Chandra and his students at Yerkes Observatory

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Abstract. S. Chandrasekhar’s interactions with graduate students in his more than a quarter century at Yerkes Observatory are described. His graduate teaching, Ph.D. thesis students, colloquia and colloquium series, and seminar series were all important aspects of this side of his scientific research career. His managing editorship of The Astrophysical Journal, his one experience in observational astrophysics, a second paper he wrote describing some of the early observational work at Yerkes Observatory, and a third on “the case for astronomy” are all discussed. A famous myth about one of his courses is corrected, and the circumstances under which the “S. Candlestickmaker” parody was written are recounted. Chandra’s computers, recruited in the Williams Bay community, are mentioned. A complete or nearly complete table of all the thesis students who received their Ph.D. degrees under his supervision, at Yerkes and on the campus in Chicago up through his last one in Astronomy and Astrophysics in 1973, is presented, with references to their published thesis papers.

1. Introduction

Subrahmanyan Chandrasekhar spent more than a quarter of a century at Yerkes Observatory, a large part of his scientific career. While he was in residence there he wrote four books and more than two hundred papers, moved up the academic hierarchy from research associate to distinguished service professor, and became an American citizen. Other papers in this memorial issue of the Journal of Astrophysics and Astronomy summarize and evaluate Chandra’s research in the many different fields of astrophysics in which he successively worked, each written by a distinguished expert in that field. My own paper is different; in it I try to describe his scientific activities at Yerkes, particularly in teaching, advising and molding graduate students, of whom I was fortunate to be one. This contribution is therefore based on the memories of a participant, but with very great help from many fellow “Chandra-Ph.D.’s,” who responded to my requests for specifics of their careers, and of their insights into our former mentor’s role in preparing them for independent scientific work. I have tried to follow the goal enunciated by my fellow author, Norman Lebovitz, to analyze Chandra’s contributions seriously and as fully as I can,
avoiding extravagant praise, of which he was wary, but taking him quite seriously, as he surely would have wished (and as I always did!).

Chandra’s biography, by Kameshwar C. Wali (1991), is an excellent record of the events of his life, as he saw it himself in his late sixties and his seventies. The treatment of his Yerkes years as given there is rather brief, however, and I hope that this paper, written from my own, quite different perspective, will add new insights into his very great contributions toward preparing the next generation of research astrophysicists. Although my main focus is on Chandra’s years at Yerkes, I continue with his teaching and Ph.D. students on campus in Chicago, after his move there in 1964, until his last astronomy and astrophysics Ph.D., Bonnie D. Miller, was awarded her degree in 1973.

2. Early history

As described fully in my book, now in press, *Yerkes Observatory 1892–1950: The Birth, Near Death and Resurrection of a Scientific Research Institution* in 1936–37 its young director, Otto Struve, recommended the appointments of the even younger Gerard P. Kuiper, Bengt Strömgren, and Chandra to the University of Chicago’s young president, Robert M. Hutchins. All three were foreigners; such appointments were unusual in those days when most scientists in American universities came from families which had been in this country for generations. But Struve wanted the best, wherever he could find them, and Hutchins backed him fully. Struve wanted to make the University of Chicago the outstanding power in astrophysics in the world; Kuiper was an observer whose interests were in that direction, while Strömgren and Chandra were theoretical astrophysicists, a very rare breed in the United States of those years. Nearly all the astronomy department faculty members lived and worked at Yerkes Observatory, in the little village of Williams Bay, Wisconsin, some eighty miles from the campus, a site selected to be out of the smoke, haze and fog of Chicago, and as it turned out, free of the growing light pollution as well. William W. Morgan, an observational spectroscopist who had been Struve’s second Ph.D. thesis student, was already on its staff. He, with Chandra, Kuiper, and Strömgren became the key members of Struve’s brilliant young research group.

There had always been a small outpost of astronomers on the campus in Chicago, devoted entirely to celestial mechanics, and closely connected with the mathematics department. Its most famous member had been Forest R. Moulton in the early years; the retirement of William D. MacMillan in the summer of 1936 created one of the openings for a new faculty member which Struve filled. He wanted to replace the celestial mechanics experts on campus with astrophysicists. Strömgren, when he arrived in September 1936, was originally stationed in Chicago, but he was so productive and valuable in research that after only two quarters Struve moved him to Yerkes, so that he could interact fully with all the other staff members who were working there.

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When Struve recruited Chandra, his initial thought had been to put him on the
campus with Strömgren. Hutchins was strong for this idea. But Henry G. Gale,
the laboratory spectroscopist who was dean of physical sciences, had been born
and grew up in Aurora, Illinois, and spent his entire adult life at the University of
Chicago. He was strongly prejudiced against anyone with a dark skin. To him
Chandra was a black, Negro, or “colored man”, the polite term of his time. Chicago
was a de facto segregated city, Hyde Park was an all-white suburb close to the
boundary of the Black Belt, and Gale would not allow a “Negro” to teach in his
division on the campus. Struve, Kuiper and Strömgren, born and raised abroad,
were completely free of this prejudice; Hutchins, the son of a liberal Presbyterian
minister who had been a professor at Oberlin College, Ohio before he became
president of Berea College, Kentucky, had been brought up to abhor it. However,
Gale was not alone in his beliefs; the University of Chicago trustees and business
agents were determined to keep Hyde Park an all-white enclave to protect its heavy
investment in residential property in the campus area, and probably a majority of the
American-born faculty members of his division shared his feelings about blacks,
although not about a high-caste Indian with a Ph.D.

Struve was aware of all this. He was determined to have Chandra on his faculty,
and although Gale advised against the appointment and forbade the Yerkes director
to station the Indian astrophysicist on campus, Struve went around him and carried
on his negotiations directly with Hutchins and Emery T. Fülbey, dean of the faculty.
The director carefully paved the way for Chandrasekhar when he came west from
Harvard, where he was lecturing, to give two colloquia and see the observatory
and its staff. Struve made a reservation for him at International House, the only
unsegregated lodging place where a short-term visitor could get a room near the
campus, cautioning everyone that Chandra was a distinguished Indian scientist, and
a fellow of Trinity College, Cambridge. Struve sent Chandra careful directions on
just how to get to the right railroad station in Chicago, had a car from Yerkes meet
him at the station near Williams Bay, insisted that he stay as guest in his home
where Struve’s wife prepared his vegetarian meals, and drove him back to Chicago
himself. President Hutchins did not have time to meet Chandra, but afterward at
Struve’s suggestion sent him a radiogram on the ship on which he was returning to
England, urging him to accept the Chicago offer.

It tipped the balance, and when Chandra returned to Williams Bay to stay in
December 1936, Struve arranged for him and his bride, Lalitha, to stay with the
Kuipers until their own house was ready for them. The director prepared a general
letter of introduction for him, attesting that “Dr. Chandrasekhar of Madras, India
and Cambridge University, England” was now “a valued member of our scientific
staff” to help smooth the way for him and his wife in the little Wisconsin community.

Chandra was proud and sensitive; needless to say he knew what was going on,
and was well aware of the slights to which he could all too easily be subjected
outside of Williams Bay. Even as late as the 1950’s, when he and his wife went
on summer vacations, he found it expedient to phone ahead to resorts and hotels to
explain that they were Indians, to make sure they would be welcome. It was not easy
for him to live in America, and it marked his personality (Wali 1991; Osterbrock 1997).

3. Courses at Yerkes Observatory

With the coming of his three new faculty members in 1936–37, Struve reorganized the graduate teaching at Yerkes Observatory. He knew from his own experiences as a graduate student there in 1921–23, and as a faculty member since then, that the previous system was woefully inadequate. Typically there had been three to six students, spending most of their time working as assistants and doing research, and taking three reading and research courses each quarter (except in summer, when their number would swell as another five or six teachers at nearby colleges and universities arrived, to work leisurely toward the Ph.D. degrees which would bring them salary raises and promotions at their home institutions). Each professor taught his own specialty, and since there were no theoretical astrophysicists on the faculty, no one taught that subject. Struve, a demon observer who had essentially no training in modern or even advanced physics, knew only what little astrophysics he had been able to pick up by reading, but was determined that the Yerkes students should learn what he had not. He set up a two-year cycle of eighteen one-quarter courses, three to be given each quarter. All the graduate students were required to take all the courses, and their final examination for the Ph.D. was based upon them. Chandra’s first teaching assignment was three quarters of stellar interiors, the subject on which he was then working, spread over his first two years at Yerkes. Strömgren taught stellar atmospheres, but in the spring of 1938 he returned to Copenhagen, and from then on Chandra taught that subject too. Struve put him in charge of the graduate teaching program after Strömgren’s departure. There were typically ten to fifteen students, most of whom were taking the courses, as the normal time required to earn a Ph.D. was three years after arrival at Williams Bay. By the immediate post-World War II period, when Chandra did the bulk of his teaching at Yerkes, the course structure and schedule were well established. No courses were taught in the morning, so the students and professors who had observed with the telescopes after midnight could try to sleep. Monday afternoon was the colloquium time, while Tuesday, Wednesday and Thursday afternoons were devoted to one course each. Typically a lecture would continue for an hour to an hour and a half. Friday afternoon any professor who had not finished all he had wanted to say in his regular class period could give a second lecture, or if a long-term visitor such as Pol Swings was presenting a series of lectures, it would be his day. Most faculty members assigned some homework in their courses, frequently based on observational data or observing assignments, and nearly all gave either a long take-home problem or a final examination at the end of the quarter.

All the courses were given in the one lecture room, at the end of the long hall of the observatory building, nearest the dome of the 12-inch refractor. All of the students had bachelors’ degrees when they came to Yerkes, many had masters’
degrees from other universities, and none of them ever had to go to the campus except to graduate.

