; but one is not reassured by the election victory statement, "We'll get a damn good contract out of this." A few of the department faculty joined with about two hundred other faculty members in public support of the pro unionists. The victory is supposed to be followed by effort to unionize various other levels of the university in turn. One hopes (at least one hopes) that it will never come to the faculty; that would seem to be the very antithesis of what a university means. Even with unionization at the lowest levels, one wonders what happens in the event they strike. Do we actively support them? Will departments, including ours, split on it? Will our jobs get

done? It is not a pleasing prospect and we should not cross the picket line until it appears. In matters such as this, however, it would seem obvious that the difference between a university and General Motors is the difference between night and day; in some enterprises it seems to this writer that a union is absolutely inappropriate, at least as we have them today.

Another cloud can be seen in the increasing government awareness of the cost of doing research, particularly that seen as not leading immediately to things practical. The budgetary concern has been heightened at all levels in government funding, except that of the military. It would appear that in such a climate, the support of fundamental research is bound to decrease. What this will mean to our department's welfare and that of physics generally remains to be seen.

To appreciate the growth of the department during the last half century, and to compare the scope of its course offerings, it is instructive to look at the Cornell Announcements for 1934-35 and for 1979-80. On the earlier date, there were eleven men listed as Faculty in Physics. Today (1983-84 Directory) 48 are listed. The old department nevertheless taught a total of 44 courses; today we manage some 60. Granted, in the earlier day, not all courses were given each year; there were quite a few offered only in alternate years. Still, the teaching load would seem to have diminished over the years to a considerable degree. Furthermore, I believe in those days, the professors read their own problem sets; I don't recall any graduate student who worked as a grader. There were probably fewer problems handed out; but that surely wasn't the case in Smith's Mathematical Methods course.

It is a surprise that forty-five years ago there was no course in acoustics, a seeming staple of earlier physics. Today, one would understand that perhaps, but back then one might have expected it. The sophistication of today's advanced courses is obvious from a perusal of the offerings. Less obvious, but it is there, is the greater extent of laboratory work in the courses of the earlier day. The introductory courses for engineers, for example had a weekly lab session in the first year and an alternate weekly session the second year. Today, engineering requires of its students only three semesters of physics, and laboratory meets on alternate weeks throughout. Graduate student requirements are

not spelled out; they are, of course, in the hands of the student's special It used to be so that a year of Advanced Laboratory was committee. required; certainly it was after World War II. For experimentalists, an additional special topics laboratory for one semester was the usual. Today we have the Advanced Lab for one semester only, and not all experimentalists get into a special topics laboratory, but go directly into the research laboratory. Equally serious in my view, is the delay the experimentalist suffers in taking the Advanced Laboratory. For most, this comes after the first year, either during the summer or later. Some manage to get here early and take it during the summer before his first year of work; but that is the exception. The student is so burdened with theoretical courses that the laboratory is seen as not feasible concurrently; that's fine enough for theorists but it would seem that an experimentalist should get started with what he hopes to do.

In one course, there is more laboratory now than earlier; that is in Introductory Physics P101-2. During the sixties, Greisen and his colleagues in the course turned it into the auto-tutorial, self paced format. It is housed in the north end of the third floor (now second floor) of Rockefeller, where the rooms are subdivided into cubicles, each with separate experiment for the student to perform in their learning of They, not the instructors, operate the machinery; in a sense, physics. that's wrong--the student is his own instructor. Extensive use is made of TV cassettes and the video screen. For some students, it must be a great lot of fun; for others it is not so clear. There is some disagreement over the effectiveness of the system overall. The student must pass examinations on a certain number of block units into which the course is divided. He may take examinations on each unit over and over until he passes one, and then goes on to the next unit. Without passing judgment on whether the system is viable or not, the many experimental set-ups would seem to be most inviting.

The health of the demonstration in lectures is good; they continue to be a main feature and strength in the large lectures.

The state of laboratory work in physics instruction at an institution and department which had been innovator in the method, is of particular interest to us, and possibly of concern. Physics is of the real world, we hope.

With the impact of physics on the affairs of men, following the World War II, the need for physics courses aimed at the non specialist became clear. Great national decisions were being made by people with little or no understanding and appreciation of the natural world. Thus there developed, here and elsewhere, courses jocularly titled "Physics for Poets." The first here was given by Bob Wilson. It has subsequently been given by a number of the faculty. During Holcomb's participation in the course, a text of sorts resulted: "My Father's Watch," written in collaboration with Phil Morrison, then at MIT. Holcomb has described it as "a flop--it dropped like a lead brick." The course has proliferated: still for poets, we have today "Great Ideas of Physics," "Reasoning about Luck," "Physics in the World around Us," and "Physics of Musical Sound" (with wonderful demonstrations by Bob Silsbee--just lovely).

There is a trend in the department which should be of growing concern to faculty on both sides of the organization, each for different reasons. In the high energy program, particularly experiment, there is notable decline in the number of graduate students involved, while in LASSP there is growth, perhaps cancerous. This is markedly brought out in recent department bulletins issued each year, largely for new students and for those seeking a thesis professor, listing professors and their research interests and students already working with them. LNS faculty would like more student involvement, those in LASSP are concerned that the number who wish to do theses on that side can not be accommodated. It is certainly not that the high energy physicists discourage student participation, they seek it. Rather, is it not high energy physics that is producing the disparity? No longer does a student there work along with his professor on a nice simple (?) experiment. With immense hardware, the work is done by teams of several professors, some from outside the university, in concert with a large number of research associates, technicians, and graduate students. The individual becomes lost, his own contribution masked, his time not his alone. That can be discouraging. Thus students may elect not to do physics with the great accelerator and giant detector; they seek something in condensed matter experiment, where the individualism is apparent. Further, there is the more apparent job market for students in LASSP. Even in theory, where the disparity is perhaps less easily understood, LASSP students are likely to find their

work of more relevance to job recruiters than is work on the ultimate constitution of the fundamental particles of our universe. Should we coerce students into this or that area? Perish the thought. Or are we to become a department carrying along a High Energy Research Institute with no student participation, its faculty teaching role only in the classroom? That is also unappealing. Further accelerator development would seem likely only to aggravate this troubling situation. The way out of the dilemma is not clear.

Another aspect of this concern lies in the future of high energy physics itself in this country. It is an expensive business, both in the acquisition and in the operation of any high energy machine. The present storage ring may have a useful existence of five or ten years. Then what? At this point it is far from clear that the hoped-for 50 GeV on 50 GeV machine will come to pass. A recent report (the Trilling Committee) summarized in <u>Science</u> suggests that any future high energy developments in this country go to Fermi Laboratory and to the west coast. (Even if CESR II were funded⁵, it too would have a finite lifetime; so still, then what Cornell?) There is no doubt that the high energy program here in large measure is what has given the department the stature it enjoys. So the possible fading away of such a program should be a matter of grave worry for Cornell. Perhaps there will be a gradual move by high energy physicists into other areas of physics undreamed of today. How this will all turn out will be for some other time.

The material on the Physics Department in the Archives of the University Library is random and spotty--mostly not too interesting. For example, there are boxes of Carl Gartlein's stuff; most of it should be thrown out. Clearly, someone just cleaned out his office and moved it all over. There are I9 large boxes of Tomboulian's material, some "classified" (and not perused by me). With some of the files, one feels he is intruding. In the Grantham box is their Wedding Book--Guy Grantham and Margaret Post united in matrimony by so and so, signed by the guests; the signed guest book at his retirement party, some lecture notes, family pictures, newspaper clippings. In the Merritt file, the family scrapbook.

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⁵ As footnoted earlier, CESR II is dead; the Wilson Lab program is not. The storage ring was converted to multibunch operation in 1983. There is talk about another ring (same tunnel) for dedicated synchrotron radiation, and other dreams.

