OBSERVATIONAL ASTRONOMY AT THE UNIVERSITY OF PENNSYLVANIA 1751 - 2007

corrected, revised, and enlarged
from

OBSERVATIONAL ASTRONOMY AT THE UNIVERSITY OF PENNSYLVANIA 1751 - 1996

published privately in 5 print copies in 2005

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2008

2010 - Revision B includes 2009 and 2010 Addenda and Errata

For
Joanne Koch

and in memory of
Harry and Veronica Koch

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PREFACE

INTRODUCTION

In May 2003 Il-Seong Nha asked that I prepare a poster paper on the history of The Flower Observatory (FO) for the IAU General Assembly in Sydney. The two-month deadline was impractical but the idea seemed appealing and so led to a part of this document. Afterwards, Ben Shen suggested that I finish the job with a description of the two following observatories. I turned these ideas somewhat around and composed a text that starts with the 18th century beginnings of the University and ends in 2007 with the almost certain termination of local ground-based observing. The personalities who appear in the document are basically those who interrogated the sky telescopically or with the naked eye or who actively used measurements collected by others. Pure theoretical analysts or model builders hardly appear at all. They were not numerous.

The justification for the dates in the title of this document is as follows. In order to be able to observe, you need to know where to direct your instrument at a specific time. We do this now by a distributed time signal and by on-line information. But into the 18th century, it was not possible to know where to point a telescope unless an object were a naked-eye one or unless you had access to a printed catalogue for a telescopic target. The beginning date is associated with an individual affiliated with the progenitor of the University who prepared and had published just such almanacs. The ending date could have been 1996 (based on my personal experience) or 2004 (from telescope log entries) or 2007. I chose the latest of these because it was more generous and less parochial and because of a particular physical event.

RESOURCES AND METHODOLOGY

Money is a significant matter in what follows. When specific sums are mentioned, they are cited first in the historically current amounts and then bracketed as nominal 2002 equivalents according to the conversion factors of Sahr (2003), who recommends that only three significant figures be kept. No matter how correct be the nominal conversions, there is no way by which uniquely local factors regarding cost-of-living can be accounted for.

A summary page dated 2001 from the University Archives purports to list documentary materials relating to the FO: (1) a fairly complete set of reprints from 1929 to the termination of the facility; (2) a file relating to observations of the intrinsic variable EZ Aql; (3) observing logs from the 1930s and 1940s; (4) a few pieces of personal memorabilia; and (5) a photo of a solar eclipse in 1932. The eclipse photo comes from a Franklin Institute expedition to Conway, NH in which observatory staff did not actually participate and no FO equipment was used. This event is described by Stokley (1932) and no scientific results were obtained. There is also a very brief historical summary prepared by an archivist, Kaiyi Chen.

Mostly through the constant attention of William Blitzstein, a much larger body of paper material and hardware was preserved in his personal files during his service as a member of the Department of Astronomy and Astrophysics. When this passed temporarily into my hands from the attentive custody of Richard J. Mitchell, the materials were not consistently organized but it was possible to sort them easily. They comprise: (1) a more complete set of reprints than the Archives possesses; (2) individual manila-folder files on FO Directors, faculty members, some graduate students, staff appointees, and astronomers and other individuals transiently associated with all three observing stations; (3) two informal renderings of parts of all the observatories' histories which supplement each other well; (4) taped conversations (both on cassette and in unedited transcription) between Blitzstein and two other people; (5) an almost complete set of the *Publications of the University of Pennsylvania Astronomical Series*; (6) a limited selection of the annual Observatory Reports; and (7) a small set of posed and candid print photos and lantern slides of people, buildings and hardware. All these holdings were at least perused by me if they were not read and studied in detail. Since the material is not professionally indexed, there is no unique and proper way to cite the individual files. I decided that I would write a text that preserves faithfully the sense of information in the materials but would not refer to individual documents as if they had really been archived. I make no attempt at a complete list of faculty, staff, and student literature citations, which would be at least three times longer than my selections. All this material (which is really in less than 3 shelf-feet of manila folders) used by me has been deposited with the University Archives or with the Department of Physics and Astronomy.

In the Archives there is essentially no information regarding the observatories subsequent to the FO. What is recorded here is from my and Blitzstein's collections, what I have been able to recover from other people still living and from technical hardware/software files that I have inherited.

A valuable external source exists in the collection of obituaries published by the American Astronomical Society (AAS) and in *Physics Today* and I have used these freely without specific attribution. I have also used without individual citations assorted editions of *American Men of Science*, *American Men and Women of Science*, Philadelphia City Directories, *Who's Who in* this and that, Ritter's (1860) *Philadelphia and Her Merchants*, Jordan's (1911) *Colonial Families of Philadelphia*, *The Lives of Eminent Philadelphians* by Simpson (1859), *The Dictionary of American Biography* and *The Dictionary of National Biography*. The 2nd edition of DeFilippo's (1992) *The History and Development of Upper Darby Township* was also helpful. If all these sources failed to yield useful biographical information, I had recourse to the web site maintained by The Church of Jesus Christ of Latter-day Saints and this was sometimes useful. Many individual photos appear in the panoramic group shots of attendees at meetings of the AAS and printed in its *Publications*. These are all small-scale and were used when no better ones could be found. Some good images are in the John Irwin Slide Collection archived at The Niels Bohr Library of the American Institute of Physics. Technically, a certain number of the illustrations are deficient either because the originals were in very poor condition or because I couldn't figure out how to improve their brightness or contrast. I'd like to think that the text is substantially better than the illustrations.

Four organizations appear sporadically in later chapters.
The first of these, due to Benjamin Franklin's initiative in 1743, is the American Philosophical Society (APS). Its writ has been summarized as pursuing equally "all philosophical Experiments that let Light into the Nature of Things, tend to increase the Power of Man over Matter, and multiply the Conveniencies or Pleasures of Life". Early members included all kinds of professionals and artisans as well as workers in the fine arts with some interest in science, technology or agriculture – it is difficult to fit Thomas Jefferson uniquely into any of these categories. With the usual difficulties a publication vehicle was started as soon as possible. It is not surprising that there would be accepted for publication a large number of singular anecdotal accounts of curious natural phenomena from the interior of North America. Something of the same kind is associated with the early issues of the *Philosophical Transactions of the Royal Society of London* which published accounts of oddities from all over the world. From the point of view of astronomical science, perhaps the finest issue of the 20th century *Transactions* of the APS is that containing the superb spectral atlas of β Lyr prepared by Sahade *et al*. (1959). Membership is still by election and the premises and events of the Society are sustained in an agreeable fashion. Its breadth of interest is what sets the APS apart from other learned societies. It offers grant support not limited to scientific and technological programs and sustains a creditable publication record.
The year 1824 saw the foundation of the second of the organizations: The Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts (FI). The formation of the organization is a testimonial to the expected continuing prosperity of the city's industrial capability. The original building now houses the Atwater Kent Museum of the City of Philadelphia and its present home is, from the outside, an impressive pile in a very advantageous site. Much of the public area of the building's interior and a certain amount of the staff's work space, however, show the dated structure for what it is. At the beginning the FI had two purposes: to educate and foster technological literacy and to support and promote local manufactures. In the latter matter, it has had some conspicuous achievements as, for instance, the 1860s promotion of nationally uniform screw and nut threads and bolt heads. For decades the FI pulled in contracts for research and engineering such as the design and engineering of the 280-mm "Atomic Cannon" but this money dried up progressively as the institution became unable to compete with larger corporations and R&D firms. Even through the present day, its annual awards for technical accomplishment command great respect. Its *Journal*'s contents are representative of their times and had wide circulation even after

World War II so part of the educational part of the mission was a spinoff from the research effort. The FI also sustained weekend and summer workshops that had a specific focus on the Philadelphia population. Especially when the research program was flourishing, the FI sustained classes in optical figuring and polishing and Fig. 1 is an old illustration of that enterprise.

Fig. 1. A supervised optical figuring class in the FI building in the 1930s. At the extreme right the teenager with his tool on the blank is Blitzstein. From Wright (1938).

After 1930 a science museum was begun and a planetarium installed. These became immensely popular with probably hundreds of thousands of children walking through an immense model of a pumping human heart. In the 1980s the mission of the institution became public education only but even that has just changed. Marketing flacks have decided that cash flow will be more vigorous if non-scientific mega-exhibits become a common practice and have convinced the governing body that a name change to The Franklin will move that business along.

The third organization is The Rittenhouse Astronomical Society (RAS) which, founded in 1888 as the Camden (NJ) Astronomical Society, assumed its current name in 1927. Since 1931 it has conducted its business at the FI. Its broad purpose has been to promote astronomy in the local area and to this end it has fostered optical fabrication and telescope building practice and monthly public lectures and observatory viewing with the FI telescopes. Originally an assemblage of amateurs, it grew to have a considerable presence of the local academic astronomers as well but is now again largely an amateur group.

The last group is the Bartol Research Foundation which grew out of a 1918 bequest to the FI by Henry W. Bartol leaving his American assets for "…the founding and maintenance of an institute…preference, however, being given to workers or those making researches into electrical science." The first Director, W. F. G. Swann, interpreted the mandate liberally and in the late 1920s moved the hardly-formed Foundation to the campus of Swarthmore College where it sustained workers in different branches of physical science with a conspicuous emphasis on all manifestations of cosmic rays. Fig. 2 shows part of a platform for high-altitude cosmic radiation work. As a result of the IGY and with support from its second Director, Antarctic science was begun and now

Fig. 2. A disarmed B-29 modified for cosmic ray observation at about 40,000 feet.

under its third Director, the BRF has continuously broadened its scientific footprint. Solar science was started long ago. The Foundation is now integrated into the University of Delaware.

A certain amount of information exists only in my memory for, from personal acquaintance, I know much more than a little about many of the people to be met in the following chapters and had met several others casually. My routine or sporadic conversations with them – sometimes about themselves and sometimes regarding other people as well as about events and things – were never written down even in summary form but I use recollections of their remarks at several points. There are also first-person recollections that no one else could know. In order to identify all this hearsay as being unverifiable, I embed the appropriate text in the notation #…#. This practice may be understood in the inverse way: all text not contained within the #-symbols can be found in the files which I used or in published papers or in both kinds of sources. The reader will find no footnotes in this document.

Two major players in this history are the Doolittles, father and son. When these men were spoken of in my time, they were always referred to by their Christian names. I have followed that practice here. Finally, it is almost inevitable that the history of an institution is that of its individuals and their interactions amid the impersonal busyness of economic, scientific and academic fluctuations and wars. After characterizing these people and their work in thumbnail accounts, I pull together my assessments of the scientific observing programs.

All mistakes (even typos) that I have made can be corrected as I learn of them at **rhkoch at earthlink.net**.

ACKNOWLEDGEMENTS

Many people have contributed background and help for my descriptions and I am most grateful for their assistance:

Dave Alexander provided much information on his father and his father's career and Stan sent me a copy of his and Philip Taylor's papers;
Linda M. Beals of Wittenberg University sent me a photo of William E. Anderson when he was young;
Bill Berner photographed several images and found information on Skellett;
Sally Bolmer of the Nisky Hill Cemetery directed me to the Doolittle grave plot;
Todd Boyette of the Morehead Planetarium furnished a photo of Marshall;
David Branch provided information about Whitney and the portrait of him;
Patricia Budlong checked numerous details in Bethlehem, PA and Steve Budlong searched out Sahr's currency conversion factors;
Janet Burns led me to Frank Warner's history of the Math Department on its web page;
Vassilis Charmandaris provided me with a short bio of S. N. Svolopoulos;
Matt Considine found the death year of Skellett and permitted the use of the final illustration;
Constance Cooper of the Historical Society of Delaware tried to find a trace of Grew in the records of that colony and state;
Susie Cooper in the Register of Wills of Delaware County recovered numerous 19th century documents;
the late Jim Crowley helped find materials at Yale's Beinecke Library;
Alan Daroff, Vice-President of the RAS, permitted me to use the Society's scrapbooks;
Irene Ferguson searched the Special Collections at the University of Edinburgh at my request;
Michelle Gachette of the Harvard University Archives attempted to find evidence of Grew at Harvard;
Tony Galatola provided pictures of the FCO and some of its hardware after 2000;
Frank Giovane of the NRL offered a very detailed critique of his dissertation;
Yukimori Go of the University Physical Plant office discovered the dimensions of the original David Rittenhouse Laboratory;
Roy Goodman and other staff members at The American Philosophical Society were most helpful with their collection;
Chad Graham and Norman Brown of the LRSM researched Knox's contribution to the Lab;
James Green of The Library Company of Philadelphia saved me from an ignorant blunder;
Ann and Ed Guinan found a photo of Dorren;

Elizabeth Gutchigian wrote a short bio of her father which I paraphrased somewhat;
Walter Haas corrected some mistakes and misinterpretations that had appeared about him in an earlier text version and had some nice recollections of Olivier;
Carol Harris of The Conwellana Templana Collection, Temple University found the printed record of the dedication of the Samuel G. Barton Hall of Science and numerous photos of the building;
Rachel Hart checked more than a century of early St. Andrews University students for me;
Vivian Hasiuk found a certain amount of information in departmental files;
John Hearnshaw updated my information on the status of the astrograph cameras;
Nicholas G. Heavens and, most particularly, Amey A. Hutchins, Nancy B. Miller and Dianna Hemsath of the University Archives staff recovered much reference material, images and many dates for older students and faculty and staff members;
Joe Heid and Karen Heid tracked down Ralph Baldwin's whereabouts;
Wulff Heintz remembered several old astrometric references that I had forgotten;
Gene Hemp filled me in on Skellett's career at the University of Florida;
Bruce Holenstein provided copies of two wills, prepared a photo of Wyller, passed on some information about Hammer and provided significant information about the termination of the FCO;
Meg Hollinger of The Episcopal Academy found photos of and historical information about Tabor;
Chun-Hwey Kim translated Yun's *cv* for me and gave me a photometric data set;
Bill Koch found a biography of Nehemiah Grew;
Katherine Koch found the year of Hammer's death;
Tom Koch and Patricia Budlong oversaw a printed rendering of an earlier version of this text;
Heather Lindsay sent a photo of Menon from the AIP photo depository;
Flip Lippincott filled in some details of old personalities;
Mark F. Lloyd graciously permitted me to take a few pictures of the University archival collection;
Tom Lubensky offered information on the current status of the Tobias Wagner Lecture Fund;
Nancy Lyon looked for Grew in the Yale Archives;
George McCook and Beth Jewell permitted me the use of the library of the Department of Astronomy and Astrophysics, Villanova University, where I also found useful materials in the main Falvey Library;
Ron Mecklin searched the records of Christ Church, Media;
Sandra Meier contributed the photo of Poss;
Rich Mitchell preserved the Blitzstein collection after Blitzstein's death and offered very helpful advice on details of it; he refreshed my memory about many other details and also created some of the illustrations;
Dermott Mullan found Wyller's birth year for me and asked Charles Swann to tell the story about Locher;
Bob O'Connell offered pictures of Haas from his personal collection;
M. K. O'Donnell located for me a reference in the library of the Kent County, MD Historical Society;
Pat O'Donnell, Archivist of the Friends Historical Library, provided the details about Miller;
Karen Parshall of the University of Virginia summarized the career of Henry B. Evans and sent me her (and three co-authors') essay about Courtenay and a picture of him;
Pat Perry and Pat Ryan offered very useful interpretations of how a stove heated a colonial brick row home;
Adam Perkins discovered the likely family line of Theophilus Grew;
William Phillips of Stetson University provided useful information about Reed Knox, Jr. and his photo;
Bill Protheroe sent along the photo of himself that I have used;
Scott Prouty, Archivist of The John Irwin Slide Collection at the AIP, found several photos;
Anne Reahm sent me news and a picture of her late husband;
Rex Rivolo offered some pictures of himself;
the late Marcello Rodonò passed to me his materials on Fracastoro;
Mike Ryan at the Van Pelt Library scanned the Rittenhouse orrery images;
Robert Sahr gave me leads to uncovering the changing value of British money;
Mike Saladyga from AAVSO searched out information on the Baltimore amateur astronomers;
Todd Seeleman offered some photos of his father;
Rev. David and Marjorie Sharp searched for British sources of Theophilus Grew;
Ben Shen gave me advice on a preliminary draft and educated me about solar cosmic rays;
Tom Smith, Archivist at the Sellers Library, kindly shared maps and volumes and directed me to a useful local newspaper morgue;
Carol Singlar at the office of the Philadelphia Prothonatory was very helpful in retrieving and understanding old court records and procedures;

Maryellen Smith checked a copyright on a University Press publication;
Peggy Tilghman, a granddaughter of Cook, had useful family information;
Crystal Tinch searched AAS files for information regarding old members;
Peggy Underwood interpreted for me some legal practices and conventions;
Alex Van Allen explained the workings of the conservation groups in Chester County;
Joe Vasturia of the Upper Darby Township Engineering Department looked up the origins of the Observatory Hills playground;
Kevin Walsh and Robert D. Solvibile, Jr. guided me through the labyrinth of the Philadelphia Register of Wills and Orphans Court;
Virginia Ward of the FI Library found numerous histories of the Institute;
Susannah Waters of the University of Glasgow Archives checked out Grew for me;
Bill Whitaker of the University Architectural Archives shared with me the surviving Seeler drawings;
John Zeller and Bob Baer of the University Alumni Affairs Office offered a number of leads on finding older graduates; and
Sarah Zimmerman told me of A. Edmund Hayes;
Numerous staff members of The West Laurel Hill Cemetery, The Pennsylvania Historical Society, The Free Library of Philadelphia, The American Philosophical Society, and the Library of the University of Pennsylvania graciously offered advice and help.
Possibly I have forgotten to acknowledge the help of a few people and for this oversight I apologize.

I have sought permission for several illustrations that have been published earlier in a variety of sources.

APOLOGIA

Before composing this tale, I had not read any of the admired historical publications by Donald Osterbrock and Dorritt Hoffleit and I avoided doing that while finishing the work. What appears here has to stand or fall on my capability alone and on the character of the local subject matter. There are certainly omissions and personal choices of emphasis in the document that might be altered at some later time or by some other editor. I will have to own up to whatever mistakes of fact and interpretation are found but the breezy language is a deliberate choice. I can imagine that the text might be criticized on the grounds that dropping names of nationally and scientifically small fry is worth no one's attention. I don't agree with this attitude because no institution functions with only chiefs and no indians and anyway I wanted to write this text to be consistent with the precept of John Donne's most famous lines. If also there appears reference to the remote background or relatives of one of these individuals, such peripheral matter means that I don't think that there are uninteresting people.

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A SELECTIVE ASTRONOMICAL PRIMER

INTRODUCTION

The chapters that follow this one are very nearly in chronological order as one observatory succeeded its predecessor and there are some scientific emphases or specialties that persist through the existence of more than one observing station. Those texts are written with very little explanatory background of their scientific content. What immediately follows this introduction is intended to be orthogonal to those descriptions and to give a generalized explanation of astronomical specialties and their development at the level of an educated layman. Specifically, it talks only about subject matter in the following chapters and it deliberately lacks the customary apparatus of scientific literature citations. These pages are assembled into a sequence of 16 ruminating essays with a broad heading for each, followed by the names of the individuals who participated locally in that specialty and the time span that covered the work of all of them. It must be understood that the contributions from a given set of workers are typically very unequal. Some of the essays are illustrated.

No physical scientist (nor, in fact, anyone who has paid attention in a Descriptive Astronomy course and remembered its content) will profit by reading the present chapter other than to discover errors in it, and he should go directly to the next one. Anyone else might jump ahead too and refer back to these pages if the recognition of ignorance becomes too much to bear.

ELECTROMAGNETIC RADIATION
1786 [Rittenhouse, Myers, Devlin, Alcock, Bernstein] 2006

Everything in the story of the observatories associated with the University has to do with radiation and most of that radiation has been light. Very little material from space forms part of the story.

Misunderstandings about radiation were commonplace ever since the beginning of science – when it was called Natural Philosophy. Some people imagined radiation to be a train of waves and commonly these waves were supposed to be propagating through a medium that could not be avoided but also could not be measured in any way. Another view held that light was a stream of unimaginably small particles; they would now be called photons. Some parts of each of these interpretations remain valid today, if only as convenient ways to think about radiation.

Modern understanding is seated in what might seem an unlikely foundation: James Maxwell's presentation in 1873 of a theory with the appropriate equations to unify the understandings of electricity and magnetism. For each of these two phenomena serious investigations went back to the 17th century but fundamental understanding had been lacking. Maxwell's theory was followed in 1887 by a so-called experiment – it was really a long set of celestial observations − designed and carried out by A. A. Michelson and C. W. Morley and the most direct and simplest interpretation of the experiment is that there really is no pervasive intangible medium whatever though which radiation moves. Finally, from Maxwell's synthesis and built into the Theories of Relativity is the premise that all radiation moves through empty space at a constant speed of about 300,000 km/s but at a slower speed through any material medium.

Here it will suffice to imagine a packet of radiation as two trains of waves that have been emitted at some point by a process that need not be specified. One of those trains is a small electrical disturbance that embodies a small quantity of energy and the other train, at right angles to the first, is a small magnetic disturbance that also contains a small amount of energy. Waves have to have a wavelength and the different types of radiation have different wavelengths – those of light, for instance, are of average dimension about 0.00005 cm for the peak sensitivity of a human eye. Because you know how fast the radiation is traveling and you know how long a sample wavelength is, you can calculate how many waveforms pass a given point per second – about 600,000,000,000,000 for the wavelength just described. Obviously, it is impossible for the eye to see these waveforms one by one.

If you looked back along the two propagating wave trains toward their source, you ordinarily wouldn't see them lined up into planes seeming to make a plus sign. Rather, they would have all possible angular aspects about that axis. It does actually rarely happen, though, that the wave trains are indeed lined up exactly as if they made such a plus sign, as is illustrated in Fig. 3. For this special case, the radiation is said to be 100% polarized.

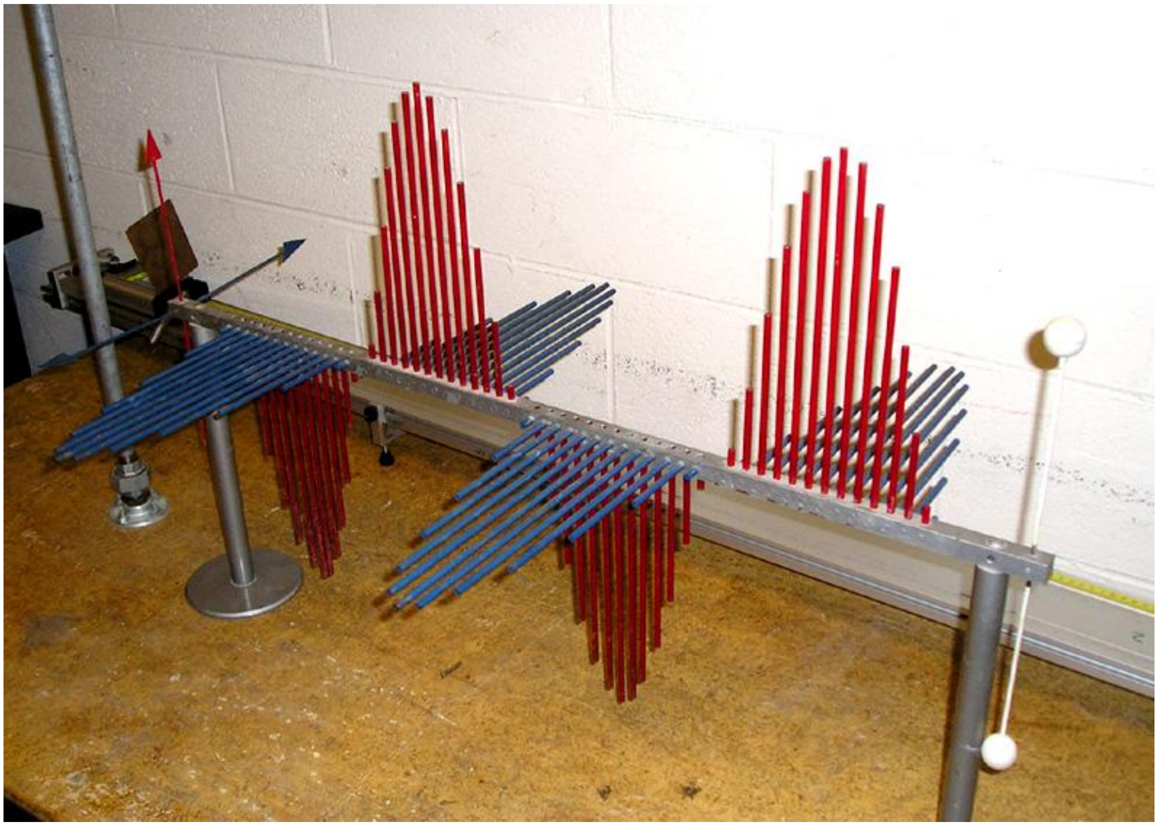

Fig. 3. A model of a 100% polarized wave with a unique wavelength; two complete waveforms are shown. In the model the radiation is imagined to be emitted by some interaction between two particles indicated by the two white balls at the right and to propagate toward the left. The red waveform may be imagined to be the electric component and the blue one the magnetic component or, equally validly, the inverse assignments.

In the more common situation, the radiative wave trains may be lined up only fractionally in this way – the partially polarized condition – or totally at random – the unpolarized situation. Similarly, it is rare that the source would emit at one time radiation with only a single wavelength. That does actually happen but much the more common situation is that the source will emit many different wavelengths and each in its individual number at a given time.

It is also possible that, for a given wavelength, two waveforms will align in space so that they add together in a display known as constructive interference. The opposite situation can also occur, namely, that the peak of one wave falls on the trough of another so that destructive interference results. Finally, the waveforms may be only partially in or out of alignment.

After about 1860 it became more and more common to disperse the collected radiation into a pattern wherein one waveform lay cheek-by-jowl with the one of next longer wavelength so that the appearance of the pattern was a spectrum, a "spectre" of the white light. Isaac Newton had first done this toward the end of the 17^{th} century but later it became possible to use another (not Newton's) interaction of radiation with matter to do the same thing and to do it more efficiently. The bright illumination along the edge of an obstacle with Sun behind it, the patterns seen through a very narrow slit in a piece of metal, a perforated screen or a coarsely woven textile, and the color patterns reflected from a piece of grating jewelry are all examples of what is called diffraction. The interaction can be used to make visible two celestial objects that are ordinarily too close to be perceived as two and also to make spectra of celestial sources.

In the early 1940s Andrew McKellar was studying spectral features of the cyanogen molecule from tenuous gas clouds between Milky Way stars emitting that spectrum and the terrestrial observer and he noticed that the strengths of the cyanogen features were consistent with the molecular gas being at a very low temperature, just a few degrees above absolute zero. Within a decade, Ralph Alpher and Robert Herman showed that the relative abundances of some of the chemical elements observed in stars and in interstellar space could be explained if the Universe had begun at a very high temperature. No one would have witnessed the event if only because most of the chemical elements which compose our bodies had not yet been made. Coupled with the expansion of the Universe discovered by Edwin Hubble in the 1920s, this understanding could be assimilated into the idea that the Universe cooled progressively as it had aged so that, from the original bath of γ-radiation, the dominant radiation form became progressively X-rays, UV radiation, light and IR radiation. Again, no human saw any of this red-shifting cooling because it had happened so long ago but it was predicted that there should, however, still exist a very cool remnant of the primordial fireball and that it should fill the sky to be discovered in the present day. The signal of that remnant radiation was indeed discovered independently at Bell Labs and Princeton in the mid-1960s and it has since been modeled with a mean temperature of 2.725 K. This very precise measure, with 4 significant figures, is an indication of the uniformity of the Cosmic Microwave Background (CMB) as it came to be known because the spectrum of the radiation peaks at microwave frequencies. This was clearly a cosmological signal of utmost importance. The essence of science is to ask questions beyond current knowledge and it might be wondered if the CMB uniformity can express itself in still another significant figure or if there is small-scale structure that deviates from complete uniformity. By the early 1990s, Earth-orbiting instruments were finding that the CMB was not of an absolutely uniform temperature but had "cool" and "warm" patches intermixed with each other. Would these nested patches be of a unique size? The answer from later observations was that there is a distribution of patch sizes with an obvious peak around an angular diameter of 1^{o}. More and better balloon and ground observations discovered one lesser peak at a smaller angular size and possibly a still smaller third one. These structures result from the curvature of the Universe and from ripples in the original fabric of spacetime. Even the polarization signature of the CMB has now been measured although the noise of the measurements is still very large. The prospect is that both ground and Earth-orbiting observations of this relict radiation are going to continue indefinitely.

It would seem an implication of all the foregoing that waveforms and photons of radiation pass through empty space in straight rays until they encounter some matter obstacle. For almost all purposes historically, this concept has been an excellent approximation to reality but it does not represent the generality of things. Albert Einstein's propounding of the 4-dimensionality of the Universe has meant that one speaks of spacettime to describe the general fabric of the world and not separable entities of space and time. A consequence of this understanding is that radiation does not travel along linear rays but along paths, called geodesics, that have a curvature depending on the masses by which they pass. The sense of the curvature is such that a great mass, such as a star or a galaxy, will act as a lens collecting more of the rays than if the paths were rigorously linear and so, at the very distant point where the focused radiation can be recognized, the source of the rays will appear to be brighter than if no mass had been in the way. The effect is called gravitational lensing and there are other effects too in addition to the brightening of the radiation. For the brightening to be recognized, either the background source of radiation must move closer to the observer's line of sight to the foreground lens and then move away from that line of sight or the lens must make similar motions with respect to the background source or both source and lens must move closer into and then further away from alignment. The closer the lens lines up with the source, the greater is the apparent brightening of the source. The optical display seen by the observer is called "strong" lensing if there also appear displaced images of the background source in the form of one or more circular images or arcs. Clusters of galaxies, having very large masses, do act as strong lenses imaging more distant galaxies behind them. "Weak" lensing can be detected even if these spectacular effects are not prominent. Both knowledge and technique have now improved to the level that it is meaningful to search for lensing of background sources by small Solar System masses.

Almost all astronomical information in this partial history has resulted from collecting electromagnetic radiation and interrogating it for its information content. The collection is done by telescopes and the interrogation by instruments mounted on the telescopes, by calculations and by conceptual imagination.

TELESCOPE DESIGN AND FUNDAMENTAL ASTRONOMY
1769 [Rittenhouse, Brashear, Charles, Anderson, H. Evans, Kast, Olivier, Blitzstein, Merrill, Protheroe, Friedman, Davis] 1970

At the beginning of the 17^{th} century telescopes, then new, were hand-held or pivoted on simple posts such as unipods used by TV cameramen. To follow the slow and predictable motion of a star across the sky this is not too inconvenient provided the angular size of the field of view is reasonably large. Since each telescopic discovery suggested a new investigation, it was inevitable that a sturdy telescopic mount should be fixed in an appropriate building that could be sited conveniently in a dark location.

An obvious improvement would be to mount the telescope tube on a pivoting axle about halfway along the tube length and to restrain that axle so that it would lie horizontally pointing east and west in a pair of stable beds. A mount of this kind is capable of seeing objects only as they pass across the celestial meridian, that imaginary circle which passes through the zenith and the north and south points of the horizon. Over the passing centuries, similar instruments equipped with measuring scales have been used in concert with clocks and recording devices to keep track of time – scientific time and civil time as well. Because of their limited motions and capabilities, these are variously known as transit instruments or meridian circles and it is the improved latter machines that are capable of measuring the celestial coordinates and changes of the coordinates of stars and Solar System objects to very high precision. Just such a device was used beginning in 1668 to make the measurements leading to the first credible calculation of the speed of light. A simple, portable transit appears in Fig. 4. The telescope objective lens is 2 inches in diameter. The tube and axle can be lifted off the pivot beds and each of these assemblies and the blue base boxed and slung on a mule. A good meridian circle, in the absence of atmospheric turbulence, would have a capability of measuring the angle which is the equivalent of a dime seen at about 200 km**.** distance. If the beam of one of these simple instruments is turned optically at right angles to its original direction, the telescope is called a broken transit but no component is mechanically broken.

Fig. 4. A portable transit made in 1886. One of the cylindrical pivots can be seen riding in its bed with a small spirit level above it and another larger level is upright on the table.

A second specialized telescope was developed in the late 1720s and re-invented more compactly in 1834 with a capability of extremely precise measures of only the north-south coordinate of position for objects that pass very near the zenith. Not surprisingly, these are called zenith tubes. Fixing the zenith (and opposite) nadir directions at any site is the first step toward evaluating very accurate geographical coordinates and opens the way to deeper geodetic understanding as well as determining the variable velocity of Earth in its orbit. It is a consequence of the design of all the instruments just described that measures of celestial objects are commonly limited to angular positions – the analogue of saying that an object is at 60º with respect to some favored point on the circumference of a circle – and are not determinations of distance – the analogue of saying that you don't know the circle's radius – and that they are intimately enmeshed with the most careful determination of time.

The two types of designs that have just been described have great mechanical stability but lack the capabilities of looking at every part of the sky in quick succession and of following an object as it is carried across the heavens by the rotation of Earth. An easy way to overcome the first of the limitations is to mount the entire transit or meridian circle on a vertical axle at right angles to its east-west axle. Altazimuth mounts, as these are called, have now returned to vogue for very large telescopes because they can easily be computer controlled but for much more than a century they had dropped out of use in favor of what are called equatorial mounts. These designs are conceptually the same as altazimuth mounts except that the formerly vertical axle is pointed to the North Celestial Pole, very near the star Polaris. If you turn the entire machine around that axle, you can mimic and keep up with the apparent motion of the heavens; all that is needed is a source of power. In the 19th century that power source was commonly a weight on a chain dropping slowly under gravity and in the 20th century a motorized gear train. This may be said another way: motion about the polar axle provides east-west motion to track a star as well as the capability to make an east-west movement to any point on the sky. Rotation about the second axle, the one perpendicular to the polar axle, offers the north-south degree of freedom that you also need. A hybrid, third design combines the altazimuth and equatorial capabilities in that the latter drives a slaved mount of the former. This was invented in the mid-19th century and the first model was mounted so as to look out a bedroom window of a Paris flat. With either of the first two designs, the old-time winter observer would most likely be cold because he had to work directly at the telescope open to the night air. With the siderostat (the third) design, the telescope could be cold in the winter but the observer would work remotely in a more comfortable room.

The function of the telescope tube is to mount all the large optical components, refracting lenses or reflecting mirrors. The functions of the optics are to collect as much radiation as possible by being as large as possible and to bring that radiation to a conveniently focused image where it can be worked with. Within limits imposed by the atmosphere, the larger the diameter of the optics, the better the detail in the image can be resolved and studied. By the 18th century simple, single lenses were found to be so unsatisfactory for the increasing demand for better images that doublets of lenses and then trains of multiple lenses began to be used. These complex lens systems persisted well into the 20th century and many commercial cameras were built around minified versions of astronomical lens designs. The reflecting telescope – first with polished solid metal mirrors, then with silvered glass ones, next with aluminized ceramics, and finally with exotic metal coatings – appeared toward the end of the 17th century and, for an assortment of reasons, these have been the preferred choice since about 1900. There are plenty of accessible references testifying to the ingenuity of mechanical and optical engineers and amateur astronomers in telescope design going far beyond this simplified description.

Optical solar telescopes are specialized instruments because heating from the potent sunlight would be severe inside an ordinary closed tube and because Sun does not move across the sky at the same rate as the stars. Numerous solar instruments have been built as variants of siderostats.

Ultraviolet telescopes such as those on space platforms in Earth orbit or on interplanetary missions are not dissimilar to optical ones and relatively small radio telescopes also resemble optical reflectors to some degree. X-ray telescopes require special optical configurations that have the same purposes as do the similar elements in visual telescopes. The largest radio telescopes, however, are immense arrays of antennae at least partially fixed onto the ground and the electrical signals from all the individual antennae are brought by cables to a central mixing and processing facility. Telescopes in Earth orbit or on interplanetary trajectories typically require large support teams to look after the health of the spacecraft and its instruments and its communications links back to Earth. They have ranged from small machines to meter-scale ones and, of course, the programs with which they are identified are very expensive.

There also have been invented "particle" telescopes which do not image (as does the lens or mirror of a conventional visual telescope) a flux of particles but rather just intercept the particles. Examples include the cosmic ray detectors which have been installed many places on ground or flown in balloons and in spacecraft and other devices which pick up the sub-nuclear particles known as neutrinos and anti-neutrinos.