4. Chandra’s teaching

Chandra eventually taught not only stellar interiors and atmospheres, but also stellar dynamics, and at times even molecular spectroscopy (after Gerhard Herzberg, whose course it had been, and John G. Phillips, his student and successor on the faculty, had both left Yerkes). Chandra’s lectures in all his courses were formal and highly mathematical, with very little discussion of the physical ideas. He wore a dark suit (or a light gray one in summer), white shirt and conservative tie, and spoke in complete sentences. Chandra presented the basic equations, such as the equation of radiative equilibrium, and then worked through the methods of solving them, doing all the mathematical steps at the blackboard, only very rarely making a mistake in sign or arithmetic as he transposed terms or factored equations, and seldom consulting his notes. His presentation was very well organized and logical, with no loose ends. Chandra’s Indian-accented English was hard for many American students to understand easily at first, and as he proceeded through the equations they often had difficulty keeping up with him. However, they soon overcame whatever language problem they had, and came out of his courses with “good sets of notes” as several of them told me, which some of them used as reference sources for their own teaching for years.

All of the graduate students who did their Ph.D. theses with Chandra were good in mathematics; he selected them for that skill or ability, and would not accept students who were not. Hence it is not surprising that nearly all of them considered him an excellent teacher, from whom they learned a lot. Some wished that he had taught more about the basic physical ideas than about the mathematical steps he had developed to go from them to the final results, but that was his style of doing science, and it took him far indeed.

In my own experience, Chandra was a very good teacher, and at his best in teaching a course which he had given two or three times previously, such as stellar atmospheres and radiative transfer in my time at Yerkes (1949–52). Then he knew the material well, had it organized in excellent fashion, and was still fresh and interested in it. All the courses he taught were in subjects in which he had worked, produced a long series of papers, taken it as far as he could go, and eventually written a book, although that generally came after he had taught it several times. A subject he had taught many times, and had not worked in for several years, such as stellar interiors in my time, was no longer of central interest to him, and in fact perhaps somewhat boring. Then he could not communicate the same interest and intensity, and as he hated to waste time in preparation for a lecture on a subject he had taught so many times, might make a minor slip in an equation and get bogged down in correcting it while he was at the board, rather than dealing with the central issues of the topic. Once Chandra had written his book, he moved on to a new subject, and
made little attempt to keep up with the developing literature of the old one. In his courses he taught the subject as he had found it, with what he and his students had added to it, but hardly anything about newer results that others had obtained. Thus we learned little of the early groping steps toward understanding stellar evolution which George Gamow had recently made, and which Martin Schwarzschild, Fred Hoyle and their collaborators were making then. Chandra’s way was excellent for preparing students to work with him on the research he was then doing, but was not so good for producing well-rounded research scientists from the students who were working with other professors on more observational topics, but needed to understand the basic ideas of stellar atmospheres and stellar interiors.

However, the majority of the observationally oriented Yerkes students of the 1940’s and 1950’s who answered my recent queries were quite positive about Chandra’s teaching. He did his best to get the basics across, and at the very least succeeded in communicating his enthusiasm to them. Those who were mathematically inclined particularly enjoyed his approach. In his eagerness to teach, Chandra could be demanding, dictatorial, sometimes even insulting with students he did not know well, although in his own mind he was simply trying to impress them with the necessity of more study to achieve understanding. Thus he could reply curtly to questions in class, catechize students he encountered in the hall or library, or otherwise humiliate them in the presence of others. A few of these students were afraid of him; one was known to flee from his basement cubicle when he heard Chandra’s characteristic footsteps coming down the stairs from his office on the first floor. Others, in self-defense, replied semi-contemptuously to Chandra (or so it seemed to him), marking themselves as enemies in his eyes. He never subjected his own students to this treatment, nor the great majority of the observational students, but some of those whom he did harass in this way have never been able to forget it. Chandra drove more than one student out of Yerkes Observatory, but surely not everyone who came there should expect to get a degree automatically, he would have replied.

5. On the campus

Chandra did not teach a course on the campus until after World War II, although he and several other professors from Yerkes had commuted to the city to give one or two lectures each in an elementary course at the University of Chicago Downtown Center in the spring of 1938. Even this had aroused Dean Gale’s ire, but Struve, with Hutchins’s support, had faced him down, as described in detail, with full references to contemporary documents, in my book (Osterbrock 1997). However, Gale retired in 1940, to be succeeded by Arthur H. Compton, a socially responsible physicist who had many ties with India. Chandra was never again unwelcome on the campus, and by 1949–50 he was an internationally renowned scientist, whom Chancellor Hutchins (as he was now titled) would have liked very much to have there. Struve had proposed various reorganization plans, beginning
in 1941, some of which involved Chandra’s transfer to Chicago, but in the end, for one reason or another, he did not make the move until years later. However, in 1948–49 the astronomy department offered, for the first time, a one-year sequence of beginning graduate courses in astrophysics on campus, designed to interest or hold the attention of students who would then go on to Yerkes to complete their training. Chandra, with Guido Münch collaborating, taught the first course in this series, Astronomy 301, Topics in the Theory of Stellar Atmospheres, in the fall quarter. It was to become the source of one of the great legends of Chandra, which President John T. Wilson loved to tell, and which the subject himself apparently enjoyed (Wali 1991). According to the president’s version, Chandra used to drive hundreds of miles between Yerkes Observatory and the campus, week after week, to teach a class consisting of only two students, but in the end all his travel and effort was justified, because in 1957 those two students, Chen Ning Yang and Tsung-Dao Lee, jointly received the Nobel Prize in Physics. Later Wilson could have added, as Chandra, their teacher, did himself in 1983.

In fact, however, there were quite a few more students in the class, and neither Yang, who was then a postdoc working with Enrico Fermi, nor Lee, a graduate student doing the same, was actually registered for the course. They were both sitting in on it, as were Fermi, Marcel Schein (another physics professor who specialized in cosmic-ray research), and several younger physics faculty members, postdocs and graduate students. Chandra was already a famous theorist and somewhat of a figure of mystery on the campus; a few of the auditors no doubt simply wanted to see what he was like. Their numbers decreased as the quarter wore on, but Fermi, Schein (who often fell asleep in the front row and snored audibly, to Chandra’s clear but never vocally expressed distaste), Yang, and Lee remained true to the end of the quarter, probably along with several others whom I can no longer clearly remember. There were actually six students registered for the course to the end, as the grade record in the files of the Office of the Registrar, signed by Chandra, shows. Three of them, Richard L. Garwin, who was then also doing his thesis with Fermi, Arthur Uhlir, Jr., later a professor at Tufts, and I went on to Ph.D. degrees at Chicago; a fourth, John Goddard, died not long thereafter, before finishing his degree. Garwin, Uhlir, and I certainly attended the classes faithfully, and learned a lot from them, and I think that Goddard did too, although Wilson’s story clearly shows the fallibility of human memory long after an event, and the value of contemporary records in establishing facts. Yang’s and Lee’s recollections also agree with mine (and with the records); Yang told Walter Sullivan, the writer of Chandra’s obituary for the *New York Times*, that there had been such a class but there were more than two people in it. The other two students who had registered for the course in addition to the four of us apparently did not complete graduate degrees at Chicago, and may have stopped attending Chandra’s lectures; in 1995 the Registrar, bound to respect the privacy of students’ records, could only write me that some of the grades “were not satisfactory.”

As time went on Chandra’s research moved from stellar interiors, through galactic dynamics, then stellar atmospheres, to turbulence, hydrodynamic and hy-
dromagnetic stability and relativistic astrophysics, and the mathematical theory of black holes and colliding plane waves. After Struve’s resignation and departure for Berkeley in 1950, Strömgren returned as director. He was one of the leading theoretical astrophysicists in the world, working on stellar atmospheres, stellar interiors and stellar evolution, and on the observational application of these subjects to gaining physical understanding of how real stars form, live, transform themselves into red giants, planetary nebulae, white dwarfs, and/or supernovae, and die. Strömgren was up to date in all these fields and it was natural for him to take over teaching them. Chandra was no doubt glad to let him do so, to free his time for his own research, in which he tended more and more to emphasize mathematical beauty and elegance. Once he gently chided me, probably during the 1963–64 academic year when I was a visiting professor at Yerkes, for spending so much time on planetary nebulae and H II regions. Everything I was doing depended on observational data, he told me, which could easily turn out to be wrong. (I did not think it wise to mention that much of that data actually came from my own observational work, and the work of my Ph.D students’ of the time!) His own work, Chandra said, was based on a few easily stated assumptions, and being mathematical, would always endure. To most of the graduate students who then were coming to Yerkes Observvatory, learning about the real universe seemed more attractive than mathematical truth and beauty. Many of the faculty members shared that feeling. Under these circumstances a growing sense of alienation naturally arose between Chandra and his colleagues, as is well expressed in Wali’s book.

With the diplomatic Strömgren at the helm, Chandra could continue to work effectively at Yerkes, but his interests were turning increasingly to the campus in Chicago. His two papers with Fermi on magnetic fields in the interstellar matter in the spiral arms of the Galaxy and their stability played a powerful role in this attraction (Chandrasekhar & Fermi 1953a, b). Chandra deeply admired this outstanding genius of physics, whose philosophy was “to use every dirty trick at your command” (combining theoretical and experimental reasoning) to solve the most important physical (and hence astrophysical) problems of his time. By then Chandra’s own approach was quite different. He had tremendous mathematical powers. Guido Münch, who worked closely with Chandra for several years, commented on one aspect of this. He could work for weeks on the solution of a complicated equation or set of coupled equations, and in the end break it, often guided by his “intuition”, actually the result of years of concentrated experience.

