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The History, Highlights, and Outcome of the Michigan - Wisconsin Echo Lake Cosmic Ray Program, 1965 - 1972: an Informal Review*

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Introduction

The nearly-simultaneous requests, by the editors of "Adventures in Experimental Physics" for a personal account of the use of cosmic rays as a high energy accelerator, and by Dr. Iona of the University of Denver for material to be used in his compilation of a history of the High Altitude Laboratory, have prompted me to set down in a less technical, retrospective fashion the background, some highlights, and the outcome of the program of cosmic ray research undertaken by a large group of physicists, mostly based at the Universities of Michigan and Wisconsin, under my direction. This summary, together with the appended bibliography, etc., may in some measure serve as an informal final report of this program.

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During 1961-1962 as a visitor to CERN I found myself impatient to do physics at ever higher energies and somewhat frustrated and disillusioned by the seemingly endless delays in getting a next generation of accelerators off the ground (following successful operation of the CERN P.S. and Brookhaven A.G.S.). At that time I was working with Dave Caldwell, Basil Zachorov, Dirk Harting, and William Middlekoop on some of the earlier spark chamber experiments at the CERN P.S.; one experiment to study $\pi^- p \rightarrow \pi^+ \pi^- n$ [1] and another experiment on $\pi^{\pm} p$ elastic scattering.[2] Both used two magnets (one to analyze the incident beam and one to analyze momenta of the reaction products) flanked by spark chambers planes. It occurred to me that the general topology of these experiments could be rotated 90° and its dimensions modified to accept incoming vertical cosmic rays. Discussions with experts such as Cocconi and a little reading convinced me that the flux of cosmic ray protons at Mountain elevations was sufficient to do interesting - perhaps exciting - physics at over 300 GeV with equipment on a scale well beyond any prior cosmic ray experiments although conservative compared to the cost of proposed accelerators. I reported my ideas in a seminar and in a CERN report at that time.[3] During the 1962 biennial International High Energy Physics Conference I kicked these ideas around with Jerry Fregeau, an old friend in the National Science Foundation. He said, in effect, "O.K., perhaps it's a good idea; why don't you write a proposal?" I frankly had cold feet; after being a party to several fruitless M.U.R.A. accelerator proposals I was not about to drop out of productive research to battle a proposal through Washington.

Two years later, at the subsequent High Energy Conference in Dubna, I was reflecting along the same lines of thought with Yash Pal and Fred Reines. The outcome of these informal discussions in the lounge of the Hotel Dubna was a delightful, small two-day conference at Case Institute in September 1964 on the use of cosmic rays in high energy physics using contemporary technologies.[4)] Bob Thompson, Luis Alvarez, Mel Schwartz, and several others presented their various ideas. Fred Mills, then at Wisconsin and director of the old MURA laboratory was also there. That meeting and subsequent discussions with Fred were the catalysts that finally produced a proposal. With the MURA engineering capability and Fred's enthusiasm we proposed a large experiment at the summit of Mt. Evans in Colorado employing two very large magnets, spark chambers, and a very large liquid hydrogen target.[5)]

During 1965 I had scheduled a sabbatical year at CERN so that Fred Mills carried the ball in the U.S. for that year. Our proposal drew a modest grant from N.S.F. to explore the feasibility of our ideas. With this the Wisconsin

MURA lab acquired an old semi-trailer which they outfitted with spark chambers and a crude ionization calorimeter together with necessary electronics, cameras, etc. This trailer was hauled to the summit of Mt. Evans at about 14,150 feet elevation in June 1965. I returned to the U.S. to participate in the setup and running of this experiment. The specific goal was to measure the flux and spectrum of cosmic ray hadrons together with information on the accompaniment of the hadrons by electrons of "air showers". To us the greatest value of the exercise was in experiencing what actual cosmic ray experimentation was like, and to be introduced to the problems of life at 14,000 feet! We learned - the hard way - our physical and mental limitations in the rarified atmosphere of 6/10 sea level pressure. We also learned a bit about Rocky Mountain meteorology; the 2P.M. daily snow squalls in July, the corona and lightning of a mountain top electrical storm, the beautiful sunsets and crystal clear mornings.

That autumn Bud Good (also in Europe on sabbatical) and I visited Bernard Peters in Copenhagen together and had some very stimulating conversations. Peters described his group's quark search experiment and also made valuable comments on my mountain top lab. proposal. Later Good and I sketched out a possible experiment, much simpler than my two-magnet proposal, to search for quarks à la Peters and to measure total p-p cross sections at very high energies (100 - 1000 GeV). The idea would be to build an ionization calorimeter, surround it by scintillation counters to detect air showers, and to surmount it with spark chambers and a liquid hydrogen target. No magnets were needed for these simpler experiments as the ionization calorimeter could be a sufficient energy detector.

On my return to the U.S. Mills and I went again to NSF with the results of our first experiments and our thoughts and experiences of the preceding year. The Foundation, interested but unconvinced, supported us for a futher year of serious study to include both mountain experiments and detailed architect-engineering studies. Accordingly, we retained the Architecture and Engineering firm of Skidmore, Owings, and Merrill, designed and built a small-scale iron-yoke superconducting magnet, and set up further experiments in Colorado. This time we incorporated one of the ideas Good, Peters, and I had discussed and arranged the experiment to not only test our ideas on instrumentation but to search for quarks as well.

During 1965 I had asked Chicovani, then visiting CERN, why he had not used wide-gap spark chambers in his cosmic ray experiments in the U.S.S.R. He had noted that the electrostatic pickup from spark chamber pulses of over 100 keV in his gas-filled proportional counters and ionization chambers, where signals corresponded to only a few hundred electrons had been insurmountable. We had the idea that protons and pions in the cosmic ray hadron flux could be separated using the relativistic rise in ionization in gas proportional counters, so one objective of our 1966 experiment was to test this notion using six layers, each 3 ft. by 6 ft., of multi-wire gas proportional counters. We forced the issue of pickup by mounting these atop an 8 inch-gap wide-gap spark chamber, all again atop an ionization calorimeter.

This time the experiment had outgrown our trailer so we built a temporary, prefab. building at the Mt. Evans summit to house the experiment and used our trailer for the electronics. During 1966 Don Lyon and P.V. Ramana Murthy (a visitor from the Tata Institute in Bombay, India) joined our group, and joined in the Mt. Evans experiments. Subsequently Al Bussian and John Learned joined us also as postdoctoral physicists, and these four provided the major mainstay of our group. Inevitably the experiment at the summit of Mt. Evans was just becoming operational as summer drew to a close. The road to the summit is normally closed on Labor Day (early September) as after that date heavy snow can be expected at any time.

The University of Denver some years earlier had taken over the operation and custody of the InterUniversity High Altitude Laboratory at Echo Lake, Colorado. This complex of real estate included the old Doolittle Ranch with living accomodations for 7 families at an elevation of 9600 ft., the Echo Lake Laboratory with a caretaker's house, another lab building, and a dormatory building at 10,600 ft. and a small shed at the summit of Mt. Evans (14,150 ft.). During the late 1940's and early 1950's most of the cosmic ray physics in the U.S. was conducted at this laboratory complex by physicists from Princeton, MIT, Cornell, Michigan, and other universities. After the operation of the Cosmotron and Bevatron, cosmic ray interests in the universities fell off and the laboratory was taken over by Denver. Hence our operations at Mt. Evans were undertaken under the umbrella of a hospitable and knowledgable organizational structure. Professor Mario Iona of Denver was of inestimable aid then and throughout our operation. During the summers our families were in residence at "the ranch" and we commuted the 18 miles each way to the summit. During the winters we stayed in the dormatory at Echo Lake, a two-story log building with a large living room, a stone fireplace, a totally modern kitchen, and 9 bedrooms.

In September, 1966 we moved our equipment, including the temporary building, from the summit of Mt. Evans to the Echo Lake site. The difference in elevation cost us a factor of three in hadron rate (due to attenuation in the atmosphere) but gave us access to year-round operation, as roads, electric power, heat, etc. were maintained continuously. That autumn the quark search was undertaken in earnest, and a very solid experiment was completed. At one point our data showed evidence for a promising quark candidate, but there was also a small probability that it was an accidental coincidence of an air shower and a normal proton. Quadrupling our running time since that event produced no further candidates, and our final interpretation was (and still is) that it was an accidental coincidence. This remains to date the most sensitive quark search using this particular (time-delay, charge-independent) search technique.[6)]

Meanwhile, our design study of a large mountain top laboratory was proceeding. Bruce Cork of Berkeley and Don Reeder of Wisconsin joined our group and contributed insight, experience, and hard work both at Echo Lake and in our study. In 1967 we finally submitted to N.S.F. a full blown, hard boiled proposal for a major mountain top laboratory to do hadron physics at up to 1000 GeV.[7)] The pricetag was $23.6 million for construction and $3.3 million per year operation. There is still no question in my mind that this scale of expenditure was justified and appropriate on a cost-effectivness basis when compared with typical accelerator operations. It was never intended to replace the need for higher-energy accelerators, but to supplement them. Still, by the time the proposal had been reviewed, two new factors had developed. First, the CERN I.S.R. and N.A.L. 200-500 GeV synchrotron had been authorized, and second, the entire funding climate for large projects at the federal level had cooled very considerably. Thus the proposal was ultimately rejected.

Meantime, encouraged by the success of the quark experiments and our understanding of our own techniques and of the cosmic ray flux, we proposed the second phase of the original experiment Good and I had cooked up in 1965. Over about 50 years of cosmic ray physics no one had even put a substantial liquid hydrogen "target" in the "beam", an ingredient regarded as essential for most accelerator experiments. We proposed to measure the total proton-proton cross section by building an apparatus consisting of (vertically, from the top down) spark chamber, hydrogen target, spark chamber, and ionization calorimeter. For good counting rates the apparatus needed to be considerably larger than the quark search experiment, so a new building was built at Echo Lake, 75 tons of iron were assembled to form the calorimeter, and a liquid hydrogen target was built for us by Berkeley (the Lawrence Radiation Laboratory). During 1967 and early 1968 the construction was carried out at Echo Lake and by May 1968 we were able to take preliminary data with carbon.

During the summer of 1968 we had convened a meeting of all physicists involved in the experiment at Echo Lake to plan the forthcoming hydrogen run. Just before noon a fellow came into our meeting at the dormitory looking for a telephone to report a fire. As good ex-boy-scouts we felt we could do quite a bit to fight the fire before a larger, more expert crew arrived. The caretaker drove us in his pickup to the road a couple of miles above the small blaze where we joined a U.S. Forest Ranger from the local ranger station. About 15 of us, with shovels, rakes, hoes, and much eagerness plunged off down the mountainside. The truck was parked at about 12,500 feet; tree line there is about 11,500 feet, and the fire was at about 10,000 feet. When we arrived, it had covered a small knoll of a few acres area. We concentrated our efforts on fighting satellite burns, spread by sparks from the largely-isolated small blaze. Two firefighters with walkie-talkie radios joined us, and about 2:00 p.m. we took a breather in a small clearing. Rather suddenly it developed that a small satellite fire down hill (east) from us several hundred yards broke into the treetops and started spreading rapidly - in particular it began moving down wind, up hill, towards us! We set out immediately to get away - up hill - from a raging inferno. The subsequent half hour was probably the most terrifying in my life. We were literally running for our lives as the "fire storm" pursued us. Back through the woods the bright flames could be seen among the tree trunks, and overhead a collosal plume of red and black towered above us. The inrush of air to feed the flames provided a fresh wind in our faces, and the blaze made a steady roar, like jet aircraft on takeoff, punctuated by an occassional exploding tree. We broke into roughly three groups, heading uphill to the right, center, and left. John Hicks (an electronics engineer) and I ran centrally up a steep ravine, and were always at least perhaps fifty feet ahead of the flame front. Bruce Cork and others on the right (north) were in more imminent danger- the fire spread up the hillside they were flanking, and Bruce was at one point hit by falling, burning wood. We were not really safe until we emerged in the clear above tree line. Utterly exhausted, five of us sat out on the rocks watching the the blaze lick up the opposite ridge. It was a spectacular, terrifying sight. Finally we slowly struggled back up to the road. All of us, miraculously, escaped injury. We are all convinced that we only made it because a propitious change in wind direction diverted the blaze to the north, away from us.