In other boxes one finds department expense accounts, pages and pages filled in for the most part with Al King's neat slanted, crisp handwriting. For the year 1939: February 27, paid to the American Physical Society for a letter to the editor by H. A. Bethe, for the January 1 issue of the Physical Review, "Energy Production in Stars," 200 reprints, \$3.65 plus 14 cents postage; March 9, same year, to the American Physical Society, cost of publishing an article by H. A. Bethe on Energy Production in Stars for the March 1 issue of the Physical Review, \$69; on June 29, to the same outfit, \$8.40 for the same author publishing "On the Meson Theory of Nuclear Forces;" \$2.25 for IOO extra reprints of the same letter plus 14 cents postage; January 2, \$6 to V. F. Weisskopf for transportation from Rochester to give a colloquium. And so on. The hours spent filling the pages of large volumes with this sort of thing! Not to mention all the apparatus, books, various supplies accounted for. No wonder she quit. In the first volume, for 1886-87, the script is cursive, neat, and beautiful; many things purchased from C. J. Rumsey, a fine old style hardware store located where now the Army and Navy store is on the Commons, closed from business in the fifties; some sums paid out in 1904 to L. W. Hartman for preparing notes and apparatus; monthly entries of \$32.50 for DeWitt Calkins, janitor. The fine script went sour in 1890 with someone else of cruder hand taking over. And it stayed sour until Miss King came along; one recognizes her hand from way back. All somewhat interesting to an old-timer but not very relevant to the present endeavor.

Then there is another box loaded with inventories. Department members apparently went through the place yearly, listing everything they could see. That for 1911 lists everything from a 10 HP, Type B, two-phase motor valued at \$1180, to 1 Ice Spud (not in my dictionary) at 50 cents, and one funnel at 5 cents! Pages and pages of such listings! Over the years, we seem to have spun our wheels over and over again. In 1932, the listings came in value to \$289,374.93. It seems the yearly practice died with that one. Who did it all? The department engaged in a number of such time consuming chores, some more worthy than others, largely to no avail. There were the several building renovation studies, space requirement studies, "Needs of the Future" sorts of studies, and so on. One looks at it all, shakes his head in wonder at the time spent with so little thereby accomplished.

Mention of Rumsey's Hardware above brings to mind the stockroom. In the prewar days, this was a simple, one man--old Bill Zeller-operation. The inventory was not great; it carried various grades of sealing wax, twine, some vacuum tubes, glo-coils, and simple hardware. There was more surely--chemicals, acids, and such--but that rather typifies the stock. It was all tucked away in wood pull drawers, not very well labeled, chemicals in a back room, acids in the dank hold under the outside south entrance staircase. Thus it was that Rumsey's was frequently called on right up to the time of its demise for innumerable items of hardware not carried in our own stock. It was a great store, old fashioned, lots of variety, and mostly real hardware, the clerks easygoing, elderly gentlemen. There was one I was especially fond of--rather portly, thinning white hair on top and ruddy bulbous nose, a W. C. Fields type. He had probably handled some of those early purchases alluded to earlier. I apparently shook him with one I made shortly before the store went under. For a personal project I needed twelve feet and five inches of rope, size uncertain. I explained to my friend behind the counter, and he suggested we go below to the cellar to see what they had in stock; which we did. It was dimly lit down there and pretty disorderly, but there were numerous spools of twisted hemp around in various sizes. I found one that would suit; I'd take some of that. "Well, how much do you want?" To be conservative, I suggested that thirteen feet would cover my needs. He cocked his head and over his glasses looked askance at me: "Hey, what are you up to?" Some of us still miss the old emporium; the new, slick, chrome-plated establishments are not quite a substitute.

Of course one did not find electronics at Rumsey's. That sort of commodity was to be found at another downtown business which was much patronized by Physics and other laboratories on the campus. That was Stallman's of Ithaca. He carried a truly wide variety of electronics components, far wider than we could stock. Art Stallman also had a mobile sound system which he rented out to various local events. So he did very well; his widow recently made a very substantial gift to Electrical Engineering in his memory. Others of us remember his yearly calendars featuring undraped ladies in artful poses; many a local electronics expert had such over his workbench to inspire him during the year.

Today the frequent isolated component purchase from such institutions is largely gone. Our stockrooms and shops carry a much wider assortment of goods, both in electronics and hardware, not to mention stationary. After World War II, people came into the department having been exposed to the copious stores of the government and industrial laboratories, and there was no going back to the prewar days. Newman Laboratory had its own abundant stock, getting tied up in some way with Central Stores, which had been established. Tomboulian and I reworked the Rockefeller Stockroom, changing it to an open bin system, which in large measure pertains today in the expanded Clark Hall Stockroom, manned with three or four people, where inventory is now even computer controlled.

The shops too have undergone metamorphosis. The machine shop, which had moved up from the basement to the Rockefeller north wing before my time, still had into the forties an overhead pulley system driving several lathes, a planer, and a mill or two, from a single motive power source, and had but one or two small precision lathes and mills individually driven. Today there is not a planer to be found; we have lathes in all sizes, mills of various sorts, all with digital read out; first in Newman and now Clark, there are one or two computer controlled--put in the material, clamp it up, push the button, and the machine does the rest. The student shops are more than adequate to serve student and staff needs, unlike the days before the war. Out-of-hours instruction is provided in the Clark Hall shop so that some truly first rate "student made" pieces appear these days in physics research set ups.

The professional practitioners of shop craft have, as in every trade, been of uneven performance; there have been artists, real craftsmen, and there have been those less talented. One recalls of the former, old Murphy and Roy Fulkerson, and later Charlie Kellogg; of the other talent, names now forgotten. The artistry is for the most part gone. The emphasis is on precision and serviceability. No more the decorative touches on the pedestal of an optical instrument; square it off, neatly of course, and send it out. No more decorative screws; an ordinary round head, six-thirty-two will do. That is as it should be; efficiency is the order of the day. But something has been lost. Some of the turn-of-the-century, European apparatus was close to art. Patience is a necessity in shop work, and

some of us don't have it. Recalled is the explosion of a volatile young machinist who became so angered at breaking a tap that he flung his machinist's hammer clear across the shop. Fortunately, no one was in its trajectory.

While the metal pump and metal vacuum systems were making their appearance in laboratories before the 1940's, and those that did were usually, if not always, home built, it was almost a foregone conclusion in those days that an experimentalist would get heavily involved with the glass shop. After the war, usage gradually tapered off. A new lathe or so was added, but the glass practice continued much the same: before his fires, the blower indeed still puffed and "paddled" the work into the desired shape. Today that is still the way it goes, but the amount of work in physics has declined to the point where a full-time blower is no longer on the premises. The shop is now gone; the Chemistry blower, however, once our own man, comes over occasionally for our needs. Vacuum systems are almost exclusively, save up in the Advanced Laboratory course, of metal--stainless steel. And they are now all commercially built.

The early experimentalist in electron and ion physics mounted his experiment in a glass tube, which was then pumped, treated, and sealed off, such work being done in a "technical operations laboratory." There one had access to a spot welder, shears, nippers, and jigs to form his parts; a bulky box on wheels, the "tea cart," housing a self excited radio frequency oscillator and four glowing transmitter tubes to feed current through flexible insulated cable to coils of various sorts to heat up metal parts in the glass enclosure for their outgassing--precursor of the microwave oven. No longer does one seal off his physics experiment; it is all done on continuously pumped metal systems, vacuua obtaining some orders of magnitude better than we managed in the earlier day. For leak hunting, the technical operations laboratory now utilizes the modern helium detector rather than our insensitive Pirani detection scheme with its carbon filament lamp as the sensitive element--there were boxes of such lamps in the attic back then, but no longer. Some individual laboratories have Today with the TOL, an MSC facility, there are their own detectors. hydrogen furnaces, evaporators, crystal growing equipment, chemical processing, indeed a scanning electron microscope. For the synchrotron

maintenance and construction, one finds similar specialized facilities to serve those needs in Newman and over at Wilson. In both laboratories, there are electronics technicians to build and service electronics apparatus, of which there was so little in prewar times that the individual worker could expect to build and service his own gear. We have become "big time."