TELESCOPIC INSTRUMENTS

1769 [Rittenhouse, Ewing, Brashear, Charles, Olivier, Protheroe, Pierce, Levitt, Gee, Whitney, Wilson, Blitzstein, Chen, Wolf, Poss, Fay, Wyller, Davis, Koch, Friedman, Giovane] 1998

When a telescope objective – its major lens or mirror − has brought all the collected radiation to a focus, it is possible to examine it and work with it. Historically, the first effort was simply to put a small magnifying glass at a convenient location so that it could collect all the focused radiation and study it. The detector was the eye that has an efficiency of perhaps 0.1%, that is to say, for every 1000 photons one piece of information is transferred to the brain. If the eye saw two stars and wanted to determine the separation between them and their angular orientation, there would be provided some movable set of cross wires whose locations could be read against angular and linear scales to give just this information. Devices of this kind, called filar micrometers, exist to this day. If the brightness or the brightness ratio of those two stars was to be determined, a trained eye could do this to a precision of about ±10% but it is necessary that the eye/brain apparatus remember the scale of brightness accurately from night to night. This is very difficult. In order to overcome that limitation, there were invented beginning in the early 19th century, one after another of what are called physical photometers. These exploit one or another property of light radiation and of geometry so as to refer the light from a celestial source to a calibrated source of radiation built into or brought into the photometer. The eye examines both the celestial source and the calibrated source and makes a measure of the brightness of the celestial source compared to the calibrating source. These instruments offer a gain in precision of about a factor of two compared to eye estimates. In principle and in practice, the eye can examine any telescopic image and the brain can tell the hand to make a drawing of it. This is fraught with every difficulty imaginable and defending that drawing to another party at a later time can be a challenge.

The ephemeral character of visual observing and the impressionistic character of the records made the mid-19th century appearance of the photographic process an obvious choice to replace the eye as a detector. After all, its efficiency is about 1% - ten times greater than that of the eye. Whereas the eye is a rate meter, much like the electric meter on a house showing instantaneous power usage, the photographic emulsion stores information. Obviously, if you expose an emulsion for minutes, you can collect more information than the 1/16 sec latency that the eye commands, which is also to say that photographically you can accumulate information about objects fainter than the eye can see. That information, when rendered visible by chemical means, can be studied at leisure and repeatedly and it is nearly impersonal. It can be no surprise that electronic cameras, appearing about 1980 as the direct ancestors of personal digital cameras, are now the detectors of choice with efficiencies about 50 times greater than that of photographic emulsions. Whereas the precision of brightness measures with emulsions can be of the order of ±1%, with electronic cameras there is commonly a 30-fold improvement in that number.

Chronologically between the eras of emulsion and electronic imagery, there was almost the entire 20th century of photoelectric detection. The photocell devices are great upgrades upon the once-familiar electric eyes that would open doors in supermarkets. With them, imagery was difficult at best but precision of brightness measures was of the order of ±0.1%. Photoelectric detection is not yet over because it is a fast and efficient way to measure brightnesses and colors of celestial objects but the technique is definitely on the wane.

Other information can be extracted from the radiation collected by the telescope. For instance, if you want to compare the red fraction of light of an object to the blue fraction of the light, you can insert colored glass filters in the beam and measure the brightnesses individually. It matters little, other than the level of precision that can be attained, whether this is done with a photographic emulsion, photoelectric cell or electronic camera. Should you wish to determine the polarization state of a beam, there are available hardware devices that are generically similar to and vastly superior to Polaroid sunglasses and these can be used with any detector too. By combining the appropriate lenses, mirrors, prisms or diffraction gratings – one of which is displayed in Fig. 5 – in some box or room, it is possible to disperse the focused light into a spectrum that can be measured - once photographically but now electronically.

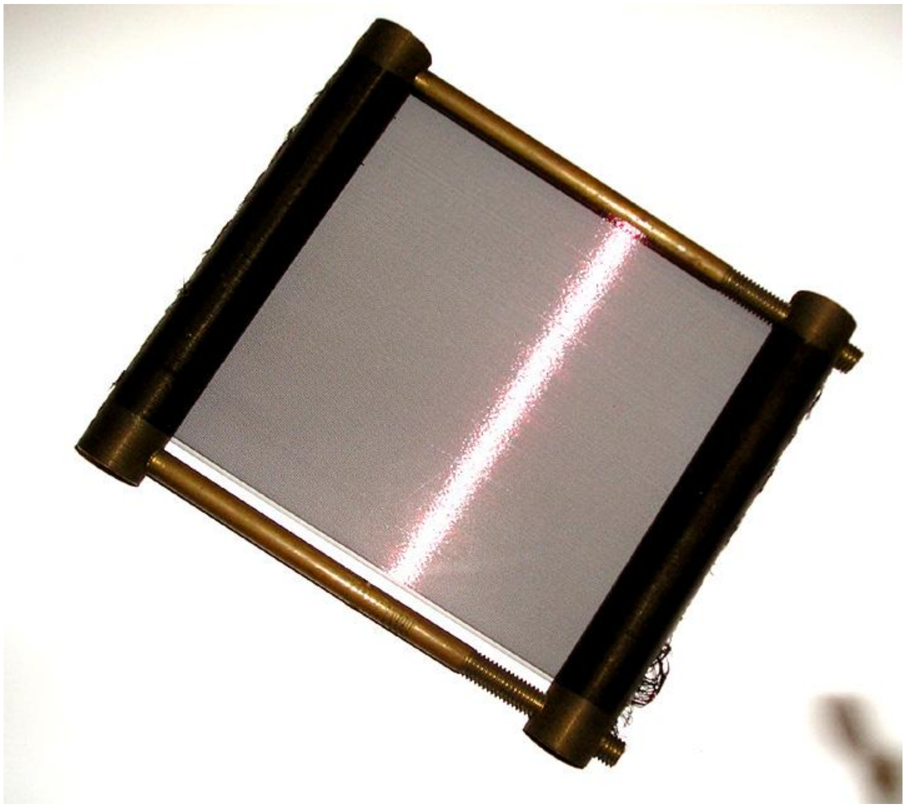

Fig. 5. The brass frame is about (5.5 x 5)-in and the thicker rods were worked on a lathe so as to cut a 0.02-in thread along their lengths. Across the frame and held taut in the threads there is wrapped a continuous 0.01-in diameter wire. Light from celestial sources passes through this coarse, old-fashioned grating onto the objective of a telescope and is then imaged into the telescope's focal plane where it appears as a spectrum from each source. The bright streak across the wires is glare from the ceiling light above the camera that was used for this picture. Modern versions of this device have microscopic grooves (the analogues of the wires) ruled into transparent or reflecting surfaces.

Whereas the original hardware items to do this were smaller than desktop size, modern installations fill entire large rooms. From the records made with these instruments it is possible to learn the temperatures, absolute brightnesses and line-of-sight velocities of many celestial objects.

Detectors and other hardware used for ultraviolet and infrared radiation have a considerable resemblance to those used for visible light. Radio radiation is a somewhat different matter since the detectors are basically radio antennae and color and polarization filters are replaced by solid-state elements.

For particle telescopes, detectors are in the nature of chemical or atomic or nuclear radioactivity laboratories because the intercepted particles interact with the atoms and nuclei in the detectors and it is the products of these interactions that are detected. Need it be said that once only analogue records were made but now digitized ones are most common.

ALMANACS, CLOCK MAKING, FUNDAMENTAL ASTRONOMY, NAVIGATION, SURVEYING
1751 [Grew, Rittenhouse, Ewing, Smith, Patterson, Courtenay, Kendall, H. Evans, Anderson, Bohjelian, Kast, Eric, Barton] 1919

Everyone has heard of *The Farmer's Almanac,* in one sense the descendant of *Poor Richard's Almanack* published by Richard Saunders, who was really Benjamin Franklin. No year goes by without North American editors assigning idle journalists the task of writing a feature article hashing over its weather predictions and glossing its success and failure record. Similarly, every respectable daily newspaper has a sidebar or almanac section that gives sunset, sunrise, moonset and moonrise timings, twilight intervals, lunar phases and tidal phenomena for locations with significant shipping interests. Many papers also carry predictions of auspicious times for sport or commercial fishing based on what are called sollunar tables.

None of this is a response to current idleness or capitalism. Instead, the origins of modern astronomical almanacs rest in the 17th century when a few European countries embarked on concerted enterprises of discovery and mapping, colonization and fishing. Blue-water seafaring was the only transportation method that was useful and land markers were out of the question for most of any voyage. The obvious two dangers were that a vessel could become lost and sail aimlessly without making the intended landfall or it could fetch up unwittingly on a lee coast. What was needed was a way to refer a boat's momentary position to the geographic grid of longitude and latitude no matter how incomplete and out of scale were the maps at hand.

While the magnetic compass had been in use for centuries, it was unreliable for a number of reasons, an important one being that it really provided only a single approximate direction for the momentary position of a ship. One start on the remedy to this ignorance was to create a tally of celestial phenomena – naked eye ones or those accessible to a telescope of about 1 cm objective diameter – that could be consulted by a mariner and referred to a coordinate grid locked into the sky. What could usefully appear in such a document? Obvious entries would be geographic coordinates of major ports and even inland cities, the celestial coordinates of numerous bright stars scattered broadly across the sky, the positions of Sun and Moon for at least daily intervals, the phases of Moon, the changing coordinates of the naked eye planets, the locations of Jupiter's satellites around the planet, and the incidence of solar and lunar eclipses. All these would typically appear as tabular entries of numbers – not sketches or drawings – and no one could know them intuitively; every one of the numbers had to be calculated by equations based on an appropriate theory. At any one time, each theory would be of limited precision and accuracy and, until the relatively recent past, a labor of dogged persistence. The end product was some kind of almanac, typically published and distributed by a national government. The entire effort began in 1679 with the appearance of *Connaissance des temps ou des mouvements célestes*. The U.S. book in its present form emerges from the U.S. Naval Observatory and is amalgamated with the British volume in a single cover as *The Astronomical Almanac* but of course all the contents are now online as well. A page from a modern national almanac, indicating that English has not completely swept the world, is shown in Fig. 6.

So, since there existed a hand-held book containing all the needed celestial information, the quartermaster of the boat could put it to use. What he needed was some piece of hardware that would permit him to observe at least one of the phenomena tabulated in the volume. Historically, such a device was either an octant (1731) or a sextant (1757), the names deriving from the angular size (1/8th or 1/6th of a circle's circumference) of the arc that formed part of the frame of the device. The gadget was held in one hand and so manipulated that the image of, say, the noonday Sun was caused to appear tangent to the sea horizon viewed in the eyepiece of a little telescope attached to the frame. This tangency was achieved by the free hand swinging a movable arm as the eye continued to look at the solar image. The reading of the location of the movable arm against the scale on the arc then declared the instantaneous angular height of Sun in the sky and an entry from the almanac permitted each of a mental subtraction and addition that led directly to the latitude of the boat. To say that this required more than a little eye-hand coordination and a firm planting of the entire body on a rocking, pitching vessel is to labor the obvious. Sextants, much the more useful instrument, are usually brass but some have been made of wood and many were elaborately chased and even inlaid with valuable materials. A sextant can be made to work on land as well as at sea. The needful thing then is to find some substitute for the natural sea horizon and this is ordinarily done by using

시 각			행 성	현 상	시 각			행 성	현 상
월	일	시			월	일	시		
1	5	00	목성	유(동-서)	7	5	20	지구	원일점
	5	03	지구	근일점		9	02	토성	합
	6	23	수성	유(서-동)		15	10	금성	최대밝기
	17	19	수성	서방최대이각(24°)		27	12	수성	동방최대이각(27°)
2	2	18	해왕성	합	8	9	14	수성	유(동-서)
	22	11	천왕성	합		18	04	금성	서방최대이각(46°)
3	4	11	수성	외합		24	06	수성	내합
	4	14	목성	충		28	04	천왕성	충
	8	00	토성	유	9	1	02	명왕성	유
	25	08	명왕성	유		2	03	수성	유(서-동)
	29	21	수성	동방최대이각(19°)		9	23	수성	서방최대이각(18°)
	30	02	금성	동방최대이각(46°)		15	22	화성	합
4	7	06	수성	유(동-서)		22	09	목성	합
	17	10	수성	내합	10	6	04	수성	외합
	29	19	수성	유(서-동)		24	19	해왕성	유
5	2	17	금성	최대밝기	11	8	20	토성	유
	5	22	목성	유(서-동)		12	11	천왕성	유
	15	06	수성	서방최대이각(26°)		21	10	수성	동방최대이각(22°)
	18	09	금성	유(동-서)		30	22	수성	유(동-서)
6	8	18	금성	내합, 태양면 통과	12	10	17	수성	내합
	11	09	천왕성	유		14	02	명왕성	합
	11	21	명왕성	충		20	16	수성	유(서-동)
	19	06	수성	외합		30	06	수성	서방최대이각(22°)
	29	23	금성	유(서-동)					

Fig. 6. As can be seen clearly from this page of the Korean Almanac, Venus was at inferior conjunction at 1800 hours Korean Standard Time on June 8, 2004 at which time it was halfway through its most recent solar transit.

a piece of obsidian or unpolished glass or a pool of mercury to provide the reflected image of the object in the sky. It is even more of a challenge to make the process work at night when one might want to measure the angular distance between, say, a star and a planet. Eventually, surveyor's theodolites (an angular measuring device more versatile than a sextant and mounted on a portable tripod) and chains and rods were put into wide use for land surveying with celestial measures still necessary to determine the cardinal directions and geographical coordinates.

It can readily be accepted that no almanac can be computed for every possible location on Earth at every possible minute of every day. Instead, standard locations and times are used as the basis for all such calculations. Suitable choices might be midnight at the Greenwich meridian or noon in Paris or Washington; Greenwich was chosen in 1884 and has been used ever since. Clearly, what was also needed is a clock to keep time out of sight of land for months on end. Inventing such a time-keeping device was a major preoccupation of practical science and engineering into the 18th century. The story of how this was done has been told numerous times and here it suffices to say that John Harrison made a sequence of such timekeepers that by 1762 proved to be accurate and correctible to his own stringent demands and he should have been awarded the very substantial prize that was on offer. The British bureaucracy stumbled and prevaricated but finally awarded him most of the sum when he was an old man. With the almanac predicting the time of a particular phenomenon at Greenwich and the mariner observing the same phenomenon with his local timepiece and hardware, he could calculate his longitude by a single subtraction since Earth is a rotating, solid, nearly rigid body.

There was also a second constituency for almanacs and clocks in the growing number of field and immobile stations dedicated to purely scientific astronomical work. The staffs of these establishments had no time to make the necessary calculations themselves and would readily buy the volumes containing the predictions of the yearly phenomena. As time went by, more and more different kinds of entries, not limited to naked eye phenomena appeared in almanacs. For the most part, these additional contents resulted from demands of these observatory station scientists. It must also be appreciated that every station needed at least one clock and this requirement was fulfilled by generations of tremendously skilled artisans who invented one device after another to improve timekeeping.

EARTH'S ATMOSPHERE AND OTHER PLANETARY ATMOSPHERES
1950 [Blitzstein, Protheroe, Negley, Giovane, Goldstein] 1990

If you cannot move above Earth's atmosphere as in a spacecraft, you must inevitably observe through that mixture of wind-blown gases and particles and organisms. The consequences of this are profound. For instance, the advance of sunrise and moonrise and the retardation of sunset and moonset are direct consequences of atmospheric refraction, a simple law of which was first enunciated at the laboratory level in 1621 by Willibrord Snell. Except at the zenith, no object is seen from Earth's surface in exactly the same direction as would be the case from above the atmosphere. From star to star, these directional displacements are not quantitatively the same across the sky at any one time. It is also true that from below the atmosphere each stellar point of light is actually dispersed into a small spectrum with the pattern of colors of the rainbow standing perpendicular to the horizon. The length of this spectrum also varies with position in the sky. The largest telescopes require correction for these effects.

There is another effect that follows from living on Earth. The atmospheric molecules scatter light in random directions from every extraterrestrial source and this is the reason that we experience the evening and morning twilights and a blue sky on clear days. Those rays of light come from beyond Earth but the same effect happens with artificial light. Every ray directed from ground level upward or scattered upward passes into the atmosphere and contributes its bit to lighting up the night sky. Inevitably, at some locations and some times faint celestial sources will be invisible against this foreground illumination and the problem is accelerating all over the planet.

Radiation from celestial objects is also attenuated as it passes downward through the atmosphere so that we never see objects to be as bright as truly they are at the top of the atmosphere. The lower they are in the sky, the greater the attenuation. As far as making measures freed of this degradation, it cannot be done from Earth's surface and the only possibility is correction for the effect by a process of extrapolation. In a loose and averaged way, we understand the mathematical form of the atmospheric attenuation and can write down a simple equation that expresses it. Suppose that we make a measure of the light level of a particular star first when it is very low in the eastern sky and then we repeat the measure as it rises higher and higher and continue doing the same thing as the star drops lower and lower into the western sky. With an appropriate *x*-coordinate we plot these data and find them to fall on a straight line. The end point of the process is that we extrapolate this line to the fictitious, impossible case that the star went through the zenith and then extrapolate it further as if we had observed it through no air at all. In principle, that value is the real brightness of that object on that night and we have evaluated and removed the atmospheric scattering and extinction. This technique was first understood in 1729 by Pierre Bouguer but was not really applied routinely until photoelectric measures became common after World War II. Naturally, it is more complicated than the simplistic explanation given here – the atmospheric extinction is not the same for all colors and all celestial objects and it is very likely to vary through a night, as actually appears in Fig. 7.

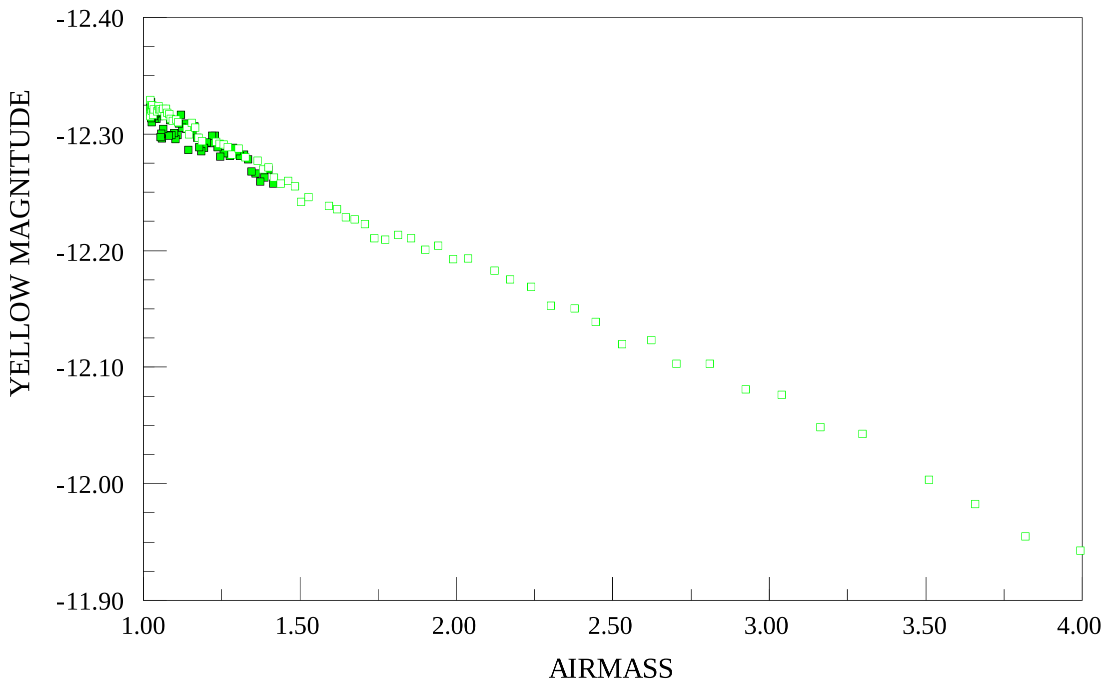

Fig. 7. The abscissa scale is a measure of the total quantity of air that has been traversed by the beam from a constant star before reaching the telescopic instrument on Earth's surface and the ordinate represents the measured, apparent brightness of that star. The right-most filled symbol shows the first measure of the night when the star was climbing in the eastern half of the sky. As these symbols progress to the left, they indicate the star approaching its maximum apparent brightness when it is as high in the sky as possible. Further and further to the right the open squares indicate the same star dropping lower and lower in the west, during which time it brightness diminished by somewhat more than 30%. At airmass = 1 the brightness would be that expected if the star actually passed through the zenith (which didn't quite happen) and, extrapolated to airmass = 0, the brightness is the expected value as if no atmospheric scattering had happened (also a fictitious situation). The atmospheric attenuation varied slightly during the night.

Nonetheless, the process just loosely described is the basis for understanding the brightness of all celestial sources whether they be constant or variable and, as well as possible, it is applied to all brightness measures made from Earth's surface.

Two other effects are caused by the terrestrial atmosphere. Because this mixture of gases is not of uniform density and because these inhomogeneities are in constant motion blown horizontally and vertically by winds, the light of a point source is caused to appear to come from slightly different directions when it is seen from ground level. Each one of these seemingly different rays has its individual brightness as well. The stars are said to twinkle and scintillate and, when you look at or photograph them, they appear as disks of finite size. The recognition of these effects became a way to study the upper atmosphere winds at heights to which instrumented balloons could not ascend.

It is easy to imagine that what has been learned about Earth's atmosphere would also have some application to the gas shells of other planets. This generality is true in the sense that we would have the set of terrestrial expectations to be tested but it should be no surprise that Venus, Mars and the giant gas planets all have atmospheres strikingly different from ours.

GEODOSY
1818 [Adrain, Charles] 1916

Newtonian gravitational theory had a direct application to Earth itself but it was not the only concept that was necessary to understand the planet. It was necessary to take account of planetary spin and initially there was no agreement about the equilibrium shape of a rotating, self-gravitating solid. This matter was settled empirically by the field expeditions set in motion by Pierre de Maupertuis in the 1730s so that by the next century there were important new questions framed about Earth's structure.

Everyone is used to atmospheric changes built into daily weather and seasonal patterns of sunlight and precipitation and the storm surges and hurricanes and cyclones that march across the oceans and land. We also accept that there are going to be avalanches, rockslides, earthquakes and volcanic events that sporadically affect Earth's solid crust. It is now generally appreciated that the crust itself is broken into nested segments that are in constant motion albeit at a slow rate.

Whereas the direction of Earth's axis of spin is, in the short term, fixed in space, its approximate axis of symmetry is not so fixed and motions of the crust carry towns, farms and people with them. The effect is not large – the cyclical excursion is of the order of the dimension of a tennis court over about 7 months – but one consequence is that the latitude of every point on the planet is constantly changing. Even before the effect was discovered in the late 19th century, the measures of coordinates of stars actually suffered from its unrecognized impact. To say this statement another way, the precision of stellar coordinates was already very high by the mid 18th century. Of course, terrestrial longitude is also changing as well as latitude but the latter coordinate is much easier to determine than the former one, as was indicated above. Nowadays, GPS and other hardware systems have a capability of determining the latitude variation much more precisely than zenith tubes but that doesn't mean that we understand this behavior completely at present. The same activity may or may not happen on other rocky planets.

SOLAR SYSTEM ORBITS AND MECHANICAL PERTURBATIONS
1896 [Eric, Rorer, Barton, Turner, Bohjelian, Mason, Bernstein] 2007

When Newton propounded his postulates of motion and the gravitational law in the 1660s, 70s and 80s, it became clear that terrestrial and extraterrestrial mechanical phenomena could be unified. The sticking point for some people was the concept of action at a distance with no tangible connection between interacting masses while for others the very idea of any interaction was a matter of concern. Here was an invisible, seemingly universal attribute that respected no shield and could not be deflected or reflected. Although it might be considered evident that solar gravitation was the agent causing the planets to move in their orbits and Jovian and terrestrial gravitation causing their respective satellites to pursue their orbits, would there not be other, as it were, cross-interactions?

At the level of introductory Physics and Astronomy course work, these questions are treated rhetorically if at all and most probably are scanted entirely. It is nonetheless true that the fundamental nature of gravity remains a matter of profound interest, its working agent just a postulate with no evidence of its existence and its own nature subsumed into the Theory of General Relativity. One can be sure, however, that the true character of gravity requires that there does exist a mutual attraction between every pair of masses that may be enumerated. It is only a question of which is the dominant interaction and what are the rankings of the subordinate ones and which of the subordinate ones can be measured or accounted for within the accuracy of observations at a given time.

One need not leave Earth to find evidence of this effect. Imagine that a plumb line and bob are hanging over a level field in Nebraska and that from the surface to Earth's center there is nothing but spherically symmetrical layering of rocks of locally uniform density; the line will point to Earth's center for sure. Now take the same line and bob to the foot of Pike's Peak while we imagine the same uniformity to exist beneath the mountain all the way down to Earth's center. The plumb will no longer point to Earth's center but will be deflected toward the mountain because of its gravitational attraction for the bob. Or consider the path of Moon in the sky. It had been known for a long time that that orbit does not repeat itself month after month but is always changing and indeed one detail of this was discovered about 150BC, long before telescopes. A third thought will make another point. If Sun causes Earth and Jupiter and Mercury to pursue their orbits, shouldn't there be motions imposed by these and the other planets upon Sun so that it moves too? This absolutely happens and, since no planet keeps in lockstep with any other, Sun's motion must be very complicated.

How does anyone ever know where to predict celestial objects to appear in the sky? To see the answer to this query, pick an object, say Mars. Then write down the analytical form of the Sun/Mars interaction; it is actually very simple in an algebraic sense. In order to put it to work you also have to know the locations of Sun and Mars at a chosen time and their velocities at the same time as well as the masses of the star and the planet. An orbit can then be computed but it won't be the correct one for either of Mars or Sun and the reason for this is that each of the objects feels a gravitational attraction from first Jupiter and secondly from Saturn and thirdly from Earth and so on. These effects are called celestial mechanical perturbations and they are important. Only when they are accounted for will all the orbits, including the solar one, be correct.

It is not only the major Solar System objects that are perturbed. Fig. 8 gives an example of the changing

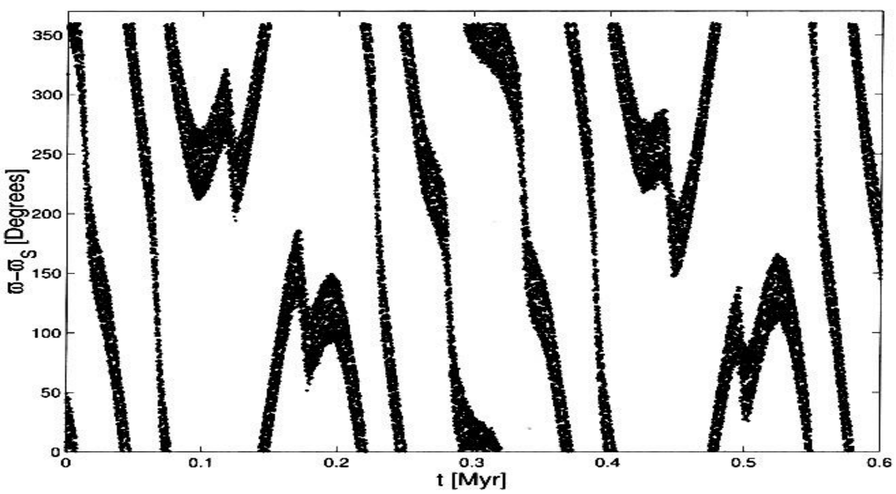

FIG. 10*a*

Fig. 8. Jupiter, Saturn, Uranus and Neptune are always working gravitationally on the satellite of Saturn named S/2000 S5 Kiviuq, that itself might be imagined to have essentially no mass. You can follow the behavior of the satellite in a computer as was done in this case for 600,000 years – many times the orbital periods of the planets. The picture shows what happens to the alignment of the orbit of the satellite as these planets pursue their orbits over that time interval. Clearly, there are some intervals when the alignment keeps rotating without interruption (as in the first 100,000 years) and then there are others intervals where that progress reverses more than once "quickly" as in the next 50,000 years. If the satellite's orbit were constantly aligned in space, the history in this plot would be an infinitely thin horizontal line at whatever was the appropriate angle This illustration of planetary perturbations was taken from 2004, AJ, 128, 1899 – an article by V. Carruba, D. Nesvorný, J. A. Burns, M. Ćuk and K. Tsiganis.

orientation in space of the orbital plane of a small satellite of Saturn over a cosmically brief time. In the past 20 years, recognition of the richness of the Solar System's cosmogony and dynamics has increased immensely with discovery of the very large number and variety of objects that populate the Kuiper-Edgeworth Belt. Attempts are ongoing to take a complete inventory of these non-planets/non-comets and to fit them into larger scenarios that will also account for disks of distributed matter around other stars and the formation of small-mass stars in general.

If, finally, some significant Solar System object remains undiscovered, not a single orbit can be correct and, if the planetary and solar masses are not correct, the orbits will also be wrong. In the days of logarithms and slide rules, calculating correct orbits was a phenomenal task relieved only by the unwitting circumstance that observations were not so precise and accurate as they are now. The variety of mathematical tricks invented to lighten the computational burden was very impressive. Nowadays, because of radar ranging and interplanetary spacecraft successes, the initial conditions and the masses are very well known and the orbits are astonishingly good. For instance, Moon's position is known to a precision of centimeters and it is these realistic orbits that underpin all modern almanacs and spacecraft missions.

SOLAR SYSTEM ECLIPSES, TRANSITS AND OCCULTATIONS
1751 [Grew, Williamson, Rittenhouse, Smith, Ewing, Kendall, Bohjelian, Marshall, Wood, Blitzstein, Gee, Poss, Shaw, Guinan, Koch, R. Mitchell] 1995

Through the 18th and a major part of the 19th centuries there was essentially nothing modern done with solar eclipses. The first known recording of the solar chromosphere had been in 1706 but this had been forgotten and it was not until the eclipse of 1842 that this solar atmospheric layer really came back into astronomical consciousness. Further, there was no conviction that the chromospheric shell belonged to Sun (and not to Earth or Moon) until the follow-up eclipse of 1851. Advances in the understanding of the chromosphere followed only haltingly even after the application of solar photography and spectroscopy. The solar corona, on the other hand, had been known since antiquity (Plutarch described it, for example) but it suffered from the difficulty of recording the details of its delicate structure until photography of the streamers became routine. By the end of the 19th century the variability of that shape with phase in the sunspot cycle had been established and there was agreement that some spectral features were due to "coronium", an unknown element that would have to be shoehorned into the Periodic Table of the Chemical Elements in some way that wasn't understood. From about 1940 onward, one advance after another has occurred in solar studies until now it is possible to build computer models of Sun that accurately describe a significant part of its invisible interior and that convincingly present a quantitative picture of how the star powers itself. Of course, questions remain and eclipses are still followed assiduously although most work is done daily rather than at the infrequent eclipse events.

Lunar eclipses continue to be more or less unrewarding scientifically. Their phenomena are too poorly defined to check the lunar orbit and the only significant discovery of the 20th century has to do with the slow rate of cooling of some portions of the lunar surface when the solar light and heat are blocked by Earth. The thermal inertia of these locales remains of some interest.

Because the orbits of Mercury, Venus and Earth are not too far from lying in a common plane, there are predictable times when Mercury (rather infrequently) and Venus (very infrequently) will be seen from Earth as circular silhouettes passing across the solar disk. For the case of Venus, the phenomenon was first predicted in 1639 by Jeremiah Horrocks and it became known where and when on Earth a transit could be seen. Then in 1679 Edmond Halley cited such an event as an opportunity to solve part of what might be called *the* major multiplexed problem of astronomy since Copernicus propounded the heliocentric model of the Solar System: how to discover the distances of stars and how to demonstrate that it was really Earth and not Sun which revolved and how to evaluate the mean linear distance between Earth and Sun. From ancient times it was known that, if Earth revolved around Sun, each star should show a minified mirror image of Earth's orbit every year, the shape of which depended on how far in angular measure a star was above or below the annual path of Sun across the sky. By Halley's time orbits of all the historical naked eye planets were known in a relative sense – so many times or so much of a fraction of Earth's orbital radius − so scientists saw upcoming transits of Venus as an immense opportunity. At one stroke the Earth's orbital dimension (for shorthand called the scale of the Solar System because the mean radii of all other orbits within it could instantly be calculated in linear units) would become known and so would the mean radii of all the other planetary orbits. What were needed were the geographical coordinates of each observing station, good timekeeping and a good value of Earth's radius. That last number was definitely in hand by the 18th century so numerous expeditions were set in train through the transit of Venus of 1882. From each event some measure of success resulted but unpredictable phenomena and human error degraded the timings no matter whether they were visual or photographic. In 1725 and 1838, two independent proofs of Copernicus's model had already become established and the scale of the Solar System was evaluated very well by an independent method in the early 20th century and eventually by Earth-based radar without any need for transit information. There never was real interest in the transits of Mercury because all effects would be smaller than for Venus.

It is also inevitable that, as seen from Earth, the eastern edge of Moon will pass in front of some star and occult its light until the star emerges at the western lunar limb some time later. This is happening all the time. Timings of these events can be used to check the theory of the lunar orbit but, by the end of the 20th century, Moon's orbit was being determined routinely by Earth-based radar much more precisely than these occultations can permit. Nonetheless, the phenomena have a value: they accidentally discover a number of binary stars which are too close to each other on the sky to be found by routine optical inspection. If the data are taken at a very fast rate, the difference in brightness between the two binary members can also be measured very accurately. Furthermore, for "nearby" giant and supergiant stars the lunar occultation record can give the star's angular diameter. A much rarer type of lunar occultation is displayed in Fig. 9.

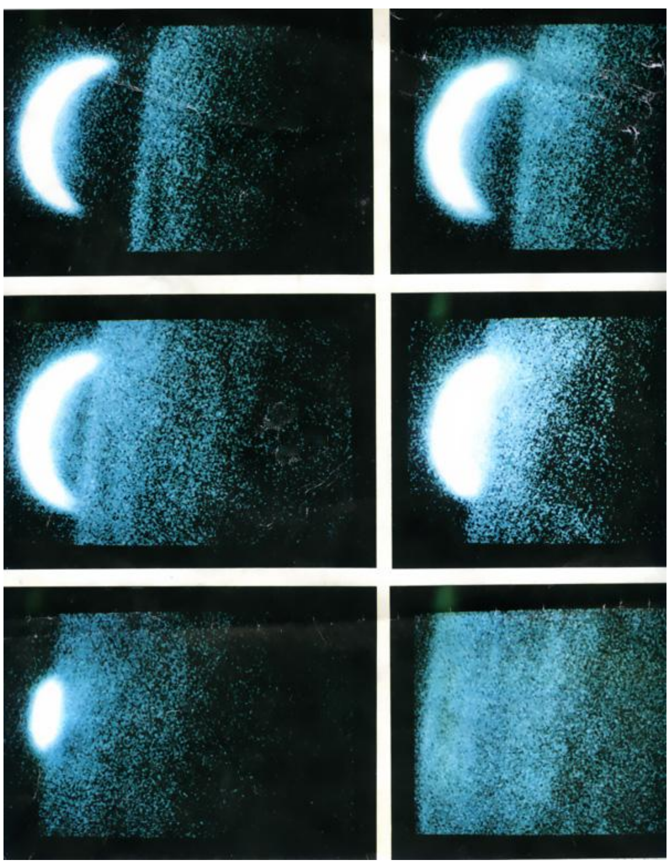

Fig. 9. These panels are to be read from upper left to lower right and were made to test telescopically a new electronic camera in the daytime - nearly at noon. The sequence of 6 frames required about 2.5 minutes. Sun is off each frame toward 10 o'clock in the upper left and the nearly vertical blue streak is the illuminated edge of Moon nearly at new phase. The crescent object is Venus, and Moon is moving leftward occulting the planet until it finally disappears. Magnification is greater in the last frame than in the others so that the curved limb of Moon can be recognized more easily.

Much less frequently, some other object in the Solar System will occult a fairly bright star. If the object has an atmosphere, the star's light will dim slowly rather than be extinguished "instantaneously" as happens for Moon with no atmosphere. On such an occasion, the occultation record can be used to find the mean molecular weight of the gases in the planetary atmosphere and this is a piece of information which may not have been known by other means, principally before interplanetary spacecraft were sent on their missions.

COMETS AND METEORS
1769 [Williamson, Rittenhouse, Kendall, H. Evans, Rorer, Olivier, Hall, MacRae, Wills, Whitney, Woods, Watson, Crout, Binckley, Weber, Whelder, Reilly, Giovane, Soberman, Xie] 1995

Pictorial and written records of heavenly apparitions go back to very early vestiges of human culture. This is hardly surprising because the daytime and nighttime skies were the scenes for astonishing phenomena. The relatively slow and orderly motions of the familiar naked-eye lights suggested, not only that their motions could be relied on and even predicted, but also that the planets, Sun and Moon might have specific purposes for humans. Out of this wishful/fearful rationalization arose astrology, the body of formless conjecture that imagines that the heavens control our destinies and even those of animals and plants.

What then to make of sudden day and night manifestations that had had no precursors and moved across the sky over a matter of days to months or else were transients lasting only a few seconds? Surely there had to be messages for mankind in these appearances. Most likely, the messages would be dire because the unfamiliar was frightening. Could one not suppose that the transients in the heavens were either warnings of something uncontrollable about to happen or prophecies that there was still time to change bad behavior before that punishment was sent from on high. Maybe the king would die or a famine was about to happen. Through most of human history kings did die with gratifying frequency and crops failed all too frequently. The expectations were self-fulfilling. (Of course, there were sometimes twists away from the pessimistic beliefs as in the case of the biblical Star of Bethlehem.)