Back at the Echo Lake dormitory we spent the next 24 hours getting our breath back while a veritable army of firefighters was marshalled; Indians from Montana, helicopters, bulldozers, the works. Two days later the blaze was contained; over a square mile had burned in those first hours, however the subsequent destruction had been relatively minor.

The liquid hydrogen target, with a capacity of almost 2000 liters, was completed and tested at Berkeley and installed in Colorado during the late summer of 1968. We had quite a bit of discussion and work concerning hydrogen safety. The safety problems concerned with large quantities of liquid hydrogen in the fully-staffed environment of a large national laboratory are significant. When translated to the national forest environment, with the full spectrum of forest fire hazards, elk and deer hunters, tourists, and wild animals, problems become very much more serious. We tooled up the lab with fire alarm, hydrogen sensors, an emergency battery electric power system, a standby gasoline driven generator, a barbed wire fence, and the works. Inside the building cotton lab coats, hard hats, conducting paint, and special electrical systems were the rule. It was never totally clear whether our major concern was protecting the forest from the hydrogen or the target from the forest. As it turned out, in seven months of operation with the hydrogen target continuously filled we had no difficulties whatsoever with the hydrogen system. Every week or 10 days a semitrailer truck came up from Boulder with liquid hydrogen, and we "topped off" the target, which subsequently boiled off hydrogen at the rate of about 3% per day. We took data almost continously during those 7 months.

The one incident which caused greatest concern was a freak snowfall in May 1969 when about four feet of very heavy, wet snow fell. Of course we were snow bound briefly, but more seriously the electric power was off for four days. First our battery system failed, then our emergency gasoline generator quit. We were then stuck, with 1500 liters of liquid hydrogen perculating in the target, with no electric power whatsoever. We rationalized that the system was at least safe from the fire hazard of accidental electrical sparks, but we did feel singularly helpless. After some effort we found that the points in the distributor of the standby generator were bad(in fact broken); with a bit of make shift juggling we were able to repair them and get the generator going again. After the snow storm the weather was warm and clear, and we were able to get our vehicles moving in and out after a couple of days, although the power and phone lines were out longer.

Following the hydrogen run some data were taken with targets of heary elements while the next phase of the program was in preparation.

In 1964 Alvarez had described a proposed experiment to explore strong interaction physics with a large balloon-borne experiment consisting of a gas Cherenkov counter, spark chambers, a liquid hydrogen target, more spark chambers, and a large superconducting magnet. He proposed to achieve very high spatial resolution through the use of emulsions of one square meter area interspersed between spark chambers. The program was subsequently funded by NASA and the superconducting magnet was built at Livermore under the direction of Clyde Taylor.[8)] At the Case Conference[4)] I had noted that the gain in primary cosmic ray flux achieved by boosting the apparatus above the atmosphere for a couple of flights a year was cancelled by the short "running" time of the ballon flight, and that the same physics could be as efficiently done by setting his apparatus on a mountain top for a year. He replied that while this might be true, a mountain top laboratory can acquire an inertia and independent existence which is hard to turn off, whereas in the balloon experiment, if a parachute fails, you have an opportunity to reapraise the program with a clean slate. Well, in fact in Alvarez' program, there was a parachute failure, although the magnet was not on that flight. Subsequently that group changed their program emphasis from particle physics to the very separate scientific question of the energy spectrum and nuclear composition of primary cosmic rays. While the instrumentation for this investigation is similar to that for the particle physics experiments, and indeed the program had been moving in that direction, the problems adressed are in astrophysics and cosmology rather than in elementary particle physics.

Consequently, after the magnet failure, the HAPPE (High Altitude Particle Physics Experiment) magnet was put on the shelf.

At the Budapest International Cosmic Ray Conference Koshiba (of the University of Tokyo) in talking with Lynn Stevenson and myself suggested that we bring the HAPPE magnet to Echo Lake and set it above our existing apparatus with additional spark chambers in order to measure accurately the momenta of incident protons and pions. We pursued this idea; there were discussions with Alvarez, Clyde Taylor, NSF, and NASA, and the magnet was brought to Echo Lake. NASA also purchased for us a large helium liquifier-refrigerator to operate the magnet system together with the hydrogen target.

The magnet is very large; its coils are saddle-shaped so that it forms an open cylinder a meter in diameter and 2.5 meters long with an 11 kgauss field perpendicular to the cylinder axis. The entire magnet weights about two tons. Starting in 1970 new, more elegant spark chambers were built, our lab building at Echo Lake was modified to give us a high bay, structural steel was added to support a new crane together with the magnet and the new spark chambers, and a major revamping on the electrical and electronics systems was undertaken. The major job was the installation of the CTI 1400 liquid helium system. After many false starts, much consultation with cryogenics engineers, and a great deal of cryogenics engineering education on our part, we got the system working and filled the magnet dewar with liquid helium. In October 1971, the magnet was energized to half current, and run in the persistent mode for over two weeks while field measurements were made. (A superconducting magnet can be short circuited and current will continue to flow through the zero resistance path essentially indefinitely without change. This is called the "persistent" mode of operation.)

Now one problem with any large magnet is that very considerable energy may be stored in the static magnetic field, and when the field is reduced, the energy must go somewhere. During December we were bringing the magnet close to full current when it "went normal", i.e. ceased to be superconducting. The magnetic field collapsed (over a period of many seconds, to be sure), great arcs occurred in the contacters of our generator, and a great quantity of liquid helium was boiled off through our vent system.

Of course such a boiloff was provided for in the design of relief valves, etc., but it was nevertheless a jarring experience. During the boiloff John Learned was on the upper deck, near the magnet, communicating with the technician down below. Later the technician noted that he must have gotten very excited the way he was shouting, with his voice squeaking so high. John replied that he wasn't all that upset, it was just that there was so much helium in the atmosphere up there that the pitch of his voice was elevated. The magnetic field of this fully-charged magnet contained a stored energy of 1.5 megajoules.

In March 1972 the entire system was finally operational, and on March 30 all was in readiness to charge the magnet to full current, switch to the persistent mode, and start taking data. Alas fate was not on our side. John Learned and Dan Jones, post doctoral physicists there at the time, have documented their experiences in detail. I was not present. The magnet was fully charged, switched to the persistent mode, and the leads removed. About 15 minutes after the magnet had been switched to the persistent mode, while they were cleaning up and preparing to quit for the night (2:30AM), the magnet spontaneously "went normal." For about a minute helium vented vigorously as the stored energy of the magnetic field supplied the latent heat of vaporization to the liquid helium. Only then, for reasons still not understood, the magnet structure failed and the helium pressure ruptured the inner dewar and outer vacuum jacket with a loud "Whomp," blowing superinsulation and splashing liquid helium generally about the laboratory. With that, over two years of careful preparation and hard work went for naught. It was indeed a profoundly disappointing turn of events for everyone connected with the project.

Fortunately, the remainder of the apparatus remained intact, and there were experiments well suited to the equipment capabilities which remained to be done. A wide gap spark chamber with a lead plate had been built earlier that year in Wisconsin to study γ-production, and that was then installed in the system. Data were taken during the spring and summer, 1972, with a graphite target and the lead plate chamber. The specific objective was to study the correlation between π° and charged π production at 100-1000 GeV. A long shot hope was to look for evidence of the magnetic monopole in the form of a large burst of γ-rays (predicted by Ruderman and Zwanziger as the probable result of production and subsequent annihilation of a monopole-antimonopole pair).[9)]

At the end of summer, 1972, the lab was formally shut down. The equipment was returned to Wisconsin and Michigan and our extensive borrowed gear was returned to the large laboratories - Berkeley, Argonne, and Brookhaven.

Along the way, over the several years, there was an almost continual sequence of incidents. For example I was at the dormitory with my family visiting with the caretaker one evening after dinner when a fellow came in and asked after the ownership of the car with the radio amateur call license plates. I acknowledged that it was mine, and he asked if I could come to assist in a search underway. It seems that a family had parked up the road a few miles and that the 80 year old grandmother had gone off picking mushrooms and not returned. When her family failed to find her they notified the forest rangers, and by the time we got into the act, a full-fledged search was underway. They had portable walkie-talkies and wanted me to carry one with a search group. As it turned out, my 18 year old son, the caretaker, his boy, and I with two others formed one search group and were assigned an area to probe. We struck off down the mountain through thick pine forest at about 10PM searching our zone. Even with a map and flash lights we were hard put to keep our way straight in the pitch black darkness. About 1AM we got back to the base station; no one had seen any sign of the mushroom lady. However this all-volunteer and wonderfully organized rescue group kept parties out all night. We went home to bed. The next morning we went up to find out how the search had come. About the time we showed up one group located the old lady -alive and remarkably well, to everyone's surprise. As we were relatively fresh, my son and I joined a dozen others who went down the 1500 feet with a stretcher and down sleeping bag to where she had been found. We carried her out down below to a jeep road - a total trip of several miles.

Considering the precipitous nature of the mountains and the winding mountain roads we fared generally quite well in our driving. On one occasion Phil Kearney was riding the road to the summit with a highway crew setting out road markers to identify the road in deep snow when the pickup slid off the road. All three men jumped clear, and even the truck was recovered later - it had only bounced down the rocky slope a dozen yards. A later winter, however, some of our families were visiting over Christmas and five of the fellows drove off down the mountain in our University of Michigan carryall. They skidded on a patch of ice, clipped an 8-inch post cleanly, and vaulted into space off the pavement. At this point the road was cut into a steep hillside and the rocks and boulders down hill were piled as steeply as permitted by the angle of repose. There are differing opinions as to how many times the vehicle tumbled in air and how it rolled, tumbled, or bounced after it struck the rocks. In any event, it came to rest over a hundred and fifty feet down hill upright, with all windows gone, and pointing uphill. Although the vehicle was a total loss, the only serious injury to the five occupants was a broken arm to Fred Mills' son. We all thanked our lucky stars and are now much more conscientious about seat belts!

At our lab at Echo Lake we learned a little about trying to run a modern physics experiment in the wilds. We were of course janitors, carpenters, and housekeepers as well as physicists in the usual sense. Although the caretaker and his wife were of invaluable assistance, we often plowed snow, hauled garbage, did the laundry, and of course made our own meals while on duty. And inevitably we were guides, nurses, and raconteurs for the myriad tourists who came by; occasionally physicists of international stature visiting the scene of their doctoral thesis research, but more often helpless vacationers from Oklahoma, lost in the fog, and desperate to find the nearest john.