In the course of time, space seems to have been a problem for the department, more often than not to be relieved only temporarily by the occupation of new quarters, Franklin Hall, Rockefeller, and Clark. In 1981, fifteen years after occupying the commodious latter structure, we again found space to be a limitation; area on loan to Science, Technology, and Society is sorely needed for offices and may need recalling; research space is at a premium in the basement. Professor Parratt, who in his Cornell career has been chairman of three building committees, has had occasion to look into measures proposed to alleviate this perennial problem. His account is summarized in what follows: In January 1931, the predicted quarter century after Rockefeller Hall occupancy, Professor Merritt submitted a memorandum to the administration which stated that "The present structure has outgrown its usefulness for reasons having to do with capacity, safety to life and property from fire risk and use of high potential. It is also obsolete from the standpoint of arrangement and character of construction and mechanical equipment." He presented the then current needs, allowing for but little growth. He needed a building of about double the floor space of Rockefeller, a little over one-fourth of it for research; the rest was for teaching and support facilities! That apportionment is surprising in view of Nichols' earlier expression for the need of research space. The administration responded by directing that plans be prepared. Architects Ackerman and Associates went ahead; Rockefeller would be replaced in two stages by a grand edifice costing upward of two million dollars. As Parratt has said, the design was probably too grand, for no money was forthcoming. A situation on Wall Street occurring a few years earlier may have had more to do with it.

In 1935, Ackerman and company would draw up plans for a small structure to be part of their planned building but separated from it. This would house high-powered radio frequency oscillators for a cyclotron and a linear accelerator. This would be in the Gibbs' chairmanship, yet to
come, when nuclear physics is about to become more than the scattering of naturally occurring radioactive products. Parratt cites the nationwide shift in academic emphasis to include research as something more than merely incidental to undergraduate teaching, and he cites the department's being right in tune with that change. If such diversity of physics was to be carried on concurrently, a separate structure for sources of electrical disturbance was seen as essential by those in low current measurements. Recall Nichols' worry about locating Physics too close to the trolley line. Rockefeller Hall was demonstrably unsuitable for housing both activities; low current measurements are tough enough at best. Again, however, there was no money.

Continuing, still further along in time, in 1945, near the end of the great war which the previous one was supposed to have made impossible, there would be another building plan. New architects, Skidmore, Owings and Merrill, would be engaged. Parratt indicates that plans were drawn but that none of any sort (or price tags) are in the department files; it was to be a structure about like Ackerman's. Nuclear physics was excluded since a new building would finally be in the offing for that burgeoning field. But by this time there would be a new discipline--Engineering Physics-to be based and commingled with Physics. The whole organization was getting more complex. Nuclear people did get their building soon with plans drawn up by Skidmore, etc. Following occupancy of that building, in late 1947, with a committee chaired by Parratt, the department undertook another study of immediate needs. On the basis of careful estimates and documentation embodied in a long report filed in our own Archives, a conservative set of specifications was drawn up for a new structure. Parratt is of the view that we had previously exaggerated our needs heedless of growing construction standards and costs. But nothing came of this effort beyond some guidelines for future planners.

The only other building project considered before Clark Hall finally appeared on the scene was a study undertaken during Corson's chairmanship for some renovation of Rockefeller under architect Fred Wood. By this time a basement corridor had been taken over for research space, a stairwell occupied, the old student shop and part of the north basement carpenter shop commandeered. Things were indeed tight. The

one thing that was achieved was a reorientation of Lecture Room B, a counter-clockwise twist of 90° looking down on it. What shortcoming in the building this corrected is not clear. The room is now deeper than it is wide rather than vice versa.

During the course of time, a number of famous physicists have been attracted to the Cornell department for varying periods of time beyond the single day visits of innumerable greats brought here for colloquia and seminars. Many have been cited already. The Andrew D. White Professorat-Large Program serves well in the regard. It has brought periodically to campus Robert Schrieffer of B-C-S fame (theory of superconductivity), stimulating Sir Fred Hoyle (astronomer), and Pierre-Gilles de Gennes (percolation and polymer physicist). For a year or semester, there have been visiting appointees not previously mentioned and whose names come to mind. There was Otto Frisch, now deceased, who spent a year here with his wife a few years after the war which he indirectly played so large a part in bringing to an end. We had likewise Herwig Schopper, until recently director of DESY in Hamburg and now director of CERN; Arnold Nordseick from Illinois, like Morrison a one-time student of Oppenheimer, here to replace Bethe during a sabbatical leave; Herman Hoerlin, photographic expert and Los Alamos diagnostician of atmospheric nuclear explosions, known also for several, first, Andes mountain ascents; Missouri's Albert Eisenstein, expert in electron emission, who came to work with Sproull, died here unexpectedly the morning following a department Christmas party. There was Seishi Kikuchi in LNS, a good golfer, but known for his work in early electron diffraction. Known also for early work in diffraction, but in X-rays, was Paul Ewald, now an Ithaca resident. While not holding official appointment, he has been nonetheless a frequent visitor, particularly at department seminars and colloquia. Like his son-in-law, Hans Bethe, he is holder of the Max Planck medal, two of these yearly awards in the family. At this date, his participation has ended; he is in his nineties, rather frail, his mind nonetheless sharp. In his late eighties he gave us a colloquium on the early days of "modern physics." David Bohm, of hidden variables in quantum mechanics, spent a summer here in the fifties. Brian Josephson spent a more recent summer here before his Nobel award. There were the notables who came in the ten-year experiment of Professor Gibbs, names we have listed. There

must have been others. Finally may be cited again the name of Paul Dirac, father of relativistic quantum theory, which for hydrogen Robley Williams' data did not quite fit. Dirac had a relative living up at Interlaken and made visits to the territory on occasion. On one such, he expressed to his host the desire to go through Wilson Laboratory. A visit was arranged and tour carried out, John DeWire and Ken Wilson conducting. The event is chiefly of interest for what followed; he wanted to go over to the Science Library in Clark Hall. It seems he has often been quoted as having written that Chemistry was no more than the application of Schrodinger's equation. But he could not recall where he had possibly ever written that and wanted to check the literature. Ken took him over but their search was fruitless. It's a nice comment, however, notwithstanding their lack of success. So much for famous visitors.

The faculty members of the department have had a wide range of avocations beyond physics. Bob Wilson has real talent in sculpture and architecture; Sievers in pottery; Corson in photography. Music has its adherents: one thinks of Smith and Littauer (piano) and Silsbee (violin). Bethe goes in for stamp collecting. But athletics seems to attract the most; graduate students field intramural teams. Interest in the college events is strong. Jogging, the "in thing" these days, attracts many. Lee and Reppy take it seriously, running miles each day, Reppy appearing creditably in the Boston and local marathons. Orear and others climb. Parratt plays tennis assiduously. Skiing and sailing are frequent activities of others. Littauer glides, Stein flies, as did Smith. All not atypical of what the general populace enjoys and does.

Thus, the Department of Physics at Cornell and some of my recollections centered around it and physics. It is seen to have been a strong department from the beginning; that it has had a major impact in physics in this country and is likely to play a significant role in the next half century is evident.

For myself, it has provided a pleasant, rewarding, and long association. It is a regret that I have not contributed as much to its welfare as it has to my own.