6. Observational research

Full-time theoretical astrophysicists were exceedingly rare in America when Struve hired Chandra on the Yerkes staff in 1936. Henry Norris Russell at Princeton was practically the only one who did no observing himself; his Ph.D. thesis student, Donald H. Menzel, who had joined the Harvard faculty in 1932, was the other, much younger, theorist in the country, and he did observational work as well. Struve was
convinced that America in general and Yerkes Observatory in particular could only really advance with first-class theoreticians on the scene, to guide the observers’ thinking and interpret their results, and he went abroad to get them. Many working astronomers were highly skeptical, and Harlow Shapley, director at Harvard, had advised Struve against adding Chandra to the Yerkes faculty (although he was simultaneously trying to persuade the young theoretician to accept a further short-term appointment at his own university). Even Struve had some doubts about a “pure” theoretical astrophysicist, and wrote Kuiper, who was then at Harvard with Chandra, that it would be “decidedly advantageous” if the latter would undertake “a small amount” of observational work at Yerkes. When Kuiper mentioned this idea to Chandra, he welcomed the idea in principle, and thought he might try some observational work on solar prominences, connected with his theoretical ideas on the outer layers of the sun.

Undoubtedly Chandra was sincere in this thought, but research is a highly specialized business, and he never had the time to learn all the intricacies of operating a large refracting telescope and a solar spectrograph. It would have been a great waste of his talents, as Struve recognized as soon as Chandra got to Yerkes and began producing papers and books packed with new theoretical results. The director never brought up that idea again.

But Chandra coauthored one purely observational paper, years later, surely of his own volition. This was a report on an eclipse expedition, on which he photographed the solar corona rather than the chromosphere or prominences. The eclipse had a relatively short totality, only thirty-seven seconds, but it occurred in July 1945, just as World War II was winding down to a close, two months after the defeat of Germany and two months before the Japanese surrender. All during the war Chandra had been working hard on weapons development at the Army Ballistic Research Laboratory at Aberdeen, Maryland, alternating three weeks there and three weeks back in Williams Bay, where W. Albert Hiltner, the leader of the eclipse group, had been working on the same type of project at the Yerkes Optical Bureau. No doubt the trip to the remote observing site on the line of totality near Pine River, Manitoba was a welcome diversion after three years of wartime tension. They were joined there by Burke Smith, a stellar spectroscopist who had collaborated with Struve at Yerkes. Chandra and Smith helped Hiltner set up the two photographic telescopes they had brought with them from Yerkes. On the eclipse date the sky was clear and Chandra got a good photograph of the corona with the shorter focal-length instrument, while Hiltner obtained two of the outer chromosphere and inner corona with the large-scale instrument. They duly published reproductions of these photographs in a two-page paper, in the tradition of the time (Hiltner & Chandrasekhar 1945), and Chandra soon went back to his important theoretical research on radiative transfer.

A second paper Chandra wrote described briefly some of the earlier observational work at Yerkes. It was one of a series of articles written at Struve’s behest, each by one of the top research workers at Yerkes, for its semicentennial in 1947, to be published as a group in Science fifty years after the observatory’s dedication. Each article described the earliest work by Hale, Walter S. Adams and the other
giants of the past in one particular subject, and emphasized the continuity by which that work had led down to the writer’s current research. This was easy enough for Struve, Morgan, Kuiper and Gerhard Herzberg, the molecular spectroscopist who had recently joined the Yerkes faculty, but there had been no theoretical astrophysicist at all in 1897 with whom Chandra could connect his work. In 1947 he was in the midst of his radiative-transfer and H⁻ period, so he wrote his article on “Solar Research and Theoretical Astrophysics” (Chandrasekhar 1947). It began with Hale’s spectroscopic confirmation, made at his own Kenwood Observatory in Chicago, that the yellow D3 emission line in the chromosphere was truly the same line emitted by helium gas on the earth, discovered in 1895; his later discovery of C₂ in emission in the low chromosphere; Edison Pettit’s studies of solar prominences; and Philip C. Keenan’s observational work on the solar granulation, all done with the 40-inch refractor. Then Chandra smoothly switched to his and his students’ theoretical work on H⁻, the continuous spectrum of the sun and its limb darkening, and scattering by free electrons in the atmospheres of hot stars. They all “serve[d] to underline a fact which Hale often emphasized,” Chandra concluded, “namely, that there is no essential difference between the attitudes of a physicist and an astronomer”.

Years later, in 1983 Case Western Reserve University awarded Chandra its Michelson-Morley Prize. Peter Pesch, a former Yerkes observational Ph.D. and a faculty member at CWRU, introduced Chandra for his prize lecture there. In his introduction Pesch showed a slide of the first page of the Hiltner and Chandrasekhar eclipse paper, listing the great relativity theorist as one of its coauthors, and another slide reproducing the coronal photograph he had taken. Chandra, picking up on the joke instantly, started his lecture with the comment that Pesch had destroyed [his] credibility!.

7. Colloquia

Struve put Chandra in charge of the colloquia at Yerkes, and he remained in that post until very nearly the year when he moved to Chicago. Monday afternoon was colloquium time, and one was scheduled every week as regularly as clockwork. Naturally there had been colloquia before he joined the Yerkes faculty, though on a more catch-as-catch-can schedule; he ignored them and began numbering the colloquia from the day his reign began, like an ancient king or emperor. There were plenty of scientific visitors to Williams Bay in Struve’s and Strömgren’s years as director, and Chandra saw that they all gave colloquia on their current or recent research while they were there. Gaps in the visitors were filled in with specific invitations to astronomers from nearby Madison, Northwestern University, the University of Illinois and other research centers, and to physicists from the campus. The senior Yerkes faculty members generally each gave one colloquium a year; younger assistant professors and instructors, and sometimes even graduate students, also gave them occasionally.
Chandra followed the English tradition of holding a regular tea after the colloquium, and in those bad old sexist days of my youth the faculty wives acted as hostesses, one scheduled for each week, pouring the tea and providing the refreshments. They vied with one another in baking and bringing rich cakes and cookies, which we students wolfed down whenever we thought Chandra’s back was turned, but he seemed to enjoy them too. The colloquia were held in the classroom down the hall from the library, and he had his own special chair, and his own special place, at the end of the second row nearest the door. Occasionally an unwary visitor would sit there before Chandra appeared to claim his seat. When he did come in a moment later, usually with the speaker in tow, he would recognize the situation at a glance and take another chair, still empty in the first row, and place it halfway outside the door, next to the visitor, and sit down there. The visitor, now embarrassed, would try to give up his chair but Chandra would not hear of it; his politeness would call forth further apologies and protestations from the visitor. Finally after three or four offers and refusals Chandra would at last accept his chair and the talk could begin. After the tea one of the students was assigned to wash the colloquium china cups, saucers and spoons, usually done in one of the basins in the men’s or women’s room, or occasionally in the one bathtub in the building.

We all certainly learned a lot of current astrophysics in these colloquia, perhaps imperfectly, but at least the central ideas. And Chandra made sure we were there to learn it; every graduate student and every faculty member was expected to attend every colloquium, and any student with the temerity to skip one, even if it was only “Recent Research at Such-and-such Observatory” by a visiting director who was more of an organizer than a research scientist, was sure to be subjected to a searching cross-examination the next day.

Chandra himself gave at least one colloquium every year, and frequently more. He always gave the even hundred numbered colloquia, making them festive occasions on which Lalitha, his wife, poured the tea and provided a special cake, but he always had a serious scientific message to bring to the auditors. His colloquia, like his lectures, were models of organization, extremely well presented and always interesting. In his early, pre-war years, Chandra gave even more colloquia, many of them didactic, for the faculty members as well as the students. Theoretical astrophysics was a subject few of them had studied, and he widened their horizons.

### 8. Seminars

In addition to the colloquia, Chandra ran a theoretical seminar during much of his period at Yerkes. He began it a few years after World War II, when the great dammed-up wave of new and returning graduate students hit Yerkes Observatory, and provided him with a steady source of good students. Most probably his seminars began operation in the summer or fall of 1948, when Chandra was seriously examining published work in turbulence, a subject astronomers, led by Struve, believed they had discovered empirically in stellar atmospheres years before. After the war
such leading theoretical physicists and aerodynamicists as Werner Heisenberg, C. F. von Weizsäcker, and young G. K. Batchelor had begun publishing papers on it, whetting Chandra’s interest. In later years he went on to magnetohydrodynamics, then to rotating ellipsoids, and then to general relativity.

The seminar was held regularly in the classroom on Monday evenings, and Chandra expected all the theoretically oriented graduate students who wanted to work with him (the two groups were identical at first, as there were no other senior theorists until Strömgren’s return in 1951) to be there, as well as any postdocs or visitors who were theorists. These seminars were Chandra’s way of getting into a new subject, and keeping abreast of the latest work in the field he was working on. He would assign papers, some recently published, and others which he had received as carbon copies of manuscripts just submitted for publication, in those pre-Xerox, pre-preprint days. Often he would report on the most interesting new papers himself. Whoever was assigned the paper was expected to study it in depth, work through all the equations, look for good new ideas and also for weak points, and report orally on it in two or three weeks. Ideally the report was very thorough; those in attendance were encouraged to ask questions, as Chandra himself always did.