Lest I leave the impression that all Echo Lake life was hard and hazardous, I should hasten to add that we all became very fond of the mountains and the forests. The lab is less than an hour's drive from the Loveland Basin, and only a bit further to Winter Park, Arapahoe Basin, and other excellent Rocky Mountain ski areas. In the winter skiing was by far the most popular recreation; normally we would work through the weekends and break away to ski week-days when the slopes were uncrowded.

Starting with Bruce Dayton, many of us became avid mushroom hunters, continuing a Mt. Evans tradition originated, so I am told, by Vanna Cocconi. Dick Roth and John Learned in particular would gather tens of pounds of superb Boletes on good August days.

We fished for trout in Echo Lake and Summit Lake; Phil Kearney in particular seemed to have the touch for successful ice fishing, and as he didn't like fish, we had fine trout dinners when he was around.

We enjoyed beautiful hikes through the fir and pine forests, although my favorite summer hike was across the arctic tundra high above tree line where the quartz-flecked granite hid nests of exquisite miniature mountain wild flowers, and where one feels literally on top of the world.

Other than the quark work I have not commented on the physics output of our high energy experiments. The film analysis at Michigan and Wisconsin, the magnetic tape analysis at Boulder, and the program developments converged to produce physics results that were altogether new, quantitative, and very interesting if not of dramatic excitement.

Cosmic ray physicists have always done almost nothing but inclusive reactions (although that term is a recent contribution by Feynman) and our experiment was no exception. The contributions of our experiment were two-fold: "high" statistics (a consequence of the large scale of the experiment) and the liquid hydrogen target. All previous experiments had been based on emulsions or on targets of graphite, iron, or lithium hydride at best. The most significant previous data had been from Dobrotin's cloud chamber experiments in the USSR, where about 70 events had been accumulated over several years of operation in the energy range above 100 GeV.

While our initial objective was the inelastic pp cross section, we found that the inclusive distributions of inelastic intereactions were available in our data and were in fact more interesting than the total cross sections. We first found that the multiplicity distributions were too broad to be explained by a simple Poisson distribution of singly produced particles, but fit better a model whereby observed secondary particles are produced in pairs, or at least multiply (e.g., as vector mesons or isobars). In fitting angular distributions (effectively "rapidity" distributions, although the term wasn't common then) to theoretical models, we found that a particular multiperipheral model adjusted to data at 30 GeV fit our 100-400 GeV data very well.[10] It was subsequently noted that this model, hence our data, follows the laws of "scaling" as suggested by Feynman, although at our first publication of the results this term had not come into use. In retrospect, scaling in strong interactions above 100 GeV was first demonstrated by our data. One consequence of scaling was the prediction that average charge prong multiplicities increase as the logarithm of energy, rather than as energy to the 1/4 power, as had been traditional in cosmic ray literature.[11)] The $E^{1/4}$ law had been suggested by emulsion data of poor statistics and great systematic uncertainty. Our data, while also subject to certian systematic errors, fit quite well to a logE dependence of average multiplicity. We also demonstrated clearly if not surprisingly that multiplicities at a given energy depended on the target atomic weight.[12)]

Our data, published[13)] and reported at various meetings and conferences[14)], stimulated considerable activity in theoretical modeling with various scaling ideas and multiperipheral models.

The Soviet physicist Grigorov had published results of a very ambitious series of satellite experiments which showed the inelastic proton cross sections rising with energy above 100 GeV in carbon, and (less significantly) in hydrogen, from data with a polyethylene target. We found the inelastic pp cross sections essentially constant, and the data from the calorimeter showed that the proton-iron cross section was also substantially energy-independent.[13), 14)]

The final analysis of data from heavy nuclei and from the lead plate spark chamber is still in progress at Michigan and Wisconsin, where it will form the substance of the doctoral theses of two graduate students.

At the Case conference,[4)] I noted that my motivation for working with cosmic rays was the stimulus of doing physics at energies not available with accelerators, and that I would gladly do experiments at accelerators were they available. During the cosmic ray program Reeder and I continued active experimental programs at accelerators at Brookhaven, Argonne, and Berkeley. Bruce Cork was made Associate Director for High Energy Physics at the Argonne National Laboratory and Fred Mills was made head of the AGS at Brookhaven during the course of this program. Now with the operation of the National Accelerator Laboratory 300 GeV accelerator, Reeder, Cork, and I are actively working on experiments there at these energies as the cosmic ray program closes.

Subsequent Developments

Data from the CERN ISR and from first bubble chamber exposures at NAL have confirmed the scaling behavior of inclusive distributions we reported, and have of course added considerable detail to the broad brush picture revealed in our data. Also confirmed were such features as the narrower rapidity distributions for higher multiplicity events. While the average charged prong multiplicities we reported are systematically lower than now seen in the NAL bubble chamber data, the logarithmic increase with energy is confirmed, albeit with a higher coefficient. The nearly constant pp cross section below 1000 GeV is also confirmed.

The physics of inclusive reactions in complex nuclei is relatively new, and our cosmic ray explorations are breaking new ground here. Calculational techniques we developed in the analysis of our data are now finding application by theorists in exploring complex model predictions. Our data on purely cosmic ray parameters such as hadron flux, energy spectrum, charge-neutral hadron ratio, etc. all represent useful contributions to the more traditional cosmic ray literature.

Less predictable has been the spinoff from our cosmic ray program in other areas. The use of an ionization calorimeter as a hadron energy detector was brought from cosmic rays to the accelerator floor by us for measurements of neutron total cross sections. Now several groups are using such iron-scintillator stacks as hadron detectors in neutrino experiments as well as in strong-interaction experiments.[15)]

The wide gap spark chamber, which we developed for the Echo Lake experiments, was found to be capable of very high spatial resolution at a price and complexity much less than in competing systems.[16)] Such chambers are now installed at NAL behind the 30-inch bubble chamber in an application well suited to their intrinsic capability.

Our experience with liquid hydrogen stimulated a line of thought quite remote from high energy physics; I became very enthusiastic about the prospect of using liquid hydrogen as a replacement for hydrocarbons (petroleum products) in vehicular and aircraft fuel systems. My publication of these ideas has drawn me into close contact with a lively group of scientists and engineers, now banded together in the "H_2indenburg Society", dedicated to the safe utilization of hydrogen as a fuel.[17)]

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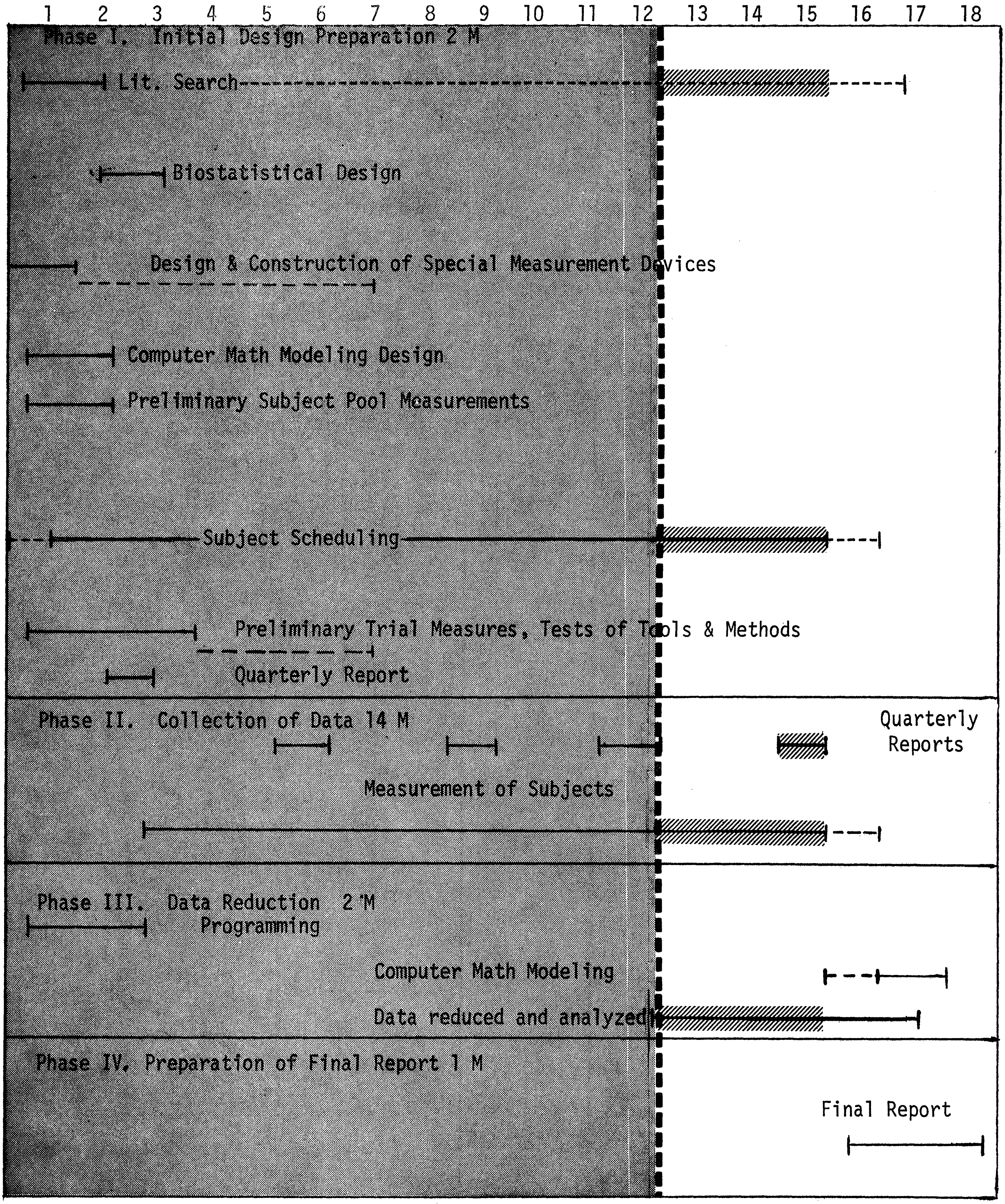
TABLE V