ACKNOWLEDGMENTS

Whatever the value of the foregoing pages, I want to thank Hans Bethe, Doug Fitchen, Dale Corson, Neil Ashcroft, Lyman Parratt, TungMow Yan, and A. W. Laubengayer, for taking valuable time to read a preliminary draft of the text, making corrections of fact and grammar and suggestions as to content. Not all of the latter have been taken. Thanks go to Don Morey and Lauritson Taylor for written material on some of their own recollections. The contributions of many others in conversation are obviously many and impossible to acknowledge individually. A collective appreciation to all. Thanks to L. Pearce Williams of History for making available to me a draft of Jeanette Lurier's paper on the department. The help of people in the University Library Archives and Gould Colman in particular is gratefully acknowledged.

I greatly appreciate the help of Jane Pedersen and others in the Physics office and of Anne Kingsley in the EP office who have all been instrumental in getting the text edited and reproduced on the Physics and EP word processors--a great saving in my not having to type it all over one more and final time.

Last, but by no means least, my deepest appreciation to all the characters therein, mentioned and unmentioned, past and present, for their devotion, leadership, foresight, and professionalism, which have made the department what it is; and for the personal associations which have meant so much to me and which have created my own fond feelings for the department and Cornell.

APOLOGIA

In working over the manuscript, I have come to recognize how woefully lacking it is as history, as writing, and as much else. Bad as it is, I can hope that those who have been, are, or will be associated with the Cornell Physics Department, and into whose hands it may happen to come, may find some of it of interest and, here and there, of perhaps some amusement. Any resemblance to characters, living or dead, is of course intentional; I sincerely hope that no one has been maligned or unfairly treated.

POST SCRIPTUM

This history was printed in a few dozen copies and was read and commented upon by several people. Some errors have been corrected and additional facts connected with the department have been pointed out; they might well have been included. In a separate department repository such items should be filed so that in another century someone can put together another volume of department history if it seems worthwhile. Before a final printing of the present writing, however, a few worthwhile additions should be included.

In a 1983 book <u>The Rosenberg File</u> and in an article in the <u>New York</u> <u>Times</u> (July 26, 1984) much more is revealed about our possible Communist agent than in my paragraph (pp. 218). The devious character in question was Alfred Sarant. He did, as we have noted, disappear suddenly with the wife of a Physics graduate student. What we did not note was that the disappearance occurred shortly after the arrest of Julius and Ethel Rosenberg, later executed for presumed spy activity connected principally with the atomic bomb. A day after the arrest the FBI from New York City came to Ithaca and accused Sarant of keeping an apartment in New York's Village for espionage activity, an apartment that had been

frequented by Rosenberg. An electronics shop "front" figured in the Village doings. The vanished wife was Carol Dayton, a pleasant person. She left her husband and two children, even as Sarant left his wife and Together the pair presumably made their way, apparently two children. via Mexico, to Russia. The <u>Times</u> article details how it was deduced by a Russian émigré and science historian that a noted Russian electronics expert, Fillip Staros, and Alfred Sarant were one and the same person. Staros was important and very successful over there but seems to have come into some bureaucratic disfavor, being demoted to a remote post in Vladivostok. He died in 1979 and Mrs. Dayton (Staros?) is supposedly in Czechoslovakia, where she has been visited by a Sarant son, if local report is correct. The Rosenberg File describes some connections between Sarant and Julius Rosenberg, such as Rosenberg's hasty automobile trip to Ithaca to see Sarant, the Village activities, the presence of a person at Cornell where he could be close to such as "Bedda" (Bethe) and "Morris" or "Morrison" (our Phil). In neither report is it in any way proved that Sarant was a spy, but it certainly is not a frivolous conjecture to believe that he was. And that Rosenberg was.

Mention was made (p. 263) of two fine oak trees sacrificed for Clark Hall. The dismemberment and felling of the first of these (an almost obscene operation) brought a response from F. C. Steward, Botany professor and director of the Cell Physiology Laboratory. He wrote in the Cornell Daily Sun (November 15, 1962) a rather moving piece, "Requiem for a Tree." He was not the only one saddened by the loss. The "Requiem" was recollected by the Cornell Chronicle of October 22, 1970, when the last Clark Hall oak suffered its demise, now replaced by the Corson oak. Ironically perhaps, Steward wound up with his laboratory in Clark Hall for a number of years. The lab was eventually moved out; he retired and now works at the University of Virginia. To this day, however, a small section of the second floor of Clark is given to Biology, space being provided for a portion of the Section of Molecular and Cell Biology in the Biochemistry Department. Steward could grow a whole carrot starting with a single cell of a previous carrot. Not bad, but I suspect that seeds are easier.

In the summer of 1984, a couple of Chemistry graduate students rummaging around in Rockefeller attic (what were they doing prowling around in <u>our</u> domain?) happened upon some old letter files filled with

early Physics Department correspondence dealing very much with the Physical Review (so it was a worthwhile prowl!) Indeed, the earliest file, dated 1893, contains letters pertaining to the then proposed journal. One of the earliest was from Providence and our first department head, Eli Blake. After some discussion on the silvering of mirrors, he expresses interest in the new journal -- "the era of specialization is on us," he notes. (Previously, physics was mixed in with mineralogy, fossils, geology, etc., in the American Journal of Science, published at Yale, "James D. and E. S. Dana, Proprietors.") From Professor Ayres, Tulane Physics and Electrical Engineering, came the comment that Nichols was just the man "to carry off the enterprise." A handwritten letter from Angstrom to Nichols accepts a request to publish (and translate) a paper in "the journal about to start"--he wishes every success. One from MacFarlane at Texas was pleased to learn of the new journal--he offers to review books on mathematical physics. Later, he's reading Heaviside's Electrical Papers; he hopes for "not a mere description of the books but a critical notice, especially of his system of vector analysis." There are many letters to and from MacMillan who was to publish the journal--send us a list of editors and others to whom to send complimentary copies; with a retail price of \$3 yearly, would not a net price to the trade of \$2.40 be all right? That would include postage! What size pages? Weight of paper? Trimmed edges or uncut? In later years there are innumerable exchanges with MacMillan and with the New Era Printing Company, Lancaster, Pennsylvania, printers of the journal: Why is this cut not present? When is that contributed paper coming to us? One from T. C. Mendenhall apologizes for his tardiness in a response but he's just back from "a sort of lion's tail twisting trip" to Montreal; he thinks he "gave the aforementioned appendage an additional torque (!) of a half turn or so"--he is very much in favor of the journal but worried about the name. "Physical" sounds too gymnastic and out-of-doors. Congratulations came from MacGregor at Dahlhousie: "The study of physics has been advancing in America at such leaps and bounds in recent years--there must be sufficient intellectual backing." From E. F. Nichols to E. L. Nichols comes the "third proof of the article for the Review"; he wishes not to have the word Professor used in front of his name unless it is to be a Review custom; he hopes the custom will be different. And so it goes. Letters

later from Millikan, Steinmetz, Lorentz, Pupin, Morley, Michelson, R. W. Wood, and even our William Anthony, by then five years gone from Cornell.