The fact that comets are objects of scientific interest began with Newton and Halley. Halley was a committed Newtonian from the beginning and among his infinite interests were apparitions of comets. The bright ones of 1531 and 1607 he found to be moving in the same orbit and furthermore that orbit was the same as the one for the bright comet of 1682. He was able to show likely repetition back to the 11th century and also to account conceptually for the same object showing up at slightly different intervals by invoking perturbations due to Jupiter. His predictions: the same comet would appear again in early 1759 and he would be dead by then. Both things came to pass (he died at 86 in 1742) and the first prediction was a very substantial triumph for Newtonian gravitation.

The busy variability of comets is due almost entirely to their fast tumblings about more than one axis and the heating from Sun and is most prominent when they come into the inner part of the Solar System where solar action on them is most intense. These small and fragile objects have ephemeral existences since all matter expelled from a comet's nucleus is lost to it forever and it accretes essentially nothing to make up for what it has lost. With the impact of Comet Levy-Shoemaker 9 on Jupiter and the discovery of astonishing numbers of small objects beyond Neptune's orbit, interest in comets is higher now than it has been since the predicted return of Comet Halley in 1759.

The matter lost from comets includes uncountable numbers of microscopic and very small dust particles as well as large rocks. Inevitably, Nature makes more small things than large ones of a given kind so the microscopic solids are the most abundant. Released from a comet's nucleus, the particles pass behind the nucleus as it pursues its perturbed solar orbit but they still continue to travel in orbits similar to the comet's. Should Earth pass through that swarm of orbiting dust particles, their passage through the terrestrial atmosphere causes a nighttime display of a meteor shower with all the meteors seeming to appear near a small patch of sky , called the radiant, and to streak outward in every direction from that location until they vanish. Not all meteors are of the same brightness, and occasionally a very bright one becomes visible and streaks a very large angular distance before being extinguished. From such a display, there may remain a smoke trail showing the path for some minutes before the upper atmosphere winds cause it to dissipate. Around the beginning of the turn into the 20th century there arose a controversy over whether the radiant appears to move or really does move across the sky. It is naturally true that many shower meteors are invisible to the naked eye but can be photographed or detected electronically. Radar echoes from meteor particles permit study of showers during the daytime.

During such a display, thousands of meteors may be visible per minute for a limited time but the usual shower is not so extravagant. Of course, any display is most conspicuous if Moon is down and the sky is cloudless. A time-lapse engraving of a rich shower is displayed in Fig. 10. Essentially all known showers have now been identified with their parent comets and are known by the name of the constellation within whose boundaries the radiant is located.

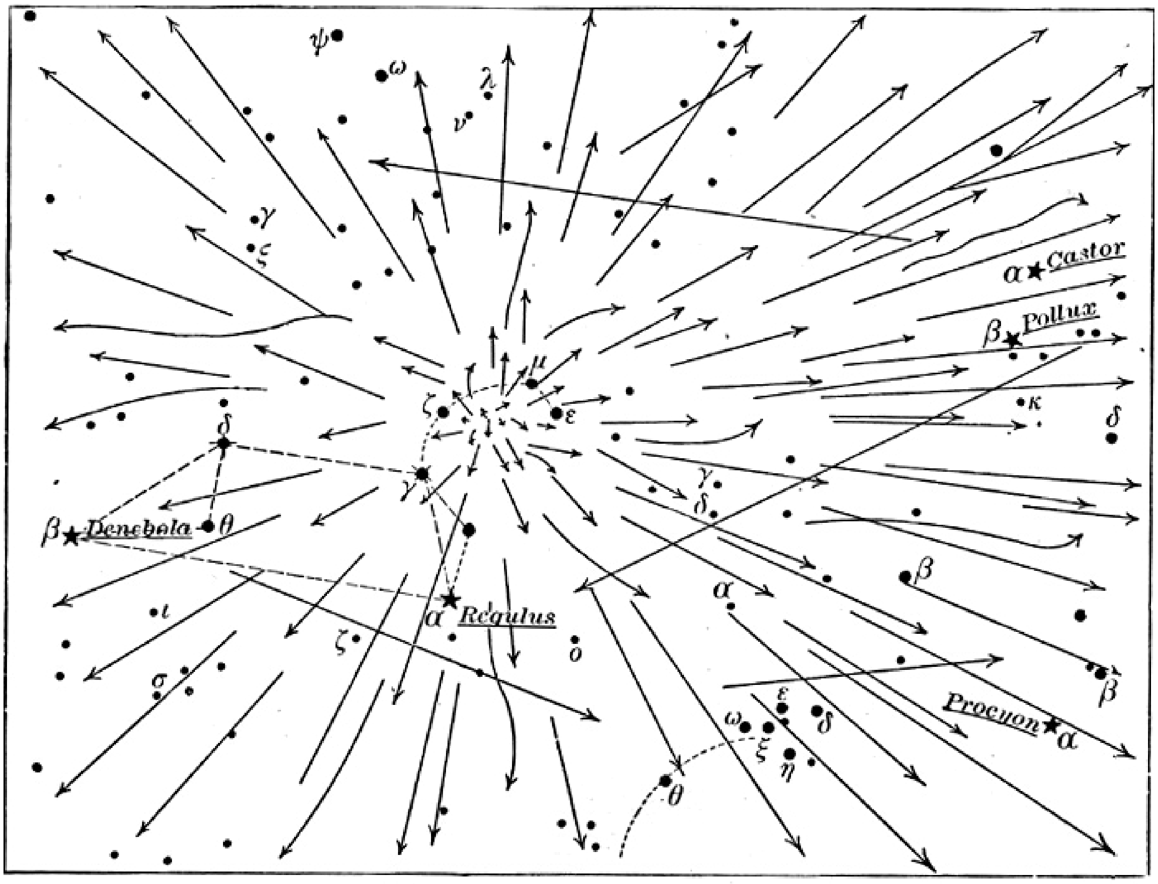

FIG. 210. — The Meteoric Radiant in Leo. Nov. 13. 1866.

Fig. 10. On part of the night of November 13, 1866 meteor tracks crossed the constellations of Leo, Gemini and Canis Minor as shown in the drawing. While some of these are clearly at random, the greatest fraction appears to emanate from a very bounded region above Regulus. This illustration appeared originally in Charles A. Young's *A Text-Book of General Astronomy for Colleges and Scientific Schools* in 1888. The phenomenon is one of geometrical perspective and the meteoric material really has been freed from Comet 1866 I Temple-Tuttle. An apparently richer display had been seen in 1833, the comet having a 33-year period.

Even without meteor showers, the sky is streaked by sporadic meteors every clear night because the Solar System is filled tenuously with dust and Earth is always orbiting through that dust. These particles also have a considerable range in size but the usual sporadic meteor is caused by something no larger than a pinhead heating the atmospheric gases as it intercepts Earth. A reasonable estimate is that about 500 tons of meteoric material falls on Earth every day. Large particles may not be consumed during atmospheric passage and will fall to Earth's land, water or ice surfaces. These meteorites may or may not be recovered and end up in museums or labs where they can be analyzed for information pertaining to the very early days of the Solar System or to a possible interstellar origin for them. The usual meteorite has arrived at Earth from an origin in the minor planet belt between Mars and Jupiter, but there are also prospected meteorites from Moon and Mars, presumably caused by impacts of minor planet meteorites on the surfaces of those planets. The present human generation has lived into a time when collections of meteorites have increased greatly because of exploration of Antarctica. *A priori*, there is no reason meteorites should have fallen only where people were around to pick them up promptly. Instead, they may indeed have fallen on Earth's icecaps eventually to be recovered in nearly pristine condition when someone walks across the ice with expectant eyes. Because these specimens are so uncontaminated, they are studied for clues about the earliest chemical reactions that occurred in the Solar System.

WIDE BINARIES
1901 [Eric, H. Evans, Aitken, Barton, Fender, Olivier, Wamer, Mason, Hammer, Knox, Binckley, Wilson, Thompson, Ambruster, Reid] 2005

In a way, there has never been a beginning to this subject because the eye of prehistoric man surely saw as two stars the object that sits at the crook of the handle of the Big Dipper. Notice of this condition for Mizar and for a few other stars first appeared in scientific literature in 1650 but nothing resulted from the announcement. Observationally, the real beginnings of the specialty are accidental, for William Herschel was intending to try to determine relative distances to groups of stars in 1775 when he was forced to conclude that two individual objects appeared to move in a way consistent with them being orbitally bound to each other. The discovery had, in fact, been predictable for more than a century for it is a direct implication of Newtonian gravity that stars could be bound at a distance from each other just as planets are bound to Sun or Moon to Earth. Herschel undertook the first systematic study of these wide or visual binaries, as they are called, shortly thereafter and interest in them has never abated.

There is a kind of a problem with the class. Consider two identical pairs of pairs of stars with the orbital dimension being many times the stellar radii. The first pair is so close to the Solar System that a modern telescope easily sees two points of light but the second is so far away that it can be perceived as only a single source. Nonetheless, it is a true visual binary, just unknown to us. There is also a question that might be considered even for the nearest binary: how widely can the two stars be separated and still be considered gravitationally bound? To this question, there is the theoretical answer that the force of gravity never vanishes but only continues to diminish according to its inverse-square law. In a practical sense, an answer would be framed so as to recognize that a wider separation means a longer period of revolution in most cases and that the unending gravitational tugs from other stars may unbind the binary. Sufficient observational persistence over centuries will certainly discover more and more binaries.

The fundamental law of binary motion was originally worked out for Mars and Sun by Johannes Kepler in 1619 and relates in a simple equation the sum of the masses of the two interacting objects, their period of mutual revolution and the average separation between them. The trick is to make observations that discover two of these parameters so that the third one can be calculated. Whereas originally positional measures were made with transits or meridian circles, eventually there was invented the filar micrometer mounted at the end of the tube of an equatorial telescope and the observer could measure the angular separation between the two stars and their orientation at any position in the sky that was convenient. Enough of these measures will show when the orbit has been completed and so the period and average separation can each be computed in, say, 1 second on a hand calculator. Of course, you don't know the real separation in useful units for two reasons. The first is that the orbit is probably not displayed to you face-on but rather at some random angle. This is not at all an insuperable difficulty for the measures that have been made can also be used to determine every apparent and true characteristic of the orbit by any of several graphical or analytical methods that date back to 1873. The second reason is more formidable: just as the linear extent of an arc of a circle cannot be known from only its angular extent, so the linear size of the average separation of the binary components cannot be known from the average angular separation. In the case of the circle, you need to know the radius as well and for the double star you need to know the stellar distance from Earth and then the calculation can be made. Unfortunately, the measures that have been accumulated by filar micrometry cannot tell you that.

The modern and efficient way to uncover this information and still accumulate the binary information was promulgated in 1924 by Frank Schlesinger. In essence, you give up making visual measures by looking through the telescope and instead you make an image of the sky field that includes the wide binary. Keep doing this until the period of the binary has elapsed and then measure through a microscope the (X,Y) coordinates of at least several stars on the images. Most of these will be very, very far away and show no change in their (X,Y) values but the wide binary, being relatively nearby, is likely to move among the background star positions. The entire track of the binary can then be analyzed to yield all the binary orbital characteristics, the rate of its motion against the background mini-constellations and its distance from the Solar System. At this point, everything needful is known and the masses of the individual stars can be calculated, again in a trifling amount of time. Most of them turn out to be more like Sun than unlike Sun. Fig. 11 shows a line drawing of the complex paths of a sample wide binary. Within the past 20 years, it has become possible to acquire all this information from Earth-orbiting spacecraft and with higher accuracy than is possible from ground.

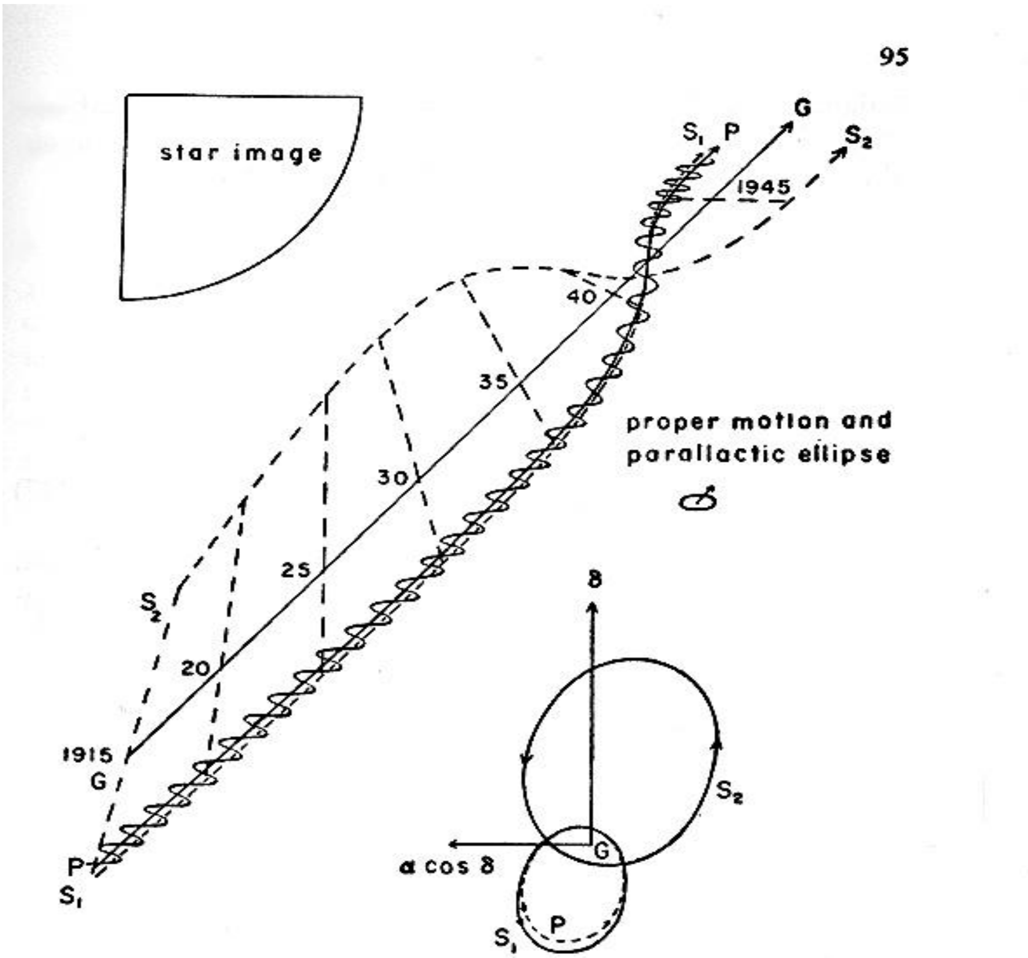

Figure 42. Observed photocentric spiral of an astrometric double (99 Her). See the size of the star image on the same scale.

Fig. 11. The straight line shows the average progress of the wide binary 99 Herculis across the sky from 1915 through 1946. The two smooth curves, one continuous and the other dashed, indicate more closely the respective paths of the more massive and less massive members of the binary but this is not the complete story. For the massive star there is shown the annual displacements from its path that are actually observed because the observer is on Earth revolving about Sun. It must be imagined that the less massive star does exactly the same thing. For scale, one quarter of the size of the photographed image of the massive star appears in the upper left corner. This illustration was originally drawn for L. Binnendijk's *Properties of Double Stars* and appears on page 95 of that 1960 volume.

It can readily be imagined that some of these wide binaries are so distant that we cannot perceive them as two sources of light. The equivalent of the micrometer measures can still be made by using an objective or eyepiece interferometer. The former is cumbersome but the latter is compact and convenient. Originally, the device was meant to be used visually and this actually has been most of its applications but it can also be adapted for limited imagery by a camera. For more than 10 years now though, very large-scale objective interferometers have been developed which have capabilities that were hitherto unimaginable by the average worker.

CLOSE BINARY STARS
1938 [Baldwin, Taylor, Alexander, Wamer, Irwin, Blitzstein, Levitt, Wood, Binnendijk, Nason, Moore, Svoloupolos, Fracastoro, Merrill, Koch, Kilmartin, Gilmore, Plavec, Dorren, many grad students, Alcock, Reid] 2005

By 1783 John Goodricke had heard not one sound nor uttered one word during his 19 years on Earth but he had discovered and publicized a new branch of astronomical science. Three years later he was dead. What Goodricke had done was, first, to observe the star Algol – a single point of light − so assiduously that he found its light to vary with a period of about 2 days 21 hours and, secondly, to offer the interpretation that the drop in light level was caused by a dark companion of the star interposing itself between Algol and the observer on Earth with that period. He had documented a stellar eclipse and set in train the study of close binary stars.

The sense of the adjective "close" is not that the stars are close to Earth but rather that the two companions are spatially close to each other – at most a few radii apart or even touching each other. These objects are not rare – tens of thousands of them are now known - and statistically we have a good feel for their characteristics and their distributions in several galaxies. Whereas Goodricke imagined that the Algol companion was totally dark, we now know that companion stars cover a wide range of brightness relative to the bright primary ones. There are binaries in which the two objects are as the peas in a pod. Because of the physical cause of the display, these objects are also known as eclipsing binaries with the two stars in mutual orbit about their center of mass and with the line of sight from the terrestrial observer being in or very close to that plane.

The measured light levels assembled graphically onto one orbital cycle picture what is called a light curve, which implicitly contains much information about the stars and the binary as a whole. The challenge is to enunciate a model that will permit one to recover this information and the first steps toward this achievement were presented in 1860 by E. C. Pickering of Harvard. Many subsequent refinements culminated in a detailed model by H. N. Russell and H. Shapley at Princeton around 1912 and this procedure was elaborated for graphical manipulation by J. E. Merrill, also of Princeton, around 1950. Since that time, numerous other workers have presented other models but since 1971 the dominant and most realistic one in a physical sense is due to R. E. Wilson and E. J. Devinney of Florida. These two men are former grad students of the University. The model returns such information as the tip of the orbit to the observer's line of sight, the brightness of one star compared to the other, the ratio of the temperatures of their radiating layers, the efficiency with which each star heats the other, the mean radii and distortions of each star, and the light contribution that a third star or some other source of light makes to the total systemic brightness. None of these results has a dimension (*e.g*., ergs/s, cm) attached to it although the inclination is given in degrees or radians as you wish. Thus, the worker gets a "relative" understanding and quantification of the binary.

It is easy to envision a binary whose orbit is so tipped with respect to the observer's line of sight that the stars do not eclipse each other. There may, however, still be changes in light level because the stars distort each other mutually; periodically their small cross sections are turned toward Earth resulting in faint light levels while again periodically the large cross sections are seen from Earth showing bright light levels. These objects go by the name of ellipsoidal binaries simply because their shapes are distorted from spheres but, in fact, they are usually distorted much more severely than a tri-axial ellipsoid. The W-D model can be applied to these light curves as well but the information content of the light curves is meager.

It is also easy to imagine that the same orbit may be progressively tipped more and more from the observer's sightline until finally no light variation is seen. However, all is far from lost in favorable cases because what should be called the Doppler-Fizeau Effect can be applied to the spectrum of the binary at any inclination. Measures of the locations of the spectral lines lead directly to at least a lower limit for the stellar orbital velocities and for this reason such objects go by the name of spectroscopic binaries. Should it happen that the binary orbit is exactly in the line of sight, the velocities so measured are the true ones. Should the orbit be somewhat inclined to the sight line, the inclination value from the light curve corrects the observed velocities to the true ones. It can be realized, therefore, that eclipsing and ellipsoidal binaries are inevitably spectroscopic binaries but that there is a subset of the latter category that varies in light not at all because the orbital inclinations are too shallow. The spectroscopic binaries give dimensioned (*e.g*., km, km/s, kg) results for the orbits and stars and these can be combined with the eclipsing modeled results to yield every stellar parameter in useful form.

At the same time, some of these objects are uncommon natural experiments. Periods of many of them are very short – just several hours. It is easy to recognize that these objects are pathological in that they represent two stars of individual energy sources that are physically connected and intermeshed. The gases of one mix directly with those of the other in flowing currents. It is confidently believed that a single daughter star can be created out of the bound parents as they get closer and closer together. Other very short period binaries are known to be natural cyclotrons because their magnetic fields are so strong and some subset of these are implicated in nova explosions. For these reasons as well, close binaries are among the objects that command very large amounts of observing time and theoretical work, and their extravagance is reasonably conveyed by the example in Fig. 12.

Fig. 12. Not a photo, this is an oil painting by M. F. Struble of the object code-named UX Mon. It appears as a single point of light telescopically in the sky but, as a result of analyzing its velocity and light variations, we know it to be double with a period of 5.9 days and we also know the stellar sizes quite well. The temperatures of the stars are indicated by color so that a red-hot object is cooler than a white-hot one. The cool star is surely variably spotted and has flares and the hot one pulsates slightly. Most of the circulating gas comes from the cool star and is heated as is passes close to the hot one before cooling again as it continues its path back toward its origin. Some of this gas is lost to interstellar space and merges with the general Milky Way environment, a small part of which appears in the background.

The real appeal of this specialty is a forceful one. With dimensioned radii and masses and luminosities of the stars, mean densities for them follow directly. Until about 1985 there was no other way to get such a mean density and even now it can be found independently only for a small fraction of stars by an independent method. Also with the dimensioned results, it is possible to confront the assorted theories of stellar aging in great detail. There should come a time when the close binary results will be so numerous, detailed and accurate that little information about the future lives of stars will be unknown. If progress is as fast as in the past 30 years, that time could arrive in this century.

INTRINSICALLY VARIABLE STARS

1795 [Rittenhouse, Olivier, Baldwin, Hammer, Cleminshaw, J. Evans, Haas, Reilly, Whitney, Taylor, Knox, Woods, Watson, Crout, Marstellers, Stevenson, Irwin, Moore, Nason, Blitzstein, Wanner, Sievers, Shaw, Avery, Koch, Koegler, Elias, Alcock, Reid] 2005

At a very, very low level Sun is currently a variable star, the word being used in the sense that its light is subject to change although not in an intuitively obvious way. There is also a very loose usage of the word even encompassing variability that is due to purely geometrical causes such as eclipsing stars. Obviously, taken to the limit this is an indefensible idea for one could then state that Sun is a variable object simply because Venus and Mercury transit its disk as seen from Earth.

Naked-eye variability of stars is well documented so it is no surprise that the total number of variables is very large and increasing quickly. The familiar modes of variability, however, are relatively few.
1-Many stars are spotted to degrees much greater than is Sun. As such a star spins on its axis, the variable spottedness may modulate the starlight by as much as 5%. Since spots do not last forever, the light modulations of these stars may be expected to abate and even vanish from time to time before growing again to a considerable level. There is a trivial case that should be recognized: if a star is absolutely uniformly spotted, even while it spins its light will not be modulated. Thus, there can be a certain number of such stars which we cannot discover by monitoring their light levels.
2-Stars pulsate, some with periods that are as brief as a few minutes and light ranges that are as small as 1% and continuously improving monitoring programs now find still smaller ranges of variability. Other stars pulsate in periods from hours up to scores of days and some of them have light ranges that are very small while others double their light output during their pulsations. The first of these was discovered by Goodricke and the second by a friend of his. These objects are all larger in radius than Sun and some of them are justifiably called supergiants. Because these variables are large and despite the fact that many are cool, they can be seen to great distances and are of cosmological importance. Many of these variables either have constant periods of pulsation or their periods change slowly for reasons that are reasonably well understood. In the 20th century it was finally discovered that some pulsating stars have more than one pulsational period active in themselves. There might, for instance, be a case in which a star pulsates with a peak-to-peak modulation of, say, 2% in 3.005 hours and simultaneously endures a 1.5% modulation in 3.012 hours. Disentangling these separate pulsations can be a considerable observational and computational challenge and finding a plausible explanation for the multiple pulsations even more of a task. Understanding these phenomena means reckoning with the structure of a star's invisible interior and its nuclear and atomic chemistry.
3-Between the extremes of the pulsational variables just described there are many stars which are warm to cool in temperature but which vary in only a semi-regular way. Many of them have large ranges of light variability and so the brightest of them have been known for a long time.
4-Explosive variables are either recurrent, as for ordinary novae, or single-shot explosions which result in the displays of supernovae.
5-Lastly, there is a zoo of minor types of variability much too lengthy to describe or even to list.

There is no reason why double star members cannot be intrinsically variable as well and it is abundantly documented that many wide and close binaries do contain very spotted stars and others contain low-level pulsating variables. It is also true that many close binaries will pass through repetitive nova events or the supernova explosion. One believable generality is that all stars will pass through multiple stages of variability and a second is that every variability stage has a finite duration.

SOLAR AND STELLAR ENVELOPES
1938 [Levitt, Fecker, Wood, Avery, Friedman, Wyller, Fay, Yun, Koch, Blitzstein, Holenstein, Pfeiffer] 1998

With the structure of Sun's outer layers known to be divided into the successively higher photosphere, chromosphere and corona (a fine image of part of which appears in Fig. 13), it would be reasonable to

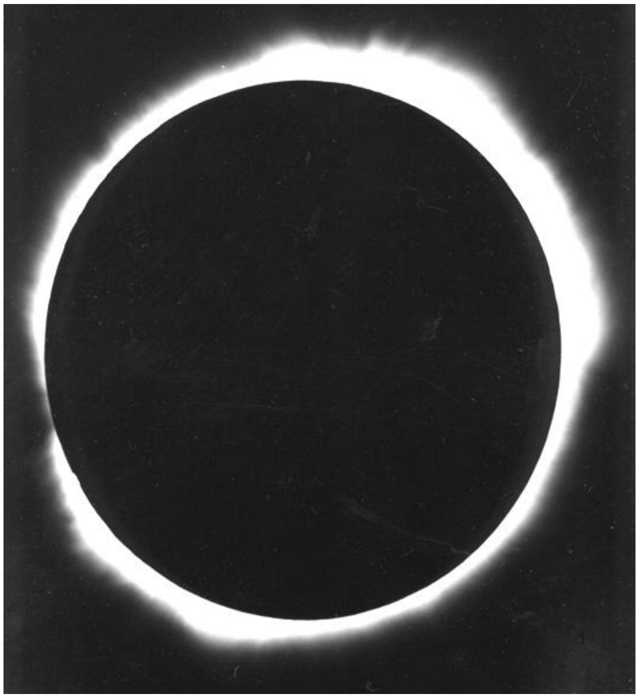

Fig. 13. This photo was taken at the March 7, 1970 solar eclipse from the vicinity of Wallops Island, VA by P. M. Perry. The inner corona and its non-uniformity are apparent but the exposure was too brief to image the outer corona. Each Sun-like star has an envelope such as this.

suppose that at least Sun-like stars are structured in the same way. Possibly stars very different from Sun in temperature and mass might have differently structured envelopes but at least they ought not to have a sharp edge at the interface with interstellar space. At the turn into the 20th century almost no information pertaining to these matters was known. One tool invented early in the century was the HRD (and later the CMD) for the Hertzsprung-Russell Diagram (and the Color-Magnitude Diagram). These diagrams represent the relationship between photospheric temperature and luminosity in somewhat different ways but always show a pattern and not a random distribution when the luminosities are corrected as if all the stars were at a unique distance from Earth. If the stars are brand new, the pattern is called a Zero Age Main Sequence (ZAMS) whereas, if their core nuclear reactions have consumed a considerable amount of their birth budget of hydrogen, the pattern is a Terminal Age Main Sequence (TAMS).

Three milestones have marked the understanding of solar and stellar envelopes: in 1929 Russell estimated the first credible chemical model for what came to be called the solar atmosphere; in 1931 M. Minnaert and C. Slob replaced Russell's estimates by measures and potentially placed all stellar atmospheres on a solid atomic physical platform; and in 1941 Bengt Strömgren refined the older quantitative analysis greatly. During the following two decades many high quality spectra were accumulated and analyzed so that the correlations among temperature, pressure and chemical species became well understood. There also appeared significant exceptions to the usual descriptions of stellar envelopes. For instance, it became clear that not every star had an extensive chromosphere and that the envelopes of close binary components could be very different from those of single stars. In the early 1960s computers began to be able to work with diagnostic physical tools much more powerful than Strömgren had been capable of doing and his methodology came to be downgraded, so to speak, as a coarse analysis. But even his methodology, applied to visual binaries and to close binaries whose stars did not come close to touching each other, had shown that their envelopes were similar to those of single stars.

Because there are so many stars that are Sun-like, study of our own star has continued without letup. Indeed, it is possible to say that the study of just the solar photosphere is more active and more searching than ever before.

Already in the 1950s the first serious attempts had been made to deal with the envelopes of supergiants that were members of binaries with periods of about 1,000 days. It was instantly clear that these monster stars' envelopes were not much denser than interstellar gas. As their companions were eclipsed by such supergiants, the diminution of the companion light dropped off very gradually rather than "sharply" as for the usual eclipsing binary. This meant that it would be possible to look for clouds in these very tenuous envelopes and to see the way in which they were dynamically active. In the late 1940s F. B. Wood and Franklin Roach demonstrated that you could even make measures photometrically rather than spectroscopically that would demonstrate these atmospheric eclipses, as they came to be called.

Around 1965 a new development began with the first Earth-orbiting spacecraft – manned and unmanned – equipped with simple spectroscopes. These observations instantly offered confirmation of some ground-based information known for a long time and expanded on it. Hot, bright and massive stars are continuously blowing off their tenuous envelopes at speeds of 1,000, 2,000, even 3,000 km/s. These speeds are so great that the matter never returns to the stars but is lost to interstellar space. The phenomenon had been known for nova and supernova explosions from much earlier in the century but here it was being displayed in nominally stable stars. Stellar winds became a fact of life. Sketches of the extent and non-uniformity for three of them are shown in Fig. 14. The expanding envelopes are so massive that these stars

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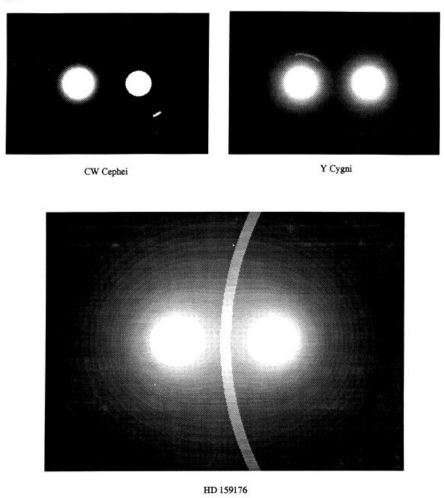


Figure 7.3.2: A schematic, pole-on view of three binaries at wind-line wavelengths, where the photospheric disks appear embedded in a binary wind 'halo'. The locations of the 'clouds' for CW Cep and for Y Cyg and the shock wind-interface layer for HD 159176 are also shown. The sense of revolution is in the counter-clockwise direction.

Fig. 14. Spacecraft measures led to the models of the expanding winds of these three binaries in the 1996 doctoral dissertation of I. Pachoulakis. For CW Cep (upper left) the winds are puny yet it was possible to detect a dense cloud in one of them. The winds of Y Cyg (upper right) are a bit more extended as is shown by the fuzzy rings around the two stars and in one of the shells there is again a denser-than-usual cloud of expanding material. For HD 159176 (lower center) the winds are very well developed and so fast and dense that a standing shock front exists where they collide. The terminal velocities are about 400, 1,380, and 2,800 km/s for these binaries in the order that they have been described. The flows do not just escape the stars but are supersonic. Very luminous stars support envelopes such as these.

are capable of sending into space the equivalent of an entire Sun while still shining in a stable manner. New diagnostics had to be developed for these expanding envelopes and this was done initially by several workers over a brief interval of time but further development continues today. The same spacecraft instruments were then directed to cool stars and another instant discovery resulted: such a star loses matter in a slow but massive wind over a long time as well. A generalized return of stellar matter to the interstellar gas was thus documented and, since the stars had originally condensed from the interstellar matter, the process could be viewed as cyclical although not conservative. Successive generations of stars can meaningfully be imagined and in a way that countenances more and more chemical enrichment of the successive stellar generations.

This process is one that can realistically be described as *stellar evolution* in the sense that successive generations of stars – each generation differing from the previous ones in nuclear and atomic makeups - have developed in a changing interstellar environment. The same term of *stellar evolution* has also a second less satisfactory usage because it is also commonly applied to the successive stages of a star's life. While it is true that an individual star does change in a nuclear sense throughout its life, consistency with the word *evolution* in the older biological sense suggests that the latter usage should be foregone. It is not to be expected that such a level of linguistic purity will ever happen.

NUCLEAR REACTIONS IN STARS
1940 [Taylor, Olivier, Davis, Lande] 1998

From about 1850 through 1930 one after another of possible ways to power Sun were found wanting. The major problem was that the suggested mechanisms could be shown to be too short lived for what was the accepted age of Earth and it was impossible to accept the idea that Earth had existed before Sun. The appeal of being able to solve this conundrum lay in the circumstance that, if you could figure out how Sun worked, you would also have understood how an immense number of similar stars generated their power. The start toward the eventual breakthrough inadvertently came in 1896 when Henri Becquerel discovered one variety of radioactive decay following the discovery of X-rays by Wilhelm Röntgen the preceding year. Further experiments clarified the structure of the atom and identified the electron, proton and in 1932 the neutron and developed the concept and quantification of the binding energy of an atomic nucleus. What was needed was identification of some almost inexhaustible energy source and, from the results of Russell, Minnaert and Slob, it had become understood that Sun was primarily hydrogen so it was natural to think of that nucleus as the significant fuel.

In the laboratory, nuclear reactions had already been initiated by man on a very small scale but stars had to generate net power rather than consume it. Some sequence of nuclear reactions, maybe a cyclical set, had to be discovered. Then in 1938 Hans Bethe and C. H. Critchfield put together a sequence of reactions built on hydrogen nuclei that would apparently account for power production in the core of Sun. In time, it was discovered that an alternative set of reactions also burning hydrogen and catalyzed by carbon would work for more massive stars.

Two directions of further research burgeoned after about 1960: attempts to discover the reactions that power stars as they age and then refinements to the solar reactions and observational discoveries of these refinements. The first of these has moved from one success to another so that we now confidently apply known reactions to stars at every stage of their lives and even agree that nuclear reactions occurring on the "surfaces" of some very old stars ignite what we term the nova events. This sequence of understandings has culminated in the recognition that it is in the stellar cores that all but the lightest atomic nuclei are actually made and that the chemical elements (*i.e*., the nuclei combined with the appropriate number of electrons) can be made when these nuclei are dispersed into space. With regard to solar energy generation, the initial reaction sequence had to be modified after it was realized that it was too simple and that further branch steps would have to be reckoned with. In order to test this entire model, attention was turned to sub-atomic particles called neutrinos and anti-neutrinos. Their flux from Sun could be predicted on the basis of each model and large-scale and long-lasting unconventional particle-trapping telescopes were set up beginning in 1965. The results could only be interpreted convincingly when it was recognized that the theories of these particles were incomplete and had to be improved. This having been done, observed solar power generation became consistent with the matter and power-propagation models of the star. Even as this success was approaching, an unexpected additional success happened with the appearance of the brightest supernova since the 17th century and the detection of the neutrino flux from the explosion by a subterranean Japanese neutrino telescope.

Eventually the budget of hydrogen in a star must be consumed or nearly so. What will happen at that time? One might imagine that in a self-sustaining system, the products of the former nuclear reactions will become a new fuel to sustain the star. Attempts to understand processes such as these started with the first investigations of how helium might fuse with itself to create new energy and new fusion products. That effort, begun more than 50 years ago, continues to this day as new reactions with ever-heavier nuclei are invoked to explain one phenomenon or another observed as stars age.

While the foregoing description implies that the all nuclear reactions take place in the deep interiors of stars, this is not always the case. As indicated above, in some very tight and old close binaries, matter captured from a donor star by a white dwarf gainer will be so dense and moving at such high speeds toward the gainer that nuclear reactions do occur at the impact location on the gainer.

THE MILKY WAY GALAXY
1939 [Menon, Fang, Kilambi, Perry, Wanner, Sobieski, Friedman, Koch, Langer, Rivolo, Alcock, Devlin, Reid] 2004

There was once a time when, in cosmically large agglomerations of matter, attention was paid only to stars. So even though the unaided eye and visual viewing through a small telescope show evidence of interstellar matter convincingly but little was made of these recognitions. That day ended in 1904 when it had to be admitted that gas exists between the stars and was followed in the 1930s by the recognition that thinly-dispersed solid particles also exist in the interstellar volume. The gas will selectively absorb radiation from sources behind it as seen from Earth and emit radiation of its own. The dust would typically scatter background radiation from other sources and also emit radiation appropriate to its temperature.

The identity of the gas was first established as both neutral atomic and ionized and finally gas in the molecular form was identified also. The dust particles are microscopic in size and probably have a variety of shapes but at least some of them are needle-like. Each of these ingredients may exist in discrete and rather sharply bounded clouds or in extended amorphous distributions or in great, massive clumps that are known as Giant Molecular Clouds. It is confidently believed that all stars are born out of the interstellar gas and dust structures.