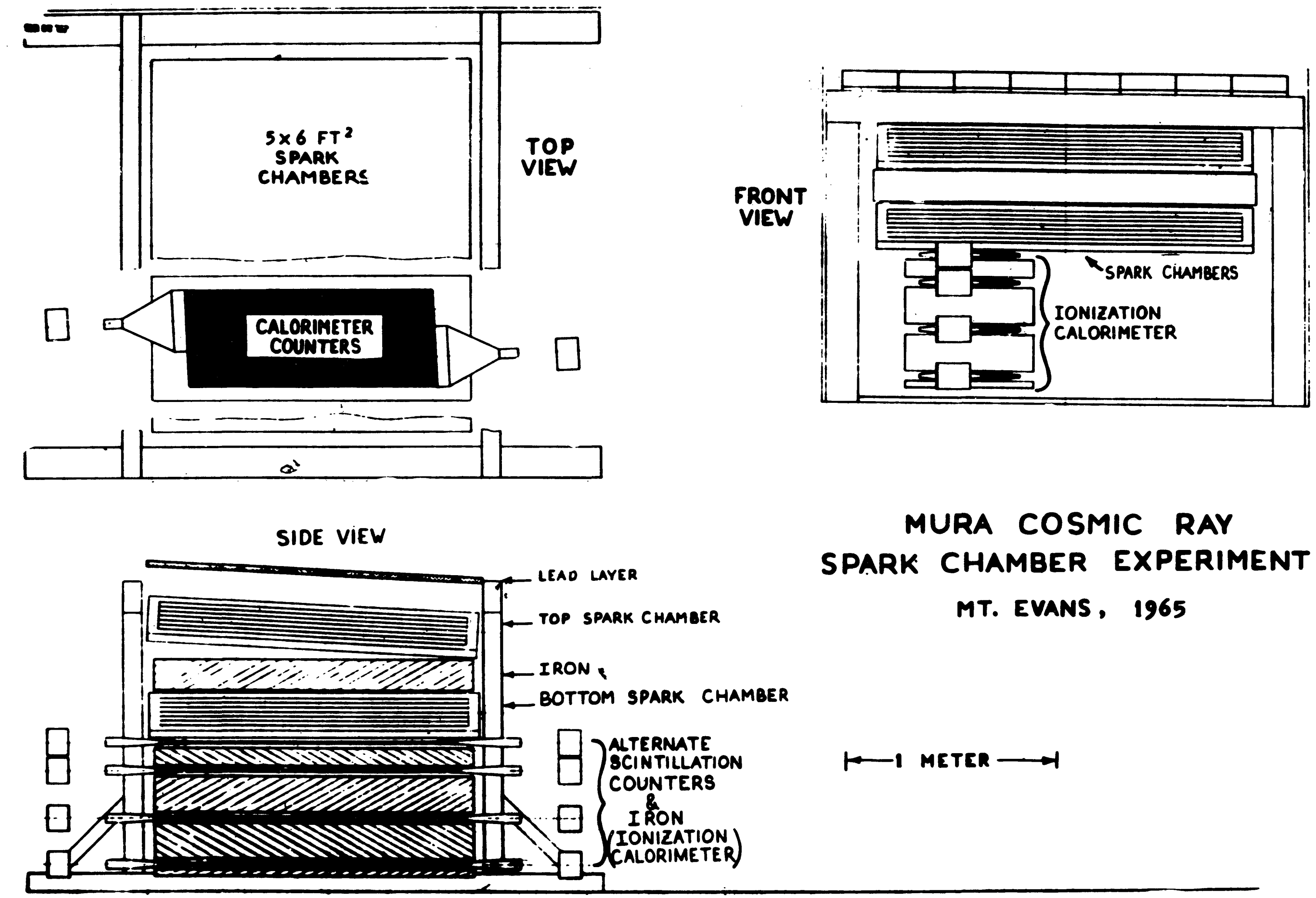
PROGRAM SCHEDULE

Months

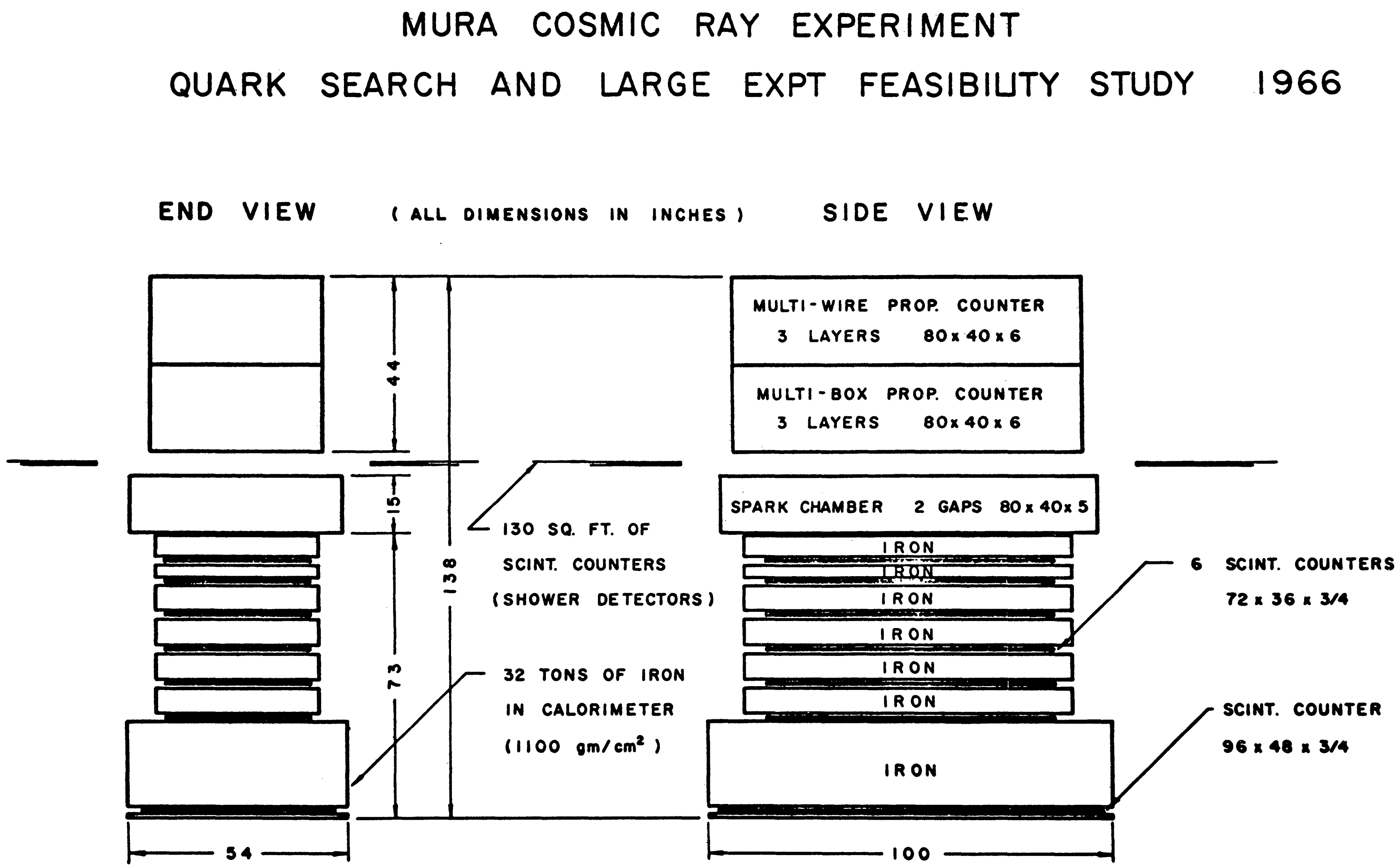


Indicates activities projected for Period 1 Jan. - 31 Mar.

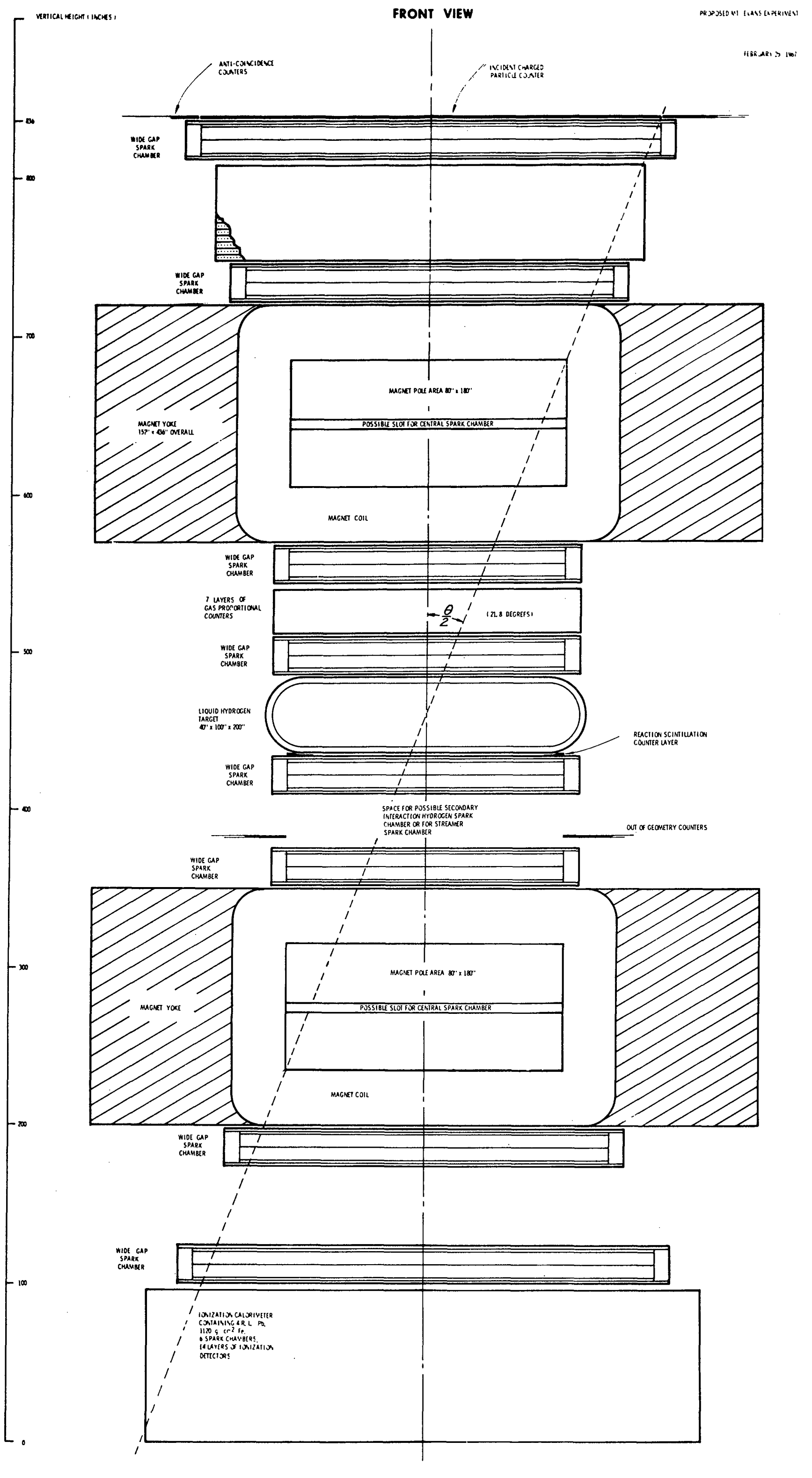
Portion of Program completed.