He gave many of the reports himself, at least in the years I was there, when he was already working on turbulence and was starting to get into magnetohydrodynamic and plasma problems. In this situation he was frank in mentioning problems he had in understanding what an author was trying to do, and would welcome comments, questions and suggestions. He was happiest when he uncovered an error, found a mathematical shortcut the author of the paper had not seen, or in the course of analyzing and discussing the paper formulated a new problem which would be grist for his mill, or for his students. More than once his report on a paper gradually changed, over a period of a few weeks, into outlining a new paper he was writing, going beyond it or straightening out some of the flaws in it.

These seminars were an excellent introduction to actual research, for students who had previously been totally immersed in undergraduate or beginning graduate course work. Chandra was demonstrating how a real theorist works, welcoming our comments and questions, never answering curtly or abruptly, as he sometimes did in class lectures, when he was frequently under time pressure to keep up to his planned pace. T. D. Lee spent two quarters at Yerkes, the spring and summer of 1950, after completing his Ph.D. thesis on white-dwarf stars under Fermi on the campus, with Chandra as the astronomical consultant. The brilliant young postdoc (then twenty-three) attended the theoretical seminars regularly, and played a prominent part in the discussions. Their styles contrasted greatly, Chandra much more mathematical in his approach, Lee more physical, and when they occasionally reached different conclusions, groping toward an understanding of turbulence, the fur could fly. But they both remained civil, and the next day would again be discussing whether the mean turbulent kinetic-energy density was approximately equal to the mean turbulent magnetic-energy density (Lee’s formulation) or to a mean-square expression involving the curl of the magnetic field (Chandra’s result).
At times it seemed to be almost a replay of the arguments between Eddington and the brilliant young Chandra over the internal structure of white-dwarf stars a decade and a half earlier, now re-enacted in the quiet halls of Yerkes Observatory instead of at the Royal Astronomical Society’s meetings.

Chandra’s Henry Norris Russell Lecture (the third ever given, following the first one by Russell himself, and the second by the recently retired, great dean of observational astrophysics, Walter S. Adams) was on turbulence, nearly all of it based on material he and his students had discussed in that first year or two of the seminars (Chandrasekhar 1949). His graduate students were stimulated by the seminar series as well; two of them, after receiving their Ph.D.’s, went on studying and developing more applied aspects of turbulence theory, Marshal H. Wrubel as a postdoc at Princeton, and Su-shu Huang as a research associate who stayed briefly at Yerkes before following Struve to Berkeley (Wrubel 1950b; Huang 1950). My own little theoretical paper on the contribution of elastic scattering of free electrons by neutral H atoms to reducing the electrical conductivity of the solar atmosphere came out of the beginning magnetohydrodynamics period of the seminar series (Osterbrock 1952b).

9. Computers

Chandra’s research depended on large amounts of numerical computing, especially numerical integration of differential equations. He, like other theorists of his time, was an expert in carrying out such calculations, using an electric-powered, hand-operated computing machine. His graduate students learned to do it too, and several of them worked as assistants for him, especially in their earlier years, doing the time-consuming numerical work. In this research, separate from their thesis problems, they might participate in the theoretical development to a certain extent, but spent most of their efforts on computing. Often they became coauthors of the resulting papers. An example is some of the early work Chandra did on H− with Margaret Kiess Krogdahl (Chandrasekhar & Krogdahl 1943). It helped support her, and at the same time prepared her for her thesis with him on the inhomogeneous Stark effect in stellar atmospheres (Krogdahl 1944a, b).

However, for maximum long-term efficiency in Chandra’s ongoing research a full-time computer (the name then used for the person who used the machine) was clearly preferable. Theodosia (“Theo”) Belland, a resident of the nearby village of Fontana, became his first full-time computer, from 1940 to 1943. Earlier she had worked for Struve, and her husband, Fred Belland, was also on the observatory staff. Chandra himself taught Theo Belland how to carry out all the steps necessary to integrate numerically whatever complicated definite integrals, differential equation, or system of equations he had derived, as he later did for his other computers. In 1944 Frances Herman, succeeded to the post; soon afterward she married and became Frances Herman Breen. She worked with Chandra through 1948, but resigned when she was about to have her first child. In early 1949 Donna D. Elbert,
like Frances Breen a graduate of Williams Bay High School, took the job. Chandra
ecluded both of them as coauthors on papers for which they did unusually large
amounts of numerical work. Donna Elbert was to continue with Chandra for more
than three decades, and to become an outstanding numerical computer.

10. The Astrophysical Journal

A prolific author, Chandra published most of the papers he wrote at Yerkes in the
Astrophysical Journal. Ultimately he published 137 papers in it, up to 1994 a record
second only to that of Struve, who published 228 papers in it in his lifetime (Abt
1995b). Like many enduring astronomical institutions, the Astrophysical Journal
was the result of Hale’s organizational activities, founded by him and his older,
then better-known astrophysicist friend, James E. Keeler, in 1895. The Journal
belonged to the University of Chicago, and was published by its Press; Hale and
the successive directors of Yerkes Observatory after him, Edwin B. Frost and
Struve were automatically its managing editors. From its start the Astrophysical
Journal was the leading journal of astrophysics in America, with nearly all the
papers from Yerkes and after its founding, Mount Wilson Observatories, published
in it, and the astrophysical papers from most other observatories and research
centers in the United States. There were many from abroad. After 1942 the
Journal was published “in collaboration with the American Astronomical Society,”
and Harvard and Lick Observatories, previously the main holdouts, also sent their
papers to it, but the University of Chicago Press retained ownership, control, and the
managing editorship. After World War II ended, Struve was tired and overworked;
in 1946 he named Chandra associate managing editor, and then in 1947 gave up
the managing editorship to Morgan. No doubt Struve still believed that an observer
should hold that post, rather than a theoretician. After Struve’s departure in 1950,
Chandra played the leading role in negotiating an agreement with the AAS, under
which it gained more control over the editorial board and policy of the Journal, in
exchange for the financial assurance the Society provided by requiring its members
to subscribe to it. Chandra remained associate managing editor under Morgan,
but the latter suffered a nervous breakdown, was hospitalized, and resigned the
editorship in 1952. Then there was no choice but for Chandra to replace him, and
he continued in the post for nineteen very fruitful years.

His first editorial assistant at Yerkes, where the papers were received, acknowl-
edged, and sent out to referees, and where he accepted, or rejected them (or more
frequently, returned them to their authors for revisions), was Mary Horvath Rich-
mond. She, like Frances Breen and Donna Elbert, was a locally recruited Williams
Bay woman.

With all the copy-editing, illustrations, make-up and other technical aspects of
the Astrophysical Journal concentrated at the Press office on the campus, Chandra
had another reason for going to Chicago frequently. When he had made only
occasional trips there, he often rode the train, a commuter line to the Loop, but
like Struve before him, he found driving his own car was much more convenient, especially in giving him the freedom to return to Williams Bay late in the evening. Lalitha often accompanied him, and he was always willing to take students or visiting scientists, up to the capacity of his car. He enjoyed company and conversation on the two-hour drives each way. The riders had to be sure to meet him right on time at the appointed corner on the campus for the return trip; Chandra made it clear that he would follow his schedule, no matter if they were there or not, and no one wanted to test him. Accustomed to rising early, he would leave Williams Bay at 6:30 a.m. or so to have a full day on the campus. This caused problems for many of the observational types (and some theoreticians as well) who tended to stay on a schedule of working until well past midnight in their offices, and not arising until just before lunch. On more than one occasion Chandra was pleasantly surprised to find a bright-eyed passenger like Leonida Rosino (a visiting astronomer from Padua) or Imam I. Ahmad (a graduate student from Egypt) waiting early for him in the dark morning as he drove up to the observatory to meet them for the trip to Chicago, not realizing that they had decided it was not worth going to bed for only a few hours, and had stayed up all night.

Chandra worked very hard on the Journal, spending increasing amounts of time and effort on it as it grew, under his watchful supervision. Struve, in his fifteen years as managing editor, and Morgan, in his five, had taken a broad view of astrophysics and had welcomed papers reporting observational results in the rapidly expanding “new” wavelength regions, radio-frequency, infrared (with sensitive new solid-state detectors) and ultraviolet (based on captured German rockets, which carried small telescopes and spectrographs above the earth’s atmosphere). They had also welcomed new theoretical ideas, Struve more warmly than Morgan, but Chandra’s long term as managing editor began as the post-war expansion of research science, fueled by massive new government funding, was just taking hold. He was the ideal person to ride it to success, highly receptive to observational papers which he believed to be good ones, and casting a wider net for theoretical papers than any of his predecessors had. There was really no place else but the Astrophysical Journal for the now rapidly expanding generation of trained American astrophysicists to publish their papers, and Chandra’s tremendous reputation encouraged many physicists to send their forays into astronomy to him rather than to the Physical Review. His great self-confidence and wide circle of contacts within the scientific community enabled him to make quick judgments as to whom to ask to referee a particular, newly submitted manuscript, and which reports to trust. He personally refereed many of the theoretical papers, and on occasion decided on the spot to accept what he thought of as especially important new observational papers. Occasionally he made a mistake (he tended to categorize certain theorists and observers as “good” or “bad,” and it was hard for him to change his thinking about them), but only rarely, and not too many people were badly hurt.