The gas structures have offered another piece of information – they are exceedingly crisp indicators of the structure of the plane of the Milky Way Galaxy and also of fast-moving clouds above the central Milky Way plane. The spiral structure of the Galaxy was initially discovered by radio telescopes mapping the distribution of neutral hydrogen and it has been found that the GMCs are equally expressive markers of the spiral arms.

UNUSUAL GALAXIES AND CLUSTERS OF GALAXIES
1961 [Sobieski, Shen, Friedman, Johnson, Ambruster, Ftaclas, Rivolo, Eskridge, Struble, Devlin] 2004

For decades astronomers in general had become accustomed to the morphological classifications imposed on galaxies by Edwin Hubble in 1938. Exceptions to his scheme could be lumped into his catchall category of Irregulars or interpreted as a relatively nearby galaxy of one type accidentally superimposed on a more distant one of another type just happening to be very nearly in the same line of sight.

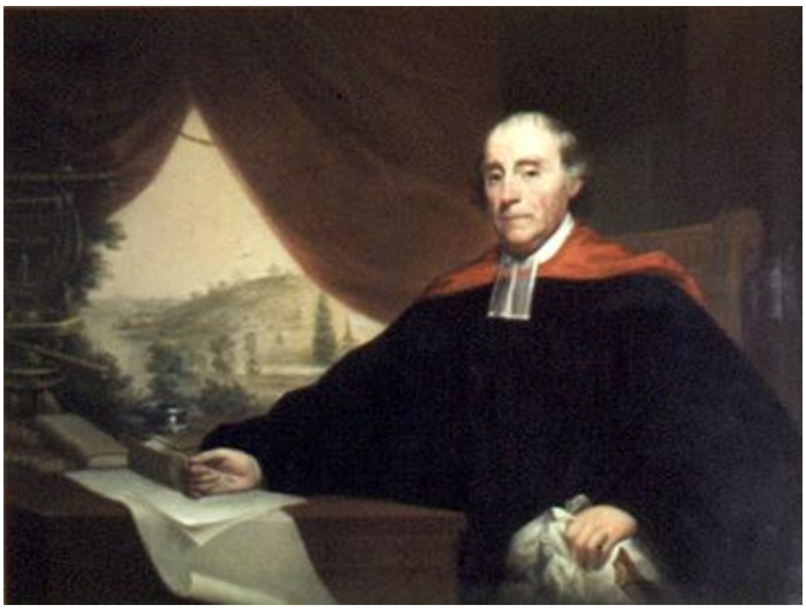
This changed significantly when, after World War II, more and more powerful radio telescopes came on line and were turned toward extragalactic targets. There first emerged detections of objects with unexpectedly large levels of radio power and these were quickly found to have curious shapes when they were imaged with visible light. Quasars were discovered in 1964 and emphasis began to be placed also on known galaxies which had uncommon levels of visible radiation concentrated in their nuclei. These came to be known by various names but they seem to form a continuum of emission and variability and all are now believed to harbor supermassive black holes that consume the gas and stars that pass through their event horizons.

By 1960, clusters of galaxies also began to assume the cosmological emphasis that is now commonplace. With world models more abundantly detailed than seemed possible hitherto, the structure, stability and evolution of these clusters and the relations of the member galaxies to the hot intergalactic gas continue to be matters of great interest. Since telescopes became larger, detectors more efficient and observations of all kinds more numerous and precise, it became possible to probe these clusters for more subtle clues about their pasts.

EARLY DAYS

UP TO 1800

Meyerson and Winegrad (1978) trace the evolution of the 1749 Academy and Charitable School of the Province of Pennsylvania into the chartered College of Philadelphia in 1755. In 1779 the College morphed into the University of the State of Pennsylvania and in 1791dropped the reference to the state. The guiding agent for the early events and for the non-sectarian (or maybe the word should be ecumenical) character of the school was, of course, Benjamin Franklin (1706-1790). Parts of two of the next four paragraphs can be traced to Meyerson and Winegrad. The remainder of this section is partly drawn from notes by Samuel G. Barton that I have edited.



In the 3-year curriculum developed by the first Provost (then the title of the top officer of the school), Rev. William Smith (1727-1803), astronomical instruction appears in the third academic year along with study of other sciences. Smith was a worldly clergyman with apparently fierce opinions, such as doing in the Pennsylvania Quakers when they would not take vigorous measures to defend the western and central part of the colony against raids during the French and Indian War. Not too long thereafter, he was caught in the political middle not wishing to give up allegiance to the British crown. In a way, he was a man about 200 years ahead of his time. When the British occupied Philadelphia, he made no attempt to distance himself from that army, his sympathies not being obviously with the rebels. Some years later, this was remembered against him and he was forced out of the University – a nice example of political correctness that would reappear in the institution during World War I. What did Smith do? He took himself off to Maryland to found another educational institution and took the University to court just as any aggrieved person would do in the present day. A level of reconciliation and recompense eventually did follow and he passed the remainder of his life in relatively placid contrast to what had happened before.

It may be believed that the studies that Smith mandated were at a meaningful level since differential calculus and natural philosophy were taught in the second year. For the academic year beginning in 1751, Theophilus Grew (?-1759) – a sometime silkstuffs merchant and tutor of Franklin's children – was appointed Academy Master "to teach writing, arithmetic, merchant's accounts, algebra, astronomy, navigation and all other branches of mathematics." Grew may have been in the line of the following men:

(1) Obadiah Grew (1607-1688), DD Oxon and a Nonconforming clergyman imprisoned for his beliefs and practices at age 75 and the uncle of Jonathan Grew (1626-1711), another Presbyterian minister, was the father of
(2) Nehemiah Grew (1641-1712), FRS and an M.D. from Leyden. He was part natural philosopher in the old style and part new-type systematic scientist. He is quite visible in the *Philosophical Transactions of the Royal Society, London*, which he edited briefly and wherein his books and lectures are reviewed. His and Marcello Malphigi's work on plant structure established this study

as a modern branch of science and he published as well on human physiology and the correct land area of England. He sustained a medical practice all his life, and at his death he left a widow, two daughters and a son (whose name is unknown). Shower's (1712) eulogy, on the other hand, did not mention any survivors.

Then there is the equivocal evidence of the New Testament name itself:

(1) A Theophilus Grew was married to Elizabeth Barrine in St. Dunstan's-in-the-East, Stepney, London on October 18, 1711 (Julian Calendar).

(2) Our Theophilus Grew witnessed a will in Philadelphia in 1729, ran a private school in the same city that was advertised in *Poor Richard's Almanack*, became a silk merchant, was imprisoned for debt in 1740, and was married on February 9, 1735 (to Eliz. A. Cosins) and on March 5, 1739 (to Frances Bowen) each time in Christ Church, Philadelphia and to Rebecca Richards in 1747 at some unknown place. Again, these are Julian Calendar dates.

If our Theophilus was the son of Nehemiah and if there was only one Theophilus, he was certainly the marrying kind all his life. If there were two Theophiluses, father and son, our man would have been quite young at the time of witnessing the will. Possibly this tissue of hypotheses is all imaginary and there were still more people, presently unknown to me, of the Grew surname or possibly the three older men were not at all related to any Theophilus. That given name was not uncommon and in itself need not be diagnostic of a religious dynasty: around 1700, Theophilus Shelton was a Yorkshire amateur astronomer, Theophilus Oglethorpe was an acquaintance of Nehemiah, and Theophilus was the middle name of two British notables named Desagulers and all of these men had technological or scientific interests. Some of the preceding information comes from LeFanu (1990) and some from scanning the *Phil Trans* in the *JSTOR* database. This source also shows that no Theophilus Grew published anything in that journal.

Our Theophilus published not only under his own name but also as *T__G__*, *Poor Tom* and with the anagram *Wreg*. He was very well known in the middle colonies, having been a consultant to the Pennsylvania colony in the Pennsylvania/Maryland and Pennsylvania/Delaware boundary controversies as well as founding in 1740 the Free school of Kent County, Maryland. The Historical Society of Kent County actually has no evidence of this and Grew does not appear in Wright (1982). In 1746 he had published "*Grew's Tables of the Sun and Moon Fitted to the Meridian of Philadelphia*". With these he then calculated the parameters of the partial solar eclipse of January 7, 1749 (Julian Calendar) and that for Moon on May 28, 1751 (Julian Calendar) and solicited observations that would be made by any observer. By 1755 he was Mathematical Professor. His other accomplishments include almanacs for New York City, Annapolis, and Williamsburg, the Barbados Almanac for 1752, and a very nice treatise on the use of terrestrial and celestial globes. This volume poses and works out numerous problems in plane and spherical geometry using globes. It is not known if he made any measures himself but he is the first person associated with any form of the University known to have been interested in celestial observation.

One may imagine Grew in front of his students on the first day of term. "Gentlemen, I hope very much that you will apply yourselves to these studies which are both practical and heavenly. There may come a time when it is useful to you, perhaps even a matter of life, that you would be able to know where you are on Earth and which direction you want to go for safety. In our mastery of the globes it will become apparent to you how this may be done and my vanity permits me to say that I address these matters to you in a way and with a background that is not possible for any other tutor in His Majesty's American domains. You will eventually become acquainted with the magnificent *Uranometria* of Bayer published almost 150 years ago and therein you will find a constellation *Grus*, most of which can be seen from our city during the dark nights of summer. No doubt, this means nothing directly to you but your classical training will already have permitted you to turn this name into the English word *crane*. I tell you that Roger le Grue lived in Somerset in 1230 and Gerard la Grue in Yorkshire in 1246. Each of these names is from an old form of French and refers to the European crane, the only variety of the bird that was common in Europe in Bayer's time and that remains common there now. These two Anglo-Normans must each have had a tall, lanky ancestor or were themselves of that physique. By 1379, Johannes, Joanna and Agnes Grewe (among others) had all anglicized the name with the articles disappearing and the old English *w* supplanting the detested foreign *u*. Through the ensuing centuries the final *e* was dropped from some versions of the name as in my own. So I myself have a personalized affiliation with the heavens that I am quite sure can be claimed by no one else on this side of the ocean. Who better to teach you this subject?" I made up this silliness but the facts of person and date that appear in the supposed quotation may be verified in Reaney

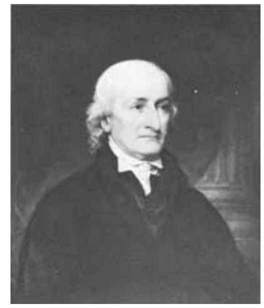
(1958). For a reason that I don't understand, Bardsley (1901) refers *grue* to the greyhound despite the fact that 115 grues appear on a menu for Henry III in 1250 and the English have never favored any preparation of dog.

At its first commencement in 1757 the College awarded Grew an honorary M.A. His probable headstone is shown in Fig. 15 and itself conveys doubt that the College Theophilus was really in the line of Obadiah and

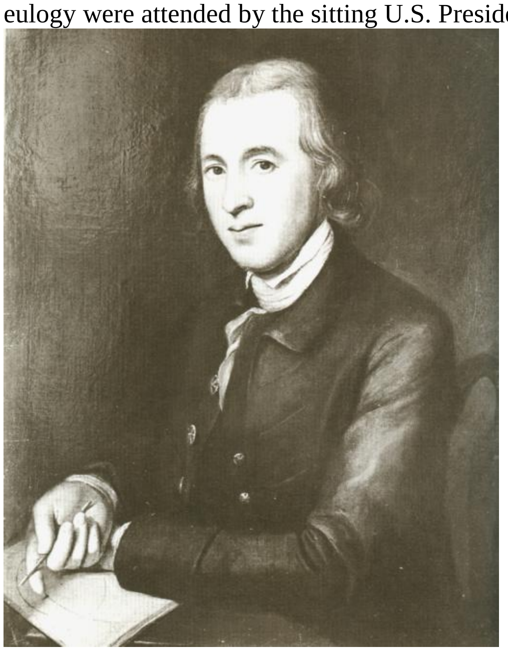
Fig. 15. The end of the beginning of astronomy at the University - the likely sunken headstone for Theophilus Grew in Christ Church Graveyard, Philadelphia. The inscription, effaced and buried, once read in part: "He distinguished himself in Life by many exemplary virtues and many valuable Qualifications. He was very deeply learned in Astronomy and Mathematics whereby He rendered himself a most useful Member of society. He served as Professor of these noble sciences in the College of this City. He discharged the trust with honor and integrity."

Nehemiah. At the time of the marriage of the one Theophilus, St. Dunstan's was in the jurisdiction of the Bishop of London and so the ceremony was an Anglican one. The two older Grews remained Nonconformists all their lives but the grave of Theophilus is in what was then an Anglican burial ground. By that time, a Philadelphia Presbyterian congregation was well established and was even represented by the University's eventual second Provost so Grew could well have joined that parish without censure or penalty if he had not wished to depart from the Nonconforming tradition. This religious association may, of course, alternately be reconstructed to mean that our Theophilus was really the man married in St. Dunstan's and that he had emigrated between 1711 and 1729.

After a brief difficulty replacing Grew at his death, a non-practicing Persbyterian clergyman, Rev. Hugh Williamson (1735-1819) (or one time Williams), filled the position from1761 to 1763 and then married a Grew daughter the next year. A graduate of the first class of 1757 with his hand in theology, politics, education and very successfully in military medicine, he was apparently interested in the bright, nearly parabolic comet of 1769 (Williamson 1771) and worked on a committee to prepare for the transits of Mercury and Venus in 1769. In the same paper in which he presented his essay on comets, he also offered some ideas concerning the origin and nature of heat. While his ideas are not outrageous for his time, they are founded on his certainty that there were long-lived Cometarians who were not rational beings. Williamson was always inclined to be convinced by the last argument which he heard in any discussion so that a French diplomat reported: "Il est difficile de bien connoître son caractère; il est même possible qu'l n'en ait pas." This didn't prevent a second marriage when he was 54 and is at odds with his forceful and principled performances in the NC colonial legislature, the Continental Congress and the Constitutional Convention to say nothing about his arguments with British General Lord Conwallis. Williamson had insightful understanding of treating and preventing medical problems among large numbers of people in order to minimize contagion from rampant infection and he put this to work after the American defeat at the Battle of Camden. After that event, he voluntarily entered the British lines to care for the Continental Army prisoners insisting that proper hygiene and diet were the keys to preventing the deaths of many of them. His argument won the day after he convinced Gen. Cornwallis that any epidemic taking off his prisoners was sure to infect the British troops as well. It is not clear how Williamson came to his understanding of these causes and effects but possibly his medical training at the University of Utrecht was at the base of it. Obviously, this man was a remarkable personality whose memorable achievements are not in science but in public service. He also had a hand in the establishment of the University of North Carolina.



Over the next 20 years, three Pennsylvania personalities (all three of them College Provosts in one form or another at different times) remained enmeshed with each other but far and away the most significant of the three was David Rittenhouse (1732-1796). He is surely the only astronomer whose memorial service and eulogy were attended by the sitting U.S. President and his lady and major delegations from both houses of Congress. This nearly self-educated, insightful scientist had been known for years as a self-employed maker of clocks and other mechanical instruments as well as optical devices but the event that made his name on an international scale was the transit of Venus on June 3, 1769. One of the most provocative tales is told of his childhood when he is purported to have written or carved mathematical problems and equations into the handles of a plow so that he could ponder them as he did his chores. The appeal of transits for 18^{th} – and even 19^{th} − century science in order to scale the Solar System has been described above and treated recently, *e.g.*, by Sellers (2001) and Teets (2003), and even in 2004 still two more accounts were published. The institutional agent for observing the 1769 transit was not the College but the APS, one of whose members was Smith. He asked Colonial Proprietor Thomas Penn for a Gregorian reflector with an attached filar micrometer so that he and Rittenhouse might observe the phenomenon from the Rittenhouse farm northeast of Philadelphia, and for a number of reasons Penn came across with the money for the instruments. All stories about him emphasize that all his life he either had fragile health or suffered from a chronic and painful abdominal ailment of unclear nature. But in an astonishing display of concentrated work, Rittenhouse (1771) calculated the timings of the tangencies of the disks of Sun and Venus and built a clock and two refracting telescopes and an observing cabin with a sliding roof opening. After considering all evidence known to him, Milham (1937) concluded that this was the first structured astronomical observatory purpose-built in North America. Whether this statement is correct depends on whether similar buildings had ever been constructed in French North America or Spanish Florida. Milham doesn't concern himself with these possibilities. The image copied from Fig. 4 of Davis (1896) into Fig. 16 is purported to be this observing structure.



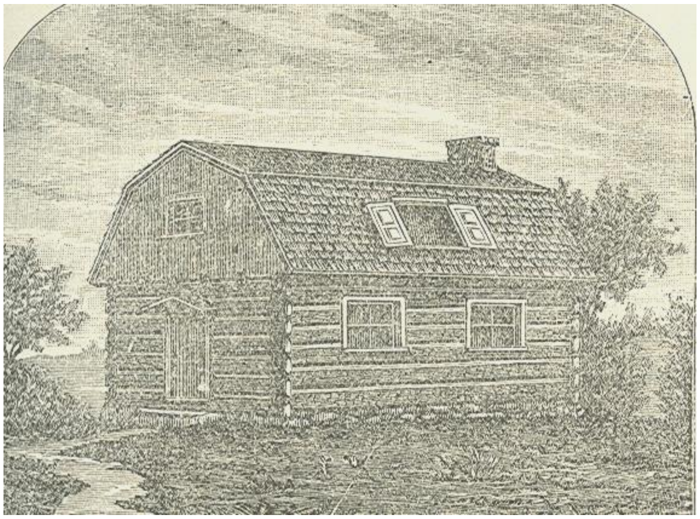
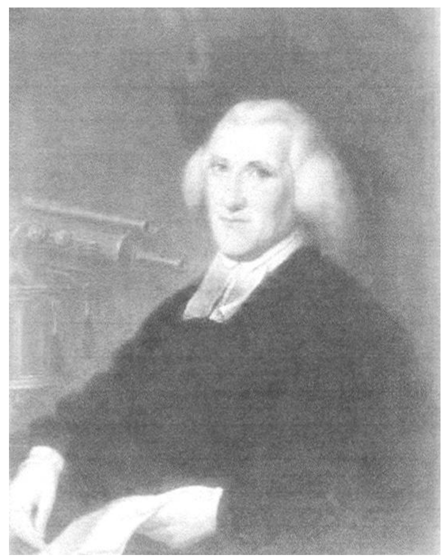


Fig. 16. If this image really represents the observing building built by Rittenhouse, it had an earthen floor. To me, it seems as if the roof windows are not large enough to show the extent of sky that would agree with the length of the observing interval which Smith reports.

Smith's enterprise was, in fact, a reaction to the opportunism of the Rev. John Ewing (1732-1802), his deputy and eventual successor at the College (and at whose church Benjamin Rush presented Rittenhouse's eulogy with Smith sitting in a pew.) Ewing had grown up near the Mason-Dixon Line and may even have crossed it daily when walking to and from school. He had apparently been interested in the natural world since adolescence. Before Smith's request to Penn, Ewing had already caused the APS to ask for funds from the Pennsylvania Assembly to buy a micrometer and install it on a telescopic platform from which he might observe the transit on the grounds of what is now Independence National Park. His observing station was completely exposed to the air – no walls and no roof. Smith and Ewing had almost nothing in common − Scot *vs*. native colonial, Anglican *vs*. Presbyterian, Tory loyalist *vs*. patriot, superior *vs*. subordinate. The only historical detail uniting them is that they are the first two known celestial observers from the College and one might stretch facts to say that Ewing's and Smith's observatories were the first and second, respectively, with which the College had any association. The only personal character trait that they shared was a very high level of self-regard, at least in matters of natural science.



Each team had clear weather for the ingress phases and tracked the march of the planet across the solar disk until near sunset. The two final contacts could not be observed from eastern North America. The observing interval was physically heroic for Rittenhouse, Ewing and a third observer using the second new telescope built by Rittenhouse. Because ingress occurred near 2 PM local time, Sun was high in the late spring sky and all except Smith had to lie supine in order to view the phenomena. Williamson is said to have supported Ewing's head during the measures but this is unlikely since Williamson was observing at another telescope near Ewing's. Rittenhouse's brother-in-law did actually offer that convenience to him. Despite their vocations, neither Smith nor Ewing lived by the precept that the meek shall inherit the earth and controversy inevitably was associated with priority of publication in London. Smith, *et al.* (1769) achieved this priority with Smith seemingly not telling Ewing of his intentions. Their separate accounts of the transit, quite nicely self-critical and showing awareness of both random and systematic errors, did appear simultaneously in Philadelphia (Ewing 1771a, Smith 1771a), wherein both Ewing and Smith (1771b) absurdly calculated values of the horizontal solar parallax to 4 decimal places: 8.6838″(Ewing) and 8.8715″(Smith). None too astutely each then patched together internal and external contact times from their own and different stations to make additional determinations of this constant without having a real grasp of the errors of timing. The scatter among these values is very large.

More than a century later, Newcomb (1890) handled this transit and that of 1761 in a more experienced way but was still partly at sea about what to do. Eventually, he settled on a model that he believed defensible and examined the dozens of data sets. That of Rittenhouse he found wanting. The problem rested in the fact that the description by Rittenhouse was very detailed and described phenomena that other observers didn't report. Inevitably, this data set really didn't fit Newcomb's model and came out with a very large residual from the model. Newcomb's critique gave emphasis to the belief that physical and emotional fatigue caused the observer to miss the phenomena because he had fainted. My belief is that this interpretation is wrong for two reasons: the detail reported by Rittenhouse gives every evidence of complete attention to the moving image and his was not a personality that would fail to include personal information if he felt it important for observational integrity. Within errors, Newcomb's value of 8.79″ is the same as the modern 8.79418″ obtained much more precisely by other techniques. In a way, this agreement is surprising because of the conspicuous systematic trends in the data sets. For instance, the second contact of 1769 shows negative residuals from essentially all the numerous stations which saw that phenomenon between 8 hr Local Mean Time and 0 hr Local Mean Time and even beyond to the final station at Tahiti. The curiosity that the mean of Smith's and Ewing's values is very nearly correct must be an accident of small-number calculation.

Rittenhouse next concentrated all his talents into finishing fabrication of two orreries, the second one for the College (shown in part in Fig. 17) and the first for The College of New Jersey (*i.e.*, now Princeton University). His intention was to display the phenomena of the then known objects of the Solar System as seen from the north pole of the ecliptic with a runout error of no more than ±1° after 5,000 years into the

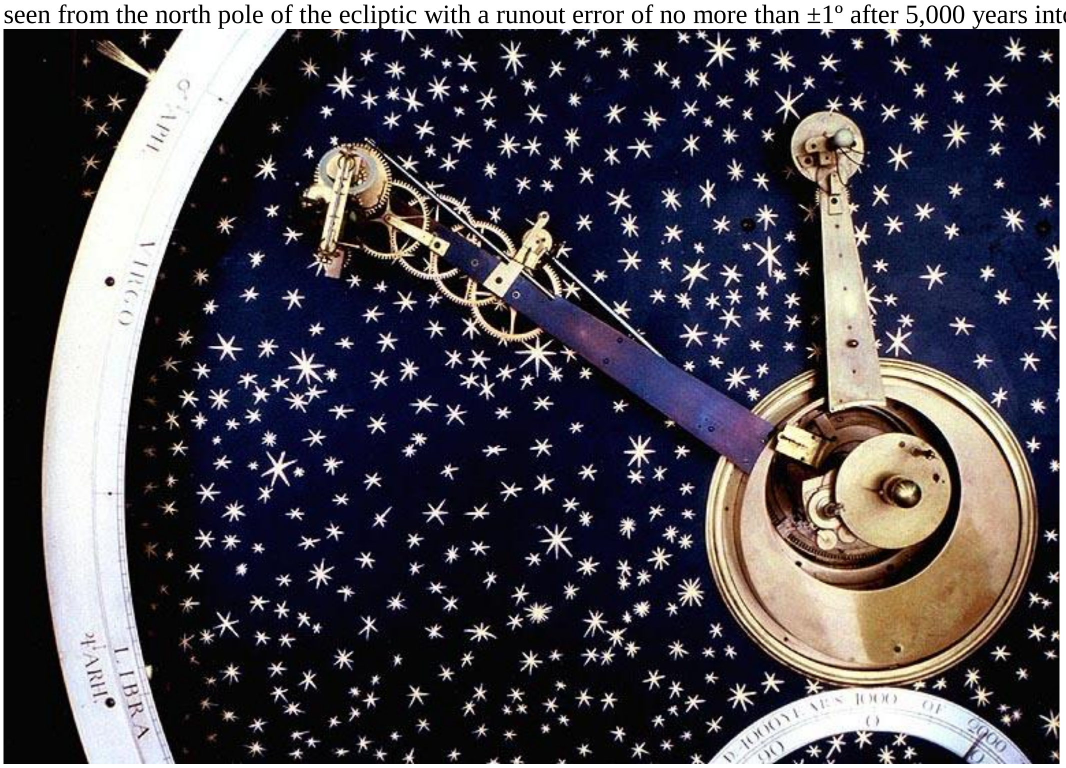


Fig. 17. A view of a portion of one Rittenhouse orrery – it's really a partial mechanical almanac. The steel ring engraved with the months and constellations and the longitudes of assorted orbital nodes is about 2 inches in width while the display inside this ring has a diameter of about 42 inches. Rittenhouse himself did not make the mahogany case which actually cost him 60% of his fee. The model for Saturn is directed to about 10 o'clock while that for Jupiter is aimed to almost 12 o'clock. Earth and Moon are just barely visible at the edge of the small brass disk to the right. The background star pattern is idealized.

past or future. From even a 18th century point of view, this has to be understood to be an unreasonable expectation. In the case of Moon, for example, that intended runout error translates into a mechanical error in initial angular position of the lunar gear train of no more than 0.06″ and on a 6-inch gear, for example, this would mean a locating tolerance of something better than 0.000001-in. Then there is the inevitable non-uniformity built into cutting the gears themselves. Another example concerns the effect of Jupiter's oblateness on the advance of the perijove of Io; that rate is of the order of 900º per terrestrial year. Nonetheless and although the devices are somewhat incomplete, they are magnificent pieces of scientific furniture and Rittenhouse's reputation became very high. During the British occupation of Philadelphia and at Smith's instigation Gen. William Howe caused the first orrery to be locked up and protected lest it be damaged by his inquisitive soldiers. Rittenhouse and Ewing became Trustees in 1779 but both resigned to become Vice-Provost and Provost, respectively, shortly thereafter. In those positions they gave astronomical instruction but Rittenhouse (the first Professor of Astronomy) resigned his appointment in 1782 because of his distaste for teaching – apparently he could not declaim in a commanding manner.

It would be interesting to know if Rittenhouse ever used either orrery as a teaching aid. The evidence available to me is that the machines were quickly recognized as anachronisms and lack of maintenance caused them to become derelict. This condition continued for the specimen that remained at the University until recent time when a concerted rehabbing effort by skilled technical people and capable restoration specialists brought it to a condition that makes a very presentable piece of furniture. Having been moved twice within the University Library, it has yet to find a suitable home where it will be displayed to advantage under controlled climatic conditions. The visible documentation for it could also be improved a lot.

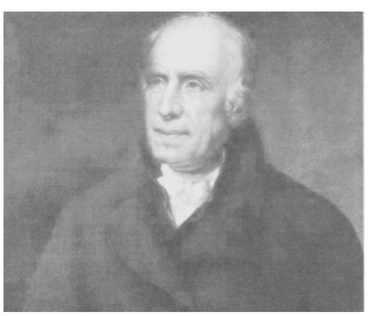
After 1782 Rittenhouse wsa re-appointed a Trustee and built the next and third observatory associated with any University person. Money was not from the University but public funds appropriated by the Assembly. The building, an octagonal brick structure, was set back from the northwest corner of 7th and Arch and is believed to be the first purpose-built permanent observatory in North America. He used it for observing Uranus and comets and attempting the light curve of η Aql, a star that had recently been discovered to be variable. Whatever else may have been his attainments, Rittenhouse was a poor estimator of stellar brightness. Pitman (1933) could not make a credible light curve of the estimates and I have done a more sophisticated reduction of them but they show no pattern at all, only noise. It is not clear to me that all of the estimates are even of the same star. At very nearly the same time, he (Rittenhouse 1786b) independently invented the use of spider silk for the marking wires in a telescope's focal plane. He also made an inventive application of the concept of collimation for any optical system and built a collimator of his own to obviate the parallax of a mark that is not many focal lengths distant from a telescope. He has numerous other scientific accomplishments to his credit as well. For instance, he was asked to address a question from Francis Hopkinson, who had held a handkerchief in front of his face and failed to understand what he saw through it as he looked at a lamp down the street. (Hopkinson himself is a most interesting and astute personality, a member of the first graduating class of the College, a responsible politician and bureaucrat and a composer of art songs that remain in the repertoire. It is not to his discredit that he did not comprehend the stationary scattering pattern as he moved his kerchief in front of his face.) To answer the query, Rittenhouse (1786a) built the first diffraction grating and empirically determined the law of spectral order spacings from a transmission grating but apparently saw no spectral lines. Had he pursued his results, he might have given up his commitment to the corpuscular theory of light and certainly would have had priority for the invention because Joseph von Fraunhofer had not yet even been born.

It is fair to say that this man was the preeminent scientist in colonial North America and the early United States but he also spent a large amount of time in activities that he could not develop further because he suffered from lack of communion with scientific equals. All kind of honors had come his way but election as a Fellow of the Royal Society happened only the year before he died. It is unnecessary to detail his political and civil service activities but they were numerous, as may be seen in Ford's (1946) and Hindle's (1980) biographies, to which I am myself indebted for many details. A much older appreciation of Rittenhouse appears in Davis (1896), who gives some rather appealing old photos and the whimsical note that the man is descended from Charles the Bold, Duke of Burgundy. This seemingly silly reference is actually used by Davis to trace the evolution of the surname from the 15th century to its modern English form. Since there are numerous web references that invalidate a more conservative claim that Rittenhouse was in the Hapsburg line from Maximilian II, Davis's claim must be spurious. Rittenhouse's name does, as it were, remain in print: the journal *Rittenhouse* is edited from Ottawa by R. C. Brooks of the Canada Science and Technology Museum. It is dedicated to publication of scholarly papers about scientific instrumentation from the 17th through the mid-20th centuries. He was first interred under his observatory floor. When the building was sold, his remains were removed to the Old 4th Street Presbyterian Church. Public and private buildings, streets and parks bear his name.

Grew had attached *Philom.* or some variant of the word to his name. According to the *OED*, this refers to *Philomath*, a word created in 1643 to signify a lover of learning or a student of mathematics, natural philosophy and the like. It is certain that he had no degree from Oxford, Edinburgh, Cambridge, Aberdeen, Yale, St. Andrews, Glasgow or Harvard although anyone could have studied and left without a degree and not be recorded among a school's alumni. (The St. Andrews and Harvard lists of students were checked exhaustively without result.) It is possible that he was privately tutored or entirely self-educated as was nearly the case for Rittenhouse. The clergymen, Smith (sketched in Hein and Shattuck (2004)) and Ewing (described in Wilson (1812)), generated plenty of acrimony during their lives but they also made some additional scientific contributions. Ewing (1771b), for example, published a paper on improving Godfrey's quadrant and has numerous biological credits to his name. Smith (1771c) measured the difference in terrestrial coordinates between Philadelphia and Rittenhouse's home in Norriton and observed a transit of Mercury (Smith 1771d). Clearly, 18th century education (in Smith's case the University of Aberdeen and for Ewing some years of private tutoring followed by a year at Princeton) was successful, turning out some priests and ministers who were comfortable making field measurements, completing the necessary geometrical constructions and numerical calculations, and presenting their results for publication. None of this is likely to happen in the present day.

Salaries are an interesting matter. Grew was paid £125PA at the start of his appointment; these are Pennsylvania pounds, not sterling. Sahr's calibration doesn't go earlier than 1800 so I used the more extended scaling of McCusker (2001) with the (dollar/pound) par value of 4.44. Grew's salary then becomes £125PA [$8,300]. Some context for this sum may be gained by realizing that the subscription to open the Academy amounted to £5,000PA [$332,000], a cord of hickory cost £1.8PA [$120] and the College building completed in 1751 came in at almost £776PA [$51,500]. Grew would have needed 3-4 cords to heat his first floor through the winter and this is about 5% of his salary. His salary scales to be about 16% of a useful academic building at that time, so he didn't do too badly. On the other hand, he never got a raise in 9 years of teaching but the College permitted him to continue his own private school and he took student boarders into the family home. Having once been jailed for debt, he perhaps always felt he needed more money.

THE NEW CENTURY



The first astronomical figure around the beginning of the 19th century was Robert Patterson (1743-1824). While he made no observations, he revised Ferguson's *Astronomy* in 1806, saw this revision through a second edition in 1814, and published *The Newtonian System* in 1808. A few low-level astronomical papers appeared as well and he served as a pipeline (*e.g.*, Patterson & Ellicot 1793) for publication of results from non-members and out-of-town members of the APS. Patterson had taught dry-land navigation to Meriwether Lewis before the start of Jefferson's Expedition and the President made him Director of the Mint.

A much more consequential man was Robert Adrain (1775-1843). His interest in geodesy is shown by his papers (Adrain 1818a, b) on the figure and mean diameter of Earth and the fact that he taught courses in analytical dynamics and "physical" astronomy. The background for his greatest accomplishment, however, goes back to about 1750. By then, many types of measurement and observation had been made in which were embedded parameters of physical significance that could not be measured directly. They were functions, in the mathematical sense, of the observables. The question of how to recover the most probable values of these unknowns from measures of finite precision engrossed many European scientists and mathematicians. In one achievement, Legendre (1805) had enunciated the least-squares principle and showed several applications of its potential power without giving a proof that the results so obtained were the most probable ones. Adrain's place in the history of mathematical-based science is due to his having discovered the first and second proofs of the law of probability of random errors of observation. This understanding is the basis for all applications of the method of least squares because that procedure undertakes to minimize the sum of the squares of the differences between measures and calculations using the values of the parameters that the procedure uncovers. Adrain's accomplishment had actually occurred

in 1808 (before he joined the University) when he was teaching in Reading, PA and editing in Philadelphia a magazine *The Analyst; or Mathematical Museum.* As the editor, he solicited mathematical puzzles and questions from readers and published the answers and proofs from the originator or other subscribers. Patterson (1808) had submitted a question to the magazine about the distribution of errors and offered a prize of $10 [$137] for the correct answer and Adrain (1808) was impelled to answer with his proofs. He had actually been anticipated by Bowditch (1808) with a special demonstration of the proposition. It must also be noted that Gauss's 1809 proof (*cf*. Davis 1857, Bertrand 1855), the one usually cited, was more comprehensive and illustrative than Adrain's and that, by Gauss's own testimony, he had known and used it since 1795. None of this diminishes Adrain's achievement in his North American isolation.

The last individual who need be mentioned here is Edward H. Courtenay (1803-1853) a formidable and congenial academic mathematician who had taught at the U. S. Military Academy for 13 years after graduating at the head of his class. He joined the University in 1834 as Professor of Mathematics and afterwards spent 11 years at the University of Virginia. In the midst of these appointments he had given up on academic life or maybe just decided that he need a change for he worked about 6 years as a practicing civil engineer for a railroad and additional short intervals for the War and Navy Departments. Courtenay (1837a, b) calculated the differences of longitude between several North American locations on the basis of his and others' observations of the solar eclipse of November 30, 1834 just after his appointment at the University had begun. He stayed until 1836. At Virginia he was described as a model tescher and there is no reason that he would have been different at the University.

E. Otis Kendall (1816-1899) had been Professor of Mathematics and Astronomy since 1855 at which time he was one of only 6 Arts Professors at the University. In 1881 he became the first holder of the Scott Chair of Mathematics. Kendall must have had administrative ability for he served as Dean of the College for 6 years and Vice-Provost for 11 years. All calculations for Jupiter, Neptune and their satellites in the *American Ephemeris* from 1855 through 1882 are his. By no means was the foundation of his astronomical work laid at the University. He had been prominent at the Central High School before his appointment and had published papers on Encke's Comet (Walker & Kendall 1843) and on longitude differences deduced from solar eclipse observations (Kendall 1841). He was a half-brother of Sears C. Walker (1805-1853), a then important Philadelphia scientist not associated with the University and who came to a dismal end after some time at the USNO.

DID IT MATTER IN ANY WAY?

Of course, the net University accomplishment is not to be compared to what was done in England, France, and Germany over this time. Individually, Adrain and Kendall would have been able to hold their own in Europe because their accomplishments are neither generically nor quantitatively inferior to those of many foreign contemporaries. Rittenhouse, on the other hand, would surely have risen close to the top in England. Freed of the burdens from his positions as Mint Director and state Treasurer, he would have concentrated on astronomy, magnetism, and meteorology and found congenial scientific associations among the best continental scientists. After all, he was one of only a small number of U.S. citizens elected to The Royal Society between 1776 and 1800.