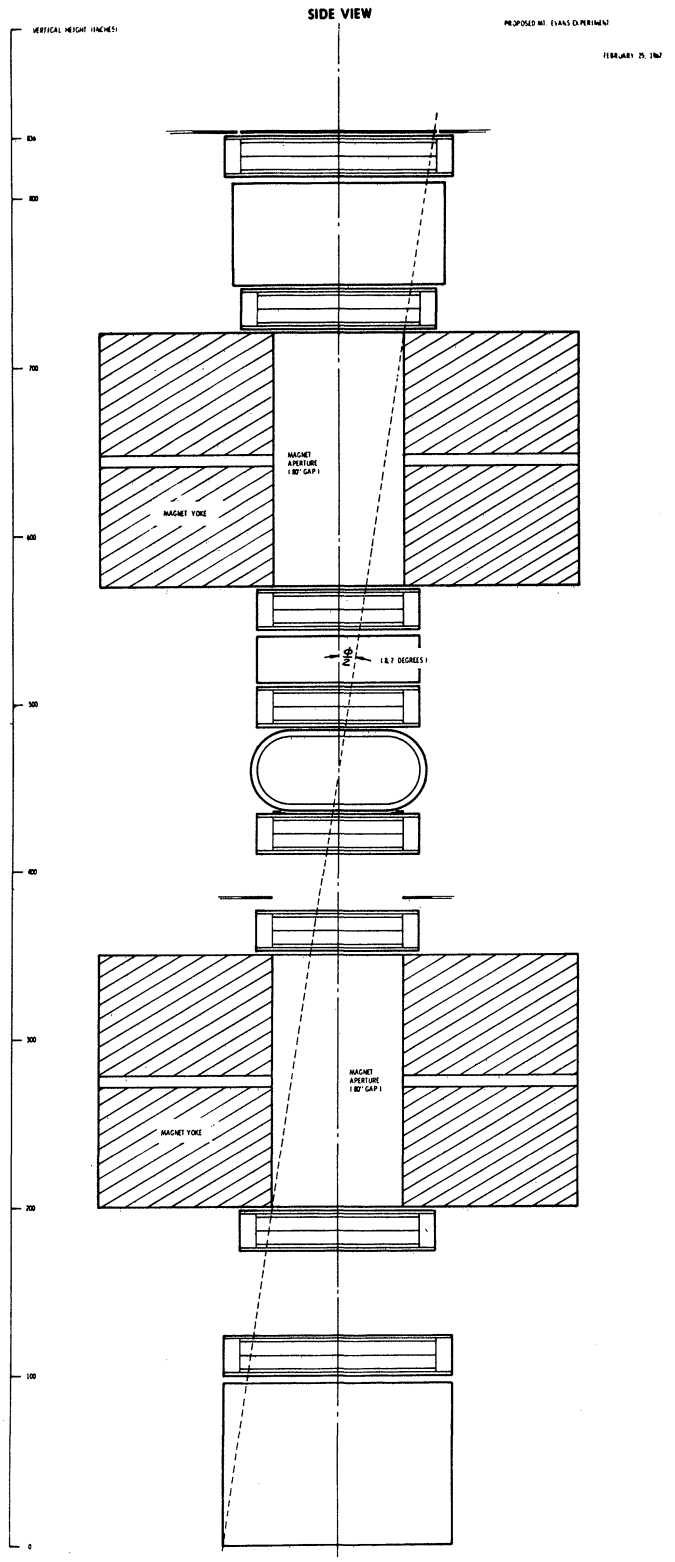
1. Configuration of 1965 feasibility study experiment, Mount Evans summit.



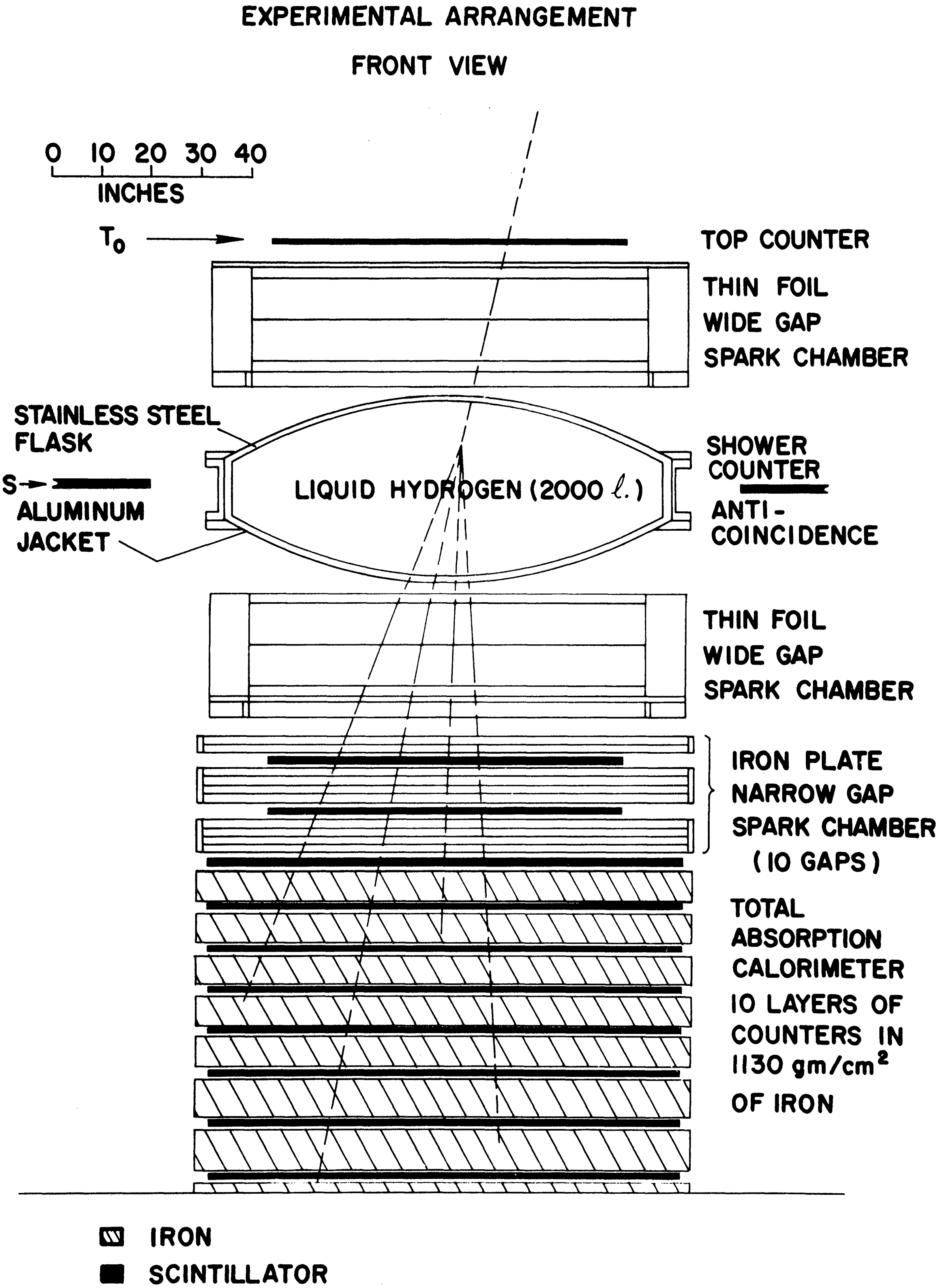
2. Configuration of quark search and large experiment feasibility study, Mt. Evans and Echo Lake, 1966-1967.



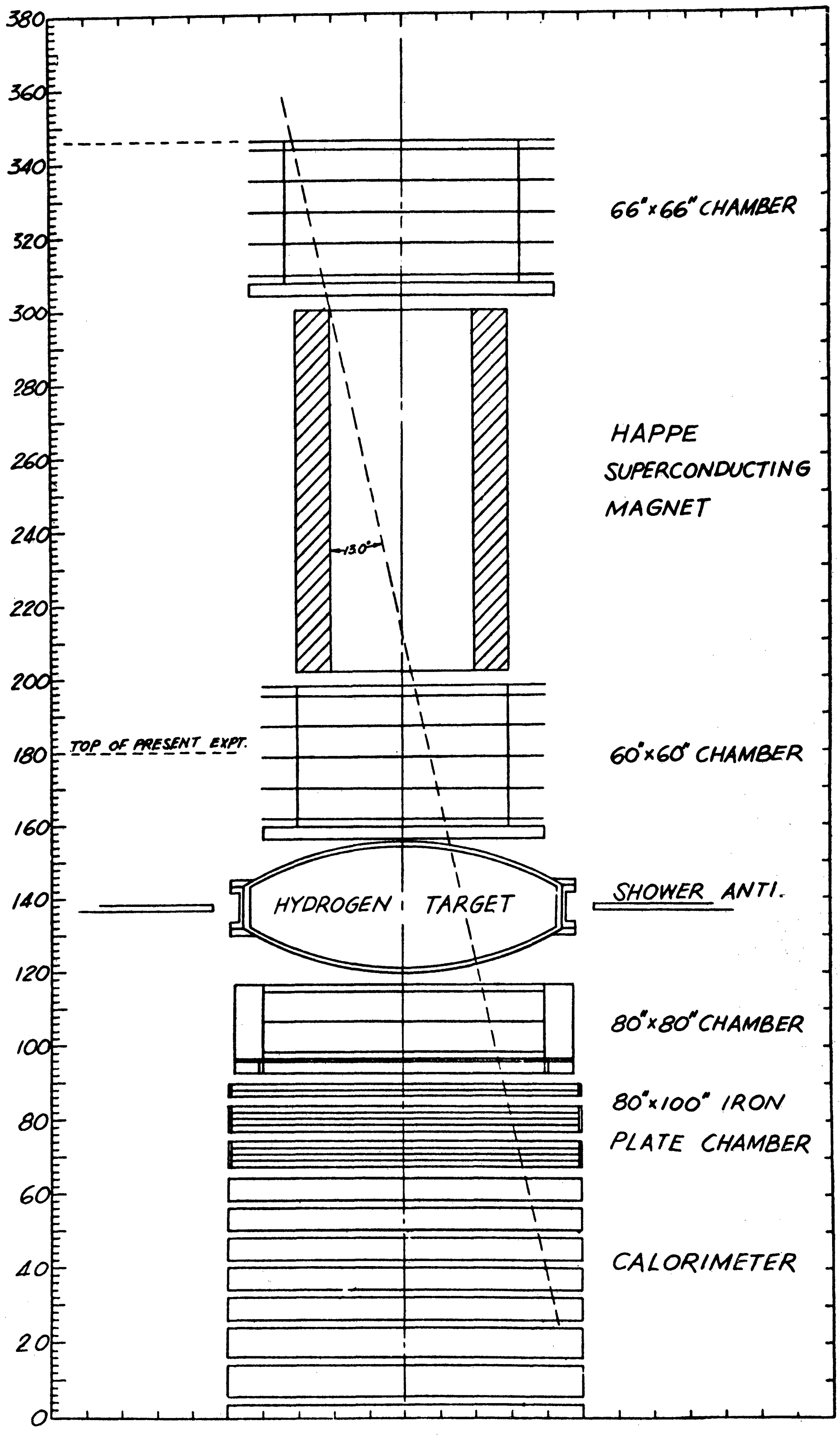
3. a) Configuration of proposed large facility for the Mt. Evans summit, 1967 (never built). Front view.