The Astrophysical Journal grew and flourished under Chandra’s management. The late 1950’s were the beginning of the post-Sputnik era in American science. The nation was prosperous and apprehensive about the U.S.S.R.; money flowed into...
space research and into its basic background, astronomy and astrophysics. In 1954 Chandra established the Astrophysical Journal Supplement Series, for less expensive publication of papers containing larger amounts of tabular data; it evolved into the preferred medium for longer papers. Then in 1967, concerned that the Physical Review Letters was draining off short papers reporting “spectacular new advances in astronomy” which some physicists persisted in sending to it for quick publication, Chandra founded a new, separate Astrophysical Journal, Letters to the Editor. It had its own fast-track schedule and was a spectacular success, recapturing the hot discovery papers to what Chandra considered their rightful place. During his reign as managing editor he greatly increased the rate of publication of research results, moving from one issue every two months to two per month; the total number of pages published grew five-fold in his nineteen years as managing editor. In 1970 he set up the Journal’s own production manager’s office in Chicago, to take some of the administrative effort off his own shoulders, and to make the task a little less onerous for future managing editors. Jeanette R. Burnett was the first holder of the post, while Jeanne Hopkins was the long-time chief technical and copy editor of the Journal. She compiled the Glossary of Astronomy and Astrophysics (with a foreword by Chandra), which went through two editions as the copy editors’ and publication secretaries’ bible.

Chandra, with his handsome, boyish charm, his well-dressed appearance, his unfailing courtesy to women, his enthusiasm, and his generous praise for a job well done, was an idol to them and to all the Press technical employees. He commanded their respect, and they were always ready to go the extra mile for him to get an issue out, if he asked them to. On the other hand, to complaining authors or recalcitrant referees he could be caustic; he terrorized more of them than he charmed. His supreme self-confidence, presence, scientific reputation and rapier-like wit made it impossible to win an argument with him. Yet for authors who met his exacting standards and whose work he respected he provided fast, efficient publication. His one failing as an editor, I thought, was his prejudice against Ph.D. theses condensed and rewritten into papers and submitted to the Astrophysical Journal; this was contrary to the Yerkes tradition, in which a thesis was written from the start as a manuscript for publication. Chandra could be brutal to a first-time author who, he thought, had not cut his thesis down enough and was trying to slip a padded manuscript past him. Wali’s book recounts a few such episodes from the editor’s point of view, but for a young Ph.D. they could be traumatic.

By the late 1960’s Chandra was tired of the job he had done so long and so well; he was ready to hand it over to a successor whom he could trust. He was greatly concerned about who this would be, and offered the post to at least four well-known research astrophysicists whom he had personally selected. (I was one of them, as Wali has revealed). It was a daunting prospect for anyone, because by then the Journal clearly absorbed so much of Chandra’s time, effort and resilience. How could any mere mortal carry on after him? But Helmut A. Abt accepted the challenge and proved a worthy successor, who worked hard and improved the Journal still further, over an even longer term than Chandra’s.
Before Chandra had given up the managing editorship, he brought about the transfer of the *Astrophysical Journal* from the University of Chicago to ownership by the American Astronomical Society. He was convinced that the leading journal in its field could no longer be the property of a single institution. It was too big, too expensive, and too important. Chandra personally negotiated an agreement under which the transfer was effected in 1971. Only his immense prestige in the University of Chicago and in the astronomical community made this step possible, and he had to work very hard to bring it about even so. But he succeeded, as he did in everything he wanted to do (Abt 1995a, b; Osterbrock 1995).

Long before he resigned as managing editor, Chandra had moved to Chicago. His interests had been steadily shifting from theories which applied to readily observable effects in common types of stars to questions of hydromagnetic stability and of rotating ellipsoids. He felt more and more kinship with the physicists on the campus, and less and less with the astronomers at Yerkes, who did not particularly want him to teach his current specialties to their students. The *Astrophysical Journal* necessitated frequent trips to Chicago. All these reasons combined to make the move inevitable. In the years immediately after World War II, he drove to Chicago nearly every week, usually on Thursday so that he could attend the physics colloquium in the late afternoon. Then when he took over as editor he began staying overnight, usually at International House, and spending Friday on campus as well. He started teaching physics there, at first the regular graduate quantum mechanics or electrodynamics course. At least one of the senior physics professors thought that Chandra did not teach enough quantum mechanical applications, and tried to make up for it when he himself taught the next quarter of the course, but all the physicists were glad to have their astrophysical colleague in his office in the Institute for Nuclear Studies on Ellis Avenue. In 1959 he and Lalitha rented a small apartment near the campus, so that they could stay overnight in their own base there, and come more frequently when it suited their plans.

Although it made all kinds of practical sense to move to Chicago and work full time on the campus, it must have been a long struggle in Chandra’s mind, whether to give up on astronomy at Yerkes Observatory, just as in 1951–53 it had been a hard decision for him to become a U.S. citizen (Wali 1991). In 1964 he published a curious article, “The Case for Astronomy,” quite unlike his typical research papers. This one, which he presented orally at a meeting of the American Philosophical Society in April 1963, was highly nonquantitative, discussing the relations between physics and astronomy in extreme generality. According to his analysis, studies of physical sciences are carried out at two levels, a primary one, seeking to formulate general laws, and a secondary level, seeking to analyze and interpret particular complexes of phenomena in terms of these basic laws. Then he gave several examples, starting with Newton’s law of gravitation (primary) and its application, by Newton himself, to interpret Kepler’s laws of planetary motion. This illustration showed, he wrote, that the primary level was “the domain of physics as commonly understood,” while the secondary level was “the domain of the various special branches of the physical sciences; ... astronomy is one such branch.” Nevertheless, he gave
several examples of particularly important “secondary” analyses, of white-dwarf stars, solar limb darkening, H-, stellar energy-production, synchrotron radiation, and non-thermal radio emission, ranging from his own earlier work to some of the most relevant astrophysical applications of 1963. Chandra continued that “the only crucial empirical evidence for the aesthetically most satisfying physical theory conceived by the mind of man — Einstein’s general theory of relativity — [was] the astronomical one derived from the motion of Mercury”. This led him to conclude that “the principal case for astronomy is the same as the case for any of the physical sciences. No less: but, perhaps more; for only in the scales provided by astronomy can we discern the largest in the natural order of things” (Chandrasekhar 1964).

But apparently that “principal case” was not enough to keep him at Yerkes Observatory, for that same year he and Lalitha went all the way, and moved to a high-rise building on South Lake Shore Drive. Their apartment was on the north side of the building, with a clear view all the way to the Loop, and Chandra liked to keep a pair of binoculars next to his chair, so he could read the time from the huge clock on the Wrigley Building. Three years later they moved back to the edge of the campus, in a modern, high-security apartment building on Dorchester Avenue close to International House. It was a walk of only a few blocks to his new office in the Laboratory for Astrophysics and Space Research, where he moved as soon as it was built. They lived in their Dorchester Avenue apartment until his death.

After Chandra moved to Chicago, four successive directors of Yerkes Observatory, Hiltner, C. Robert O’Dell, Lewis M. Hobbs, and D. A. Harper, kept his office there unoccupied and waiting for him for more than twenty years. No one else was assigned to use it, and only rarely, many years after he was gone, was even a short-term visitor allowed to occupy it. Likewise, for the first three years after he and Lalitha moved to Chicago, their university house, once E. E. Barnard’s and after him occupied by Frank E. Ross and his family, was kept vacant for their possible use. Chandra came back to Yerkes for departmental faculty meetings, but as they shifted to Chicago he appeared at Williams Bay less and less frequently, usually on a Sunday and often with a visitor who wanted to see the observatory. Finally in 1989 Chandra’s former office was turned over to a staff engineer for a year, and since 1991, James W. Gee, Jr., who became the Yerkes manager then, has occupied it.

11. Chandra’s Ph.D. thesis students

Chandra was an outstanding research scientist, recognized by membership in nearly every elite honorary scientific society to which an astrophysicist might conceivably aspire, and by every prize, medal and award right up to the Nobel Prize. But in addition he was a great teacher, particularly as a thesis adviser or supervisor for more than thirty years. For his own Ph.D. students, he was an outstanding teacher, mentor, opener of wide new horizons, and supporter. He liked to have bright, mathematically inclined students working with him, and I have yet to find one who
does not remember being his student as anything less than a wonderful experience and training for the future. He made them all work hard, but looking back on it, they all thought it had been good for them.

In his younger days Chandra’s reputation as a great theorist and teacher was not yet made, and theoretical astrophysics did not enjoy the importance it does today. His first Ph.D., I believe, was Gordon W. Wares, who had been an undergraduate student at the University of Washington, and then a graduate student at the University of California. He had spent three years at Berkeley and completed all his course requirements for the Ph.D., but theoretical astronomy still meant celestial mechanics and orbit determination there. Wares wanted to become a theoretical astrophysicist and transferred to Yerkes. Louis R. Henrich was a student at Columbia with the same aim; his professor, Jan Schilt, told him he should go to Chandra and he did. Wasley S. Krogdahl, his third Ph.D., had an excellent undergraduate record on the campus in Chicago and came to Yerkes by that route.

In 1945, after World War II ended, the big rush of former students back to graduate schools began, and of new ones whose academic careers had been interrupted. Chandra demanded a high degree of mathematical preparation, but he wanted thesis students, and he was a realist. Henry G. Horak remembers that in one of his classes in that period, Chandra, looking at the students, remarked, “I don’t think that you’re very good, but you’re the best that there are.” His comment was “quasi-humorous,” but it expressed his feelings well; few students were as well-trained and expert in mathematics as he was, but he took the best he could find and made the most of them. By that time Chandra would only accept a student to work with him whom he had already taught at Yerkes and who had done well in that class, or who had other credentials of mathematical skills and strong work habits. Horak had done a master’s thesis at Kansas on the application of vector methods to orbit theory, which gained him Chandra’s respect.