There were, therefore, almost 140 years of observational background by College and University faculty before any bequest of money was implemented at all. Most of the observational efforts concerned transient or ephemeral events and it was only with Kendall that a programmatic approach appeared. His theoretical and analytical work on the outer Solar System had both practical and scientific values: almanacs employing the phenomena of the Jovian satellites would continue to be useful for navigation and surveying for quite a while and the discovery of Neptune on the basis of perturbations of Uranus could, in principle, be repeated with one hoped-for result being a new ninth planet. What is really missing in the sporadic observational history is any understanding that astrophysics was going to grow out of spectroscopic experiments, theory and observations. That recognition had been made at many places in Europe and at several in North America and was partly to account for the eminence of astronomy at those institutions for most of the rest of the 19th century. There was no one around Philadelphia to start a comparable effort. The situation was, in fact, worse still. As a result of the small public appropriation, Rittenhouse had been able to establish his own observatory near his Philadelphia home after 1784. As far as is known, no one from the University had access to this facility and well into the 19th century the only operational local observing capability existed at the Central High School where Walker was teaching. All this negative interpretation is consistent with Rothenbreg's (1975) unpublished survey. There appear to be no mechanical or optical University artifacts left from this time.

By the end of the century there were functioning observational installations at North Carolina, Cincinnati, Yale, Harvard, Hamilton and Union Colleges, Dartmouth, Princeton, Missouri, Pittsburgh, Chicago, Northwestern, Virginia, Haverford and Swarthmore Colleges, Wisconsin, and Michigan as well as at numerous smaller schools. The no-longer-new emphasis on astronomical spectroscopy was already prospering nearby at Dartmouth, Harvard, Columbia, Pittsburgh and Princeton but the 19th century people at the University showed no awareness of the promise of atomic astrophysics. It would have been unreasonable to expect Kendall to give up his productive and modern research and change to spectroscopy. What was needed was the foresight to create a second faculty position with that specialty. This didn't happen and therefore the University observational enterprise would inevitably be a catch-up one in the framework of U.S. research and higher education. The best that can be said is that a reputable man was on the spot when money began to come in.

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THE FLOWER OBSERVATORY (FO)

INTRODUCTION

Reese Wall Flower, aged 68, died of pneumonia around 8 AM on June 27, 1875. He had inherited considerable wealth (about $80,000 [$1,270,000]) from his step-grandfather John Wall and a paternal uncle John Flower and then had prospered in the lumber business. The Philadelphia City Directories show him maintaining a downtown residence from the early 1840s to the late 1850s and describe him as a gentleman. This was a euphemism for a man of independent means who did not have to work for a living. Although he also kept a home in West Philadelphia, the U.S. Censuses for 1860 and 1870 show his residence to be in Upper Darby Township, Delaware County at the Flower farm. In fact, an 1848 map of the township shows him as the owner of the farm with two structures on it; he had retired from active business operations. As far as is known, he had joined no business associations or social clubs. No likeness of him is known to exist. Late in life he endured some reverses with investments in small, undercapitalized railroad lines but still left a considerable estate at his death, which happened at 3402 Baring Street, the West Philadelphia residence. He had moved to this address only in the year of his death.

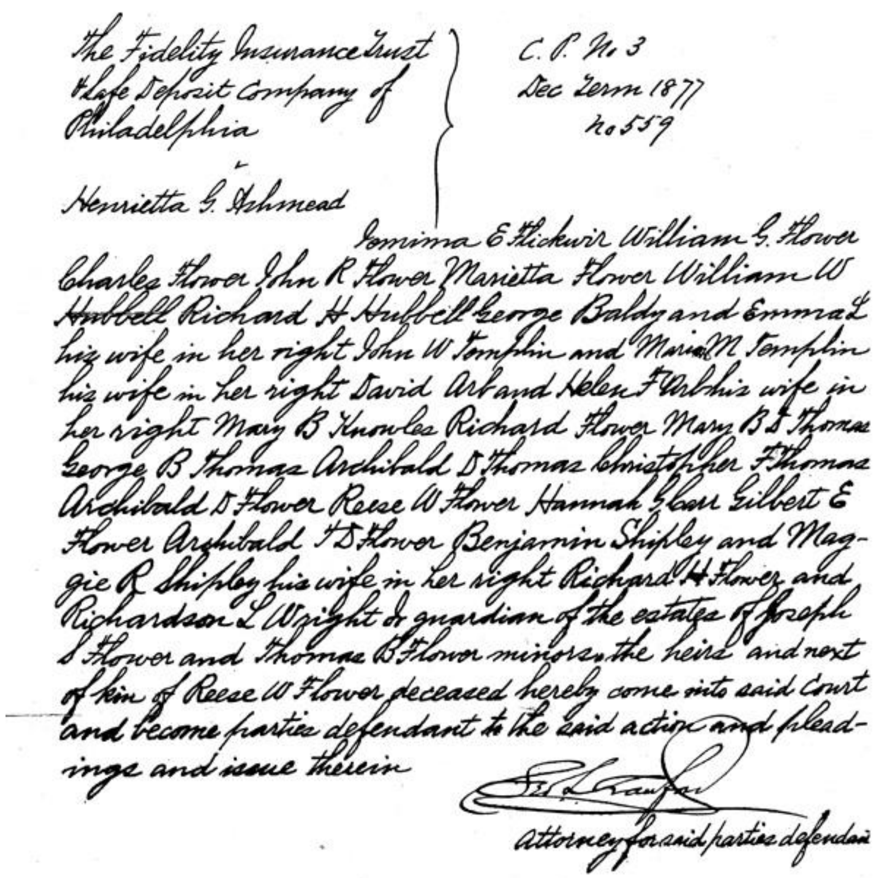
Flower's maternal great-great-uncle George Graham (1673-1751), the famous instrument maker in London, England, was an early supporter of John Harrison whose struggles to invent and get paid for a stable time-keeping device for deep-sea navigation are well known (*e.g*., Andrewes 1996). Graham (1727) had conceptualized the temperature-compensated pendulum, made fine instruments for Edmund Halley and James Bradley, and invented the deadbeat escapement in 1715 in order to minify the recoil force from a pendulum back onto the pallets. It is unknown if Flower was aware of any of these accomplishments or indeed took any notice whatever of scientific affairs. Nonetheless, his 1870 will provided that his residual estate should "…apply the same to the erection, construction, and maintenance of an astronomical observatory, and to the advancement, in connection therewith, of the science of astronomy, and for that purpose I authorize and empower said trustees to purchase all necessary apparatus therefor, and to appropriate annually, from the income of my said estate which shall be remaining after the thorough completion and equipment of said observatory, a sum sufficient to pay the salary of a competent astronomer to take charge of said observatory and apparatus…". Other clauses apply to possible locations of the observatory. The trustees are those of the University of Pennsylvania, with which Flower had no known connection.

A LEGAL TUSSLE AND ITS RESOLUTION

The *University Magazine* of January 1877 records the remarkable circumstance that in the 80 years prior to 1868 the University received not one dollar in the way of endowment but in the 8 subsequent years $1,092,000 [$15,400,000] had come in. The second largest item in this sum is $200,000 [$3,170,000] from the estate of Flower. That $200,000 [$3,170,000] was in the cup but it never got to the lip.

Flower had created a will in 1862 by which all his estate was to be left to a Philadelphia soup society. This will was superseded by the later one of June 18, 1870 according to which there were some beneficiaries other than the University. Christ Church in Media, PA was to receive $450 [$7,100] and some family members were to receive small sums. By the year of his death, Flower was the youngest and only survivor of three brothers but there were also two living sisters. Flower had never married. The next generation was represented by many nieces and nephews, and their own children were also numerous. For legal purposes many of these people were named heirs-at-law and at least some of the family looked to set aside the 1870 will on the grounds that Flower was insane and that they had been disinherited unjustly. For this purpose, they challenged the will's probate procedure and had it replaced by Letters of Administration as if Flower had died intestate. The Administrator was John B. Gesh, Vice-President of The Fidelity Insurance Trust and Safe Deposit Company, who attested that a valid will did exist. This can hardly be a surprise since his own company was the Executor and two lawyers witnessed the signing. Gesh set in train a first appraisal of Flower's effects and goods. These included his silver watch and chain, shares of preferred stock in 4 railroad companies, bonds and coupons issued by 10 different railroads and municipalities, assorted farming implements, the furniture at the Philadelphia home where he died and the equity in the mortgage on that home. The total appraised value came to $79,677.34 [$1,370,000]. The farm and other real property were not included in this appraisal. A later re-appraisal, taking account of the real property, increased the value of the total estate considerably.

Apparently on the basis of Gesh's finding, Orphan's Court then granted the trying of the validity of the will. This led to the one-day jury trial of *The Fidelity Insurance Trust and Safe Deposit Company, Executors of the Estate of Reese Wall Flower decd. vs. Henrietta G. Ashmead* in the Court of Common Pleas No. 3 at the December Term, 1877. Ashmead was an older married sister of Flower and cared for him in his last illness. She was the lead defendant for 27 other adults (including 4 wives in their own right) and for the guardian of two minors. This case is No. 559 for the term and its docket survives, as is shown in Fig. 18.


The Fidelity Insurance Trust & Safe Deposit Company of Philadelphia

v.

Henrietta G. Ashmead

C. P. No 3
Dec Term 1877
No 559

Jemima E Flickwir William G. Flower Charles Flower John R Flower Marietta Flower William W Hubbell Richard H Hubbell George Baldy and Emma L his wife in her right John W Tomplin and Maria M Tomplin his wife in her right David Orr and Helen F Orr his wife in her right Mary B Knowles Richard Flower Mary B S Thomas George B Thomas Archibald D Thomas Christopher F Thomas Archibald D Flower Reese W Flower Hannah Glass Gilbert E Flower Archibald T D Flower Benjamin Shipley and Maggie R Shipley his wife in her right Richard H Flower and Richardson L Wright Jr guardian of the estates of Joseph S Flower and Thomas B Flower minors the heirs and next of kin of Reese W Flower deceased hereby come into said Court and become parties defendant to the said action and pleadings and issue therein

[illegible]
attorney for said parties defendant

Fig. 18. The summary page for Case 559 showing one of Flower's sisters as the lead defendant and the gang of co-defendants. The first of these people is Flower's second surviving sister. One name on the 8^{th} line of co-defendants repays attention. The attorney may be George L. Crawford. He or his clerk seems to have had no regard for punctuation.

The stenographic recording of the courtroom testimony has not been found in the City of Philadelphia archives and the trial is not summarized in *The Legal Intelligencer.* There are, however, three sources of information pertaining to the event and they should permit a close reconstruction of what happened. One is a document in the University Archives that appears to have been handwritten by one of the Executors' attorneys or clerks or by a University witness and is a paraphrase of witness testimony. The second is a pair of summary articles in *The Philadelphia Inquirer* and *The Evening Bulletin* of June 5, 1878 by an unnamed legal reporter. The texts of the newspaper accounts are identical so the papers used a single legal reporting service. The third source is typewritten and apparently contains the *verbatim* instructions of the judge to the jury. According to the newspaper article, the Executors put in evidence the 1870 will and rested their case calling no witnesses. The University Archive document of the ensuing testimony of the defense witnesses is summarized in the following.

(1) Flower had left his property to a heartless, useless charity.
(2) His choice of the location for an observatory was useless for astronomy of which Flower knew nothing.
(3) Although some witnesses thought him a loved and tractable child, others considered him a peculiar adolescent. He had behaved well toward his family until about 1835 but thereafter had become abstracted and selfish and contentious toward family members and acquaintances believing everyone was after his money.
(4) An accident with a horse and dray in 1836 left him with head problems for years.
(5) After his mother died, he gave each sister $50 [$980] with no explanation. When his father died, Flower received more of the estate than his siblings despite already having benefited from the estates of his step-grandfather and uncle. He failed to account properly to his siblings for the disposition of the father's estate.
(6) He hardly worked more than a year or so in his life.
(7) After 1844 he was habitually drunk and abusive, encouraged both men and women to become drunk with him, sometimes appeared only partly dressed or even naked at the farm, lived immorally (but not simultaneously) with two housekeepers and had a child by the first of these low women, spoke obscenely in public even to children, kept a library of obscene books, and even cursed a toddler.
(8) Because of a medically diagnosed softening of the brain leading to mental instability, Dr. Isaac Ray testified that Flower was forbidden to read or write. Dr. Ray lived a block away from Flower and was probably the family physician. According to the testimony of his Aunt Abigail Graham, Flower suffered from the family trait of insanity. Brother Zedediah had been feeble-minded and brother William insane for a year before his death and the latter's daughter had shot herself in a fit of insanity. An uncle was said to have been eccentric.
(9) After about 1868 he had become delusional sometimes wishing his mother dead (she had died in 1841), sometimes imagining that her body had been exhumed and sold to doctors, and at various times threatening to shoot a drover, a minister, and a brother-in-law.
(10) He had cruelly left nothing to two penniless widowed sisters or to the remaining sister, who was 75 with an 88-year-old husband, both of them without means.
(11) He was cruel to animals. He had cut off the tail of a handsome horse which had happened to flick him, stabbed the dog of one married sister, kept dogs and rats so that they could fight, swore at a ram, cut the throat of his pet crow, shot a neighbor's bull although not fatally, and cut off the ears and stones of a dog and had them cooked for his supper.

For the most part, there is no way of knowing which family member or acquaintance uttered any of the specific comments. I have condensed the testimony considerably but preserved the sense of the pejorative remarks as they appear in the Archives document. I have also chosen not to put in quotes the archaic terms that appear here and there in this summary. The Executors attorney did not cross-examine the defense witnesses.

Perhaps the witnesses could have worked harder to blacken Flower's name and character but this is doubtful. There is certainly a feeling that some witnesses were not listening to what they themselves were saying. One might well agree that the University is heartless but it is probably not useless and it is hardly a charity. On the other hand, a soup kitchen really is a charity and Flower thought well of it in 1862 long after the family decided that he was a selfish individual. There is no way that a layman would know whether any particular site eventually to be chosen by the Trustees was or was not suitable for an observing facility. Even the casual reader can note the anecdotal nature of many of these remarks, and envy and greed are transparent. It is known, for instance, that Flower was a responsible citizen while residing at the farm, which he had bought in two pieces in 1831 and 1850. In 1849 he had joined with six neighbors and fellow stockholders to raise money in order to finish the planking of the West Chester Turnpike from Philadelphia to Newtown Square by the following year. If he were still in the lumber business, he could have made a considerable profit by this enterprise. As late as 1873 he was engaged in conventional and reasonable real estate transactions. The evidence is that he was not irresponsible.

Instruction to the jury by Judge Thomas Finletter was expressed in great detail and this survives in the typewritten copy in the University Archives. Therein much was made of the major precedent *Banks vs. Goodfellow*. It was expressed that an individual has no higher obligation in life than the disposal of his legal property and that the law must respect that obligation and the way in which the testator wishes to fulfill it. At the same time, it is necessary that the individual be of sound mind, and a traditional and forceful evidence of that mental condition is regard for the future of surviving family members. Should they be in need but ignored by the individual, it may reasonably be inferred – in the absence of justifiable antagonism – that the will was not conceived with a sound mind. Numerous other supporting legal precedents were also cited. In deliberations that must have been brief, the jury found for the defendant on June 4, 1878 and the jury fees were paid 24 days later.

The final detailed appraisal of the estate was presented at the July, 1878 term of Orphans Court and survives as Docket 223. From the devastating characterization of Flower given by the defense witnesses, one would never anticipate what the newspaper reporter finally describes – the trial had been staged to bring about an amicable settlement between the parties. And indeed this appears to have happened, for shortly thereafter the heirs-at-law assigned to the University half of the judgment. The only alternative is that University lawyers, chagrined at their ineptitude during the trial, had threatened to tie up a settlement by endless court appeals unless the defendants split the assets. I have found no evidence for this idea. The first effect of the settlement appears in the ledger of Christ Church, Media where $427.50 [$7,370] is noted as a bequest from the Flower Estate. The parish minutes are silent on how this sum was used. The church records are far from continuous but they do not show Flower as a communicant even in 1860. That idea is itself implausible since in good weather it would have taken him at least 45 minutes to drive a buggy or ride a horse from the farm to the church and there were Episcopal churches closer than the Media one. It could not have been his childhood church since the parish was not organized and incorporated until 1854. I have been unable to discover his attachment to this particular congregation.

According to an Indenture of July 2, 1878 the University received the following:

(1) the Flower farm of 100.5 acres;
(2) extensive improved and unimproved real estate in Philadelphia's First Ward;
(3) a dwelling (really two numbered addresses) on 8th Street in Philadelphia; and
(4) cash and securities valued at $23,239.10 [$401,000].

There were conditioning clauses to the settlement that the University had to observe:

(1) a Flower Professorship had to be created with appropriate salary and equipment and books;
(2) an astronomical observatory was to be established in the 27th Ward or any other suitable place and was to be suitably equipped;
(3) any remaining monies could be used for instruction in mathematics and physical science; and
(4) provided none of the foregoing were compromised, the Trustees could dispose of property.

The Indenture shows that the University had to turn over to the Heirs-at-Law $50,000 [$862,000] in exchange for the properties.

Some adjudications among the plaintiffs delayed final settlement until July 1881. Each heir-at-law then received his or her individual fraction of less than half of the total estate (their lawyer having taken 20% of that half) in portions down to 1/175 of the total. Henrietta Ashmead had died before the final disbursement. An interesting event becomes apparent at this time and seemingly had its origins 46 years earlier. For some reason, Zedekiah Flower had became unable to care for at least some of his children and he petitioned The Court of Common Pleas of Delaware County to appoint guardians for two boys over 14 and two girls under 14. On November 24, 1835 the docket for Orphans Court Case 1532 records that the court did appoint a guardian for John and Thomas Flower. Among the surviving heirs-at-law in 1881 was Reese Wall Flower, Jr.; without the generational indicator his name appears in Fig. 18. This adult man (1842-?) is further described as a son of the deceased John Flower, himself the son of the deceased Zedekiah Flower, and the latter in turn the brother of Reese Wall Flower. In view of the trial testimony and the designation of Jr. on this man's name, he is almost certainly the natural son of Reese Wall Flower but formally or informally adopted by the nephew John Flower when John was himself just a young adult. No adoption papers have been discovered but this is not unusual for that time. Reese, Jr's estate share, $613 [$8,330], was equal to that of each of John Flower's own children. Apparently John remembered a kindness done for him when he was still a child and he and his wife acted when they saw a human need so not all of the family were grasping people. There is no evidence that Flower or the mother provided any support for the boy's upbringing but entries for Reese, Jr. in the City Directories suggest family contact. In 1875 he is described as a General Agent and lived at 3411 Baring Street, *i.e*., almost across the street from his father. From 1880 through 1894 he resided a few blocks away at 3600 Powelton Avenue, part of the time with his son, Henry, who is described as a Draughtsman. Reese, Jr. was Superintendent of the property at 927 Chestnut Street from 1880 through 1891 and then from 1892 through 1894 he is listed as a Clerk. After that year, he vanishes from the records that I studied.

A NEW OBSERVATORY – THE FOURTH, THIRD, SECOND OR FIRST?

After the legal settlement, the farm was let to tenants but no University records exist between 1878 and 1884. Beginning in that latter year there are records of annual income of the order of $3,500 [$61,400] from the Reese W. Flower Endowment but nothing active was done for another 8 years. Eventually, in 1892 a Flower Professorship was created, the chair was filled and observations were begun in 1895, albeit not by the first Flower Professor and not on the eventual site. That chosen site, decided by action of the Trustees on June 4, 1895, was on the property of the Flower farm. The endowment continued to increase in value and apparently the 8th Street properties were sold; at least they no longer appear on the University property list. The 1897 Treasurer's Report values the real estate at $30,000 [$625,000], the Observatory (presumably including installed and portable equipment) at $12,796.84 [$267,000], and the residence and library at $11,808.25 [$246,000]. The year's income from the endowment was $1,793.03 [$37,400]. All but 5 acres of the farm were sold at the end of 1904 and the endowment rose considerably with that sale. At the same time an area of a bit more than 1 acre south of the Observatory was added to try to protect the station from encroaching development and another small parcel was bought in 1919. Over this same interval small pieces were sold in order to achieve a tidy boundary line.

Dedication of the FO on May 12, 1897 was attended by University officials, lots of locals, John A. Brashear and Mary Proctor but E. Otis Kendall, the first Flower Professor, apparently was not present. Brief remarks were offered by different people and Simon Newcomb of the Nautical Almanac Office, who, it will be remembered, knew more about transits of Venus than anyone else in the world, was the invited speaker. He talked for more than 30 minutes on *The Problems of Astronomy*. Almost inevitably, this lecture presented a mixture of solid accomplishments, misinterpretations that are now considered dated, reasonable speculations, and personal biases. Newcomb had a few, probably unwitting, barbs too, commenting on the "little" new Observatory and noting an impressive accomplishment of Allegheny Observatory at the other end of the Commonwealth. There, Keeler (1895) had recently finished the observational demonstration of the true character of Saturn's rings, for that time a remarkable accomplishment. There were a few other short speeches and then the 400-odd guests enjoyed refreshments.

The installation, as it existed in the decade of the 1910s, is shown in Fig. 19. The main instrument was an 18-inch, f/17.5, 2-element visual refractor by Brashear and Warner and Swasey. The objective was not new, having been displayed at the World's Columbian Exposition in Chicago and then installed at the Lowell Observatory on the same mounting as a 12-inch Clark refractor. There it was used for observing the 1894-1895 opposition of Mars and then returned to Brashear. Its optical quality was very fine. In addition, there were a 3-inch universal instrument (really a broken transit), a 4-inch zenith tube, a 4-inch meridian circle, a Self-Winding Clock Co., NY sidereal clock and chronograph and some peripheral apparatus. Apparently as a one-time stunt, the USNO clock signals received telegraphically were fed into a primitive version of an oscilloscope with the help of an engineering faculty member. The locations of the mire-mark lenses do not appear in Fig. 19 but they had focal lengths of the order of 175 feet. There was also a substantial variety of instructional accessories including a spectroscope. Quimby (1898), a noted local amateur, remarks on the reward for sitting through the dedication speeches: you got a very satisfying look at the solar spectrum at the focal plane of the refractor. There is no record of the spectroscope ever again being mounted on the telescope. Because the staff was never sufficiently large, other optical components and telescopes were never broken in and put to routine use. All optical components were by Brashear and all mountings by Warner and Swasey.

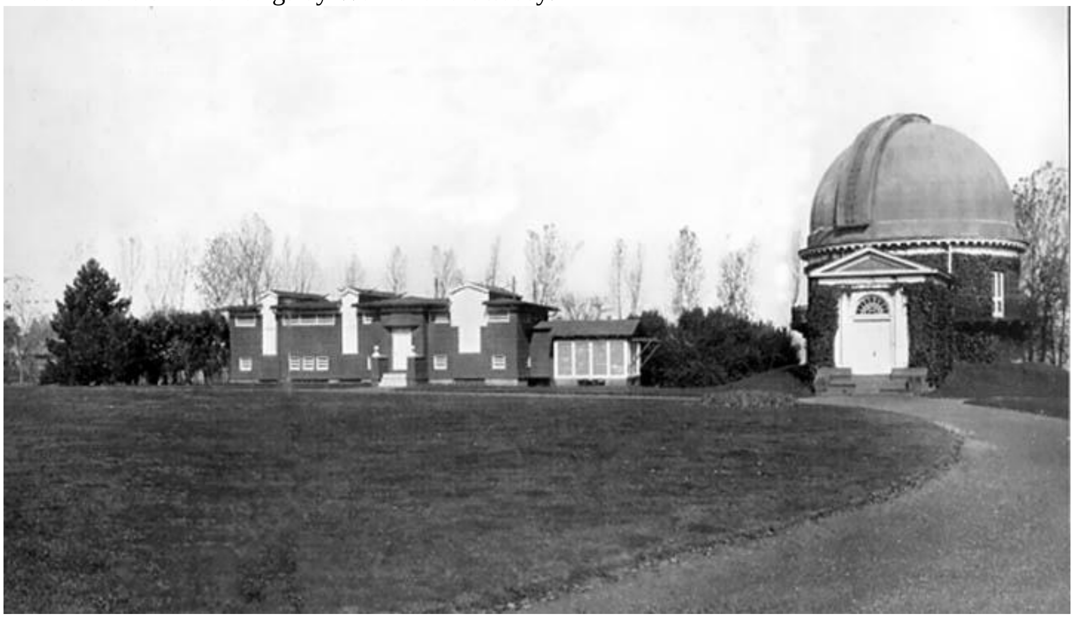


Fig. 19. A panoramic view of about 25% of the Flower Observatory property looking northwest sometime between 1904 and 1916. This was once a cornfield. The location of the 4½-inch refractor building after 1922 is off the right edge of the picture. West of the dome and continuing westward the frame shelters housed the Wharton instrument, the zenith telescope, the meridian circle and the universal instrument as well as the Self-Winding sidereal clock and chronograph. This mage was cropped from a photo with a larger extent to the west and south. At the left of the larger picture is PA Route 3, which was not paved until 1922. The property was on the highest ridge of the surrounding countryside.

An additional structure housed the residence of the Director and initially a library of thousands of volumes. This was not Flower's home *The Roadside* but a new construction. The architect of the residence was Assistant Professor of Design Edgar V. Seeler (1867-1929), who actually spent 12 years on the University faculty in one position or another but from 1895 also had a private practice. Trained in Philadelphia and Paris, he had a very visible career designing public and private structures in and around Philadelphia, a number of which continue to decorate the urban scene. The downtown home of The Curtis Publishing Company, with its Tiffany & Company stained glass panorama of Maxfield Parrish's *The Dream Garden*, is among them. Seeler's elevation drawings for the residence still exist in the Architectural Archives as No. 047, 203 and are shown in Fig. 20. Presumably he was paid for the Observatory design as the Principal of his firm rather than the task being just another of his academic duties. Seeler designed the other buildings as well and these were the only technical structures of his career. The refractor's steel dome skeleton was sheathed with 1-inch thick pine (visible from the inside) and supposedly by an outer shell of tin. The choice of this fragile metal is actually improbable. Rather, the outer shell would most likely have been of terne, an alloy of lead and tin. It remains unclear how the outer plates were put together; no photo shows clearly enough the seams or ridges on the slightly-faceted surface. About twenty idler wheels supported the steel base flange of the dome. It all seemed to work: around 1900 Eric could rotate the dome 180° by hand in less than 1 minute and apparently it never leaked.   Much of the foregoing is described by Doolittle & Doolittle (1912).

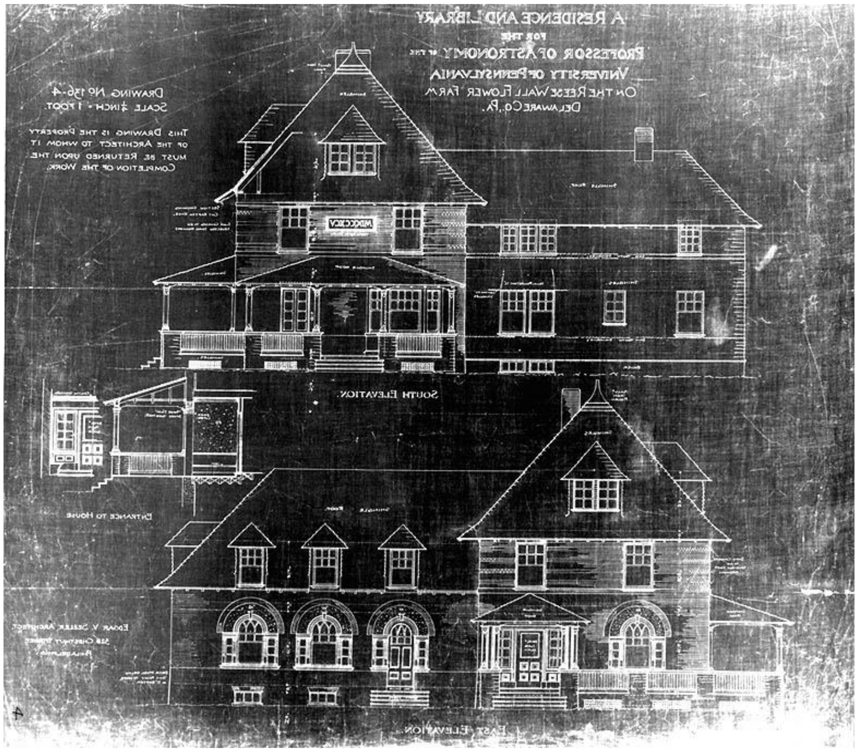
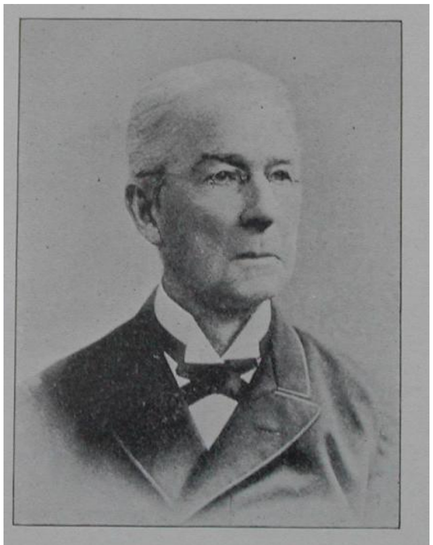


Fig. 20. Above, E. V. Seeler's drawings for the south and east faces of the Director's residence. Below, the appearance of the residence about 1910. For some time H. B. Evans lived in the room directly above the library at the northeast corner of the dwelling. A caretaker's apartment was on the west side of the structure. #When the building was demolished, the coal-fired heating system needed replacement but the exterior and interior were structurally sound.#

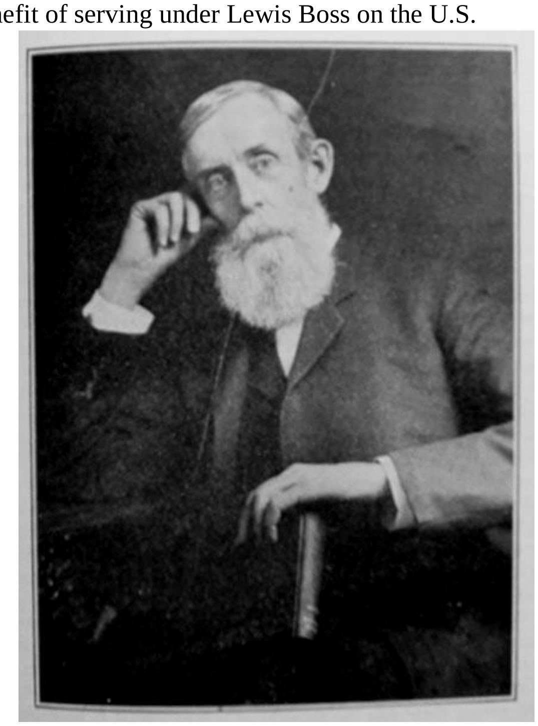
THE DIRECTORS, FLOWER PROFESSORS AND OTHER FACULTY

As soon as the Flower bequest materialized in the chaired Professorship and the FO, astronomy at the University changed to a more structured enterprise and continued the concentration on particular subjects in the manner that Kendall had practiced. Its development can be traced in the personal and scientific sketches that follow, which are in neither chronological nor academic order but try to present the story in a reasonably coherent fashion.


Ezra Otis Kendall.

E. Otis Kendall (1816-1899) was appointed first Flower Professor in 1892. By this time his very successful career, described in the previous chapter, was in the past but the award of the Professorship was a logical and deserved appointment. He held the chair until retirement in 1896. In the language of Victorian times, Kendall was "loved" by many of his students and must not really have been so stiff as he appears in the accompanying portrait taken around 1890 (and which was copied from.page 1 of volume 3 of *The Alumni Register* of 1899). There does exist the curious circumstance that Kendall apparently did not attend the FO dedication and there is no evidence that his successor consulted him in its design and furnishings although his background before coming to the University would suggest that he was amply knowledgeable about such matters. Perhaps it was that he was uninterested in the coming emphasis of the new station or possibly he was ill on that day. He would live more than 18 months after the dedication event.

Charles L. Doolittle (1843-1919) started his education at the University of Michigan, enlisted in the Union Army, married, returned to Michigan and finally graduated with a civil engineering degree in 1874. That ended his formal education but he had already had the benefit of serving under Lewis Boss on the U.S. Northern Boundary Commission for part of the tenure of the Commission. He participated in the portion of the survey of the Canada/U.S. border from the Lake of the Woods to the Rocky Mountains. After 20 years service at Lehigh University beginning in 1875, he came to Pennsylvania as Professor of Mathematics, second Flower Professor, and Observatory Director. It was during his service that the Astronomy Department was calved off from the Mathematics Department. When the AAS was organized, Charles served as its first Treasurer for 13 years and at the 12th meeting in 1911 he became Chairman of a committee on cooperation in the teaching of astronomy. He was a gregarious man outside the family circle although his formal appearance and weary gaze might lead you to think otherwise. The image is actually his obituary photo published in *The Pennsylvania Gazette* issue of March 1917. The date of the picture is unknown but other photos suggest that it might have been taken around 1900. Married twice, he was the father of five sons and a daughter Hilda (1886-1961). Hilda endured an insecure and hyper-sensitive adolescence (or perhaps it is more correct to say that her parents endured her) and recorded this part of her life piecemeal in several works (*e.g.*, H.D. 1956). As a teenager, she encountered William Carlos Williams and Ezra Pound when they were University students and Williams (1948) has a few things to say about the



Doolittle family, more about Hilda herself, and much more about Pound. At 25 she moved to Europe and, partly patronized by Pound, established her literary identity under the *nom de plume* H. D. With literary critics and feminists her reputation has grown in the past 25 years. One Doolittle son became an USNO staff member and another was killed in France less than 2 months before the 1918 armistice. Hilda considered this to be the cause of Charles's death but he was already 76 years old. The very sympathetic obituary in *The Philadelphia Press and Public Ledger* describes him as a representative of the "old astronomy".

The proper way to energize a new field of science is exemplified by Chandler's (1891a, b) first two papers on polar wandering. New unexplained measures or observations require novel hypotheses that can be tested against still newer as well as retrospective data. Charles came to the FO as an expert in the new phenomenon of the variation of latitude, specifically from the Sayre Observatory of Lehigh University. The foundation of latitude variation rests on Leonhard Euler's theories of how a rigid object of any shape should spin if the axes of spin and figure are not collinear. If the direction of the axis of figure is not constant with time, what you would intuitively think of as constant characteristics, such as the latitude of a station, should vary. Discovery of just this effect had been announced by Küstner (1890). Charles had early indications of the latitude variation at Sayre but he seems to have been expecting a secular (probably a long-term periodic) and not a cyclical effect. His evidence for cyclical variability does not become convincing until about 1889 largely because he had too few data that were too widely spaced in time. This may be judged in Fig. 21, where it can be seen that he had problems with the stability of his zero point.

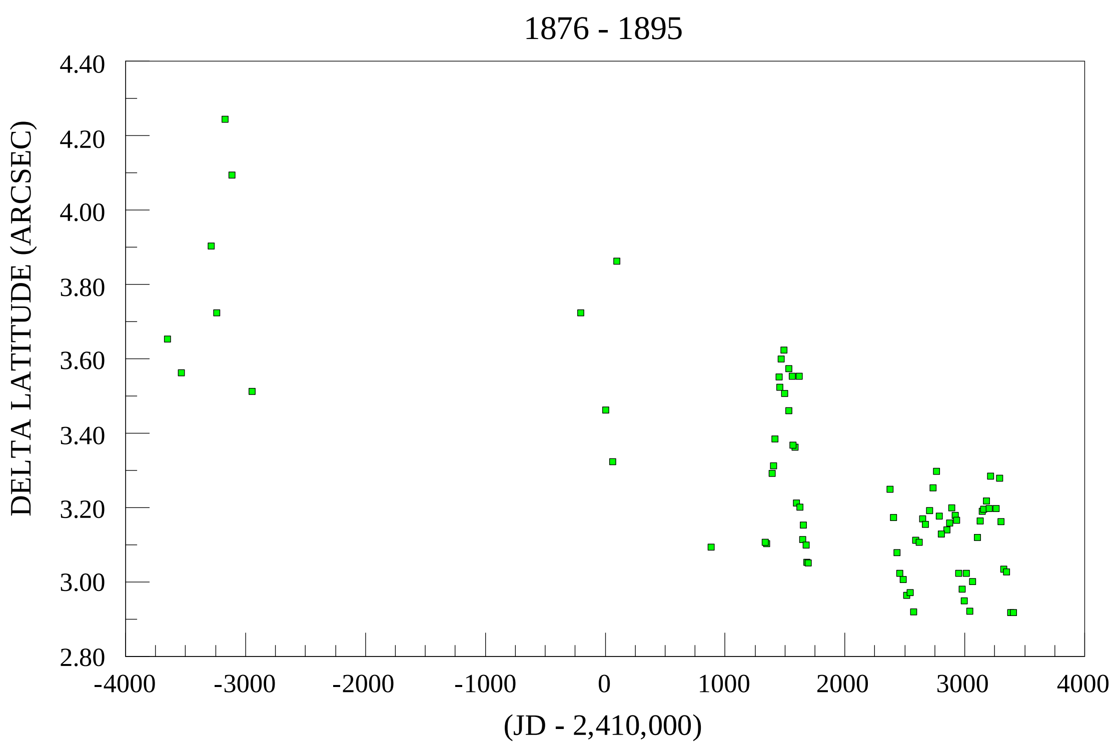


Fig. 21. The 1876-1895 record of latitude variability at Sayre Observatory by Charles. The secular trend is spurious but oscillations of credible amplitude and cycle length begin to be detected in 1889-1890.