SIDE VIEW
VERTICAL HEIGHT (INCHES)
PROPOSED MT. EVANS EXPERIMENT
MAGNET
APERTURE
MAGNET YOKE
DEGREES
836
800
700
600
500
400
300
200
100
0

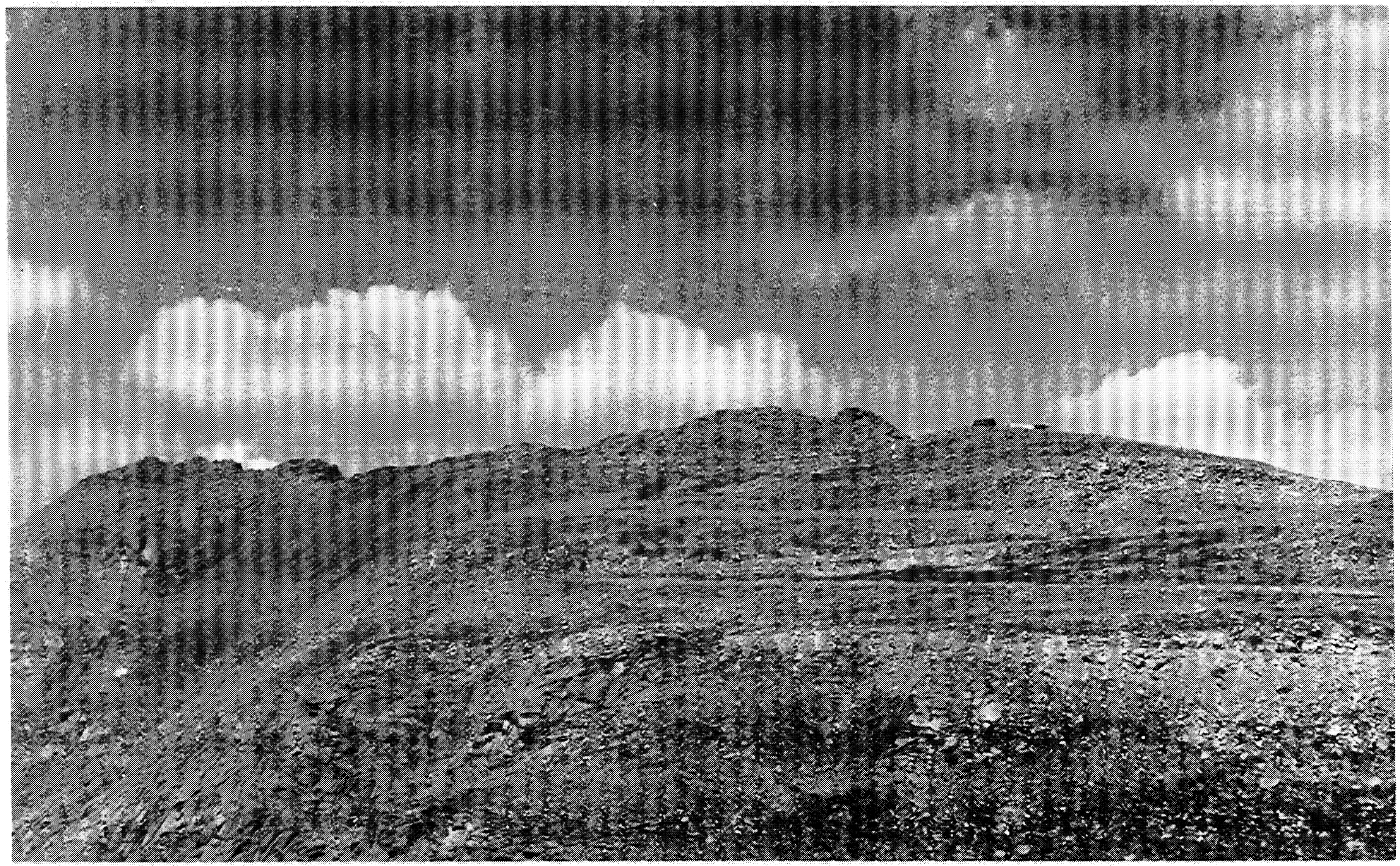
b) Side view.



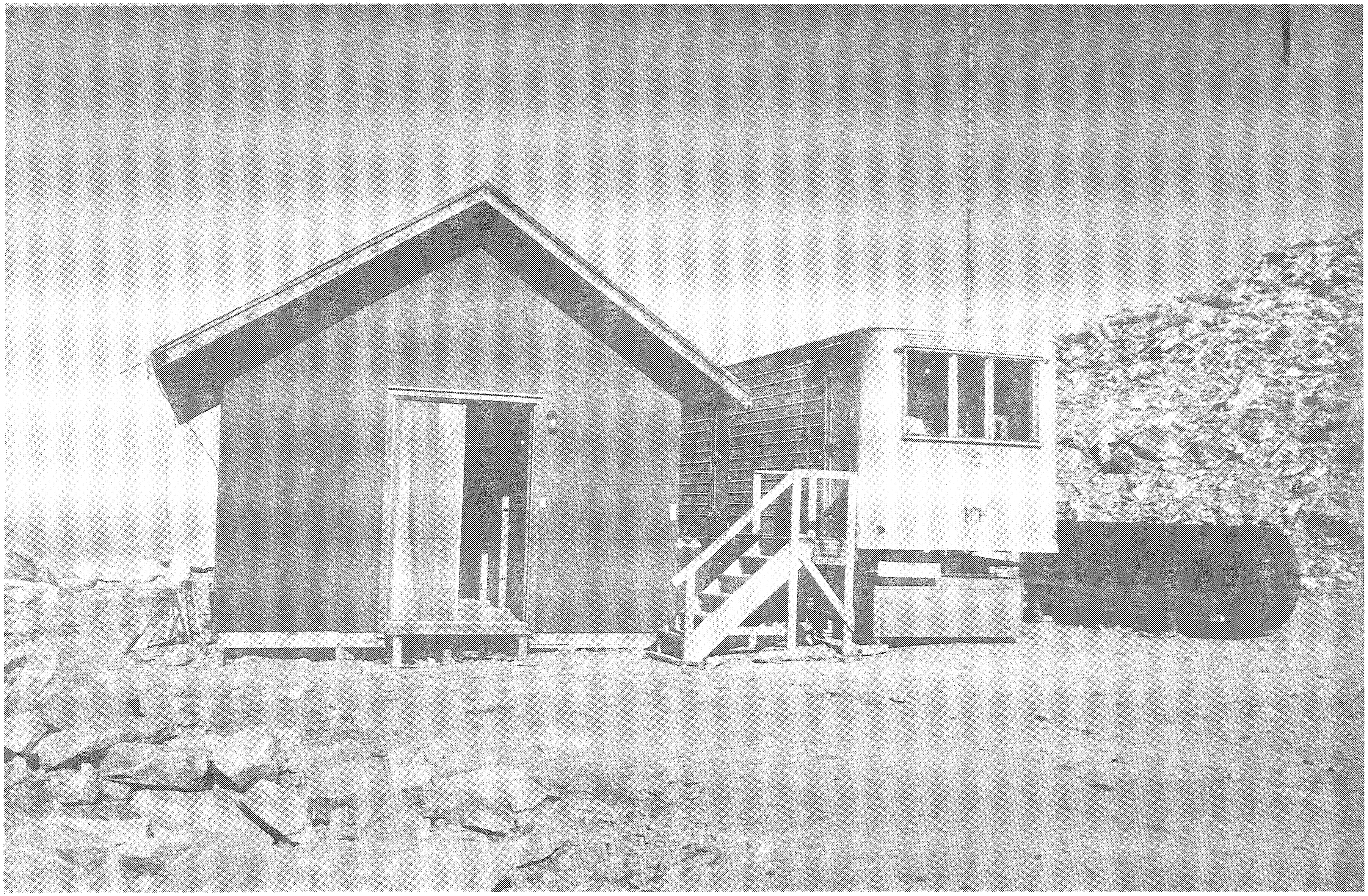
4. Total cross section and properties of pp interactions experiment, Echo Lake, 1968-1970.



5. Total cross section experiment with addition of HAPPE magnet, Echo Lake, 1970-1972 (constructed, never operated for physics).