When I came to Yerkes in 1949 with a master’s degree from the campus, Chandra knew that I had done well in his course there the previous year, and on the combined physics-astronomy “basic examination” of that time. D. Nelson Limber arrived from Ohio State University a few quarters later with glowing recommendations from Geoffrey Keller, his professor there, a theorist whom Chandra knew well. No doubt his other thesis students of that era had similar recommendations or backgrounds which convinced him to take them on. He simply assumed that any mathematically oriented students at Yerkes would want to work with him, and until Strömgren returned as director in 1951 nearly all of them did. A decade later, when Maurice Clement wanted to do his thesis with Chandra, the pool of applicants was larger. Clement had to prove himself first, by computing a Cowling stellar model using an electrical hand-calculating machine just before Yerkes bought its first IBM 1620 electronic computer. He passed with flying colors, as much for the excellent grammar and sentence construction of his written report (always a point of pride for Chandra himself and an absolute requirement for his students’ theses) as for the correctness of his numerical calculation, which his new major professor had expected but wanted to see confirmed. One later would-be thesis student found
an error by a factor of two near the end of one of Chandra’s own papers, and thus proved that he was worthy to work with him.

Esther Conwell was Chandra’s first Ph.D. thesis student in physics, rather than astronomy and astrophysics. She started working with him in 1945, on improved wave functions for the negative ions O\(^-\) and H\(^-\). She had taken all her course work on the campus, and had not even met Chandra before it was time for her to start her thesis. World War II was still in progress, and he was glad to have a thesis student who would not be drafted. She spent less than six months at Yerkes, driving back and forth with him between there and Chicago occasionally, and then completed her thesis and received her Ph.D. after she had moved on to a teaching post at Brooklyn College, New York.

In the early post-war period many of the male students were veterans, supported by the “GI Bill of Rights,” which paid tuition and living expenses. There were not many other sources of support, except a few graduate-student assistantships. Struve was generally willing to allot one to Chandra, for a student to do computing work for him, but reserved the rest for observers with the 40-inch refractor. Thus some of Chandra’s women students, like Merle E. Tuberg and Marjorie Hall Harrison, had to work several nights a week with the big telescope while they were doing their Ph.D. theses. It was no easy task to “reverse” the telescope, pushing it around the pier from one side to the other, particularly on cold winter nights when the oil was stiff and the observers’ heavy insulated flying suits made moving awkward.

After about 1954 most of Chandra’s Ph.D. thesis students were in the physics department. Generally when they started to work with him they would stay on the campus, but commute to Yerkes on Monday, driving together in a car. He was almost certain to be there for the colloquium, and in the evening they would take part in the seminar before driving back to Chicago late in the evening. But when they began working seriously on their theses, he preferred that they move to Williams Bay so he could discuss their work more frequently than he could on his busiest day there, or on his only day on the campus, Thursday. The physics students who moved to Yerkes were even more bored in the little Wisconsin village than the astronomy students, for they did not get much out of the colloquia, nor sit in on the specialized courses on stellar spectroscopy, radiative transfer, galactic structure and the like. One of Chandra’s physics students from that time vividly remembers his arrival in Williams Bay, when he asked one of the long-term Yerkes students what you did there all winter. He got the one-word answer, “fidget!” Another, Fred Bisshopp, was commuting for one or two days a week for several months, because he was having difficulty finding a landlord who would let him keep his dog, a boxer named Robert Maynard Hutchins (“Hutch” for short) for the recently departed chancellor who had been a hero to all the Chicago students. Chandra, growing impatient to see Bisshopp more often and not knowing what the problem was, abruptly asked him one day why he was so slow in moving to Williams Bay. The eager student told him about the reluctant landlords and how attached he was to Hutch. Chandra, taken aback, replied, “You have a dog, is it? Being attached to a wife I can understand, but sell the dog!” However, the story ended happily as
Bisshopp was able to find a one-room apartment with a willing landlord soon after that little exchange, and keep his dog, although he probably had to pay a higher rent than he had planned.

When Chandra moved to the campus in 1964, the physics and astronomy students who were then working on their theses with him moved too. He had not been teaching at Yerkes for several years, and most of them, and the observational students of the time as well, had only taken a general relativity course from him, which he had taught in Chicago. They regarded it as more of a “cultural” course than as one in which they would learn material that they might actually apply in research themselves. In fact, Chandra himself was getting seriously into the subject, and very soon began publishing in it. More than a decade earlier, around 1951, H. Lawrence Helfer had expressed interest in doing his Ph.D. thesis in general relativity, but Chandra said that he was not working in that field then, although he expected to do something in it when he neared retirement. Actually, of course, he did not wait that long, and he went on to do a great deal of research in general relativity, much of it after the “normal” retirement age. Donna Elbert had moved to the campus before him, in 1958, and continued working as Chandra’s computer and secretary for many years there. She coauthored eighteen papers with him in all, and did the numerical computations for many more, as well as typing his papers and correspondence. She was a friend to all his graduate students and postdocs, but in 1979, as his human computing needs decreased, she moved on to become office manager for the astronomy department.

I have tried to compile a list of all of Chandra’s Ph.D. thesis students through the last one he had in astronomy and astrophysics, Bonnie D. Miller, who finished in 1973. Chandra had such a list, but I was unable to obtain a copy of it. Therefore I began with a tentative list of those I knew personally, and asked all of them, and also many other Ph.D.’s from Yerkes, to add whatever names they could to it. Proceeding iteratively in this way, I ended up with the list of forty-six given in Appendix 1. Chandra may have had more thesis students who finished their degrees after 1973, and if so they are important too, but not as relevant to the subject of this paper. Evidently I missed a few of his students up to 1973, for more than one near the end of the list has written me that they were higher (by one or two) on Chandra’s list. Partly it may be a matter of definition; some students had more than one thesis adviser, and I have included Anne Underhill (who had Chandra, Struve, and Jesse L. Greenstein as advisers) and Russell Kulskud (Chandra, M. L. Goldberger and Strömgren). There may have been others whom Chandra included on his list, but whom the rest of us did not perceive as his students. Of course, I would appreciate very much learning of any more of Chandra’s Ph.D. thesis students whose names should be added.

It is striking to see in the table how closely the subjects of his students’ theses tracked Chandra’s own research. Whatever general subject he was working on (as described in other papers in this memorial issue), his students were working on it too. He always had many problems which he could assign to students who were looking for thesis topics. These problems were hard, but doable. He knew the
subject well, and could give good advice on how to proceed at each stage. Chandra never gave a student a problem he could not have done himself. Often they were more applied than he wanted to do, but they were all problems that, in some sense or other, needed doing. I never heard of a student of Chandra’s who did not finish a Ph.D. thesis; he was a realist.

Furthermore, he was always interested in a student’s thesis. He wanted to know what was going on, wanted to discuss the work, wanted to see progress. Although he was a great scientist, with many calls on his time, and the Astrophysical Journal was a constant drain of his energy for nineteen years, he was approachable to his students and would always make time for them somehow or other. He demanded a lot; nearly all of them commented that he made them work harder than they ever thought they would, but practically every one, looking back on it, thought it had been good for them. Chandra was an excellent, and highly productive, thesis adviser.

12. Chandra to his Ph.D. thesis students

I tried to get in touch with all of Chandra’s thesis students to survey their thoughts on Chandra. Seven of them are no longer living, Gordon Wares, Ralph Williamson, Marjorie Harrison, Su-shu Huang, Marshal Wrubel, Frank Edmonds, and Nelson Limber. I sent all the others listed in Appendix 1 a fairly long form letter; thirty-five of them, ninety per cent of those who are alive, responded. Overwhelmingly they thought that Chandra had been a good teacher for them. We may differ a little in just how mathematical he was, or how physical, and whether he would have been a little better if he had been a little more in one direction or the other, but we all believed he was very good indeed. One respondent even thought that any criticism of Chandra’s teaching or research might simply be a sign of a deficiency in the criticizer, but all the rest considered him a human being! Chandra liked students, but he would not tolerate any nonsense from them. Always formal, a bit reserved, well-dressed, he was also eager to discuss science, most approachable, and full of stories, especially of the great men of his youth and of their idiosyncrasies. In his later years Chandra became somewhat more aloof, and a larger fraction of the students who completed their degrees in the late 1960’s and early 1970’s considered him a bit cold or inhuman.

He was famous for inviting his students and postdocs to come over to his house for “some fun” on nice fall weekend days; this turned out to mean helping to rake leaves which had fallen from the huge trees which grew everywhere on the Yerkes grounds. With the wives of his students and colleagues he was unfailingly friendly and polite; to their children he was a kindly uncle-figure on the rare occasions when he saw them. To the men students he could be harsh if they did not measure up, but it was for their own good, he thought, and many of them agreed in retrospect. In his later years he evidently mellowed, for there are fewer reports along this line after he moved to Chicago. Perhaps this was an aspect of his aloofness. None of the
women Ph.D.’s who wrote me, his own students or the students of others, reported the slightest unkindness from Chandra; he was unfailingly friendly and polite with them. One, Anne Underhill, believed that he preferred women graduate students because “they tended to work hard all the time, while the men had sense enough to say, ‘I have done enough; I will stop here’ ”.

In colloquia Chandra was quite capable of interrupting a speaker to criticize his work; he had dedicated his life to scientific truth and therefore felt it his duty to combat error. One former Chandra Ph.D. likened him to President Harry Truman, who stated, “I didn’t give anybody hell, I just told the truth and they thought it was hell!” But this could be quite discomfiting to faculty colleagues whose work he criticized in their presence and the presence of their graduate students. Sometimes “the truth” was not so apparent to them as it was to Chandra.