In many cases during all the years, it is not clear always who at Cornell is responding to the correspondence, but it must have been Merritt and Bedell mainly. Some correspondence is purely technical: Franklin has sent Nichols twenty standard candles--"there is no charge." The new Lummer-Brodhun photometer he has acquired "is a beauty." George Ellery Hale inquires of Merritt as to his galvanometer and his bolometer materials--Hale finds the "smallest Swiss watch hair springs to be the best." A letter again from E. F. Nichols, then in Germany, to Merritt includes a drawing showing the interference mirrors of Boltzmann, quite modern and of more than passing interest to our Professor Sievers, whose own present-day research has utilized essentially the same technique. From the GE Lamp Company in Harrison, New Jersey (not yet Nela Park, Cleveland) to Merritt, a letter on his Edison Effect curves taken on some of their lamps. A curt, carping letter from Brown University: As apparently requested by the <u>Review</u>, the writer had sent in a notice and photograph of Professor Blake (has the latter died?). At the time of the writing, he has not received a copy of the <u>Review</u> or any evidence it was even published. "Yet, free of charge, I expended both time and money in getting my copy ready--your part of the task ought to be attended to." From J. Willard Gibbs to Bedell: he can't undertake the review of Nernst in Palmer's translation. A letter from Manhattan, Kansas, in handwriting recognized before seeing the signature of the writer: L. W. Hartman to Merritt. An earlier one from Franklin to Merritt congratulating him on his coming marriage, wishing happiness. A different letter: one to Merritt from a student sojourning in Paris; he's received only 54 percent in his Physics course, passed only nine hours; trouble. And a couple of last ones: from Henry Lomb of Rochester's optics firm, Bausch and Lomb, declining an invitation to write a review on optics; he would like to do it, but it would be poor, he having had no college education. But his son could do it; all right? And in December 1894 a letter from our Nichols to Merritt from Berlin, where he is on leave and enjoying it: "--delightful and most of all the music. . . . I am fascinated with Planck as lecturer and like Reubens very much in my laboratory work." Two months later he reports heavy

snows in Rome, Sicily is blockaded, in Algiers (!) the railroads are stopped. Some winter.

Copies of the <u>Review</u> did not always reach the customer. In 1910 W. A. Bragg wrote that he was not receiving his copies; and he wants a letter published. E. Rutherford was also missing some copies; he'd "be much obliged" if they can be sent.

All in all, a nice discovery by our Chemistry colleagues.

One of the interesting items in the find was a bound book of 1000 numbered, tissue-thin pages, on each of which was a copy of a letter written in the department during the period in which it was used, September 1894 to May 1896. The typewritten copies are all in blue print, those handwritten, and signatures, come black. "The Standard Letter Copying Book" "Highest Grade of Excellence." Not all the correspondence therein relates to the <u>Review</u>. The method of reproduction is not clear:

"To obtain a Perfect Copy, place a sheet of Oiled Paper under the leaf of Copying paper, dampen the leaf uniformly, remove the surplus water with blotting paper, lay on the letter and over it a sheet of Oiled Paper and put the Book in the Press under quite tight pressure for a few seconds.

"The paper will Dry Perfectly Smooth if the Book is Replaced in the Press, under some pressure after copying. Leave the oiled sheets between copies to prevent setting off."

The copies, and order which results, are rather neat, if the process itself remains somewhat obscure.

Like their professor, physics graduate students have not remained aloof from societal concerns. In the mid-forties a group of physics graduate students successfully persuaded the University to remove questions of race and religion from our application forms--the first lvy League institution to do so, according to Jane Faggen, a member of the interested group. She also "implicates" as part of the small group, Bethe, Lenny Jossem, and John Trishka, my former "housemate" down on Williams Street. And in about 1957, the enterprise of a Physics graduate student, John W. Taylor, had the result which even today favorably affects graduate students throughout the university. The matter hinges on the income tax paid by graduate students to the Internal Revenue Service. Taylor reasoned it was all well and good that he should pay income tax during his early years here when he was paid as a teaching assistant. But when one

leaves that position and becomes a graduate research assistant, things change; he is then receiving remuneration for working on his thesis. It seemed to Taylor that when this happened to him, he became a fellowship holder and so could be relieved of income tax payment. A visit to the university counsel to inquire of the situation drew only a negative response. So it was for John off to the Law School library and a day with the law books. He became convinced that he was right and he managed to convince the university counsel, who successfully pursued the matter. Thus it came about that graduate research assistants are today relieved of income tax worries from that source of remuneration. They should know the name of John W. Taylor and be thankful to him.

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BRIEF CHRONOLOGY (APPROXIMATE) OF HIGHLIGHTS

- 1865 Cornell University Charter.
- 1866 A. D. White named first president.
- 1867 Eli Blake in Physics chair.
- 1868 Cornell open for business---Physics set up in Morrill Hall.
- 1869 Physics moves to "wooden" building with Chemistry over near future Goldwin Smith Hall.
- 1870 John Brown succeeds Blake in Physics chair.
- 1871 Francis Loomis succeeds Brown in Physics.
- 1872 William Anthony comes to Cornell to take over Physics---Real beginning of department---Physics moves west to McGraw Hall.
- 1873 Physics moves from McGraw to White Hall---Carver named as assistant to Anthony.
- 1874 George Moler replaces Carver.
- 1875 Famous dynamo construction.
- 1881-2 Franklin Hall construction.
- 1884 Department moves with Chemistry into new Franklin Hall---Great tangent galvanometer constructed.
- 1885 Electrical Engineering (proposed two years earlier) set up as part of Physics. White resigns---President C. K. Adams takes over.
- 1887 Anthony resigns in favor of industry---Nichols named to head up Physics.
- 1889 Merritt is instructor.
- 1891 Chemistry moves into Morse Hall leaving Franklin to Physics.

- 1892 J. Gould Schurman becomes Cornell president. Merritt becomes assistant professor---Bedell takes Ph.D. in Physics, first to be granted, later named instructor.
- 1892-3 <u>Physical Review</u> founded at Cornell by E. L. Nichols.
- 1899 American Physical Society formation in New York---Merritt begins long stewardship in society.
- 1900 Nellie Lyons begins fifty-seven years of service to department.
- 1906 Rockefeller Hall occupied.
- 1906-7 Richtmyer and Gibbs both are instructors.
- 1916 Morse Hall burns---Chemistry homeless---Rockefeller Hall attic sequestered for Chemistry laboratory courses.
- 1917-8 World War I.
- 1919 Nichols retires---Merritt new department head.
- 1921 Livingston Farrand becomes new Cornell president. Baker Laboratory dedicated---Chemistry home at last.
- 1925 Total solar eclipse in Ithaca---lots of activity.
- 1926 Lorentz spends a semester with the department.
- 1931 Lloyd Smith is assistant professor.
- 1933 Decision made by department to go into nuclear physics.
- 1934 Gibbs becomes department chairman---Livingston comes on as instructor---cyclotron construction started.
- 1935 Hans Bethe arrives as acting assistant professor---Merritt retires.
- 1936 Bacher arrives as instructor.
- 1937 Ezra Day becomes Cornell president. Bethe's carbon cycle for stellar energy production.

- 1941-5 World War II---Department decimated.
- 1943 First (?) contract from a government defense department to a university for work at the university taken on.
- 1945 War ends---Smith department chairman---Laboratory of Nuclear Studies established; Bacher, director---Engineering Physics established under Smith.

| | 1951 | First class graduated. |
|-------------|------|--|
| | 1955 | Cuykendall, director. |
| | 1962 | Marriage with MS&E under John Howe. |
| Engineering | 1965 | Return to normal. |
| Physics | 1968 | Norman Rostoker, director. |
| | 1970 | Rostoker goes westJohn Silcox, director. |
| | 1974 | Bob Batterman, directorCuykendall retires. |
| | 1979 | Silcox, director, again. |
| | 1983 | Watt Webb, director. |

- 1949 Funding for 300 MeV synchrotron by ONR---Newman Laboratory authorized---Bacher to Atomic Energy Commission---Corson acting director of LNS.
- 1947 R. R. Wilson named director of LNS.
- 1951 Deane Malott becomes Cornell president.
- 1952-3 Construction of 1.1 GeV machine at LNS.
- 1956 Smith resigns and leaves Cornell after two five-year terms as chairman---Corson named to position.
- 1959 Corson tapped for Engineering's deanship and beyond---Laboratory of Atomic and Solid State Physics set up; Sproull, director--Parratt named department chairman---Low temperature physics starts under David Lee.
- 1960 Cornell acquires one of ARPA's Materials Science Centers---New building in the works---Sproull named first MSC director---Krumhansl LASSP director.