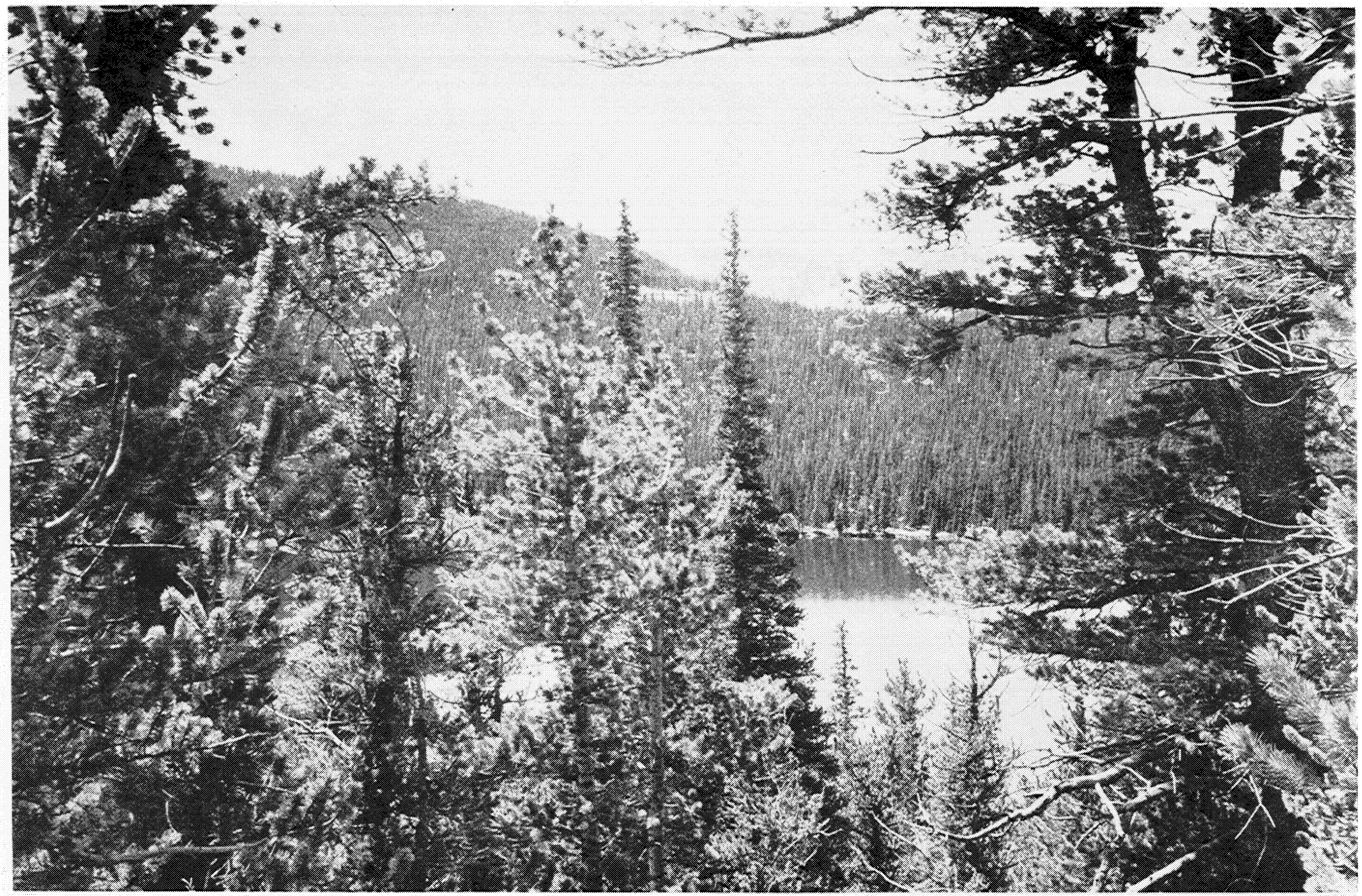


6. Summit of Mt. Evans.



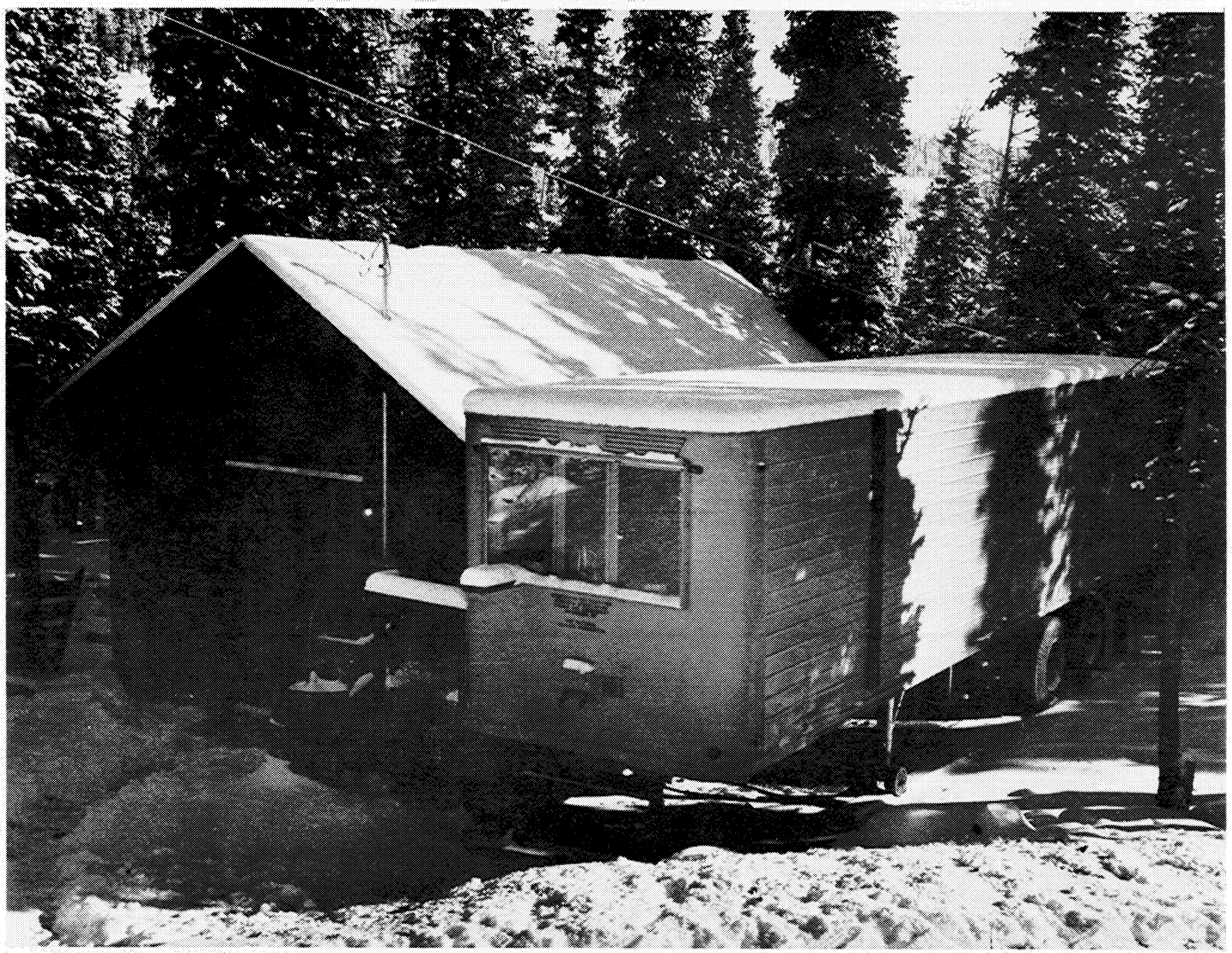
7. Quark search experiment, Mt. Evans summit, 1966.

8. O. Haas, C. Radmer, R. Brown, and R. Jones (adjusting short wave antenna). Mt. Evans summit, 1966.

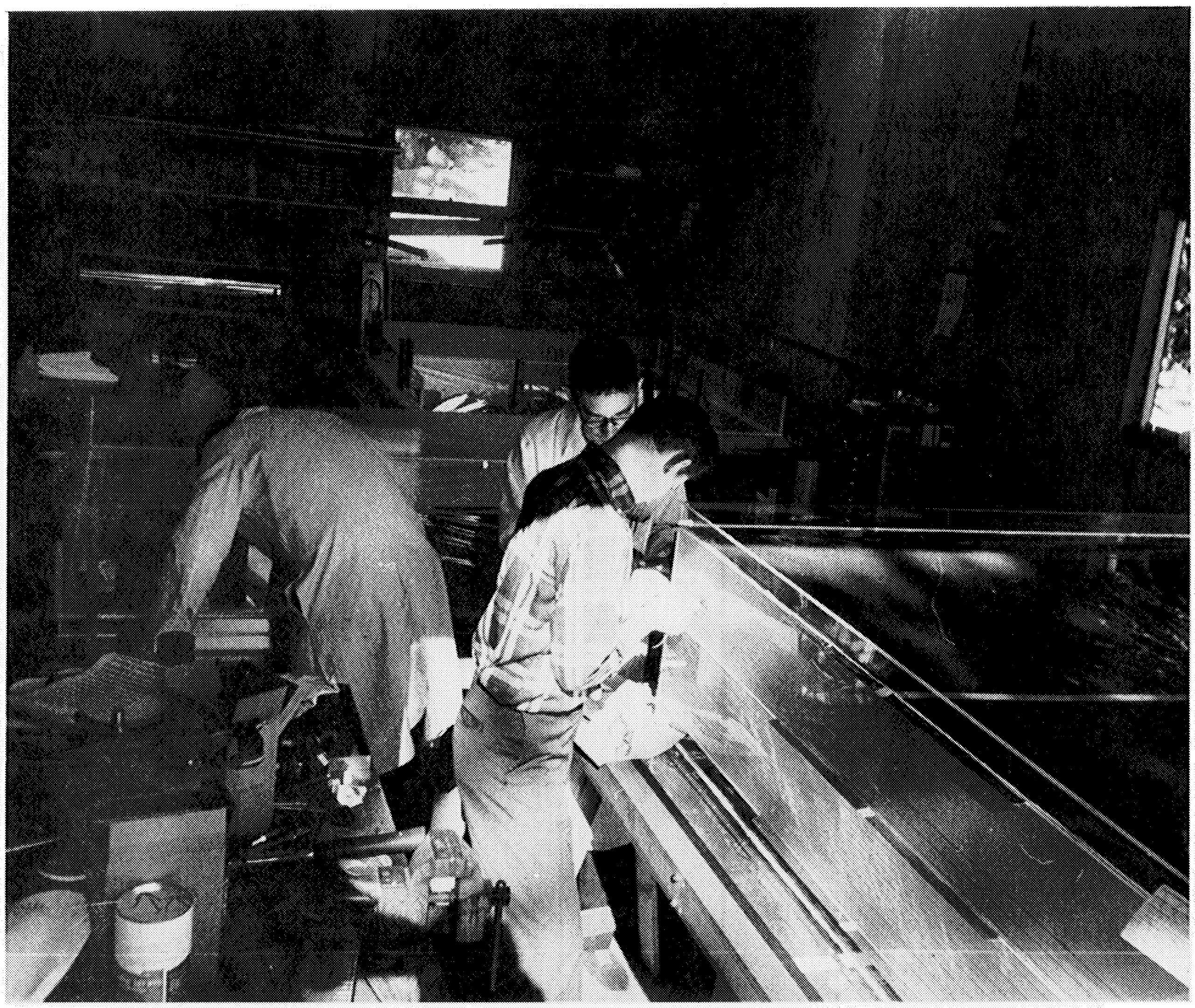


9. Echo Lake.

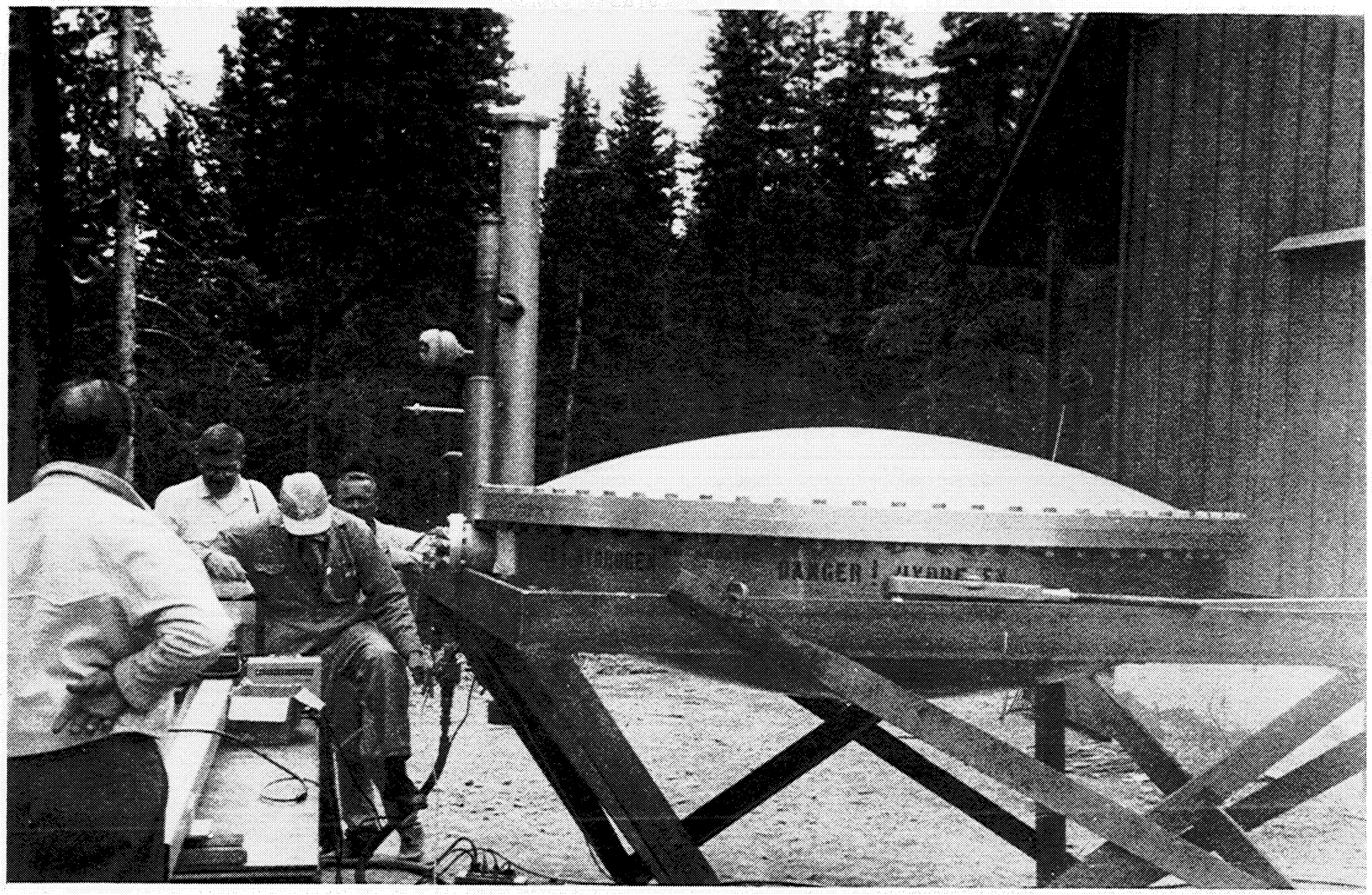
10. Echo Lake Dormitory.



11. "Quark Hall" and trailer, 1967.