These difficulties were undoubtedly caused by the limitations of an inferior telescope and by less-than-definitive declination values for the program stars. The constant of aberration (one of the separable unknowns to be recovered from the data) was also too large. This work culminates in Doolittle (1902a, b), delayed by his duties in establishing the new Observatory. The description of that particular effort appears in Doolittle & Doolittle and it is an expression of Charles's managerial capabilities that the entire establishment was dedicated and its coordinates established after only 2 years of construction and instrumental installation.

Even before the FO dedication, Charles had set about continuing his geodetic measures in the garden behind the Botany Building on the University campus. This jury-rig effort could not possibly reduce his errors and he soon stopped the work. At the new Observatory he had a better zenith tube than the Sayre one. Not only that, there was also money to pay Eric and H. B. Evans as assistants. With the meridian circle and zenith tube he could now determine stellar equatorial coordinates locally rather than depend entirely on the work of others. There recurred persistent problems with the stability of the eyepiece micrometer just as he had experienced at Sayre. Damage by a storm caused the zenith tube to be entirely rebuilt around a larger objective and some time later he contracted for a second and different instrument – the Wharton Reflex Zenith Tube, named after the physician donor and built to a 50-year old design by G. W. Airy. This telescope was large – a pierced 8-inch, f/12.4 doublet with a beam folded by a mercury pool. With two instruments, two data sets could be taken nearly simultaneously and thus, in principle, reveal the instrumental errors of each telescope. A series of very discursive papers (Doolittle 1903, 1905a, 1908, 1912, which are replicated piecemeal and more cryptically in early volumes of *The Astronomical Journal*) reveals his 35-year concentration on latitude variation and may be considered a fine, early contribution to the quantification of the phenomenon. His expectation for simultaneous observing was not, however, consistently validated. The latitude-variation curves are consistently in phase for the two instruments, but the zero point and amplitude of the variability are not the same over an interval of some years. His aberration constant also remained too large by about 0.3% and so his horizontal solar parallax was too large by about 0.2%. Sources of error among the observing series with his four different instruments are seated in the possibility of poor proper motions for the stars, the observing protocol itself, a troublesome mercury reflector folding the beam in the Wharton instrument, and the method of analysis. Küstner's observing procedure was followed faithfully so as to separate, in principle, the aberration and latitude variation components but there was never a least-squares analysis to treat a data set in a unified way so as to verify that there were no correlations between the unknowns. Whatever may be criticized about Charles, it cannot be his dedication to research: past age 70 and in all seasons, he was still hustling from one instrument to the other as fast as possible throughout every workable night.

Eric G. Doolittle (1869-1920), a founding member of the AAS, was trained and worked briefly as a Civil Engineer and then taught mathematics at Lehigh University and the University of Iowa. In 1895 he matriculated at the University of Chicago for astronomical studies, wrote a master's thesis that was apparently acceptable as a PhD dissertation, but did not complete the requirements for either degree. Instead, he accepted an Instructorship at Pennsylvania in 1897 and enjoyed the usual succession of academic promotions. Upon Charles's retirement in 1912, Eric succeeded him as Director and Professor of Astronomy and third Flower Professor. Eric is the top figure in this group photo from the 1916 meeting of the AAS at Swarthmore College. There are no really satisfactory images of this man but there are numerous testimonials to his engaging personality, uncertain health, dedication to teaching, and inability to decline requests for personal or professional favors. When S. W. Burnham retired from Yerkes, he passed on to Eric all his catalogued materials on visual binaries. This was no sudden decision for Burnham (1902) had praised Eric's early work unstintingly. Too old to serve actively in World War I, he founded, at the University, the U.S. Shipping Board Navigation School for proper instruction of mariners. Progressively worse heart disease killed him just a year after Charles died. *The Evening Bulletin* considered his death front-page news. The portrait shown just above is from 4 years before his death. It is pleasing to read that the Trustees voted on January 16, 1922 to make up for a year the difference between

$1,200 [$12,900] and the salary of Eric's widow as a University library worker and to award this sum to her.

There were two main components to Eric's scientific career. The first part played out in celestial mechanical perturbation problems in the inner Solar System. Hill (1882) had presented his method of calculating secular perturbations (the kinds of effects with which Rittenhouse's mechanical almanac, of course, could not cope) but there had been no extensive application of the procedure until Eric's work. Beginning with Doolittle (1896) and ending with Doolittle (1905c) he presented a series of 12 papers calculating the secular perturbations of each planet by all the others. This culminated in a substantial monograph (Doolittle 1912b) pulling together all the calculations for Mercury through Mars and comparing them with the orbital parameters. Eric's interpretation is a commonsensical one showing concern for the errors in the planetary masses, which errors, of course, had to propagate into errors of the calculated perturbation terms. Even after he had reckoned the effects of these problems, discordances remained in three details: (1) the perihelion advance rate for Mercury, (2) the perihelion advance rate for Mars, and (3) the rate of motion of the node for Venus. The last publication of this series ends with Eric beguiling himself with calculation of perturbations exerted by a model of the zodiacal dust cloud in order to try to explain these discordances. Today, it is clear that the matters troubling him have other explanations. In the first place, his synthesis preceded the theory of General Relativity by 4 years. Secondly, the discovery of Minor Planet 433, Eros in 1898 shortly offered a way to improve planetary masses beyond the values that Eric used and, after that, planetary masses would not be improved significantly until the *Pioneer* and *Voyager* interplanetary missions many decades later. Thirdly, the real errors of old astrometric series are just now being appreciated, as, *e.g*., in the recent omnibus paper by Kolesnik and Masreliez (2004). In a second career Eric prepared an astonishing series of monographs. In these (Doolittle 1901, 1905b, 1907, 1915b, and 1923 posthumously), he records filar micrometric measures of more than 5,900 visual binaries and visual multiples, each one measured a few times. It is true that many of these objects are not limiting close pairs in terms of the Flower refractor's resolution and so may not have been the most stressful at the telescope but this physical effort of about 20 years and the folding of his own program with Burnham's were remarkable achievements. Any reader of all his works has to be impressed by the superabundant observing energy and unremitting attention to detail in calculations which were all done by hand arithmetic and logarithms. How did a man in fragile health do it and not scant his administrative and academic duties?

Charles P. Olivier (1884-1975) had a social conscience formed by the unacceptable end of the War Between the States and remained marginally reconstructed all his life. He held early appointments at Lick Observatory and Agnes Scott College and was a civilian scientific staff member in the Anti-Aircraft Division at Aberdeen Proving Grounds, MD during World War I. From there he was appointed at the University of Virginia where he worked on both stellar and Solar System problems. It was on the basis of his remarkably varied and successful early career that he was recruited as the fourth Flower Professor and served almost 25 years in this position. He was an effective undergraduate teacher. #In one story he recounted how, from Mt. Hamilton, he had seen Venus set and rise three times before finally setting, all within one minute. This was intended to get across the message that the world is not one to be understood with simple textbook models. He also took pains to modernize the Russell, Dugan, and Stewart *Astronomy* textbook explaining how the march of science had left behind quite a bit of its second volume but that no better could be found in English in the 1940s. Time and again, he emphasized the problems of reconciling the time scales from Earth, stellar evolution, and the expansion of the Universe. By the 1940s he was enduring a neurological disorder which apparently affected only his right arm but made his handwriting very angular.# In 1954 mandatory retirement for age caught up with him and he left some of his correspondence to the APS. At age 90 he still worked weekly in his departmental office and had not aged greatly compared to the portrait shown here which was taken in his home study around 1940.

When he arrived at the FO Olivier instantly electrified the dome motions and provided electrical power to the circles and focal plane of the refractor. His early and continuing work at Flower resulted in a number of papers (*e.g.*, Olivier 1957a) on visual binaries whose impact is smaller than that of Barton's. Much more profit should have resulted from his decision in 1929 to broaden the Observatory's research. He decided to concentrate on the general domain of intrinsic and geometric variable stars and purchased a wedge photometer from the J. W. Fecker Company. The device bore a strong resemblance to the one illustrated in Fig. 3.10 of Hearnshaw's (1996) book. The Fecker wedge was not really satisfactory and Olivier had a grad student make two others with ranges of 6 magnitudes. With this improvement, the photometer was used on both the Flower refractor and its finder and the entire program lasted more than 22 years. Olivier (1940), himself, was primarily responsible for the magnitude scales of the comparison stars. His own Argelander-type step had a value of about 0.10-mag and was apparently very consistent and stable. Many staff assistants and grad students participated in the wedge photometry but Olivier still made a major observing contribution himself. The final publication in the series is Olivier (1957b). In addition, he made numerous naked-eye estimates of bright variables – a hobby he had started as a boy and continued all his life. His final publication on the subject appears as Olivier (1952).

Olivier published *Meteors* in 1925 and *Comets* in 1930 and these well-regarded compendia stayed in print for some years. His interest in comets was the inevitable adjunct to the main concentration of his life. In 1911, he had founded the American Meteor Society (AMS). In *Annual Reports* and *Meteor Notes* (many of which appeared in *Popular Astronomy*) he collated an untold number of typically handwritten reports of sightings and observations of naked eye and telescopic sporadic and shower meteors, bolides, meteor trails and explosions, and meteorites and meteorite craters sent him by AMS members and others. The observers were men and women from all over North America, a few from Europe, a certain small number from India, Japan, New Zealand, Australia and South America, and officers and enlisted men on U.S. Navy and merchant marine ships. The membership included professional as well as amateur astronomers and casual observers.

Fig. 22. Olivier and John A. Kingsbury, a Wall Street MD, pretending to observe a Leonid at the latter's summer home in the Catskills in 1931. Olivier holds a protractor for estimating the trail's angular length. Kingsbury characterized his partner's coat as a moon suit and it would be interesting to know what he thought of his own costume. This image appeared as the cover photo for the December, 1931 issue of *The Amateur Astronomer*. It cost 15 cents.

With a phenomenal load of postal correspondence, Olivier made an unremitting effort to create a constituency of citizen scientists to serve a neglected domain of physical science. His attempts to enforce uniformity on the field reports, to work toward completeness of description, and to foster an inquiring attitude among both domestic and foreign members show consistently among his commendations, hopeful wishes and cajolings. One indication of the random errors with which he had to deal appears in Watson & Cook (1936). Of course, few of the AMS publications passed under the eyes of a referee and he was really the only functionary of the Society. Nonetheless, the papers read credibly and his was a personality that loathed sloppy science. Until wide-field photographic monitoring statistics were generated, as from the Prairie Network, he had compiled the world's largest body of meteor information. His only contemporary with a comparable accomplishment was Cuno Hoffmeister. Olivier was also acutely aware of the need for physical understanding of meteor phenomena as when he took pains to recommend a paper by Skellett (1935) on the ionization state of meteor trails.

One indication of Olivier's standing in contemporary U.S. astronomy is the agreement with Frank Schlesinger whereby he would assume responsibility for working up and publishing the measures by W. Lewis Elkin of Yale. The latter was apparently the first person to design and build hardware that would record meteors photographically using constantly rotating shutters in front of a telescope objective. The plates could be analyzed to recover the linear velocity and position of the meteoroid, to measure the deceleration of the particle as it passed lower into Earth's atmosphere, and to determine radiants and orbits of shower meteors. Olivier's (1938) presentation saved for the future Elkin's pioneering work over the 1893-1909 interval. Nationally and internationally, Olivier was a significant scientist. He was an AAS Councilor for one term and he must be one of a small number of people to have been President of the bodies that evolved into two different IAU Commissions.

Olivier's intention to act promptly on the bequest to the University of the Roslyn House Observatory equipment was forestalled by World War II – there was neither staff nor money to implement the acquisition. By the end of the war he had convinced himself and the University that the only proper decision was to scavenge useful equipment from both the FO and Roslyn House and to relocate the salvable instruments at a new site further removed from Philadelphia's light pollution. This task was left to his successor but reports for both places begin in 1948 while Olivier was still Director. He seems to have intended to re-locate the refractor to the new site in order that the visual binary program might be upgraded.

F. Bradshaw Wood (1915-1997) was a transitional photometric worker – one of the few people who were equally capable in visual, photographic and photoelectric measures (Wood 1946). An intercollegiate swimmer, he was a Research Associate at Princeton and then Assistant Professor at the University of Arizona after World War II service as a U.S. Navy navigator flying PBY missions in the Pacific. Recruited to Pennsylvania in 1950, he was Executive Director of the Observatories until 1954. Promoted to Professor in that year, he finally became the fifth Flower Professorship in 1958. He held the post for 10 years and then resigned to go to the University of Florida. Wood had to oversee the closure of the Flower and the Roslyn House facilities, storage of some of the apparatus (*e.g.*, the Flower refractor), the selling of some more of it (*e.g.*, the Flower universal instrument), arranging long-term loans for still more equipment (*e.g.*, the 10-in astrograph from Roslyn House), and the commissioning of the new combined Observatory.

#Wood was a spontaneous type of teacher who did not evidently prepare his class presentations other than at red light stops on the morning commute# but he throve on committee work – in one particular year he served on 17 of them. Obviously, this could continue only for a brief interval. At times that were critical for the departmental and Observatory's well-being, he was a forceful and intelligent and generally successful advocate. #Wood dealt not at all in small talk and, to the best of my knowledge, never cracked a joke during the workday but he had an ironic appreciation of numerous faculty members. One day he came back from a committee meeting in a bitter mood. They had wasted two hours trying to define *moral turpitude* as the basis for denial or removal of tenure: at the end of the meeting the precept was that wives were fair game but hands were to be kept off the grad students and, if you violated at least the last part of the statement, you were guilty. It took him many years to get over the war and well into the 1950s he was not keen on German astronomers' participation in the IAU. He also had deliberately to rationalize admission of Japanese grad students on the grounds that they were too young to have been guilty of aggression.#

Wood believed in international observational collaboration because he was certain that single-station observing neglected significant classes of variable stars. For instance, should an eclipsing or pulsational period be very close to 1.000 days or a multiple of that number, a single observatory at a temperate latitude could not compile a complete light curve in a reasonable length of time. A grid of stations around the globe could, in principle, deal with the problem. He strove busily to foster links in Germany, New Zealand and Iran but these mostly came to naught. There was also a scientific impediment to his understanding of linking independent datasets. Because of his own photometric experience, he did not reckon with the differences of spectral response that always exists between two independent observational hardware systems. What is loosely called *photometric standardization* he did not understand.

If attention is confined to the four years between his appointment and the FO closing, he published at a much more than minimal rate. Many of these papers have not been of lasting significance but four of them made important contributions. In Wood & Roach (1951, 1952) there appears the first comprehensive photometric treatment of atmospheric stellar eclipses. It is safe to say that these results would not have been possible without the collaboration between the authors. *The Finding List for Observers of Eclipsing Variables, 3rd ed.* in Wood (1953a) built on the previous editions and had a very useful life in guiding observing programs. Lastly, his organizing an AAAS Symposium (Wood 1953b) in 1951 on photoelectric photometry and then editing the contributions promptly brought a comprehensive historical and current understanding of that specialty within the covers of a small volume. At the time, there was nothing to compete with this very fine work. In the remaining 14 years before he left for Florida, Wood published abundantly giving no indication that his administrative work impeded him but by 1967 he was burned out locally. He served one term as an AAS Councilor and was a founding member and then President of IAU Commission 42.

<u>Samuel G. Barton</u> (1882-1958) received his PhD in 1906 after graduating from Temple University as a Math major in 1903. His dissertation topic was *Secular Perturbations Arising from the Action of Saturn upon Mars – an Application of the Method of Arndt*. This is the most original of the early Pennsylvania dissertations. After 4 years at Clarkson College of Technology and two more at Swarthmore College, he was appointed to the FO staff in 1913. #His personality was a singular one – in everyday and classroom encounters a gruff and tactless man. It took a feeling of desperation to ask him a question in class and to endure the condescending answer if it actually came. The general impression was that he loved astronomical science but hated having to teach it to the average student. Yet, if you showed interest in celestial mechanics or spherical astronomy, he warmed to you. His view of fundamental astronomy was that it was not to be trifled with. He spent large amounts of time poring over Bowditch's *American Practical Navigator* and Smart's *Spherical Astronomy* to find the errors in them.# At the same time he could and did write gracefully about astronomical interests for the educated layman: *e.g.*, calendrical oddities, the origins of the names of the satellites of Saturn, Dürer's contributions to old star maps. #Well into his 60s he played tennis energetically with anyone whom he could find.#

Barton is the near-central figure in this group photo from the 1916 AAS meeting held at Swarthmore College. There is one close-up shot of Barton in files accessible to me but this one seemed more expressive of his career; by happenstance, Olivier is standing behind Barton and overlooking him. The better photo shows Barton as President of the Rittenhouse Astronomical Society in1927 when he was still only an Assistant Professor. Obviously, the local astronomers valued him more highly than did the University.

Barton never married and apparently made money by buying up rowhouse properties in West Philadelphia and then renting them. At his death it was discovered that he had given $150,000 [$1,120,000] to Temple during his lifetime and designated another $150,000 [$932,000] to the same institution in his will. The Samuel G. Barton Hall of Science, shown in Fig. 23, is the result. Of course, Barton's contributions were

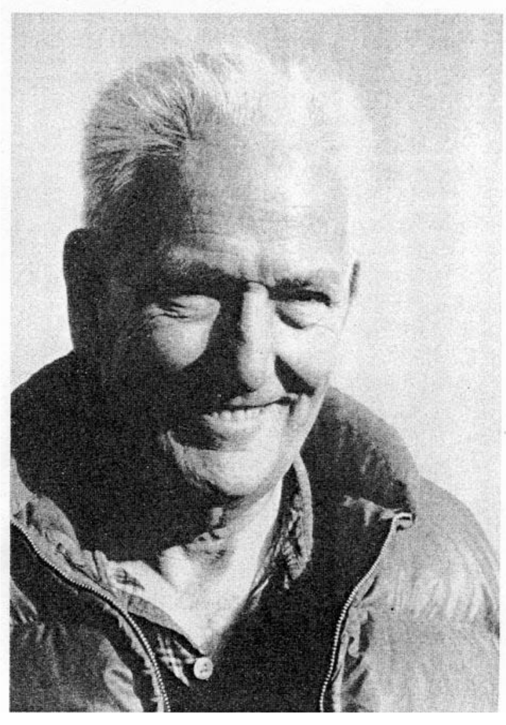
Fig. 23. A view of the Samuel G. Barton Hall of Science at Temple University. The Hall does not include the distant structure at the left edge of the view. A dedicatory plaque in the Hall lobby permits the misleading interpretation that Barton was a Professor at Temple.

only seed money for the eventual $4,000,000 building of 155,000 ft^2 but the nearly simultaneous effort to start the David Rittenhouse Laboratory (originally supposed to be called The Franklin Physics Building) at Pennsylvania could have profited from his money. He presented no motives for these donations but departmental speculation centered on the following. Barton admired Eric greatly and was disturbed by his premature death. Over the next 8 years, the University left him as the only faculty member to sustain astronomy as Acting Director and Chairman and offered no support for development. In 1928 Olivier was recruited as Chairman and Director and inevitably supplanted him. #Their relations were typically formal – in small adjoining offices for more than 25 years they spoke to each other only when necessary and used the Mr. form of address rather than Sam and Charles.# When Olivier got Barton promoted to Associate Professor, he received the response: "That's no promotion whatever!" Nonetheless, he would graciously pick up Olivier's classes when the latter couldn't meet them, actively supported the meteor program and was very sympathetic when Olivier's first wife died. It is known that Olivier tried repeatedly to get Barton promoted to Professor but was unsuccessful. Presumably, Barton felt no need to be grateful to the University but had an attachment to Temple. Olivier (1957a) actually wrote a very warm appreciation of Barton's work when he retired but couldn't or, at least, didn't go to his funeral.

It is possible that Barton is indirectly related to Rittenhouse through Esther's, David's sister, marriage to Thomas Barton (1731-1780). After Esther died, Thomas Barton married again. He and his wife being Loyalists, they left all the Barton children by his first wife more or less in the care of Rittenhouse's housekeeper when they departed Philadelphia for British New York City in 1778. Barton died there and Rittenhouse actively fostered the education and careers of the two older boys. The question of this possible tenuous relationship can be decided only by more genealogical research than I have felt motivated to undertake.

Did his personality complexes permit Barton to do reasonable astronomical research? Early on, there appear two papers on observed positions of selected minor planets, two more on artifices in plane geometry, and a manual for the U.S. Shipping Board Navigation School of which he was named Director by Eric. The manual was meant as a companion to Bowditch. Then there are 9 miscellaneous contributions of which 3 examples have been given three paragraphs above. The impression from this selection is that of an educated dilettante enjoying himself, but this is far from a comprehensive understanding of Barton. First, three more papers give determinations of declinations of more than 1,000 stars – an effort not for the faintly committed when each determination typically embodies 4 measures with the zenith tube. #But it was his admiration for Eric that led him to a lifelong concentration on visual binaries.# The primary expression of this attitude is the massive effort of 14 papers (*e.g.*, Barton 1937) in which he trawled through Zone Catalogues for known or possible visual binaries. The major criterion for new candidates was a separation of 5" or less with some weight given to a plausible magnitude difference. For some zones accessible at the latitude of Philadelphia he then made new micrometric measures as well. Not only was this work pursued assiduously from the mid-1930s through 1950, some of it was supported financially with extra-departmental funds. Presumably these monies were used to pay the assistants whom he acknowledges. Lastly, he (*e.g.*, Barton 1932) observed hundreds of binaries with the filar micrometer on the Flower refractor and discovered not a few new ones himself. He had to have been a very concentrated worker and perhaps it was this trait which led him to disdain the casual undergraduate.

<u>John B. Irwin</u> (1909-1997) was appointed Assistant Professor in 1946 and #it quickly became clear that his and Olivier's concepts of what was and was not important astronomically could not be reconciled. This apparently happened because the pre-appointment negotiations between them failed to examine that prospect deeply enough. Olivier's mistake of judgment was that he believed that he was taking on a junior colleague whose reputation was already founded in photoelectric observation of eclipsing binaries so he should logically be bringing very considerable experience that he would want to continue and that could also be exploited by advanced students. Irwin, on the other hand, was not prepared to continue what he had already accomplished but intended rather to move into more generalized understanding of the possibilities of still better observing practice.# Despite Olivier's expectations and demands, Irwin failed to observe when scheduled. #He also did not get along with the grad students whom he expected to do the observing for him.# It was inevitable that his service would be brief but it was not insignificant. The Irwin (1947) tables for least squares improvements of eclipsing light curve analyses were intended to be implemented on electro-mechanical desk calculators. A year later Irwin (1948) was addressing the possibilities of using the EDVAC machine at the University for a variety of mathematical, astrometric, photometric, stellar statistical, celestial mechanical, spectral line identification, and radial velocity reductions and analyses. Eventually some unspecified uproar came to the attention of the Dean's office and, shortly thereafter, he left for the University of Indiana spending the rest of his career there. This included serving a term as an AAS Councilor. His finest late accomplishments are the forceful recognition of the value of cluster Cepheids for the cosmic distance scale (Irwin 1961) and his visionary paper, *The First 70,000* (Irwin 1960). It is finally now, after many decades of minimum timings of close binaries, that Irwin's (1952, 1959) formalisms for analyzing light-time effects



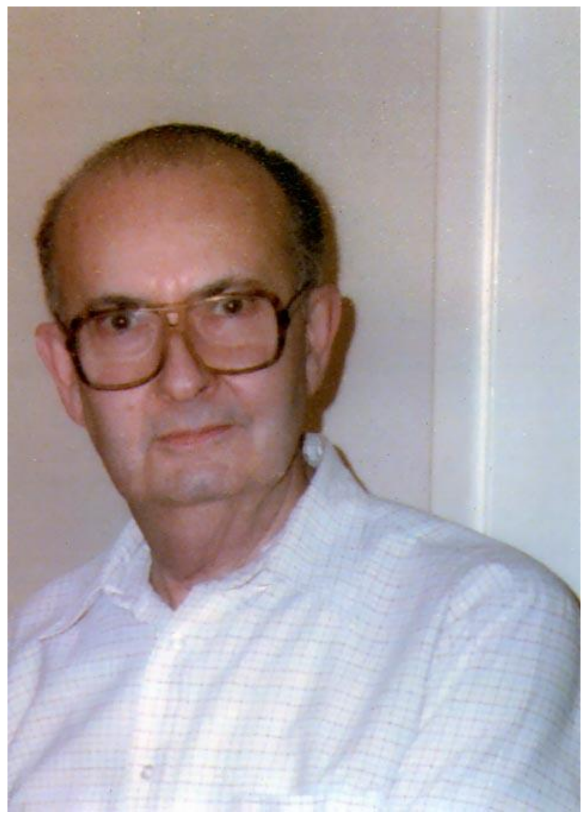
in (O-C)-patterns are demonstrating their value with numerous applications to triple and higher-order multiple stars. The photo was taken sometime around 1980.

Leendert Binnendijk (1913-1984) wrote a dissertation, *A Study of Stars in the Pleiades Region, Based on Photographic Magnitudes, Colour-Equivalents, Spectral Types and Proper Motions,* supervised by Ejnar Hertzsprung. This appears as Binnendijk (1946). As a young man, he had a life more adventurous than most: #a courier for a Resistance newspaper, he narrowly escaped a Gestapo search while on a delivery round and he endured the horror of watching some countrymen starve to death during the "hunger winter" of 1944-1945 when the German Army continued to occupy major parts of Holland after the failure of Operation MARKET-GARDEN. He arrived in the U.S. in 1947 with one valise. As far as he knew, his most important possession was his tuxedo since his Dutch mentors had told him it was indispensable in North America.# After a year as Lecturer at Swarthmore College, he spent three more at Carleton College but was ready to leave after that time. #For the last two years of publication of *Popular Astronomy* he was the *de facto* Editor of the journal due to the illness of C. V. Gingerich and this had taken time away from research. The Minnesota winters were more onerous than he had imagined and he was scandalized that an institution of higher learning would award a degree to a businessman whose only credentials were that he had donated a considerable sum of money to the place.# He did not leave behind his ideals and naiveté when he came to the University. Binnendijk was a self-contained, blunt-spoken but not aggressive man with a bit of humor: #he insisted that he was certainly not the most stubborn of all Dutchman since he didn't come from the Frisian Islands; we were made to understand that this remark referred to a rather well-known contemporary astronomer. Binnendijk also had a few curious tales to tell about Hertzsprung. He was not an inspiring teacher and, when he lectured during the winter in the library of the FO residence with its virtual heating plant, the grad students found it difficult to believe they had chosen a rewarding career.#

Between his arrival at the University and the closing of the FO he published nothing but he had already begun a life-long attachment to contact binaries and their modeling. His observations are very much still used and, quite likely, his most enduring contribution will continue to be the (Binnendijk 1970) sorting of contact objects into the A- and W-subtypes. It is still unclear if this represents a secular stellar-evolutionary effect or is a cyclical phenomenon so that a binary might move back and forth between the subtypes as it evolves, or if it is a superficial expression of the spottedness of a photosphere, or if is a consequence of the depth of the common convective layer.

#Binnendijk was a demanding *paterfamilias* giving his wife a specific household allowance and requiring the children to account weekly for every penny of their allowances, which were apparently quite ample. Although seemingly severe, he was, in fact, an admiring husband and enjoyed and encouraged Marthe's artistic talent. When she died, a lot of energy vanished from his life. Even so, he was looking forward to an active retirement. It would not be back to Holland for him since it wasn't the agreeable place that he had expected it to remain. Rather, he would stay in the States (although he never became a U.S. citizen) but would travel. Partly because of his own early education, he wanted to visit eastern Mediterranean sites to admire the remnants of their classical cultures. A blood platelet disorder suddenly took him off before he could realize that hope.#

William Blitzstein (1920-1999) #made many a ring of jeweler's rouge in the family bathtub as he washed his homemade optics and became the most experienced amateur in the silvering of mirrors in Philadelphia.# He accumulated a BA in Physics, an MS in Electrical Engineering, and a PhD in Astronomy but poor health and shortage of family money made his education a discontinuous process. A succession of teaching and industrial jobs provided enough money for him to attempt a PhD degree in astronomy that would exploit his engineering background. The development of the low-noise multiplier phototube and the availability of World War II surplus high-speed, digital counters from nuclear physics experiments caused him to conceive of a pulse-counting capability for astronomical photometry. In collaboration with I. M. Levitt, he began experiments that resulted in successful measures with the Flower refractor. Along the way, this work was materially assisted by R. W. Engstrom of RCA but it was Levitt's father-in-law who provided $1,000 at a critical time when the FO budget was very tight. At the 77th AAS meeting in Evanston, IL, the two men were personally encouraged by A. E. Whitford and published a brief note (Levitt & Blitzstein 1947) quickly thereafter. At about this time N. L. Pierce began an instrumental collaboration with Blitzstein in the Princeton Observatory workshop. The PhD dissertation *Photometric Researches, a Pulse Counting Photometer, the Eclipsing Variable XZ Andromedae* was finished in 1950. This work was published rather late (Blitzstein 1954) but probably represents the very first use of part of Merrill's (1950) Tables that were not yet in print at the time the dissertation was finished. Blitzstein held a succession of industrial and DOD appointments and consulting positions and became an Assistant Professor of Electrical Engineering and Astronomy just as the FO was being closed. His appointment and promotions in the Astronomy Department ran from 1954 until retirement. Internally he functioned as the expert in fundamental astronomy, celestial mechanics and atmospheric extinction, as an astronomical systems engineer, and as the most organized teacher of the staff.



Blitzstein was a conservative, self-effacing, gentlemanly scientist who tolerated neckties if he had to. #Not much amused him – he expected to see Murphy's Law fulfilled every day – and his only frivolous remark that I can recall had to do with a particular grad student: this fellow was about as efficient as a one-armed paperhanger with hives. He and I were the best of friends and ate lunch together almost every working day for nearly 30 years. From one year to another he offered various interpretations of his mother's arrival in the U.S. as a wetback. From childhood he had health limitations which were seated in hormonal problems – he believed that he was the first person in the country to have received doses of synthetic adrenalin. In very early 1999 he called to say that his system was shutting down and he was dead within days.#

THE SUPPORTING CAST

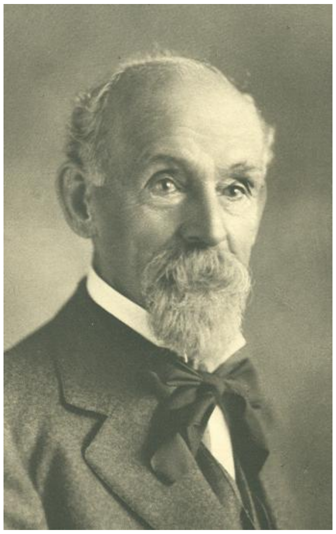
A small assortment of extra-mural astronomers and other people had some kind of association with the FO establishment, typically for a brief time. For some of these people I have been able to discover only minor contributions to activity at Flower but possibly the modest citations here will cause others to remember things that I haven't found.

R. G. Aitken (1864-1951) was an intermittent visitor to the FO drawn by the visual binary interests that he held in common with Eric. It is recorded in Aitken's (1932) catalogue that Eric asked for the promise that he (Aitken) would continue the card cataloguing of visual pairs should Eric be obliged to give it up. Within a year of this conversation Eric died and Aitken inherited all the documentation, most of which appeared in his extensive catalogue in some form or other. Of course, that catalogue also contains much of Aitken's own data and citation collection and he had the advantage of more than a decade after Eric's death to add to its contents. It isn't at all clear that, at the time of his death, Eric was intending to publish the contents of his catalogue in the near future.

Ralph B. Baldwin (1912-), the well-known expert on lunar surface features and origins, was an observing assistant at Flower over the 1937-1938 academic year. His primary responsibility concerned the light curve of the eclipsing pair TT Her which he accumulated with the wedge photometer and the Flower telescope stopped down to 11 inches. This work appears in Baldwin (1940) (by which time he was at Northwestern University) and shows that visual measures, when sufficiently

numerous and distributed with uniform phase coverage, are capable of leading to a credible binary model. One of the episodic spectral changes of γ Cas is summarized by Baldwin (1938) in a rather routine contribution but a more penetrating interpretation (Baldwin 1939a) followed. A somewhat more interesting effort shows Baldwin (1939b) wrestling with the physical characteristics of ε Aur. Baldwin also had a significant commercial career in the woodworking machine industry with the Oliver Machinery Company of Grand Rapids, MI, and nationally and internationally he was an expert in matters of product liability.

John A. Brashear (1840-1920) began as a 16-year old mechanical pattern maker in a steamboat engine



manufacturing works. Neither his posthumous autobiography (Brashear 1924) nor the somewhat sappy life by Gaul & Eiseman (1940) really explains how or why he sat out the Civil War. Testimony is clear that he was captivated by the night sky as a result of the friendly times spent with one of his grandfathers The story of his early life as a millwright and his first optical creations are well known. With assistance from the industrialist and philanthropist William Thaw Sr. and by choosing superb workers, he created the top factory for large astronomical optics in North America. The Flower objective is the mid-range of the lenses and mirrors that he designed and figured. Admired universally for his personal qualities and his civic and educational services, he received a very large number of honors. At the same time, his self-made character had to admire the efficiency of the occupying Japanese and to deplore the demeaned appearance of the conquered Koreans when he toured the Far East as an old man. He was a friend of Charles and a houseguest of the Doolittles a few times. His ashes and those of his wife and Keeler are immured beneath the Allegheny 30-inch reflector.

Gustavus W. Cook (1867-1940), a wealthy businessman and amateur astronomer, was inadvertently to contribute to the termination of the FO. An avuncular man who cultivated numerous hobbies amid his commercial obligations, Cook had disposable monies in large quantity all his life. After his introduction to rooftop astronomy with a small telescope, he was able to establish a personal observatory on his own estate about 3 miles from Flower. This had a small salaried staff to pursue Solar System and stellar observing programs and must have been the best amateur establishment in North America at the time. Around the mid-1930s Cook donated a 4-inch, Ross-lens astrograph to the FO and in 1937 told Olivier of his intention to leave all his instruments to the University. Olivier's original idea was to run the two observatories jointly but, as was noted above, he opportunely changed his plans. This story will be detailed in the next two chapters.

Alan E. Gee (1916-1991) was an U.S. Air Force colonel assigned to the Frankford Arsenal in Philadelphia during the early 1950s. He had served in World War II, passed through The Command and General Staff School at Fort Leavenworth, and then taken an MS in optical engineering at the University of Rochester. At the same time, Blitzstein was Electronics Scientist at the Arsenal for fire-control problems and #they found common hardware interests that resulted in a certain number of detectors and other hardware items migrating to the FO supposedly for testing. To the best of my knowledge, none of these items was ever returned.# They were presumably military surplus and some were actually put to scientific use. Gee was also interested in science for its own results and he, Blitzstein and Wood decided to observe the November 14, 1953 solar transit of Mercury with a voltage-tunable narrow-band *Hα* filter on the 18-inch refractor. They had already tested the filter and found prominences to be easily visible, but it isn't clear what motivated them to try the transit observations. One possibility would have been to see if the transit phenomena were more decisively timed when scattered light was greatly diminished. A second possibility would have been to attempt to observe an *Hα*-profile of the chromosphere by using the planet as a probe. A third option would be simply to learn the performance of the filter under the burden of the substantial solar heating. The event was lost to clouds.

Asaph Hall, Jr. (1859-1930), after retirement in 1929 from the USNO, where his father had made his name with the discovery of the satellites of Mars, was invited by Olivier to work as a guest and volunteer observer at Flower. At the USNO he had been concerned that the accomplishments and status of the civilian staff be recognized adequately. He did indeed move to the Upper Darby area, observed a bit by participating in the concentrated 1929 monitoring of the Perseids, and delivered a lecture of reminiscences about his life and the history of the USNO. His chronic ill health worsened shortly thereafter and he died at the Naval Hospital in south Philadelphia.

Carl Hammer (1914-2004), although born in Chicago, took his university degrees in Mathematical Statistics at Munich and then returned to the States. While a Senior Research Engineer at the FI in the mid-1950s, he was a volunteer variable star and visual binary observer at the Observatory. Both before and after that interval he held academic appointments. Hammer subsequently became an engineer first at Sylvania Electric Products, Inc., then at RCA, and afterwards held numerous other industrial positions in the States and Europe. Although he had changed job locations several times and moved far from Philadelphia, he continued to work with Olivier remotely. In the 1960s he and Olivier were writing back and forth about making machine-readable files of the observations of the intrinsic variable EZ Aql and Hammer apparently did this on his own time. He then completed several canned Fourier fits to the light curve of this star and the results (which are not very satisfactory) were published in an internal document at RCA. This early attempt at mainframe processing may have set in train Hammer's future career which was of more than a little consequence. From 1969 to 1971 he was President of the American Society of Cybernetics and in 1973 he was given the Computing Sciences Man-of-the-Year Award by The Data Processing Management Association. In 1979 he was elected Fellow of The Association of Computing Machines.