| | 1963 | Sproull to ARPA in WashingtonSack named director. |
|-----------|----------------------|---|
| Materials | 1965 | Move offices into Clark Hall. |
| Science | 1967 | Sack steps downR. Hughes named director. |
| Center | 1972 | Funding taken over by NSF. |
| | 1974 | Hughes to NSFH. H. Johnson named director. |
| | 1984 | R. H. Silsbee named director |
| Center | 1972 1974 1984 | Funding taken over by NSF. Hughes to NSFH. H. Johnson named direct R. H. Silsbee named director |

- 1962 2.3 GeV machine constructed at LNS.
- 1963 James Perkins becomes Cornell president.
- 1964 Holcomb becomes LASSP director---Contract signed for funding of Wilson Laboratory and 10 GeV synchrotron.
- 1965 Clark Hall occupied by Physics and others.
- 1966 Construction of 10 GeV and Wilson Lab under way.
- 1967 Bethe receives Nobel Prize---Wilson leaves Cornell for Batavia and future Fermi Lab---McDaniel named LNS director; DeWire, associate director.
- 1968 Chester takes over LASSP directorship.
- 1969 Dale Corson becomes Cornell president. Holcomb named department chairman.
- 1974 Littauer named department chairman---Silsbee becomes director of LASSP.
- 1977 Frank Rhodes becomes Cornell president. Littauer steps down as chairman in favor of storage ring---Fitchen named department chairman.
- 1979 Real renovation of Rockefeller Hall under way---Ashcroft named LASSP director .
- 1980 Storage ring operational---e+-e- collisions observed.
- 1981 Superconducting solenoid for CLE0 installed---Liquid helium plant for LASSP installation under way.

- 1982 Ken Wilson receives Nobel Prize---Holcomb returns to front office as chairman .
- 1983 First woman professorial appointment in Barbara Cooper's assistant professorship .
- 1984 Mermin named LASSP Director; Berkelman becomes LNS Director as McDaniel steps down.
- 1986 Holcomb finishes as Chairman, replaced again by Fitchen; microkelvin laboratory finished and dedicated.
- 1991 Fitchen finishes his second term and is replaced by Kurt Gottfried.

PHYSICS FACULTY (ASSISTANT PROFESSOR AND HIGHER RANKS) CHRONOLOGICALLY ARRANGED (DATES AT LEAST CLOSE)

Symbols: R, resigned; D, died in service; E, became Emeritus; AP, Assistant Professor. Institution where Ph.D. earned indicated in parentheses.

Active Years

- 1867-1870 R <u>Blake</u>, Eli W.--Our first Physics Department; in at the first days of Cornell; went to Brown University after three years.
- 1870-1871 R Brown, John J.--Second Department try.
- 1871-1872 R Loomis, Francis--Inactive because of poor health.
- 1872-1887 R <u>Anthony</u>, William A.--Real founder of department; applied electricity; pioneer with early generator, giant galvanometer; strong on laboratory and lecture demonstrations; established E.E.; recommended Roberts to head Ag School; went to industry and thence to Cooper Union.
- 1875-1917 E <u>Moler</u>, George S.--Partner of Anthony in famous DC generator; general all-around man in department for near first half century.
- 1887-1919 E <u>Nichols</u>, Edward L. (Göttingen)--Department head and successor to Anthony; worked in fluorescence; founder of <u>Physical Review</u>; great influence on department future and American physics generally through many people trained here.
- 1889-1935 E <u>Merritt</u>, Ernest G.--Department head, succeeding Nichols; long time partner of latter in fluorescence work; radio propagation; first and many years secretary of American Physical Society.
- 1892-1937 E <u>Bedell.</u> Frederick (Cornell)--Our first Ph.D. in Physics; applied electricity, particularly AC; stabilized oscilloscope; hearing aids; early editor of <u>Physical Review.</u>

- 1894-1922 D <u>Shearer.</u> John S. (Cornell)--X-rays and radiology; radioactivity; high and "low" temperature work; supervised liquid N2 plant and large introductory courses.
- 1901-1919 R <u>Blaker</u>, Ernest (Cornell)--Initiated work in advanced laboratory; tough man to sophomore engineers; went to Goodrich.
- 1906-1946 E <u>Gibbs.</u> R. Clifton (Cornell)--Department chairman succeeding Merritt; spectroscopy, hydrogen fine structure, isoelectronic sequences; started department on its nuclear and modern way; held fort during World War II; went to National Research Council.
- 1906-1939 D <u>Richtmyer</u>, Floyd K. (Cornell)--Big X-ray man; photoelectricity; prolific in outside activity; editor, <u>Review</u> of <u>Scientific Instruments</u> and <u>Journal of Optical Society of</u> <u>America</u>; catalyst for sponsorship of National Geographic Society aurora program; first modern physics (and text) at Cornell; dean of graduate school.
- 1909-1952 E <u>Murdock</u>, Carleton C. (Cornell)--X-rays in crystallography; long-time teacher of electricity and magnetism; dean of faculty.
- 1909-1912 E <u>Howe</u>, Harley E. (Cornell)--Long time teaching of nonengineers.
- 1910-1940 E <u>Trevor.</u> Joseph E. (Leipzig)--Physical chemistry; thermodynamics, came over to Physics from Chemistry Department.
- 1912-1927 R <u>Bidwell</u>, Charles C. (Cornell)--Thermal and electrical conductivity of metals; went to Lehigh.
- 1913-1946 R <u>Kennard</u>, Earle H. (Cornell)--Quantum mechanics, electricity and magnetism; went to Taylor Model Boat Basin and its hydrodynamics.
- 1918-1947 D <u>Collins</u>, Jacob R. (Cornell)--Infrared spectroscopy, optics generally; theory and experiment; versatile.

- 1919-1920 E <u>Grantham</u>, Guy E. (Cornell)--Long time teaching of freshman 1928-1955 engineers; effective demonstrations; also member of EP faculty.
- 1923-1926 R <u>Tucker</u>, Forrest G. (Chicago)--Photoelectric effect; went to Oberlin.
- 1927-1956 R <u>Smith</u>, Lloyd P. (Cornell)--Department and EP chairman, establishing latter school. Physical electronics; ion sources and linear accelerator; quantum theory continuous X-ray spectrum; mathematical methods for physics course; experiment and theory; pushed Bethe for department position; department chairman for ten years after World War II; went to Avco, then points west: S.R.I., D.R.I., and so forth.
- 1930-1932 R <u>Barton</u>, Henry A. AP (Princeton)--Electron and ion impact in X-rays and ionization; went to the American Institute of Physics.
- 1929-1967 E <u>Barnes</u>, Leroy L. (Cornell)--Mass spectroscopy in metal ion emission; medical and biophysics; premedical student advisor for a long time.
- 1934-1938 R <u>Livingston</u>, M. Stanley AP (Berkeley)--Constructed and made work the original cyclotron; cyclotron and machine development; nuclear physics; went to MIT and thence to Brookhaven.
- 1935-1949 R <u>Bacher</u>, Robert F. (Michigan)--LNS, first director. Spectroscopy and nuclear physics; took on cyclotron after Livingston left; went to AEC as first science member, thence to Cal Tech.
- 1935-1975 E <u>Bethe</u>, Hans A. (Munich)--LNS. Theoretical physics generally all over the map; two <u>Handbuch</u> tomes (solid state, Q.M. of one and two electron atoms, revised by Salpeter); <u>Review of</u> Modern Physics nuclear "bible" series; energy production in stars; nuclear structure; nuclear power; Nobel Laureate; much public service.
- 1935-1973 E <u>Parratt</u>, Lyman G. (Chicago)--LNS and LASSP. Chairman for ten years following formation of LASSP; high resolution Xray research and instrumentation; absorption edge structure; much concerned with pedagogy.