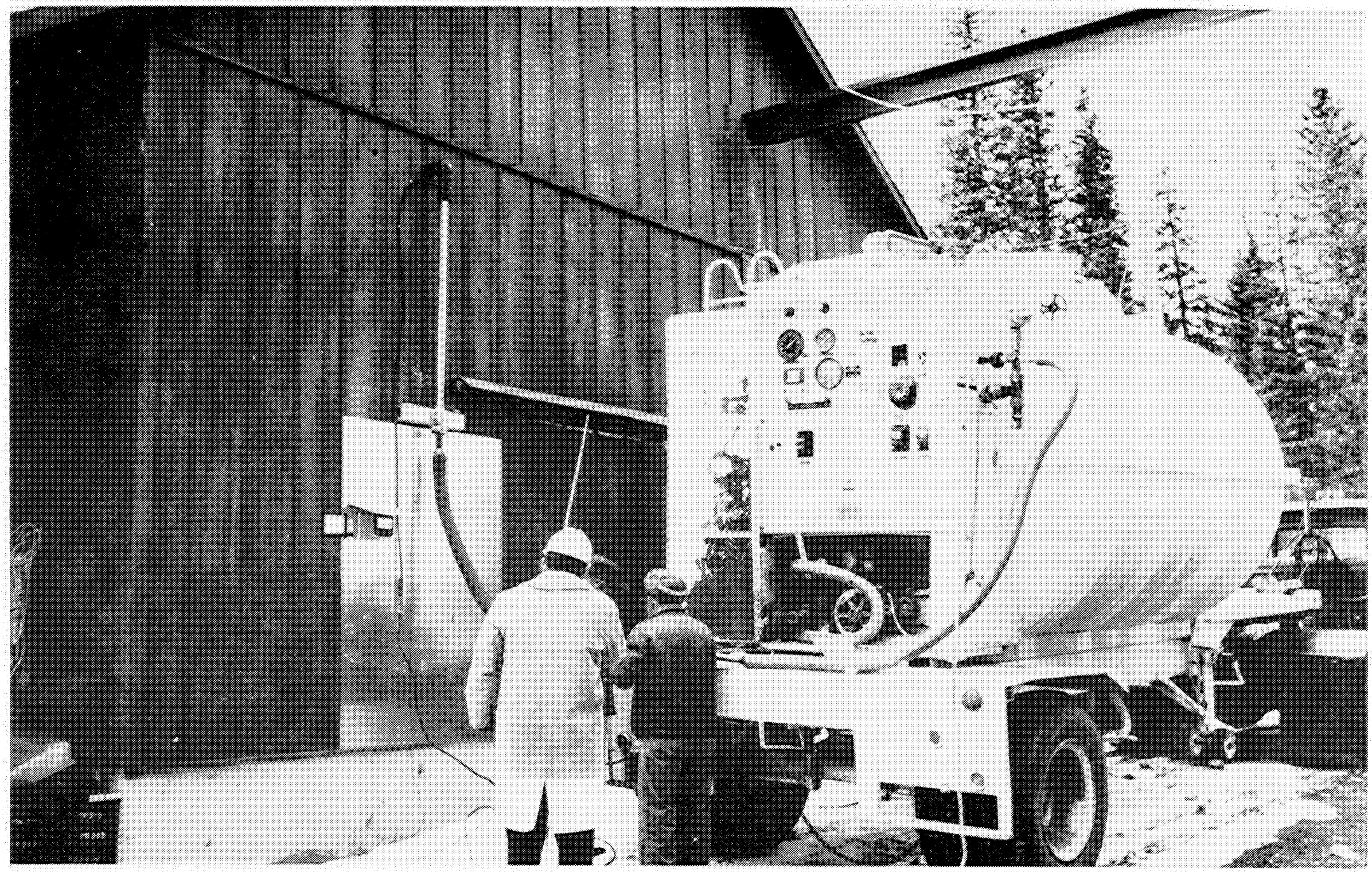


12. Large, wide-gap spark chamber construction, 1967. L. to R. K. Ericson, G. DeMeester, B. Loo, J. Pluta.



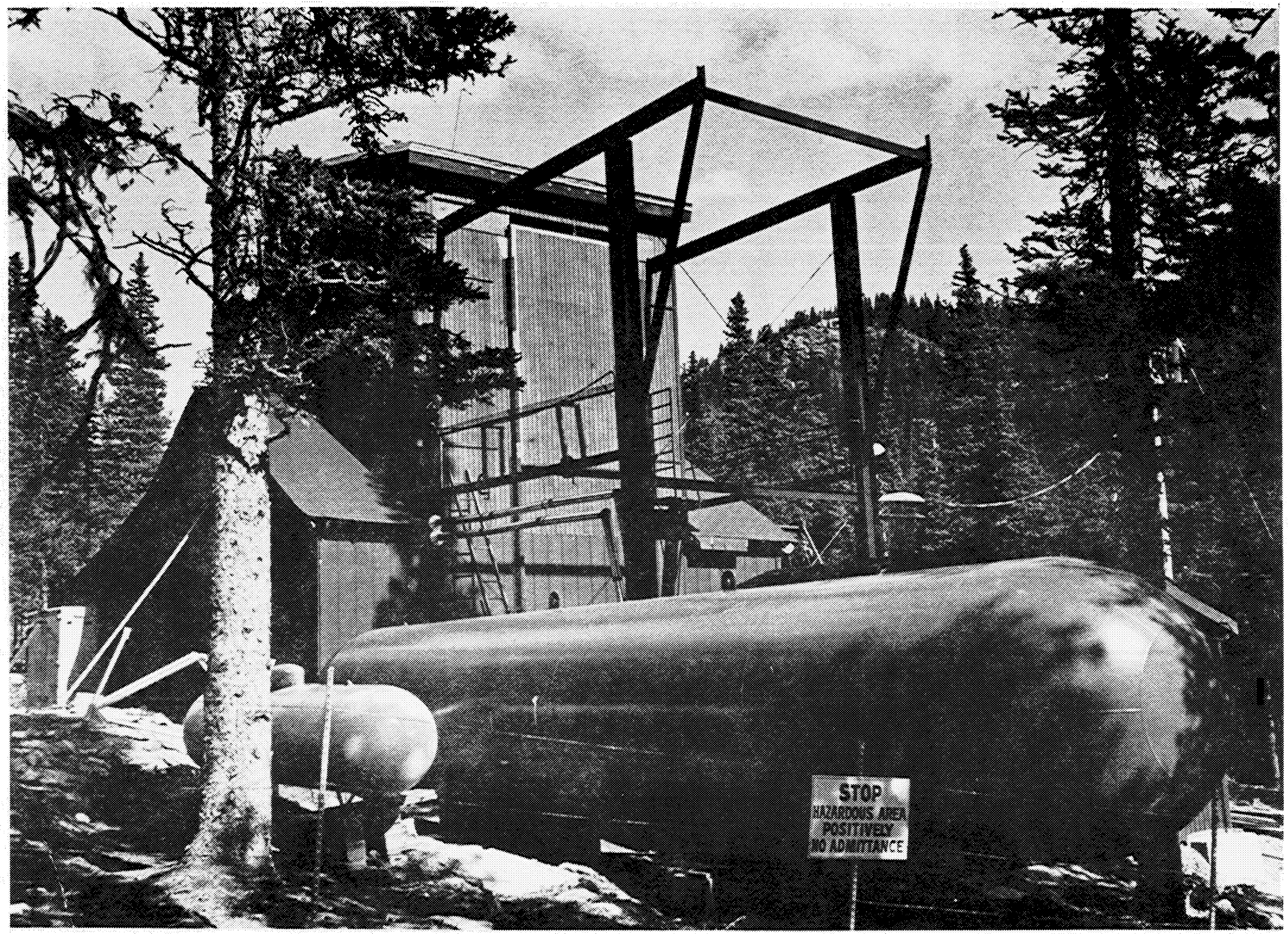
13. Hydrogen target before installation, 1968. L. to R. F. Mills, O. Haas, W. Winter (all facing camera).

14. "Sigma Hall," 1967. Bruce Cork in doorway.



15. F. Mills and Beech Aircraft Co. crew with hydrogen tank trailer. Transferring liquid hydrogen to target, 1969.

16. HAPPE magnet about to be lifted into "Sigma Hall," 1971.



17. "Sigma Hall" with helium storage tank, 1971.

Appendix A: Personnel

The Mt. Evans - Echo Lake experiments have drawn in a large number of physicists, students, engineers and technicians over the period of operation. For the record they are listed below:

Faculty:

Bruce Cork	Lawrence Berkeley Laboratory University of Michigan, and Argonne National Laboratory
Bruce Dayton	Los Angeles State College
Lawrence W. Jones	University of Michigan
Phil Kearney	Colorado State University
Yong Lee	Brookhaven National Laboratory
Robert March	University of Wisconsin
Erwin Marquit	University of Colorado and University of Minnesota
Donald I. Meyer	University of Michigan
Frederick E. Mills	University of Wisconsin and Brookhaven National Laboratory
Donald D. Reeder	University of Wisconsin
Richard F. Roth	University of Michigan and Eastern Michigan University

Foreign Visitors:

S. Lal P.V. Ramana Murthy A. Subramanian	Tata Institute of Fundamental Research, Bombay, India

Post Doctoral Physicists:

Al E. Bussian

Richard Hartung

Jordan J. Jones

John G. Learned

Donald E. Lyon, Jr.

Shoji Mikamo

David E. Pellett

Clifford Risk

Graduate Students:

Gordon D. DeMeester Billy W. Loo P.R. Vishwanath	University of Michigan
Kenneth N. Ericson	Colorado State University
James Borgwald Jeff Wilkes David Burress	University of Wisconsin
Alberto Benvenuti	University of Minnesota
Steven Schindler	Colorado State University

Engineering and Technical Staff:

William R. Winter Carl Radmer John Hicks August Springstube Rozwell Brown Paul J. Luxem Robert Beck	University of Wisconsin Physical Sciences Laboratory

Engineering and Technical Staff (cont'd):

John Vocelka Carl A. Baumann Glen Fisher H. Buer	University of Wisconsin Physical Sciences Laboratory
Orman Haas James Pluta Roger Rowell James Hassberger Paul Kolen	University of Michigan
John Starkey	University of Denver

Consultants:

Robert Venutti	University of Denver
Homer Lawrence	Beech Aircraft Corporation
William Dunlap	Skidmore, Owings, and Merrill
Ron Borden	Cryogenics Technology, Inc.
Clyde Taylor	Lawrence Livermore Laboratory

Appendix B: Financial Support

This cosmic ray program has been supported by a succession of grants and renewals from 1965 to 1973 by the National Science Foundation. The total direct financial support over this period has been $1,638,090.

In addition, some support was received from the U.S. Office of Naval Research (most importantly in the procurement of liquid hydrogen) and from the high energy research grants from the National Science Foundation to the University of Michigan and the Atomic Energy Commission contract at the University of Wisconsin. The HAPPE magnet was borrowed from the University of California Space Sciences Laboratory, which also purchased the CTI 1400 helium liquifier-refrigerator for our use. Computer facilities and time were made available by the National Center for Atmospheric Research in Boulder. Great technical assistance and important equipment loans were obtained from the Lawrence Berkeley Laboratory, the Argonne National Laboratory, and the Brookhaven National Laboratory. Valuable cooperation by the Beech Aircraft Corporation in Boulder was crucial to the success of the hydrogen experiment. The Colorado Universities, Colorado State University (Fort Collins), The University of Colorado (Boulder), and Denver University (Denver) contributed faculty time and other assistance. In particular, the entire series of experiments would not have been possible without the facilities, administrative support, and enthusiastic cooperation of Denver University.

Appendix C

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DOCTORAL THESES IN PROCESS

P.R. Vishwanath, University of Michigan.

R.J. Wilkes, University of Wisconsin.