He was tremendously supportive of all his Ph.D. students, recommending them for assistantships, fellowships, and, later, jobs, and following their progress with interest. Naturally, if he thought they were not working hard enough, or not choosing important enough problems, he did not hesitate to set them straight. In my own case I feel certain that he recommended me strongly for the pre-doctoral fellowship at Yerkes, the post-doctoral fellowship at Princeton, and my first faculty job at Caltech, which got me started in astrophysics. And in those bad old pre-open-recruitment, pre-search-committee days, a recommendation from Chandra meant a lot! Several years later, when I was in charge of the astronomy colloquia at Wisconsin, I managed to persuade him to drive up to Madison from Williams Bay and give one. I arranged for him to get a parking permit, and explained in detail just where he should pick it up and how important it was to display it on his car. When he arrived at our department office, I immediately asked him if he had found the campus police station and gotten the permit without any trouble. “No,” he replied, “I didn’t bother. I just parked out in front!” “But you’ll get a ticket!”, I half-screamed. “If I do I’ll just write Arthur D. Code [our chairman, another of his former students] on it,” he calmly said, “What’s the use of having friends if you don’t use them?” Then I did not feel so bad about getting all those recommendations from him. A year or two later, soon after I became an associate professor, I wrote to ask Chandra some question about one of the methods in his book on radiative transfer. In his reply he joked that he had been surprised to hear from me, because most of his students stopped writing after they got tenure! As “Conversations with Chandra”, the epilogue to Wali’s book, shows, he was keenly aware of the academic hierarchy and rat race, and had a good, if slightly cutting, sense of humor.

Henry Horak, who taught at the University of Kansas for many years, had to persuade Chandra to travel much farther to give a colloquium there. He was understandably reluctant to make a special trip to do it, but finally, after many requests, told Horak he would come if he could gain an interview for Lalitha and himself with former President Harry Truman, then retired and living in Independence, Missouri. They had both become American citizens and liberal Democrats, and were active supporters of the party. No doubt Chandra thought that this condition would put
a stop to Horak’s importuning, but in fact a friend of his on the Kansas faculty was a ghost writer who had worked on Truman’s memoirs. Thus he easily met the condition, so Chandra traveled west, gave the colloquium, and with Lalitha and Horak met Truman in the replica of the Oval Office at his presidential library. According to his former student, Chandra, awed, was for once almost speechless. So Horak, whose inclinations ran more toward the conservative side, did more of the talking than he had intended. Asked about his recall and relief of General MacArthur, Truman bluntly replied, “He disobeyed orders, and I fired him!” But then the former President went on to express a genuine appreciation of scientists, and they could all agree with that. Later, of course, Chandra accepted the National Medal of Science from President Lyndon B. Johnson, in 1967.

Chandra was critical of a few of his former students, particularly ones who continued to do research in fields in which he had once worked, but had abandoned. I witnessed two such cases, in which my sympathies were all with his former students. They were not continuing along his lines, but were trying to go beyond him using their own, different methods. To my mind he was unnecessarily harsh with both of them, and did not show an open mind about examining their work on its merits. But he was just applying his own very high standards to them, he thought.

As the other articles in this memorial issue describe, Chandra wrote numerous books, and in particular, a series of major monographs summarizing his and his students’ work on each of his successive fields of research, as he left it for the next. One of his students, Surindar K. Trehan, compiled notes from a course in plasma physics which Chandra gave on campus in 1957–58 into a book. He approved highly of it, and the book was published by the University of Chicago Press (Trehan 1960). It eventually went through two reprints, in 1962 and 1975. Chandra was very pleased with it, and especially liked to stress its analogy to the famous notes on Enrico Fermi’s course on nuclear physics, compiled by three of his students and also published by the University of Chicago Press (Orear, Rosenfeld, & Schluter 1950). Trehan’s book is a faithful rendition of Chandra’s lecturing style, highly mathematical and somewhat uneven in its treatment of different topics, but containing some real gems of derivations. Quite naturally it is less polished than Chandra’s magisterial summaries of a subject, but Trehan’s book was immediately available to the many students and working scientists all over the world who were then hastening to learn as much about plasma physics as they could.

Perhaps the all-time story of Chandra’s support of his students is that of Carl Rosenkilde. He was a physics graduate student on campus who admired Chandra’s approach to theoretical subjects from taking his course on classical electrodynamics, in the years when he still had his office at Yerkes but was teaching on campus. Rosenkilde wanted to do a thesis with Chandra, and made an appointment to drive to Williams Bay to visit him and discuss a manuscript he had already written on the transmission of a charged particle through a kink in a magnetic field. But driving west from Kenosha on Wisconsin highway 50, then a two-lane road, Rosenkilde’s car was hit by another one and totaled. He suffered head injuries and an ambulance
took him to a medical clinic near the crash scene to be examined and treated. Dazed and half-conscious, Rosenkilde was worried about keeping Chandra waiting, and conveyed the information to someone that he was on his way to visit him. While he was still being examined, Rosenkilde heard Chandra’s distinctive voice outside in the waiting room, asking if he were still alive! He had gotten the news by phone, and came as soon as he could to help out. Relieved to find Rosenkilde not only alive but conscious and rapidly improving, Chandra insisted on driving him back to the observatory, telling him on the way a story from India about the Grim Reaper’s early arrival on some other occasion. When he got to his office with Rosenkilde, Chandra would not allow him to discuss his manuscript, but instead drove him to the station and put him on the next train back to Chicago, accompanied by another of his physics graduate students, Lawrence Lee. However, Chandra kept the manuscript, gave it to a referee, and receiving a favorable report on it, published it in the *Astrophysical Journal* (Rosenkilde 1965). It was his first publication, and it convinced Chandra to accept him as a thesis student. Luckily for Rosenkilde, Chandra moved to Chicago soon after that, and the young graduate student never had to drive back to Yerkes again!

Chandra called upon many of his students to check his papers or books before publication. In my own case, for my last two years I was his senior graduate student at Yerkes after Horak finished his degree and Guido Münch, his thesis student earlier, and then his colleague on the faculty, left for a position at Caltech. When Chandra had finished writing a paper, Donna Elbert would type it for him, simultaneously making several carbon copies. He would fill in the complete equations on the original in ink, as well as the mathematical symbols in the text, all in his bold, characteristic writing. Then he gave me the original and the carbons; I filled in the equations and symbols on them at the same time I checked through the mathematical manipulations, step by step. Presumably there would only be misprints in signs, or omissions of a symbol by that point, but he cautioned me to check each step carefully. For two years I did, but I almost never found an error; I think it proved that he did not make them, but I could not be sure. Many of his other thesis students had similar assignments, and those who were working with him when he wrote a book checked the equations in his manuscript, and later the proofs. It was good practice for the future.

All of Chandra’s Ph.D. thesis students enjoyed working with him. They learned to do research under a master of it, and those who stayed in his general area of highly mathematical theoretical astrophysics were masters of it too by the time they had completed their theses. He taught, seasoned, encouraged, and broadened all of us while we were his students, and he supported us greatly after we had left the nest.
13. Post-docs and parody

Besides the many Ph.D. thesis students he trained, Chandra had quite a few postdoctoral research associates who collaborated with him over the years. I have not made a systematic attempt to obtain their stories, but can briefly mention a few of them. One was Mario Schoenberg, who came to America from the University of Sao Paulo, Brazil with a Guggenheim Fellowship. Under it he worked at George Washington University with George Gamow on neutrino cooling of dense, hot stellar objects by what they named the Urca process, which they proposed as the mechanism for initiating supernovae, an idea that has lasted very well. Then Schoenberg moved on to Yerkes, and with Chandra worked on the evolution of stellar models with burnt-out, gaseous, isothermal cores (Schoenberg & Chandrasekhar 1942). Their paper followed one published the previous year by Louis Henrich and Chandra (not part of Henrich’s thesis). It analyzed mathematically the idea of Gamow and Edward Teller that red giants are stars in which D-, Li-, Be- and B-burning in shells just outside an isothermal core in which these light nuclei had already been exhausted were the main energy sources (Henrich & Chandrasekhar 1941). They discovered the upper limit to the fraction of the mass which could exist in a burnt-out nuclear core, now called the “Schoenberg-Chandrasekhar limit.” That later paper in fact extended this concept to include a discontinuity in molecular weight, and carefully traced the evolution of shell-source stars burning H rather than light elements. It was one of the key first steps toward the recognition of the true nature of red-giant stars as late stages of normal stellar evolution.

With these two papers and Gordon Wares’s thesis on partially degenerate models, completed in 1940, Chandra was very much in the thick of the beginning of the study of stellar evolution (Wares 1944). In 1938 and 1939 he had taken part in the very important conferences on nuclear energy production, organized in Washington by Gamow and Merle A. Tuve. Then in 1941, with Henrich as his assistant, Chandra made a pioneering study of the equilibrium distribution of nuclear abundances at very high temperatures and densities, related to what we now call the $r$-process in supernovae. In it they supposed that “the” abundances of the elements were fixed under prestellar conditions in an expanding universe, and by fitting the abundance and isotope ratios then considered universal, derived $T \approx 8 \times 10^9$ K and $\rho \approx 10^7$ gm/cc as the conditions under which the elements O through Si had formed (Chandrasekhar & Henrich 1942). They recognized that the iron-peak elements could not be formed under these same conditions, but stated that perhaps they had frozen out under earlier, even more extreme conditions. There are many similarities between their “scenario” (in words of today) and our current ideas of the formation of the $\alpha$-element nuclei in supernovae, including their estimate of the mean temperature and density at which these elements were made.