Reed Knox, Jr. (1916-1994) was one of the FO visual binary volunteer observers when he lived in Upper Darby, PA. This man had a BA in Mathematics from Stetson University in 1938 – the picture being taken from the Stetson yearbook −and then over 6 semesters from 1938 through 1954 enrolled in engineering and science courses at the University – but apparently not in astronomy ones. At the Alanwood Steel Co. in Conshokocken, PA he practiced the skills of a metallographer, an artisan who mechanically and chemically prepares metal surfaces for microscopic inspection. He put this skill to work as a short-term volunteer in the Laboratory for Research on the Structure of Matter after that lab was established in 1960. Subsequently, he was a volunteer at the Museum Applied Science Center for Archeology. He gave money (about $13,000 [$20,000] in securities) to each of these University entities but not to the Astronomy program. Knox was active in the RAS throughout his life and accumulated a collection of several dozen small meteorites by purchase and not by prospecting. Some of these

he gave away to friends, sometimes sectioning, polishing and etching them, but most of the collection went to the Geology Department of Bryn Mawr College. They are still there and used for instruction.

Knox came from circumstances different from everybody else in this story. Other than Cook, he must be the only present personality to have appeared in *The Social Register*. His father's antics as a young man were subjects of feature articles in *The New York Times* because of the prominence of the grandfather. That man was Philander C. Knox, the immensely wealthy and immovably conservative trust lawyer who was a U.S. Senator, Attorney General and Secretary of State over more than 20 years. According to Gaul & Eiseman, the Knox family came from the same small town as Brashear. It is not known why Reed, Jr. left the family base in western PA for college in FL and then his mature years around Philadelphia but perhaps it had to do with WW II service for he almost surely served in that war.

Donald A. MacRae (1916-), while still a grad student at Harvard, was offered a 9-month observing position by Olivier with very few duties at the FO. He has noted particularly how undemanding and gentlemanly Olivier was in his requests for assistance. Because MacRae's interests then were centered on a calibrated magnitude scale for photographic photometry (MacRae 1949), he could not fail to notice the unfavorable sky brightness at Flower by the early 1940s. He assisted Olivier in the preparation of two papers concerned with a bright meteor and a fireball. After joining Toronto in 1953, he developed his well-known career in galactic structure studies. The photo shows MacRae long after his time around Philadelphia.

Newton L. Pierce (1905-1950) received his PhD from Princeton in 1937 and by the end of World War II was an Associate Professor on that faculty. The photo is cropped from the 1949 picture of the Astronomy Department faculty. Pierce (1947) composed the second edition of *A Finding List for Observers of Eclipsing Variables.* Convinced that visual photometry would never surmount its accidental and systematic errors, he fell in with Blitzstein, then a University grad student, in order to design and build a simultaneous, two-source photoelectric photometer with an automated observing duty cycle. The two men brought complementary capabilities to this enterprise with Pierce offering work space and bench test equipment as well. #According to Blitzstein and Wood, Pierce had a fiery temper symptomized by his red hair (which they might have envied because both of them were thin on top) and didn't regard the senior departmental members with overly much respect if he thought they were wrong.# He died suddenly of heart disease before the total photometric system concept was fully operational. Lyman Spitzer, then Director at Princeton, quickly permitted a purchase of all of Pierce's hardware by Pennsylvania in order that the simultaneous 2-channel photometer be built. The Pierce-Blitzstein Photometer went through several modernizations during its 40-year existence.

Tobias Wagner (1793-1868) had such a successful auctioneering business with his brother that he retired in 1831 but continued to sit on boards of a bank and a fire insurance company. An 1845 booklet published anonymously estimated his inherited *plus* self-made wealth at $50,000 [$1,136,000] and described him as a good whig(*sic*). He had numerous benevolent and philanthropic interests and became a University Trustee in the 1840s serving for 24 years. At his death, he bequeathed $10,000 [$122,000] to his unmarried sister-in-law Elizabeth Rhoads. She, in turn, gave it to the University in 1874 in order to fund a perpetual memorial for Wagner. This memorial was to consist of a growing collection of newly bought books, maps and other printed material to be kept separate from the general library collection. In 1922 the Trustees changed the purpose of the endowment so that, at the choice of the Provost, about 30% of the income could be spent for lectures on astronomy. No reason appears in the minutes to justify this change. The motivation cannot have been anything so obviously dramatic as Hubble's recognition that the Universe was expanding or that Sun was primarily hydrogen or that Pluto had been found. All these discoveries were still in the future in 1922. In any case, The Tobias Wagner Lecture Fund was the result and it continued making money after the closing of the Flower establishment. There is no evidence that Barton or Olivier ever asked that its revenue be used for any specific speaker or purpose and the Fund was forgotten. At some undetermined time it was rediscovered by Wood and put to work. The Tobias Wagner Library Fund also still exists.

Doris M. Wills (?-?) was co-author with Olivier (Olivier and Wills 1933) of a brief paper regarding daily and monthly meteor rates and he gives her credit for approximately half the work. For a few years thereafter, she published meteor height determinations under her own name 8 times (*e.g.*, Wills 1935, 1936). It is, of course, likely that these were examined by Olivier before submission. During these years she worked as secretary and data handler as well. Never a student, she was nonetheless an AAS member. When she had twins, she resigned.

Joseph L. Woods (1890-1963) was an amateur astronomer (shown to the right in the fuzzy image) with a private observatory at Sykesville, MD who ran the Winchester & Woods, Inc. metals-processing business making toy watches and possibly other items. Assisted by another amateur, Paul S. Watson (1905-?), he made photographic star maps for Olivier. These two men were capable, industrious workers with a few publications (Woods 1936, Woods & Watson 1935) and probably no other astronomer ever made cross wires from the hair of the tail of a Persian cat as Woods did. Watson (1949) presented his own revision of Messier's Catalogue and for a few years (*e.g.*, Watson 1948) was responsible for publishing and interpreting *The Graphic Time Table of the Heavens* compiled annually by The Maryland Academy of Sciences. Each of the men contributed a few thousand estimates and measures to the AAVSO program and Olivier has written that some of their plates formed the basis of maps distributed by the AAVSO. Woods, Watson and another amateur P. G. Crout (1898-?) contributed photographic magnitudes of EZ Aql, which were incorporated into the Olivier & Others (1961) omnibus light curve of that object and the three men also participated in the AMS observing program.

Two other women worked as assistants apparently exclusively on the meteor program. Nancy Weber (?-?) was a summer 1948 assistant who reduced meteor reports. Then there was Evelyn E. Whelder (?-?) whose name appears only once as a meteor data reduction assistant. A small number of men contributed to the visual binary and photometric programs: Walter (?-?) and Ross P. Marsteller (?-?) worked at variable-star photometry for two years but made only a slight impact whereas John S. Stevenson (?-?) was a productive worker generating 6% of the variable star data over about 15 months. R. A. Binckley (?-?) lived in the township adjacent to that of the FO and participated in the sporadic and shower meteor program for some years. The further careers of these 6 people remain unknown to me. James S. Thompson (?-?) had worked at the RHO during 1939-1940 and was an undergraduate student in some department who was hired and then fired three times for inefficiency. Olivier must have been relentlessly hopeful and possibly it paid off for this man. He is perhaps the James S. Thompson, Class of 1939, who became an engineer for The Aerospace Corporation. A. Edmund Hayes (?-?) is only a name on one of Olivier's lists but he actually married Alice, Olivier's daughter by his first wife, and then left science entirely.

GRADUATE STUDENTS

The activities and careers of graduate students can give an indication of the vitality of an observatory and academic department. Not many students were awarded graduate degrees during the active lifetime of the FO. The earliest dissertations were apparently printed by Philadelphia or Lancaster, PA printing firms only for internal use and personal distribution. If a PhD dissertation was subsequently published in the conventional astronomical literature, that citation is noted below. For students who received degrees not later than 1954, I give a brief interpretation of each dissertation and remarks about their careers. The basis for these remarks is partly the presence or absence of citations in the *Astronomischer Jahresbericht*.

R. Stanley Alexander (1909-2005) was awarded a Graduate Assistantship after obtaining an M.S. degree at the University of Kansas. His initial duties were to assist Olivier with Argelander estimates of long-period variables but this ended after only a few months and Alexander was free to concentrate on his own work. His dissertation *Photographic Photometry of the Eclipsing Variables AD Andromedae, V343 Aquilae, and ER Orionis* was based on photographic measures and estimates with the 4-inch Ross/Fecker camera that had been donated by Cook. This was published quickly as Alexander (1940) and was correlated with P. H. Taylor's visual measures of the same stars. After finishing his degree, he spent an interval in defense work and then went to Washburn University, Topeka, KS where he worked the rest of his career teaching the entire suite of astronomy and physics course offerings and serving as departmental chairman for a length of time. Despite a high teaching load, he sustained a research career emphasizing light curves of eclipsing variables. A very congenial man, Alexander has filed a letter of his recollections of the FO and some of its staff members. His characterizations of Olivier and Barton show humane understanding of their personalities.

William E. Anderson (1875-1960) arrived with a B.A. from Wittenburg University, OH in 1902 and eventually presented a PhD dissertation *Determination of the Mean Declinations of 136 Stars for the Epoch 1912* in 1913 while serving one year as Instructor of Astronomy. This work surveyed selected near-zenith stars over 7 hours of Right Ascension with the Wharton Reflex Zenith Tube. The observations resulted in the calculation of significant declination components of proper motion for 10 stars after comparison had been made with earlier zone catalogue results. The dissertation was supposed to lead to follow-on measures for parallax determinations of these 10 stars but there is no indication that Anderson attempted to do this. In actuality, the focal plane scale of the Flower refractor is not so favorable for parallax work as that of the Sproul refractor at Swarthmore College. It is not known why Anderson would have been attracted eastward for grad school when he could have found a satisfactory mid-western institution just as easily and he was certainly older than the norm when he finished the dissertation. He subsequently became a faculty member at the institution that is now Miami University of Ohio and was still attending AAS meetings as late as 1941. One cannot be surprised at anything that shows up on the Web but it was still an eye-opener to find his 16-page dissertation there and even more startling to see that it had had 25 hits before I found it. Who could have been interested in those long-ago observations?

Krikoris G. Bohjelian (1889-1951) obtained a PhD in 1915 as a result of determining anew the station's longitude from lunar occultations of 13 stars with the Flower refractor. The title of the dissertation is *Observation and Reduction of Occultations of Stars by the Moon*. With the availability of timings of the same events by Asaph Hall, Jr. at the USNO, Bohjelian generated very small corrections to the lunar orbit and a problematical correction to Moon's semi-diameter. The root of his analytical problems apparently rests in the poor conditioning of the unknowns in the equations of condition and the mathematical tactics that he used to overcome this limitation. An acceptable determination of station longitude did result. Since, however, no parameter errors were calculated, the significance of even this result is unclear. Apparently he (Bohjelian 1915) published only one more paper, this concerning the secular perturbations by Jupiter on the minor planet 153, Hilda. This paper ends with a curious Editor's Note: "Because of travel impediments, it has unfortunately not been possible to check a few of the paper's numbers" (my translation). This may refer to the 1914-1915 warnings by the British and German governments of the dangers of traveling in the English Channel and the North and Norwegian Seas. It may also imply that the *AN* could find no European, and specifically German, referee familiar with Hill's development of Gauss's method of perturbations and didn't want to waste time with a 2-way Atlantic checking of the manuscript.

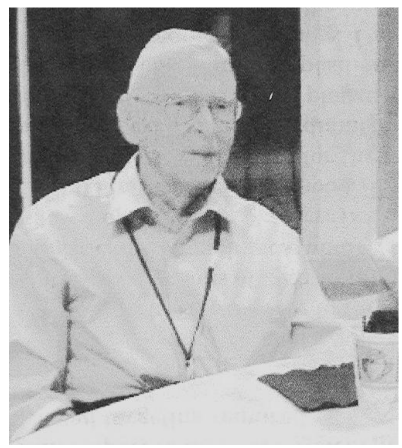
This man, of Armenian Turkish descent, had a harrowing beginning to life – when he was 6 years old, his father was sadistically murdered during an Ottoman pogrom. American Congregationalist missionaries accepted him into their orphanage and eventually set in train his emigration to the U.S. at age 16. He was sponsored by a first cousin operating a tailor shop in Philadelphia for whom he worked as a teenager. It seems surprising that a non-WASP would be accepted to the University in those days but apparently Bohjelian's academic record was compelling. After his PhD, he worked as a WW I defense team member developing gyroscopic–control applications for navigation and then taught both Mathematical Physics and Mathematics to pre-med and med students for a few years. Resigning from the University, he went into business with a brother and eventually sustained The Photo-Electric Engraving Company successfully until his death.

C. H. Cleminshaw (1902-1985) served as a variable star observer during the 1935-1936 academic year but obtained no advanced degree. By 1942 he was training Army Air Corps navigators at the Griffith Observatory and Planetarium in Los Angeles, CA. He was Director there from 1958 to1969 during which time he trained 26 of the Apollo astronauts in star identification and celestial navigation. Cleminshaw presided over an impressive modernization of the Griffith facility and was a most effective popularizer of astronomical and space science. Essentially all of the foregoing may be found in Hansen, Wang, & Cook (2003), who also note that Cleminshaw had a degree from the Harvard Law School and actually practiced for a few years before seeing the night light.

Henry B. Evans (1871-1962) was at Lehigh when Charles was recruited to the University and came along with him. He was Instructor in Mathematics and Astronomy from 1895 through 1901 even though the departments had been administratively separated in 1899. For a while, he lived in a small apartment on the second floor of the Director's residence and was on familiar terms with the members of the Doolittle family. He received the first departmental PhD degree in 1901 after submitting the dissertation *Mean Right Ascensions and Proper Motions of 254 Stars*. As a first evidence of scholarly work at the Observatory, this is a presentable piece of work. The dissertation shows a reasonable awareness of errors and their evaluations. Working with Charles, he measured the equatorial coordinates of the selected stars, presumably with the Flower meridian circle and zenith tube. His other work concerned positional measures of two comets in 1898 (Evans 1899a) and a tentative fit to the observed fraction of the orbit for the tight visual binary Δ15 (Evans 1899b). In this year he joined the AAS and continued his membership at least through 1931 but did not come to the 1934 meeting of the Society in Philadelphia. He accepted an appointment in the Mathematics Department in 1904 and served until 1942 with, apparently, particular interest in vectorial handling of engineering problems. For one year he was Dean of the Towne School of Engineering.

John W. Evans (1909-1999), while a grad student from 1932 to 1934, worked on the variable star program but made only a small contribution to it. Not unreasonably, he decided that he could profit more from a larger and more vigorous graduate department and transferred to Harvard. From 1938 to 1942 he was a faculty member at Mills College publishing on interstellar lines (Evans 1941) and the distribution of interstellar dust and stars in Cassiopeia (Evans 1938). He had subsequent appointments at Minnesota and the High Altitude Observatory helping to install its first coronagraph and became the founding Director of Sac Peak Solar Observatory. In the picture he is shown measuring pieces of natural quartz possibly for use in the optical system of a coronagraph. Evans was a scientist greatly admired by his staff and general acquaintances in solar science.

Fred G. Fender (1908-1976) was a volunteer visual binary observer from the Engineering School (where he picked up an MEE in 1930) for about a year beginning in 1929. Olivier also notes him as an Astronomy grad student but I found no confirmation of this statement. He probably was quite a good observer, for he records using a magnification of 423X very frequently and he also participated (Fender 1929) in Barton's scrutiny of zone catalogues – unlikely unless Barton trusted his work. Rather ungraciously, he refused to be a paid observer apparently disliking the idea of closer supervision and direction and transferred to Physics, although the University records show that his PhD is in EE. In principle, the dissertation *On the s-States of the Two-Electron Atom* in 1936 could have come from either matriculation. It was all of 16 pages long. Fender eventually became a faculty member at Rutgers University, and after World War II he held several positions in the aircraft industry and then taught again at Rutgers.



Walter H. Haas (1917-) was briefly a grad student and also a variable star observer but for only 15 months. Still, fully 19% of the measures over that time are due to him alone. Although he has consistently described himself as an amateur astronomer, he has actually taught mathematics repeatedly, first at Mt. Union College and The Ohio State University, where he obtained an MA in that subject. Partly because of the common interests in meteors between Lincoln La Paz of OSU and Olivier, Haas was induced to become a graduate assistant at the University in 1941. For more than 2 years he handled navigation and the associated math classes in the U.S. Navy V-7 and V-12 programs. In his teens he had done some estimates on the AAVSO program but his major attachment to Solar System interests finds expression in the large numbers of papers (*e.g.*, Haas 1938, 1944) on all things lunar and planetary beginning in 1937. As late as 1950 when Haas was associated with La Paz of the Meteoritics Institute located at the University of New Mexico and was teaching math at UNM, he and Olivier were still corresponding about mutual interests. In 1947 he became founding President of the Association of Lunar and Planetary Observers. The Association currently has more than 550 members and Haas continues to serve on the Board of the Association.

Edith D. Kast (1880-1967) wrote the dissertation *The Mean Right Ascensions and Proper Motions of 130 Stars* in 1909. This work was not based on Flower observations but rather on reductions of already catalogued positions for stars that Charles was using for his studies of variation of latitude. No errors are given for the proper motions. This is the weakest of the early dissertations. A departmental record says that she also completed an M.A. but I have found no trace of this effort. She had come to the University with a BA and ΦBK from Marietta College, OH so she must have been uncommonly intelligent. Kast was elected an AAS member in 1904 but apparently didn't pay dues after that year. In 1907 she married Leon W. Hartman, who had completed his Physics PhD at the University in 1903 on spectrophotometry of the emissivity of a Nernst glower as he varied the current. The marriage apparently signaled the approaching end of her scientific career. The Hartmans seem to have moved to NV and raised two children. Edith died in Reno so it is possible that the family lived in that town thereafter. It isn't obvious why a young student from Ohio would come to the University for graduate work but there may be a hint in the Bethlehem cemetery where Charles is buried. A nearby plot is that of a Kast family and it might be imagined that these people had some association with the family of the same name in Ohio and also had good relations with the Doolittles.

I. M. Levitt (1908-2004) went on the Conway, NH eclipse trip of 1932 described earlier in this text. He next spent the 1935-1940 interval working at Roslyn House with a spectroheliograph on an international solar flare patrol program. Thereafter, he assisted Blitzstein in the testing and fabricating of the prototype single-channel, pulse-counting photometer. An indication of the rudimentary hardware for their earliest successful experiments appears in Fig. 25. His 1949 dissertation *Photometric Research on the Eclipsing Variable ZZ Cassiopeiae* was based on visual measures with the wedge photometer mounted on the Flower refractor. Levitt calibrated the wedge with a then-new 1P21 photocell and the thesis work was published quickly thereafter (Levitt 1949). It appears evident that Levitt could have had a significant research career had he wished but he decided to spend his life in public education and joined the FI staff with duties in the Fels Planetarium. This was a perfect match of interests, personality and duties, for Levitt prospered in the situation and became Planetarium Director for many years. He records the nearly simultaneous high and low points of his life when in 1950 he was granted a meeting with Albert Einstein in the hope that a planetarium show could be developed around the astronomical consequences of GR. This conversation went nowhere but Einstein, interested in Olbers Paradox, asked if the pulse-counting photometer could measure the background sky light. The simple answer is "Inevitably!" but in his over-enthusiasm Levitt found himself explaining the physics of the photoelectric effect to the great man. Einstein's interruption: "Yes, yes I know." Levitt was a publicly visible scientist in the Philadelphia area speaking in person and on TV and writing abundantly for the newspapers. In 1952 he originated the concept of an Earth-orbiting metallized balloon, which was realized later with the *Echo* satellite. Near the end of his career, Levitt was Executive Director of the Mayor's Science and Technology Advisory Council and spent a year as Senior Lecturer in Astronomy at the University. More than a public scientist, he enjoyed translating English texts into Russian and Japanese.

A. Hewlett Mason (1905-1974) was a grad student and staff observer on the Flower double star program from 1929 into 1931. He had been one of the first two grad students recruited by Olivier. His single astronomical publication records that he had vision limitations (unable to use magnifications greater than 212X) but he had many other health problems too and Olivier did not re-appoint him in 1931. Perhaps because Olivier thought him very intelligent, Mason was permitted to return eventually for his PhD and presented the dissertation *A Study in the Range of Solution of Orbital Elements of Minor Planet 534, Nassovia, on Short Arcs, by the Method of Laplace* in 1953. This is a curious piece of work built around 6 positional measures from the Perkins Observatory of the minor planet taken at its 1947 opposition. The measures were made on only 3 nights and Mason selected one from each night and solved for the orbit and then the remaining 3 measures and solved for the orbit again. It is hardly a surprise that the two orbits are not in complete agreement but there is no discussion of reasons for the differences. It is not a very impressive effort and is the last dissertation awarded before the end of the FO. Subsequently Mason worked for The National Geodetic Survey, The Maritime Commission, The National Bureau of Standards and the U.S. Navy and Army.

Roger C. Moore (1925-1988) and Martin E. Nason (?-?) were originally variable star workers with the wedge photometer. They then turned to photoelectric work with the first Blitzstein photometer and departed with MA degrees after developing a joint work (Nason and Moore 1951) on timings of minimum light of eclipsing variables. Moore joined the National Bureau of Standards for a time. In the early1960s he was part of the process charged with JPL development and choice of on-board instrumentation for the Mariner B mission to Mars. One finds him corresponding with Abe Silverstein and Homer Newell about accepted and rejected instrument packages, but ultimately the project went nowhere because of troubles with the Centaur rocket. In the late 1960s up to 1970, he was a staff member in the Planetary Sciences Department at The Rand Corporation writing an assortment of studies (mostly with Gerhard F. Schilling) concerned with multiply-scattered light seen from interplanetary vehicles around planets with atmospheres. The last of these Rand efforts appeared in Freeman, *et al.* (1970) and is a logistical study of operating a lunar astronomical facility. The authors concluded that this would be a doable task in a couple of decades. He then moved on to NASA where he is said to have become Head Planetary Scientist for the agency but I have been unable to verify this assertion. I was not able to trace the later stages of his career and he seems to have died before retirement. Moore appears to have been a versatile physical scientist. I have been unable to follow Nason after he left the University.

Edith F. Reilly (1917-1988), apparently gregarious when young, took a BA degree at the University in 1940 and then had a brief affiliation with Olivier in a comparative study (Reilly 1944) of the (then) four known methods of determining heights of meteors. She also worked as his daytime assistant on the variable star program and subsequently as a mathematician at the Frankford Arsenal, achieving an MA in 1943. These attainments are shaded by Bok & Reilly (1947), which empirically defined the structures now called Bok globules. Perhaps because she endured a physical infirmity, Reilly had not the most winning of personalities. Olivier believed her incapable physically of completing the PhD program and refused her admission as a doctoral student. Harvard came to the same conclusion and, still later, Wood made the same decision.

Johnathan T. Rorer (1871-1948) received his PhD in 1910. He presented a dissertation *Definitive Elements of Comet 1898X (Brooks).* In fact, his is a parabolic orbit and, although it agrees well with an independent analysis by Sharbe (1904), Rorer gives no errors for his parameters. His attempt at an elliptical representation failed despite the fact that Sharbe had already shown his own elliptical fit to be better than his parabolic one. Rorer became a math teacher in the two best schools of the Philadelphia public school system and President of the RAS in 1935. His photo shows him at that time and his retiring presidential address appears as Rorer (1937). A founding member of the AAS, he was actually present at the dedication of the FO. He continued his interest in astronomical topics all his life for he appears in the photo of the 65th AAS meeting in December 1940. In the small world of middle-class Philadelphia, coincidences are to be expected. For example, Rorer lived less than 150 feet from the home where Flower died.

Philip H. Taylor (1905-1971) was a grad student contemporary of Alexander and observed meteors for a few years. He had already started observing visual light curves of eclipsing binaries when Alexander arrived. His PhD degree was awarded for *Visual Photometry of the Eclipsing Variables AD Andromedae, V343 Aquilae, and ER Orionis,* which analyzed these light curves and was published as Taylor (1940). Taylor (1941a, b) appears to have been interested in anomalies of close binaries rather than in the representation of textbook light curves. His interpretation of light curve asymmetries, founded on the idea of tidally-forced resonant oscillations of stars, was novel for the time. A similar commitment to physical explanations for astronomical phenomena appears in Taylor & Olivier (1941) where he is trying to impose different nuclear reactions on the cool supergiant EZ Aql in order to explain the details of its variability. He was apparently unable to find an academic or research position and worked partly as an engineer and partly as a staff scientist in the aircraft industry. As late as 1953 when he was an employee of Northrup Aircraft, Inc., he was continuing his astronomical interests and a study (Kaufold & Taylor 1955) of photoelectric detection of stars in the daytime sky at Flagstaff appears in the *Proceedings of the Flagstaff Conference on Photoelectric Photometry*.

Arthur B. Turner (1872-1948) taught mathematics at Temple while he was a grad student. He presented a dissertation *Secular Perturbations Arising from the Action of Jupiter on Mars* in 1902 and was granted a PhD for the work. The work appears distinctly less original than Barton's, with which it is otherwise comparable. He was Eric's student, published his dissertation (Turner 1906), had an appointment at Lick, and joined the CCNY faculty. It is possible to trace his astronomical career through 1916 when he (Turner 1916) was discussing what he called an effect of the Equation of Time. It might more properly be called an inevitable effect of the orbital motion of Earth. He had previously published a very nice orbit for the SB1 object ω Dra (Turner 1907a). There were other papers on the concavity of Moon's orbit toward Sun (Turner 1912a), the spectroscopic determination of the

solar parallax (Turner 1912b), and the maximum brightness of Venus (Turner 1914). Overall, there is an impression of a man enjoying himself without greatly taxing his powers. He was still paying his AAS dues in 1931. In 1939 he retired from CCNY after teaching mathematics for 35 years.

William P. Wamer (1908-1964) #was considered by Olivier to be a brilliant student, the first he had recruited# and for two years he was an observing assistant on the visual binaries program and he participated in the AMS program. (Unaccountably, his name is sometimes misspelled by Olivier as Warner.) The FO's practice was to hold weekly open nights for walk-in visitors and at one of these events Wamer's observing book of 175 measures and a summary of all his accumulated work disappeared. It wsa never recovered and he started again. His 1936 dissertation *The Eclipsing Variable SX Draconis* was built upon blue-filtered, wedge-photometer measures taken with the Flower refractor. It includes two limiting analyses for this complex Algol-type object. The work was published privately in the same year. Wamer finished his education when the Depression was at least not becoming more difficult #but his family disapproved of scientific interests. Even so, Olivier succeeded in obtaining for him a position at the USNO. Unfortunately, he succumbed very shortly thereafter to a mental illness – probably a short-lived depression − and was not able even to begin a career.# Despite this setback, he lived a fruitful life in South Carolina. A newspaper correspondent for *The Charleston News and Courier* for 29 years, he served as President of the local Lions Club and Vice-President of the Dorchester County Farm Bureau at different times. He and Mason apparently kept contact with each other until Wamer's death.

Balfour S. Whitney (1903-1993) made his first astronomical contribution (Whitney 1936) with a detailed description of a method to compute the heights of meteors. He was an effective photometric observer and it was he who made the successful wedges for the photometer. Family demands terminating his graduate study before he could matriculate at Berkeley for that PhD program, he secured an appointment at the University of Oklahoma. During World War II he served 3 years in the U.S. Army Signal Corps. He must be one of the few men who published a paper (Whitney 1945) while on active military duty. The subject was the complicated, slow-mass-exchange close binary QY Aql for which he had both visual and photographic light curves. Both observational error and the system's complexity prevented a satisfactory solution but he showed understanding of these problems. His paper, however, provoked an immediate and rather graceless comment by Henry Norris Russell (1945): "Captain Whitney has presented his results so concisely that he has hardly done justice to his own methods." Russell's re-consideration of the light curves did not improve on Whitney's results. After the war Whitney continued his career at Oklahoma where he taught astronomy and worked tirelessly on variable stars. More than 12,000 plates were photometered for light curves and timings of minimum and maximum light. His last publication (Whitney 1978) can be consulted as an illustration of all the preceding ones. After retirement he suffered the handicap of increasingly failing eyesight.

Raymond H. Wilson, Jr. (1911-1989) showed awareness of the importance that relative radial velocity measures can have for proper control of a visual binary orbit determination. An MA thesis developed around this concept was published promptly (Wilson 1933). His 1936 dissertation topic is *Interferometric Measurement of Double Stars with an 18-inch Refractor*. This inventive work implements a focal-plane-slit assembly mechanically coupled to an eyepiece micrometer for the purpose of observing very close visual binaries and is published in Wilson (1936). The paper lays out very nicely the advantages and limitations of interferometric observing and remarks on the increasing brightening of the night sky at the FO. Four years later Flower and Sproul Observatories shared his affiliation in four consecutive papers (Wilson 1942a, b, c, d) on visual binary orbit determinations. For some time Wilson had been receiving some support from the APS. Still later, he was affiliated with the Mathematics Departments of Temple and the University of Louisville but still collaborating with Olivier. Much later and with his productivity accumulating all the time (but not detailed here), Wilson's (1950) presidential lecture to the RAS gave a very nice summary of what stellar interferometry had produced in the interval. Still later, Wilson (1954) developed a different eyepiece interferometer and was receiving ONR support.

It can be seen that the first PhD degree was awarded only four years after the founding of the FO. The eight degrees given before 1916 were not followed by others until the 1930s. Most likely, this can be explained by the 8-year interval when Barton was the sole faculty and staff member and the following few years when Olivier was just getting started. The sample is not large but Barton and Blitzstein should be counted as well. The later students showed intellectual vitality and, had the Depression and World War II not lost productive years for some of them, their records would certainly be more abundant. About half of them had academic appointments, sometimes in unpromising situations, and had to make a sustained effort to keep their scientific interests alive. Most of those who followed non-academic careers seem to have done well and appear to have been good scientists.

A few MA degrees were given after Olivier came. It is possible that earlier ones have not come to my attention. Authors, titles and dates of award appear in the following list:

William Blitzstein, *Vector Methods in the Discussion of the Curvature of the Geocentric Path*, 1947;
Israel M. Levitt, *The New Combination Spectrohelioscope and Spectroheliograph for the Cook Observatory*, 1937;
Alvin H. Mason, *An Investigation to Determine the Position of Radiant Points of Fireballs*, 1931;
Roger C. Moore, *Improved Light Elements for WW Cygni, SZ Herculis, W Ursae Minoris, and RZ Comae Berenices*, 1951;
Martin E. Nason, *Results from Recent Photoelectrically Observed Times of Minimum of Four Eclipsing Variables*, 1951;
Edith F. Reilly, *A Statistical Study of Meteor Heights*, 1943;
William P. Wamer, *Study of Meteor Observations Made by a Group of Amateurs*, 1931; and
Raymond H. Wilson, *The Orbit of Σ2294 Computed by the Thiele-Innes Method*, 1933.

It is not out of place to note that only a few Bachelor's degrees were granted over the lifetime of the FO. Reilly has already been mentioned but Sarah Lee Lippincott (1920-) was a much more significant scientist. Her careful and abundant studies of astrometric binaries wherein one recovers simultaneously the parallax, proper motion components, visual binary parameters, and the evidence for astrometric companions are very well known.

SCIENTIFIC AND GENERAL APPRECIATIONS

This section contains an attempt to weigh the programmatic results over the lifetime of Flower Observatory. It is intended to be a synthesis orthogonal to the individual contributions that have already been described. There is no point in presuming that the FO work is all old hat, for it must be judged both in the framework of its own time as well as by what it contributed to the total ensemble of knowledge by the present day. My understanding is highly personalized and it must be limited because I am not expert in all these specialties.

Celestial Mechanics

Eric's work was inventive enough to be easily publishable and Moulton (1914) comments very favorably on it. Eventually electromechanical calculators and digital computers permitted calculation of effects that were not possible in his day. There is, however, no indication that his results were ever incorporated into the planetary positions in *The American Ephemeris and Nautical Almanac*. Instead, the USNO continued to use, *e.g*., Newcomb's older results from 1895-1898 most probably because Eric's results were consistent with Newcomb's and not a significant improvement of them and, after all, Newcomb was USNO Director for many years. Eric's studies were really tests of Hill's methodology and showed that it worked. In this field, nothing would change until radar ranging led to improved orbits and artificial planetary satellites and flyby trajectories were developed for improved masses and orbits.

Eclipsing Binary Research

The foregoing shows that Alexander, Baldwin, Blitzstein, Irwin, Levitt, Taylor, Wamer and Whitney all spent at least part of their efforts on photometric observing and/or analysis of close binaries. Since neither Barton nor Olivier – the two faculty members active over that time – had competence in the specialty, motivation and some supervision for all these efforts had to have been external. This was indeed so with a number of acknowledgements of gratitude to R. S. Dugan, J. E. Merrill, Pierce, and Russell, all of Princeton. Certainly in this subject these four men were the top workers in the world at that time but the record is silent on how their supervision and advice originated. Olivier's correspondence (File B/OL 45 at the APS, files in the University Archives, and departmental files) reveals nothing. Presumably some arrangement was made verbally.

One may ask if the Flower contributions resulted in valuable close binary advances. All these workers generated visual or photographic light curves of only limited precision and accuracy until Blitzstein compiled his unfiltered photoelectric data for XZ And and this was not done at the FO. Two of the stars chosen by Alexander and Taylor suffered from the fact that no model adequate for the contact configuration existed at the time and the insightful concept of simultaneous photographic and visual measures to obtain the ratio of mean stellar surface brightnesses was not powerful for those two systems. They did have success with V343 Aql and this was quite an accomplishment. Baldwin's choice of TT Her was an excellent one and his measures are comparable to the best visual ones which have ever been made. The very complex systems observed by Levitt, Wamer and Whitney could actually have been modelled better with more precise and accurate measures but even so the Russell Model is inadequate for them. Despite, *e.g*., Stebbins's (*e.g*., 1915) very early work, photoelectric photometry was not above Olivier's horizon until Blitzstein began his experiments. Thereafter, he fostered Blitzstein's and Levitt's efforts as well as money permitted. The other feasible options for Olivier to cause better light curves to be observed would have been by more thoroughly calibrated photography or by buying or building an instrument similar to Dugan's polarizing photometer. He took none of these options at Flower. A fair assessment would be that all these men were creating light curves at about the level of most other observers in North America and Europe and having comparable success in modeling. Time has left their analyses behind and new modeling codes extract more cogent stellar parameters from modern light curves. Their efforts do survive in the histories of period variations to which the old light curves continue to contribute and in the motivation of newer observers to make better light curves. A more positive conclusion can be drawn about Irwin's work for it looked ahead more than 10 years and might be said to underpin a large amount of light curve analysis and synthesis to the present day. It is true, however, that Snowden & Koch (1969) give one of the very, very few applications of Irwin's least squares methodology and even this was not done on a desk calculator as he intended originally but on a mainframe. Wood's (and Roach's) contributions are something of an anomaly in that the interpretation of ζ Aur was actually completed before he arrived in Philadelphia.

Fundamental Astronomy

It is not to be expected that the casual astrometric work at Flower could compete with what was being done at the fundamental astronomy stations in Europe and at the USNO. Most of the local effort was due to graduate students with no external experience or guidance, the data strings customarily did not close around the sky so that internal error determinations hardly existed, and it was really intended just as input to Charles's calculations. No one continued Charles's program after his retirement and it would have been impractical to attempt to do so. The suburbanization of the neighborhood and development further to the west meant a significant increase in motor truck usage of PA Route 3 and this increased when the road was paved in1922. The astrometric instruments were not adequately isolated from the traffic vibration.

Geodetic Studies

Charles was working on a modern, important problem which Chandler, Küstner, and he had helped to define. He was an insightful scientist who could develop a resourceful team and keep driving it by his own example. For the early work on latitude variation, his observations provide one baseline set of measures. This subject continues to be important with ever-widening applications and increasing instrumental capability. Anyone in doubt about this matter should read the entirety of Dick, McCarthy, & Luzum (2000). A summary of his work with his two telescopes appears in Fig. 24. . It is easy to see that he must have been greatly encouraged by his beginnings with the Wharton instrument; he would have said that his reasoning in using two instruments was correct. With the second cycle of latitude variation, however, the two sets of data fail to accord. The record indicates that he persisted in trying to understand the continuing inconsistencies and finally in 1910 managed to obtain compatible results. Shortly afterward he announced that he would not continue with the Wharton instrument for the foreseeable future. Photographic reflex zenith tubes were developed successfully not too long thereafter.