- 1940-1964 D <u>Tomboulian</u>, Diran H. (Cornell)--LASSP. Soft X-ray spectroscopy and instrumentation; electron distribution in metals; synchrotron radiation; teaching of sophomore engineers.
- 1940-1946 R <u>Rossi</u>, Bruno (Bologna)--Cosmic ray instrumentation and research of importance; went to MIT.
- 1945-1981 E <u>Newhall</u>, Herbert F. (Cornell)--Mass spectroscopy on oxide coated cathode products; principal interest in teaching of underclass courses, educational TV, role of computers in teaching.
- 1946- <u>Greisen.</u> Kenneth I. (Cornell)--LNS and Astronomy. Cosmic ray research, high altitude balloon flight, extended air showers; audio-tutorial teaching; dean of faculty.
- 1945- <u>McDaniel.</u> Boyce D. (Cornell)--LNS and director. Low energy neutron spectroscopy on early cyclotron; synchrotrons and storage ring development; accelerator and particle high energy physics.
- 1945-1950 R <u>Feynman</u>, Richard P. (Princeton)--LNS. Quantum electrodynamics, Feynman diagrams; rotons; Feynman trio of texts; share in Nobel Prize; went to Cal Tech.
- 1946-1959 E <u>Corson</u>, Dale R. (Berkeley)--LNS and A&EP. Nuclear physics; astatine discovery; energy loss measurement in synchrotron; design in 300 MeV machine; went to administration and Cornell presidency, token member of department thereafter; chairman for part of term.
- 1946-1983 E <u>Hartman</u>, Paul L. (Cornell)--LASSP and A&EP. Physical electronics; radiation in solids; synchrotron radiation; commitment to Advanced Laboratory course.
- 1946-1965 R <u>Morrison</u>, Philip (Berkeley)--LNS. Theory interests all over the place in high energy physics, astronomy, intelligence in space; Scientific American book reviewer; Babson Gravity Prize Essay with Cocconi; went to MIT.
- 1946-1963 R <u>Sproull</u>, Robert L. (Cornell)--LASSP, first director. Properties of oxides in thermionic emission; phonon physics;

defects in nonmetallic crystals; first director MSC; went into administration, subsequently to the University of Rochester, where now president.

- 1947- <u>DeWire</u>, John W. (Ohio State)--LNS. Design and work on 300 MeV synchrotron and subsequent machines; electron and meson high energy physics; associate director LNS for many years.
- 1947-1967 R E Wilson, Robert R. (Berkeley)--LNS, long time director. Cyclotron focusing; high energy machine design par excellence; high energy physics experiment; early Monte Carlo shower calculation; went to Fermi Lab as director and chief architect; named professor emeritus later at Cornell; received the Fermi award in 1985.
- 1948-1952 R <u>Baker</u>, Charles P. AP (Cornell)--LNS. Nuclear physics; cyclotron and arc source development; low energy neutron spectroscopy; went to Brookhaven and its cyclotron.
- 1948-1954 <u>Krumhansl</u>, James A. (Cornell)--LASSP and term as director. 1959- Theory of atomic motions in condensed matter; nonlinear dynamics of lattices; properties of alkali halides, solitons; period at NSF, director Physical Sciences.
- 1948-1982 E <u>Woodward</u>, William M. (Princeton)--LNS. High energy photon interactions, machine development; long battle with ill health, taking early disability leave before emeritus status attained.
- 1949- R <u>Cocconi.</u> Giuseppe (Milano)--LNS. Cosmic ray and high energy physics experiment; Babson Gravity Prize Essay with Morrison; went to CERN.
- 1950- <u>Salpeter</u>, Edwin E. (Birmingham)--LNS and Astronomy. Quantum theory of atoms; quantum electrodynamics; nuclear theory; energy production in stars; astrophysics generally.
- 1951-1956 R <u>Moore</u>, Frank AP (Princeton)--Soft X-ray spectroscopy; went up to Clarkson.
- 1951-1953 R Dyson, Freeman J. (Cambridge, B.A.)--LNS. Quantum electro-

dynamics, Schwinger vs. Feynman approaches; space colonization; rocket propulsion; went to Princeton Institute for Advanced Study.

- 1952- <u>Silverman.</u> Albert (Berkeley)--LNS. High energy physics experiment; machine and detector development.
- 1953-1958 R <u>Overhauser</u>, Albert W. (Berkeley)--Solid state theory; nuclear polarization via electron polarization; spin density waves; gravity and quantum theory; went to Ford Motor Company, thence to Purdue.
- 1954- <u>Holcomb.</u> Donald F. (Illinois)--LASSP, term as director. Department chairman, one-plus terms; NMR; spin resonance in solids; metal-insulator transition; conduction electron systems in metals, alloys, and metallic compounds.
- 1957-1962 R <u>Bradley</u>, Richard C. (Berkeley)--LASSP. Mass spectroscopy; went to Colorado College.
- 1958- <u>Silsbee</u>, Robert H. (Harvard)--LASSP, with term as Director. Channeling in crystals; magnetic resonance (ESR) in solids; radiation damage; optical properties, point imperfections, conduction electrons.
- 1958- <u>Cotts</u>, Robert M. (Berkeley)--LASSP. Pulsed and cw NMR; atomic transport in solids.
- 1958- <u>Kinoshita</u>, Toichiro (Tokyo)--LNS. Quantum electrodynamics; anomalous moment of electron; gauge field theory; elementary particles; symmetry laws.
- 1958-1960 R <u>Brout</u>, Robert (Columbia in Chemistry)--LASSP. Statistical mechanics; went to Universite de Libre, Bruxelles.
- 1956-1960 R <u>Dresselhaus</u>, Gene (Berkeley)--LASSP. Theory of metals; magneto-resistance; excitons; surface states; close associate of Millie; he went to Lincoln Laboratory, she to MIT.
- 1959- <u>Lee</u>, David M. (Yale)--LASSP. Started low temperature program; superfluid He³ and He⁴ in magnetic fields; spin aligned hydrogen; anisotropies; share in Buckley Prize, Simon Prize.

- 1959 Littauer, Raphael (Cambridge)--LNS. Department chairman, partial term; high energy physics, accelerator design and instrumentation; control electronics magician.
 1959 Orear, Jay (Chicago)--LNS. High energy physics at Brookhaven and Fermi Labs; large angle scattering of high energy elementary particles, proton on proton, pion on proton.
- 1960-1979 D <u>Bowers.</u> Raymond (Oxford)--LASSP. Metal physics experiment; helicon wave discovery; Science, Technology, and Society.
- 1960- <u>Stein.</u> Peter C. (MIT)--LNS. High energy physics experiment; machine and detector work; investigation of charm dynamics.
- 1961- <u>Berkelman</u>, Karl (Cornell)--LNS. High energy experiments, e⁺-e⁻ interactions, tests of quantum electrodynamics.
- 1961- Pohl, Robert 0. (Erlangen)--LASSP. Phonon physics, thermal conductivity measurements in pure and defected crystals, glasses, and amorphous solids.
- 1960-1964 R <u>Bearden</u>, Alan J. (Johns Hopkins)--LASSP. Mössbauer experiments, biophysics applications; went to UC La Jolla, then Berkeley.
- 1961-1973 R <u>Carruthers</u>, Peter (Cornell)--LNS and LASSP. Theory of strong interactions; elementary particles; symmetries, SU3, etc.; went to Los Alamos to head theory division.
- 1962- <u>Ambegaokar</u>, Vinay (Carnegie-Mellon)--LASSP. Theory of super-fluidity of thin He films; two dimensional electron crystals on He liquid; collective modes in superconductors.
- 1961-1962 R <u>Frautschi</u>, Steven C. AP (Stanford)--LNS. Boot strapping, Reggi Poles, S-matrix theory; went to Cal Tech.
- 1963- <u>Fitchen</u>, Douglas B. (Illinois)--LASSP. Term as department chairman, Rockefeller renovation; high pressure experiments; Raman scattering in crystals; inelastic scattering by phonons and magnons.
- 1962-1982 D <u>Mahr</u>, Herbert (Erlangen)--LASSP. Optics in solids, largely with lasers; picosecond pulse techniques and applications;

nonlinear optics; mixed crystals; UV laser development.