But, as the study of stellar interiors and evolution was becoming more physical and detailed, Chandra was shifting his research to more mathematical stochastic and statistical problems. Ironically enough, he did some of this latter work jointly with John von Neumann, the great exponent and facilitator of applications of nuclear
fission and fusion chain reactions in the “real world.” Chandra himself next went on to radiative transfer, and left further developments in understanding stellar evolution to Martin Schwarzschild, Fred Hoyle, Louis G. Henyey and their co-workers.

In the late 1950’s Chandra had an especially large and active group of postdocs working with him at Yerkes on plasma physics and hydromagnetic stability. One was Lo Woltjer, who had completed his brilliant thesis on the Crab nebula at Leiden Observatory with Jan H. Oort, earning his Ph.D. in 1957 and then coming to America to work with Chandra. Others were John Hazlehurst, Paul H. Roberts (who later joined the Yerkes faculty for a time) and John Sykes from England, René Simon from Belgium, and William H. Reid, an American who had done his Ph.D. at Trinity College, Cambridge with Ian Proudman. Kevin Prendergast and Nelson Limber, then young Yerkes faculty members, were working with them on some of these problems. In addition Dave Fultz, a geophysicist, and Russell Donnelly, a physicist, both faculty members on the campus, were involved with him through their experimental work on hydrodynamic and hydromagnetic stability, along with Yoshio Nakagawa, a research associate.

From this period dates the famous parody “On the Imperturbability of Elevator Operators. LVII”, by “S. Candlestickmaker,” printed as a reprint from the Astrophysical Journal. Full of outrageous puns, double entendres, overstatements, and inside jokes, it is written in a style reminiscent of one of Chandra’s papers, but with all his idiosyncrasies greatly exaggerated. In fact the Candlestickmaker parody is closely modeled on a paper by Chandra entitled “The Instability of a Layer of Fluid Heated Below and Subject to the Simultaneous Action of a Magnetic Field and Rotation. II.” It had appeared in the Proceedings of the Royal Society of London (of which Chandra was a Fellow) the previous year, and the twenty references in the parody to previous papers by the purported author, from “S. Candlestickmaker (1954a)” up to “(1954t)” are attributed to different actually existing but somewhat implausibly named journals such as the Transactions of the North-east Coast Institute of Engineers and Shipbuilders, 237, 476, the Journal of Dairy Science, 237, 476, and Scientific Progress of the Twentieth Century, 237, 476, all with the same volume and page numbers as the real paper (Chandrasekhar 1956). There is also one reference to a paper by Candlestickmaker and Miss Canna E. Helpit (Donna D. Elbert), whom the author thanks for her laborious numerical work in obtaining the approximate solution for the single case in which the problem can be solved explicitly, “admittedly a case which has never occurred in living memory”, but “from past experience with problems of this kind one may feel that any solution is better than none”. The equation for this case is

$$\ln \Omega_{2t} = 1,$$

and the approximate solution is given as

$$\Omega_{2t} = 2.7.$$

The date the paper was received is given as October 19, 1910, Chandra’s date
of birth, and the institution of the author is stated to be the “Institute for Studied Advances, Old Cardigan, Wales.”

The paper has to be read carefully to be fully appreciated; it has been republished in the Quarterly Journal of the Royal Astronomical Society (Sykes 1972) and in an anthology edited by Weber (1973). However, these versions as printed do not do justice to the original, which was widely circulated as a “reprint” from the Astrophysical Journal, printed exactly in its style, typeface and format, as if it had been published in volume 237, number 1211, November 1957. Of course the first page is numbered 476. The month and year are when the “reprint” was actually printed, but the volume number then seemed so large that it would appear impossibly far in the future (126 was then current). In fact, that volume number appeared in 1980, and when Chandra died in August 1995 the Astrophysical Journal was publishing volume 450.

The author of the parody was John Sykes, listed as the member who had “communicated” the paper, although no doubt some of the other postdocs contributed additional touches to it. Sykes, a brilliant mathematician, linguist, translator, and puzzle solver, was working with Chandra as a postdoc for a year to get into magnetohydrodynamics, before returning to Harwell to join the United Kingdom fusion project. Soon afterward, however, he became a full-time translator and lexicographer. Sadly, he died of a heart attack a few years ago. In 1957 Sykes submitted the parody by mail to the Astrophysical Journal office as an ordinary manuscript intended for publication. The secretary who opened it recognized or at least suspected that it was a joke and took it to Chandra, fearing that he might explode. But he was delighted with it, and showed it to everyone who came into his office. He authorized printing it in the Astrophysical Journal reprint format, and the postdocs and students, headed by William Reid, George Backus and Kumar Trehan, took up a collection to pay for it. Everyone who saw the reprint was amused by it; the more closely they had studied his papers, the better they understood some of the allusions in it. Chandra thought it was a wonderful joke, but he also recommended it seriously to more than one of his students as a good example of the correct style in which to write a scientific paper. Evidently Sykes had captured his style nearly perfectly.

14. Conclusion

Chandra was a great scientist, who was also an excellent teacher and thesis adviser of graduate students. He was an extremely productive research worker, who published a prodigious number of scientific papers and research monographs. In addition, he guided a very large number of graduate students to their Ph.D. degrees and started them on their own research careers. He supported most of them with praise and recommendations in their later lives. To many scientists outside Yerkes Observatory and the University of Chicago, Chandra seemed a remote, forbidding figure. But to his own graduate students he was highly approachable, even outgoing. All
the graduate students who worked with him at Yerkes Observatory, and on the University of Chicago campus up through 1973 felt that they had learned much from him, and had been fortunate to have been his students. A few thought of him as a god; most recognized him as an exceptional human being.

I am very grateful to the many former graduate students, most of them ones who received their Ph.Ds. at Yerkes and on the campus, for their letters, e-mail messages and phone calls in response to my request for information on their memories of Chandra and their interactions with him. I am especially grateful to Peter O. Vandervoort, Bonnie D. Miller, Henry G. Horak, Donat G. Wentzel, Philip J. Greenberg and John L. Friedman, who all played major roles in filling in the list of names of Chandra’s Yerkes Ph.D. students given in Appendix 1. I am indebted to Maxine Hunsinger Sullivan, University of Chicago Registrar, for providing the names of all the students who were registered for Chandra’s astrophysics course on the campus in 1948, even though, as she wrote me, she would have preferred to preserve the legend. I also wish to thank Chen Ning Yang, Tsung-Dao Lee, Richard L. Garwin, and Arthur Uhlir for their individual recollections of this class we all attended. I am most grateful to Donna D. Elbert for her memories of some of Chandra’s interactions with his students, to William H. Reid for his account of some of the particulars of the writing and “reprinting” of the S. Candlestickmaker parody, and to Roger Tayler for his recollections of what the late John Sykes had told him about his role in it.

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Phys., 12, 1159.
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Chandra and his students at Yerkes Observatory


Chandra’s Ph.D. Students at Yerkes Observatory and in Chicago: Thesis Topics and References.

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<th>Name</th>
<th>Ph.D. degree</th>
<th>Thesis topic / Reference(s)</th>
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<tr>
<td>Gordon W. Wares</td>
<td>1940</td>
<td>Partially degenerate stellar models Wares (1944), SC &amp; Wares (1949)</td>
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<td>Louis R. Henrich</td>
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<td>Margaret Kiess Krogdahl</td>
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<td>Guido Münch</td>
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<td>Anne B. Underhill</td>
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<td>Henry G. Horak</td>
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<td>Donald E. Osterbrock</td>
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<td>D. Nelson Limber</td>
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<td>Analysis of galaxy counts in terms of absorption by a fluctuating density field Limber (1953a, b; 1954)</td>
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<td>Eberhart Jensen</td>
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<td>Magneto-hydrodynamic oscillations of a conducting, fluid sphere in a magnetic field Jensen (1955)</td>
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<td>George E. Backus</td>
<td>1956*</td>
<td>Non-existence of axisymmetric fluid dynamos Backus (1957)</td>
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<td>Surindar K. Trehan</td>
<td>1958*</td>
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<td>Peter O. Vandervoort</td>
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<td>Norman R. Lebovitz</td>
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<td>James P. Wright</td>
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<td>Jerome Kristian</td>
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<td>Jeremiah P. Ostriker</td>
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<td>Maurice J. Clement</td>
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<td>Yavuz Nutku</td>
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<td>Amagh Nduka</td>
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<td>John L. Friedman</td>
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* Indicates Ph.D. in physics.
** Indicates Ph.D. in chemistry.
No symbol indicates Ph.D. in astronomy and astrophysics.
In the row behind her, Irene Hansen (later Osterbrock) and G. H. Whipple are seated on the third row, with Margaret Hess-Koga (sixth). Chandrasekhar, computer, Francis Herfinn (seventh), and L. E. Thomas (eighth) are on the rear row. Among the students in the picture are, clockwise from left to right, Pol Swings (6th), William M. Morgan (5th), Otho Struve (4th), Gerald Herter (3rd), Gerard P. Kuiper (2nd), and G. H. Whipple (1st). The students in this picture, the first of the "Four Musketeers," received their doctorates within the next two years. The last student (fifth from left) is unidentified. The students in this picture are all members of the U.S. National Academy of Sciences, and the three who followed them are all foreign associates of it. Perhaps the greatest collection of astronomers ever assembled in one place up to that time.
Chandra with a group of magnetohydrodynamists theorists at Verdes Observatory.

Chandra with a group of general relativity theorists at the Laboratory for Astrophysics and Space Research, University of Chicago, April 1970. Left to right: Robert Geroch, Sotiris Bonanos, Harry P. Ross, G. F. R. Ellis, Bonnie D. Miller, Richard Carter, Richan Hassen, F. Paul Esposito, Roger Steiner, Sotiris Perelis, Chandrak (Photograph courtesy of Sotiris Perelis).