- 1963- <u>Talman</u>, Richard M. (Cal Tech)--LNS. Colliding beam dynamics, nonlinear and intensity dependent dynamics; particle physics from identification system developed for CLE0.
- 1963- <u>Wilson</u>, Kenneth G. (Cal Tech)--LNS. Quantum field theory for hadrons; gauge theory in phase transformations; emphasis on very large scale computer calculations; shared Wolfe Prize with Fisher and Kadanoff; Nobel Prize 1982.
- 1964 <u>Chester.</u> Geoffrey V. (Sussex)--LASSP, with one term as director. Classical and quantum theories of fluids and computer experiments; metallic hydrogen; crystallization and melting; thin He films.
- 1964- <u>Mermin</u>, N. David (Harvard)--LASSP, with term as director. Theoretical solid state physics; superfluid He³; statistical mechanics; topological methods in defects; boojums.
- 1964- <u>Sievers</u>, Albert J. (Berkeley)--LASSP. Properties of solids via infrared and far infrared experiments; EM surface waves; submicron metallic particles; alkali halide defect spectra; optics instrumentation.
- 1964- <u>Yennie</u>, Donald R. (Columbia)--LNS. Quantum field theory; high energy EM interactions; Lamb shift theory; infrared divergence; muonium.
- 1965- <u>Gottfried</u>, Kurt (MIT)--LNS. Theory of nuclear structure; many body problem in quantum mechanics; elementary particles; e⁺-e⁻ resonances; weak interactions in heavy quarks.
- 1965- <u>Hand</u>, Louis N. (Stanford)--LNS. High energy physics experiments, mostly at Fermi Lab; neutral current; detection of short lived particles.
- 1965- <u>Wilkins</u>, John W. (Penn)--LASSP. Surface physics; localized perturbations in metals; valence fluctuation systems.

- 1965-1979 R <u>White</u>, D. Hywel (Birmingham)--LNS. Collaborator in Brookhaven experiments; weak, strong, EM interactions in high energy physics; went to Brookhaven.
- 1966- <u>Ashcroft.</u> Neil W. (Cambridge)--LASSP, with term as director. Disordered systems; theory of homogeneous and inhomogeneous liquids; metallic hydrogen; photoemission and surfaces.
- 1966- <u>Cassel</u>, David G. (Princeton)--LNS. High energy physics experiment; kinematic and final physics analysis of CLEO data.
- 1966-1969 R <u>Gross.</u> Franz L. AP (Princeton)--LNS. Theory, high energy phyiscs; went to William and Mary College.
- 1966- <u>Reppy.</u> John D. (Yale)--LASSP. Low temperature experimental physics; quantum properties (rotons) of superfluid He³ and He⁴ via torsional oscillator; London Prize.
- 1967-70 R <u>Delvaille</u>, John P. AP (Cornell)--LNS. Cosmic rays experimental; went to MIT.
- 1967- <u>Richardson.</u> Robert C. (Duke)--LASSP. Low, low temperature experiments; superfluid He³ properties; magnetic behavior of solid He³; share Simon Prize, Buckley Prize with Lee and Osheroff.
- 1969-1969 R <u>Groom</u>, Donald AP (Cal Tech)--LNS. Cosmic rays, high energy physics; went to Utah few months after appointment.
- 1969-1972 R <u>Peoples</u>, John AP (Columbia)--LNS. High energy experiment; sonic spark chamber spectroscopy; positrons in muon decay; went to Fermi Lab.
- 1969- <u>Hartill</u>, Donald L. (Cal Tech)--LNS. High energy physics detector development, CLEO; final states of e+-eannihilation; free quarks, B mesons; electronics wizardry.
- 1970- <u>Fisher</u>, Michael E. (London)--LASSP, Chemistry, and Mathematics. Phase transitions, critical and multicritical

phenomena; statistical mechanics; share Wolfe Prize with Wilson and Kadanoff.

- 1970- <u>Gittelman.</u> Bernard (Stanford)--LNS. High energy physics experiment; design and construction of CLEO; analysis of CLEO data.
- 1970- <u>Yan</u>, Tung-Mow (Harvard)--LNS. Quantum field theory; structure of elementary particles; quark bound states in psi and upsilon particle models.
- 1973- <u>Siemann</u>, Robert H. (Cornell)--LNS. High energy physics experiment; accelerator design, beam interactions.
- 1974- <u>Teukolsky</u>, Saul A. (Cal Tech)--LNS and Astronomy. General relativity in astrophysical problems; gravitational radiation; accretion flows.
- 1974-1978 R <u>Kogut</u>, John (Stanford)--LNS. Theory of elementary particles; statistical mechanics; went to Illinois.
- 1975- <u>Tigner</u>, Maury (Cornell)--LNS. Particle accelerators; applied superconductivity; in charge of CESR construction.
- 1976-1981 R <u>Scott</u>, J. Campbell AP (Penn)--LASSP. Magnetic, optical, and electrical properties of organic and polymeric molecules; one and two dimensional materials, experimental; went to IBM San Jose.
- 1978- <u>Siggia</u>, Eric D. (Harvard)--LASSP. Theory of fluid turbulence; dynamics of phase transitions; Bose condensation in atomic hydrogen.
- 1979-1988 R <u>Gilchreise</u>, Murdock (Stanford)--LNS. High energy physics experiment; detector development; electron-positron annihilation physics; went to SLAC.
- 1979-1985 R <u>Gregory</u>, Stephen AP (McMaster)--LASSP. Two dimensional physics experiments with absorbed oxygen; magnetic and optical effects; went to Bell Labs.
- 1979- <u>Ho</u>, Wilson (Penn)--LASSP. Surface physics experiment; low energy electron loss spectroscopy; surface EXAFS with synchrotron radiation at CHESS.

- 1979- <u>Lepage</u>, G. Peter (Stanford)--LNS. Theory of strong interactions, quarks and gluons.
- 1981-1987 R <u>Feigenbaum</u>, Mitchell J. (MIT)--LASSP. Discovery of metrically universal behavior in dynamical systems; onset of chaos; turbulence; went to Rockefeller University. Received Wolf Prize in 1985.
- 1981- <u>Galik</u>, Richard S. (Cornell)--LNS. High energy experimental physics; CLEO analysis.
- 1982- <u>Franck</u>, Carl P. (Princeton)--LASSP. Experimental; liquid interfaces.
- 1982- <u>Shapiro</u>, Stuart L. (Princeton)--LNS and Astronomy. General relativity, astronomy.
- 1983- <u>Cooper</u>, Barbara (Cal Tech)--LASSP. Surface physics; atom and ion scattering from surfaces; charge exchange processes; ion-surface interactions.
- 1984- <u>Sethna.</u> James P. (Princeton)--LASSP. Theory of amorphous materials--glasses; quantum tunneling in crystal defects, diffusion; liquid crystals.
- 1985-1988 R <u>Kawai</u>, Hikaru (Univ. of Tokyo)--LNS. Theory in high energy and particle physics; went back to Tokyo.
 1986- <u>Parpia</u>, Jeevak (Cornell)--LASSP. Low temperature physics; microkelvin research.
- 1986- <u>Rubin</u>, David L. (Michigan)--LNS. Accelerator physics.

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