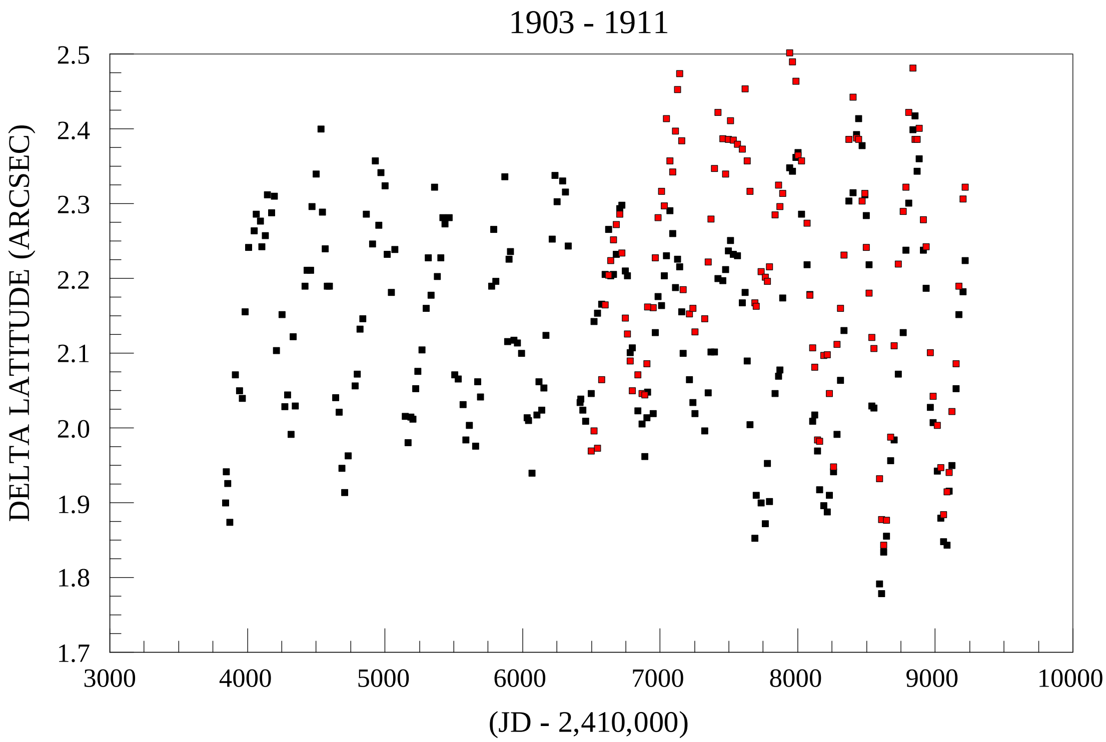


Fig. 24. The latitude variability records with the Zenith Telescope (black squares) and the Wharton Reflex Zenith Tube (red squares) over the last 8 years of Flower Observatory monitoring. Note that discrepancies persist for three cycles before the data sets finally mesh.

Instrumentation

There can be no doubt that Charles, Wilson, and Blitzstein were all first-rate inventors of novel astronomical hardware and showed years of tenacity in understanding their instruments and improving them. Each of these men also had the capability to describe and implement the application of their instrumentation and to enunciate the scientific value of their developments. The case for Charles has been indicated in Fig. 24.

Wilson found himself in a physical situation where he could reasonably expect sympathetic intellectual support and possible collaboration at first hand from both Barton and Olivier. Both people were experienced visual binary workers and additionally there was the Sproul program not too far away. The scientific appeal of his work would be to see if he could turn up binary stars with components too close on the plane of the sky to be seen and measured with the filar micrometer. His instruments were small and cheap to make and could easily be maintained, but there was a conceptual barrier against capitalizing on his insight: interferometry was a hard sell to many people because it demanded familiar understanding of physical optical effects. Wilson obviously made an attempt to spread his word by his activity in the RAS but there seemed to be no secure future in the Philadelphia area and he was fairly itinerant for most of his career moving in and out of academic and research appointments.

Knowing of the work of his contemporary Joel Stebbins, Olivier could have invested early in photoelectric photometry but that was not within his own experience so he didn't do it as early as would have been rewarding. In the mid-1940s he found money to feed into Blitzstein and Levitt's experiments as is suggested in Fig. 25.

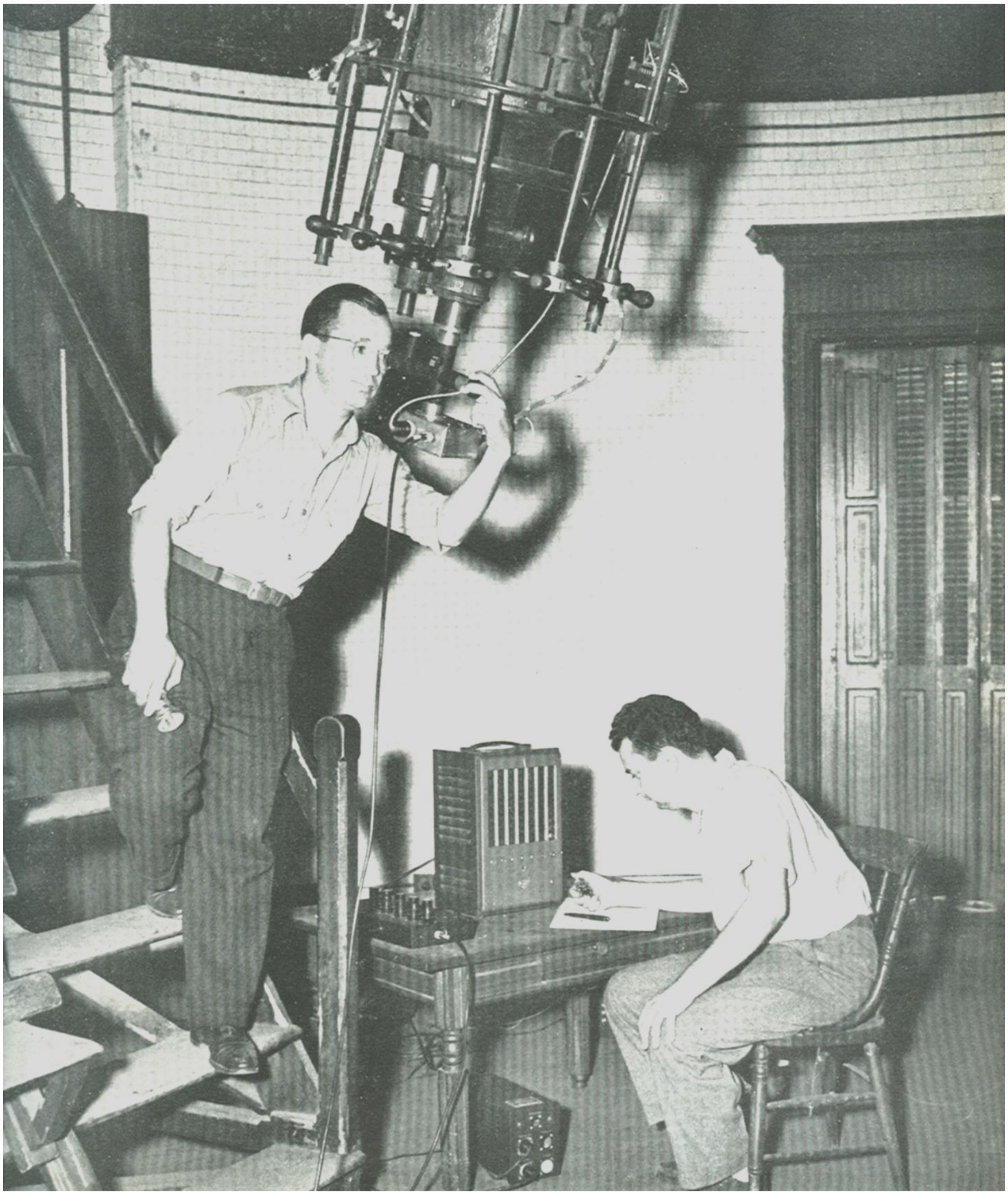


Fig. 25. In this posed and floodlit photo Levitt is pretending to have centered a star in the diaphragm of a single-channel, pulse-counting photometer mounted on the Flower refractor. It is daytime outside but a flashlight is still a useful prop. Blitzstein is purposely illustrating most of the laboratory tabletop experimental elements of a successful system: a stop watch standing in for a timing circuit, a small neon lamp counter for the digital displays, and a notepad for the eventual hardware recording the digitized time, counts, star codes, and filter codes. The power supply is on the floor and the amplifier and pulse-height discriminator at the left edge of the table. This picture is the cover image for *The Pennsylvania Gazette* issue of October 1947.

Afterwards, Olivier made up for his own lack of expertise by recruiting first Irwin and afterward Wood. With the latter man, he chose well as can easily be seen by looking anew at Wood (1953).

Intrinsic Variables Photometry

Olivier intended to broaden the FO mission by buying the wedge photometer in order to set in train the intrinsic variable program. This was something which he knew from his own background and it was a way to use nights when the seeing was not good enough for visual binary measures. The modern impression is of a program on a miscellany of about 100 objects with no defined precept for their selection. Far too many of the measures are incidental ones with no element of completeness to the light curves. The years of work made little impact.

What might have been accomplished is shown by the visual estimates and measures of EZ Aql in Fig. 26.

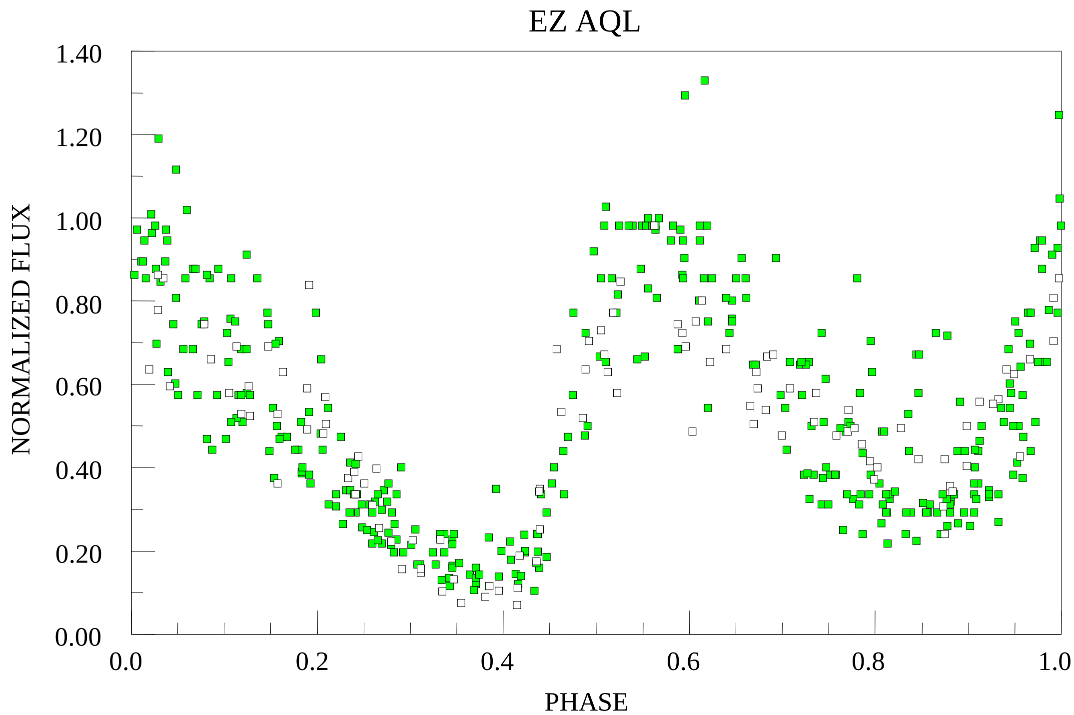


Fig. 26. The visual estimates (green squares) and measures (open squares) for the intrinsic variable EZ Aql transformed into a normalized flux scale. In magnitude scale the variability ranges from about +10.5 to about +13.0. Phases have been calculated from: Maximum Light = 2,428,596.45 + 38.64*E*. Numerous observers contributed to each annual data set over the interval 1930 through 1939.

Clearly there is a small number of inconsistencies among the 367 estimates and the 107 measures (which overlap in time with 185 of the estimates). These discrepancies would have to be resolved in detail but high-frequency stellar transients or extraordinary atmospheric conditions are not the causes of them. Rather, familiar observational noise and reading or transcribing mistakes must be the problem. According to the SIMBAD database, this is the only light curve of the variable with dense coverage that has ever been published although timings of minimum light have been made by other workers at other times. The light curve is impressive with normalized light dropping to about 10% of the maximum light value. There are also blue and red observations that are not shown here. The most remarkable thing about the light curve is its stability over 10 years. This star is a low-mass giant variable of the RV TauA-type, which class is poorly understood. Through the cycle, temperature changes by about 1,000K and the surface gravity by about a factor of 100; the star is coolest and the gravity smallest near minimum light. According to Jura (1986), such stars are post-AGB objects with fast mass loss now finished. They may or may not be going to pass through the planetary nebula stage on the way to becoming white dwarfs. The compilation by Percy, *et al*. (1997) shows that EZ Aql has the least random period variations of all 15 members of the class that they studied. Consequently, it is the most favorable of all such variables for recognizing the secular evolutionary changes that should take place on time scales of a few thousand years. It is possible that this light curve is worth modeling even now, 60 years after its compilation. Had Olivier concentrated on, say, two objects per season, the intrinsic variables program could have multiplied its contribution several fold.

Meteor Studies

Of course, Olivier's work does not dominate modern meteor studies but in his day he was *the* worker who kept insisting that this was important Solar System science. The character of his work is that of the solidification of a scientific specialty and he also deserves credit for bringing Elkin's technique and results to broad attention.

Visual Binaries Astrometry

After more than 200 years of visual binary studies Eric and Barton rank 13th and 25th, respectively, in the number of measures made during their careers. This comparison includes workers with modern instrumentation which is much more efficient than the Flower visual detections. The internal precision of their results is quite satisfactory: when objects with separations between 1" and 2" are sampled, the 1σ - errors for one measure are about ±(0.11"/1.19º) and ±(0.10"/1.95º) for Eric and Barton, respectively. In both coordinates Olivier's measures are about 30% noisier, possibly an indication of the tremor in his arm and hand, but possibly also he shouldn't have disparaged Eric's and Barton's deliberate pace of observing. I have not investigated their systematic errors. At first thought, it might be considered surprising that none of these men attempted photographic observation. While they could certainly have diminished their errors for objects with separations greater than about 2", the station seeing and the turbidity of the old emulsions would have worked against increased precision for closer pairs. Few orbits are ascribed to them. Nonetheless, in the long-term, their observations live because they will contribute to the orbits that will be generated eventually. Of course, some of their doubles will turn out to be optical. Wilson's much closer pairs were also very well measured. This entire program has to be counted an enduring success with one qualification. In the end, all the measures can generate only relative orbits so that individual stellar masses cannot be a product of their work. It is as if, for the pairs with separations above the size of the seeing disk, photographic detection and plate measurement had never been developed to isolate the separable contributions from parallax, proper motion, and absolute orbital motions.

In sum, no new branch of astronomical science was discovered or invented at the FO. Newcomb implicitly predicted this in his inaugural speech. Large-aperture telescopes were already the driving instruments of the science and Flower's was small compared to the prime telescopes at Lick (36-inch in 1879 and 1888) and Yerkes (40-inch later in 1897 when Newcomb also helped to inaugurate the station). Furthermore, the first Mt. Wilson reflector was just over the horizon. Even had the entire Flower bequest come to the University, it is doubtful if this would have led to a refractor of more than 24-inch aperture. Had a reflector design been chosen, a 30-inch instrument would have been possible with the actual endowment and it would have had a longer useful life. Support for this opinion exists in the 30-inch Keeler reflector, now modernized and re-instrumented and capable of credible research. To the small light-gathering power of the refractor add the staff's persistence in visual detection and only late invocation of photographic and photoelectric detection and the impossibility of spectroscopy, and the net impact of the establishment could have been predicted from the start.

To do more and more diverse astronomical science, more money would have been needed. In the general intellectual and philanthropic climates of Philadelphia through 1950, no citizen came forward to support this type of science for its own sake.

The staff and students certainly included a share of very inventive people and these were supplemented by a comparable number of tireless workers who channeled their careers more narrowly. Most of the latter identified themselves as observers, perhaps captivated by the romance of working on the night sky and interrogating it but not dedicated to the analysis of the results that they accumulated.   It should also be recognized that all these people were over-extended in the usual manner of academic researchers and that major amelioration of the FO's limitations could have followed by doubling the faculty and staff size.

THE UNIVERSITY ENVIRONMENT

As scientific administrators, Charles, Eric, Barton, Olivier and Wood are very mixed. Partly because of money and perhaps because of his personality, Barton had no chance to develop the FO but there is no evidence that the other Directors had uncommon and enduring difficulty with the University administration. One reason could be that they didn't push hard enough. Olivier's judgment was questioned because of the fuss over Irwin and he had to wait a year before recruiting Wood but that is the only incident on record. Charles and Eric showed no interest in developing any program that was not their personal ones. Similarly, Irwin, Blitzstein and Binnendijk did not foster teams to implement their own interests though the latter two men could and should have done so. The dissertations supervised by Charles are not so impressive as those done under Eric. Almost certainly, this can be traced to Charles having no grad school exposure of any kind. Eric and Olivier did make significant changes in their own careers, recreating and broadening themselves successfully. Only Olivier and Wood understood that scientific breadth had to be cultivated. Olivier was frustrated in large part by World War II when he and Barton had to teach large classes of U.S. Navy V-12 students as well as those in other military courses and most of the grad students had left for military service or war work. It is very likely that Olivier suffered the most frustration. Knowing the research already accomplished at Roslyn House, he must have imagined that after 1940 he could add spectroscopy to the Observatory toolkit. Wood did succeed in realizing photoelectric photometry because he could capitalize on Blitzstein's developments but he did nothing for spectroscopy. In peacetime, none of the teaching faculty suffered from excessive classroom loads: typically two courses or sections per semester and with quite modest enrollments. The University even tolerated class registrations as low as two students for intermediate-level courses.

It was inevitable that the original Flower endowment should endure fluctuations and these are shown in Fig. 27.

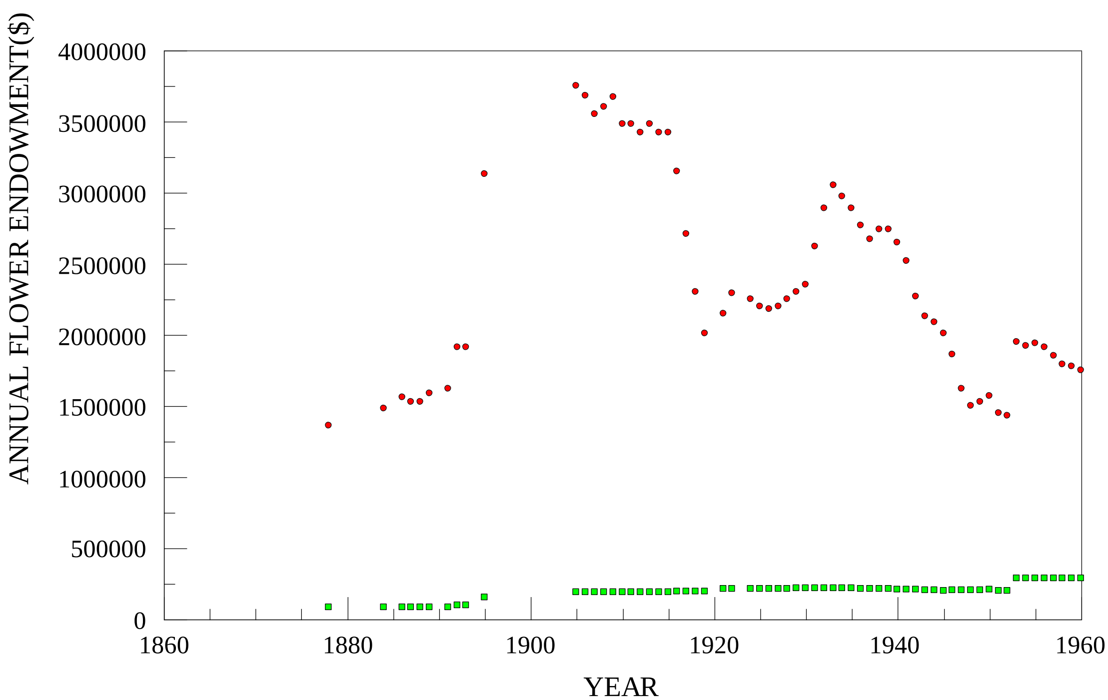


Fig. 27. The real-time (green squares) and normalized-to-2002 (red symbols) principal of the Reese Wall Flower Endowment Fund until 1960. The initial endowment is for $84,596 [$1,360,000]. Possibly the uptick around 1922 is due to counting the Tobias Wagner Fund as part of the Flower endowment. If this is so, it was only band-aid relief from the deterioration of the value of the Fund.

Eventually, after about 1920 the investment committee arrested the deteriorating financial situation and by the end of the decade had improved the portfolio considerably. It can be seen, however, that the endowment never came back to its 1905 purchasing power. The history of the income from the Flower Endowment Fund is shown in Fig. 28. By 1922 the Trustees noted that income from the endowment was

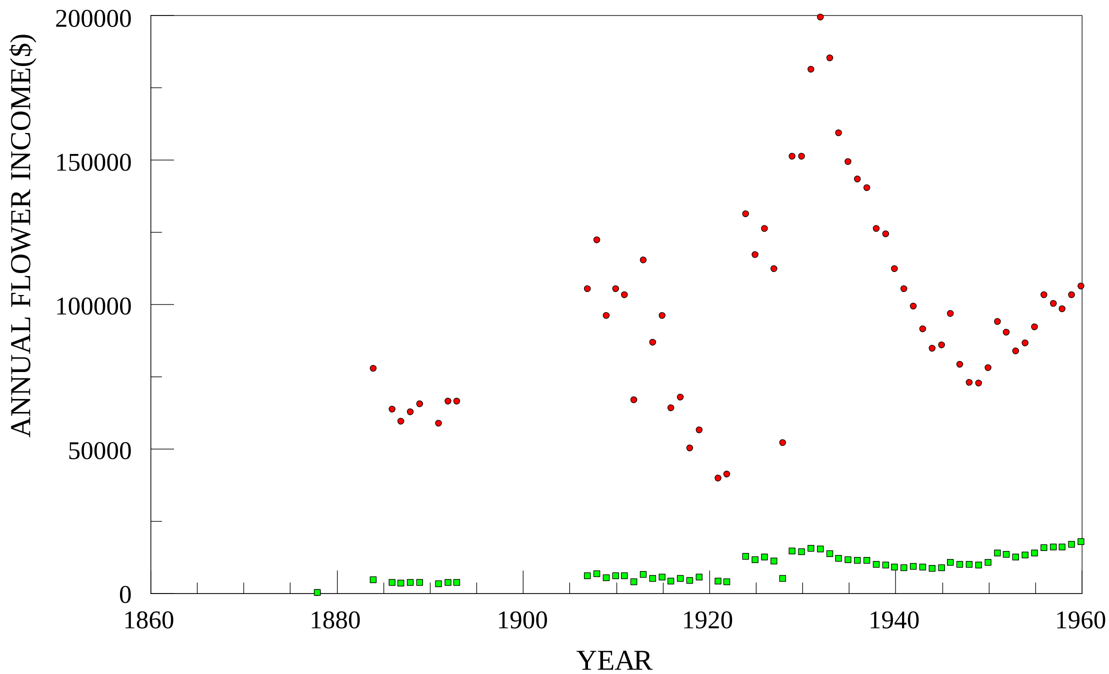


Fig. 28. The real-time (green squares) and normalized-to-2002 (red symbols) incomes from the Reese Wall Flower Endowment Fund over the existence of the Flower Observatory. The initial yearly income was $4,414 [$77,500]. For some years the Treasurer's Report is missing from the Archives. In the early 1920s income was particularly meager and the improvement thereafter may be due to counting the income from the Tobias Wagner Fund as Flower income.

inadequate to pay the salary of a Flower Professor. The book value of the endowment had suffered more than a 40% loss in 15 years. Eric was now dead only a year and Barton the only faculty member. If the Flower Fund was to be the only source of support for astronomy, there was no possibility of bringing in new faculty. Most likely, this was never explained in detail to Barton who certainly understood money. Rather he was probably left to suppose that the University would never do anything to advance him or the FO.

There is a larger and ludicrous financial context for this situation too. In 1925 Oliver Hardy was already an established comic and heavy character actor though not yet the nominally wealthy (a lot of his income went to ex-wives and girlfriends) comedian that he would become when teamed with Stan Laurel. Hal Roach Studios paid him $2,116.67 [$21,800] (*cf*. Louvish 2002) that year so the salary for Barton – whom some people would think of as a hybrid of The Grinch and Eeyore – compares favorably with that of a Hollywood personage. Near the end of the 1920s, improvement was sufficient to bring in Olivier as the new Flower Professor. In 1929 Olivier had Barton's salary as Assistant Professor raised to $4,000 [$42,100] but Ollie had made more than $21,166.67 [$218,000] the previous year. Olivier's own salary in the Depression years around 1935 was $6,000 [$78,900]. In another selected example, Louvish (2003) reports Charlie Chaplin's signing salary with Mutual Film Corporation in 1915 to be $10,000 [$179,000] a week. The local astronomers were destined not to get rich from the FO or from the University. Of course, it is possible to make any number of selective comparisons but it is unnecessary to develop the obviously favorable situation of the astronomers compared to the average working man of the day. In Barton's case, free enterprise occupied at least some of his time although not to the scanting of his observing.

THE END AND DISPOSITION

The FO was sited more than 200 feet north of the right-of-way of PA Route 3 in Upper Darby Township, PA. By 1909, 60 acres directly across this turnpike had been sub-divided by The Wood, Harmon Company into slightly more than 430 residential lots enterprisingly named Observatory Hill. By that date also, 152 acres encircling the Observatory property to the west, north and east were owned by The Homestead Real Estate Company – mostly from the sale of the farm in 1904. Although none of these areas was industrialized, light pollution and particulate pollution from coal furnaces were going to degrade the site progressively. It was only a question of how long this would take. For the visual observing programs of Charles, Eric, and Barton the increasing light level would be only a slowly accumulating annoyance because there is such a wealth of visual binaries. When, however, photometric programs were introduced by Olivier, the situation had to change more quickly. Variable atmospheric extinction would overwhelm the single-channel photometers of the day unless comparison and program stars were very close together on the sky. Olivier understood this well and worked in tight stellar configurations. The light curve of EZ Aql, shown in Fig. 26, is an example of his good practice. Measures and estimates on this object do not end before 1952 so, even in the brightening night sky, it was still possible to work on 14th-magnitude stars effectively. By the usual precept for visual observing in a good site with favorable atmospherics, the limiting visual magnitude for the Flower refractor would have been of the order of +15.1. There is no record of this ever having been attained but such a star might still have been detected even in the 1940s on a clear, dark summer night.

As early as 1922 the University was receiving solicitations from developers to sell the property. The committee appointed that year to examine the proposition submitted a report that apparently did not favor a sale and nothing was done. The Sesqui-Centennial Realty Co. made another offer in 1926 but this was rejected also. In 1949 William K. DuBarry, the University's Executive Vice-President, declared that the station was now useless for science – not exactly the most informed judgment – and reported an offer of $107,000 [$811,000] for the site. Again nothing was done. At about this same time, an external committee of Pierce, Fred Whipple from Harvard and Peter van de Kamp from Swarthmore was convened to advise the University on the future of its astronomy program. Since Pierce had already invested his time and mind in that future, since Whipple was a long-time friend of Olivier with similar Solar System interests, and since van de Kamp was an expert in astrometric binaries and also a friend, it may readily be imagined that their report was not negative but rather emphasized the foundation upon which future science could be developed. On the basis of these conclusions the University decided to try again to sell the Flower property and plan for a new station that would combine the best Flower and the Roslyn House instruments. In mid-1952, therefore, a 90-day exclusive agency for sale of the property was given to Albert M. Greenfield and Co. This was a noted metropolitan realty firm but it did not have a large presence in the suburbs. This company failed to achieve a sale, but in November 1953 the University spent about $585 [$3,900] on kerbing and tree removal at the site – a strange decision for a property that was to be vacated and its structures razed. Eventually, sale of the 6.8928 acres to Harry S. Jacobs of 1348 Garden Road, Phila.31 generated $140,000 [$933,000] on August 12, 1954 – the year after DuBarry had been Acting President. I have been unable to find any correspondence or documents that refer to the sale other than the Indenture filed with the Register's Office in the Delaware County Courthouse.

The sale sum bears on comments by Blitzstein and Wood that were not denied by Olivier when he had opportunity to do so. #These men believed that they knew of a higher sale price and perhaps they did. In sunny moments they considered the transaction to be for a derisory amount and attributed the low value to the lack of acumen by the University financial officers. Their point was that, after more than 70 years the University made too small a profit on real estate that was appreciating in the 1950s development of the township.# Because this was such a recurrent conversational topic in the Astronomy Department, it is worth examining. #The Wood and Blitzstein point of view was that there remained no nearby 7-acre plot of available land and so it should have been a seller's market situation for either residential or commercial development.# The opposite interpretation is that the edge of development in the township and county had moved significantly to the west and the FO property no longer commanded the value that it had years earlier. One may consider the current situation. There are now no 7-acre plots left in the township at all, but a local realtor provided me with the asking prices of 6 nearby improved properties that were on the market in 2005. These range in size from 0.14 acres to 0.37 acres and their asking prices, normalized to 7 acres, scale from [$1,330,000] to [$2,360,000]. The smallest of these properties is directly across PA Route 3 from the FO location and is the site of a small stucco and vinyl-sided residence and garage. If the improvements were demolished on all the plots or only the smallest one, it would be possible to make a valid comparison with the Observatory sale. That hypothetical exercise would probably diminish the value of the smallest property to something of the order of [$1,200,000], which is 30% larger than the price realized for the sale of the Flower property. The best speculation based on this extrapolation is that Blitzstein and Wood were possibly correct in ridiculing the sale price but this is far from a proved proposition.

Alternatively, one may conjure up those darker thoughts that the same faculty members also voiced. #They repeatedly expressed the idea that DuBarry had somehow profited personally from the low sale price. This was based on Wood's assertion that he knew of an offer in excess of $200,000#. DuBarry had had an interesting career at the University. Beginning as an unsalaried second assistant to the Provost in early 1923, he was earning $1,800 [$18,900] two months later. Working his way through the University bureaucracy, he became Acting President for several months between the tenures of Harold E. Stassen and Gaylord P. Harnwell. #When Wood was being recruited to the faculty, DuBarry described ambitious plans for the development of astronomy and Wood asked where the money was coming from. DuBarry patted him on the shoulder and answered, "Now, my boy, just leave everything to me. Show us how to get the best observatory possible and let me worry about the funds." Never one to be patronized, Wood distrusted him afterwards.# I have found no evidence that would support the suspicions of Wood and Blitzstein. One would have to show that DuBarry and Jacobs created a scheme for their mutual profit and this is essentially unprovable. The Classified Directory for 1954 shows no Realtor Harry S. Jacobs although it is possible that he was incorporated under some other name. Had he been a member of a real estate firm and closed a deal personally without making his company aware of it, he would surely have been fired and possibly lost his license. There are 17 individuals in the white-pages directory of 1954 with names of Harry Jacobs or Harry S. Jacobs. None of them lived close to DuBarry's residence at 18th Street and Rittenhouse Square and I could find no association between any of them and DuBarry. Finally, I have found nothing in DuBarry's papers preserved at the University that would suggest a less than straightforward individual. Most likely, DuBarry's personality was just not congenial to academic and research mentalities and the faculty members were prepared to impute to him deeper motives than he could imagine.

Jacobs remains a mystery for a number of reasons. First, he did not reside at the address given on the Indenture and I could not verify that this address was a real one in 1954. Secondly, Jacobs did not accomplish the Indenture himself. This was done in his name by Robt. D. Armstrong, a notary and the Settlement Clerk for The Land Title Insurance Company. Thirdly, the Company did not have offices and Armstrong did not reside at the supposed address. None of these circumstances proves anything of an underhanded nature; a person may wish to disguise a transaction for any number of reasons and do nothing illegal. Something more, however, is known and still more may be surmised. On October 12, 1954, two months after he bought the 6.8928 acres, Harry S. Jacobs *et ux*. of 2223 E. Allegheny Ave., Philadelphia sold 2.37796 acres, again through the agency of Robert D. Armstrong, to Effjay Corp. Neither the white nor yellow pages show a Jacobs at the Allegheny Ave. address. There is no documentation that Pauline W. Jacobs actually owned any part of the land that she purported to sell on that date but still that is what the record shows. For the interval 1953 through 1955, Pauline W. Jacobs did reside at 1348 Grdn Rd., Green Hill Farms, Philadelphia but not at either of the other addresses, and her husband is not listed at her address. The selling price from the Jacobs to Effjay Corp. was $1 so Effjay must have been a dummy company set up in the intervening two months by the husband and wife probably for tax relief purposes.

In order to learn whether Jacobs profited unconscionably by the purchase, I went through the documents of The Recorder of Deeds of Delaware County and learned only a fraction of what might have been known. It was instantly clear that Jacobs divided the property into 7 parcels. For one of them, now occupied by a vacant building, six transactions take history back to October 31, 1958 when Effjay Corp. sold 2.33796 acres to Joseph B. Simon and Co. for $45,000 [$280,000]. For three other parcels the records go back only to 1968, 1986 and 1993, the parcels had long since passed out of Effjay's hands, and much improvement had happened. For two other parcels, absolutely no history can be traced. If the 1958 price for the single parcel is scaled up linearly to the entire property, Jacobs and Effjay would have taken a small loss. Most probably, that didn't happen because location has to be factored into any selling price and property away from PA Route 3 should have commanded more money than the parcel directly adjoining the turnpike. The best reading of the fractional evidence is that Jacobs did not get a sweetheart deal and a windfall by an unrealistically low price from the University. Therefore, there is no hard information to support the astronomers' suspicions.

The Flower Observatory was finally closed in 1954 and demolished within the year. The photo in Fig. 29 dates to June 30, 1954. The 18-inch refractor was stored temporarily and then mechanically rehabilitated

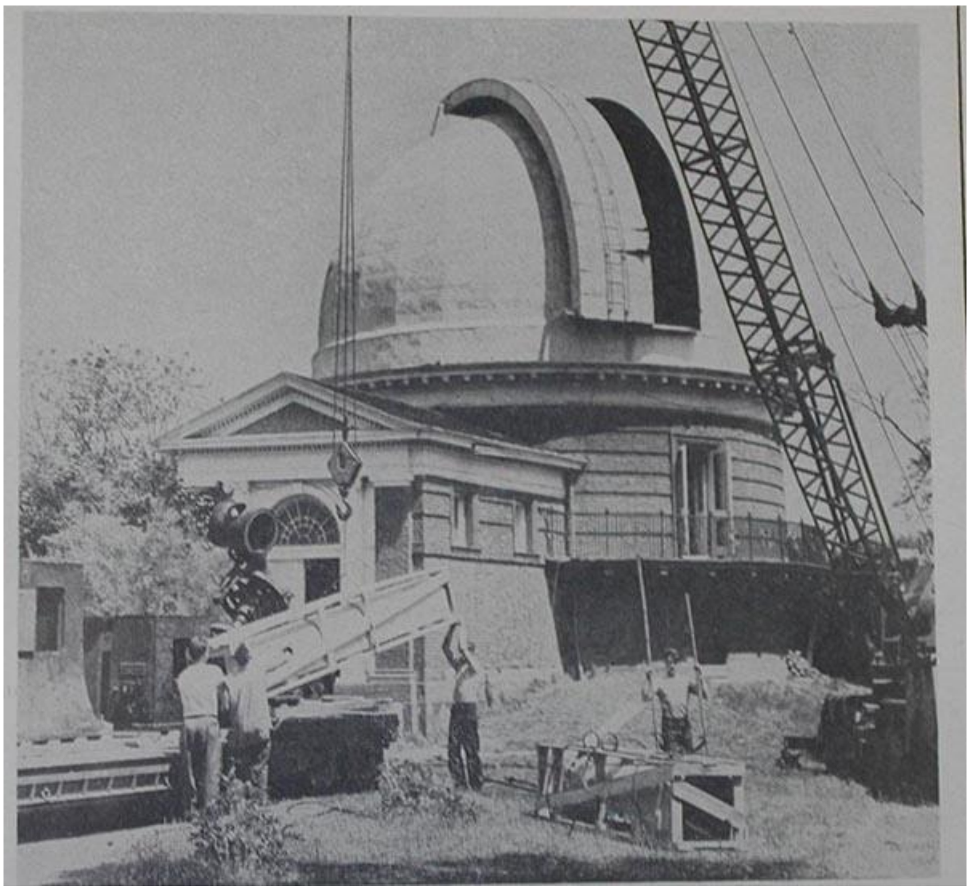


Fig. 29. For the Flower Observatory, the end of the end. All the refractor components have been removed from the dome in this photo of June 30, 1954 from volume 54 of *The Pennsylvania Gazette*. Tube segments are already crated and the polar axle mount is on the ground in the left background. No photos of the demolition of the pier, dome and other buildings on the property are known to me.

by the Wilmot Fleming Engineering Company of Philadelphia with funds supplied by the Space Sciences Lab of General Electric. It was afterwards shipped to New Zealand to be erected at the Mt. John University Observatory in the South Island. Fortunately, lack of funds prevented this potential anachronism and it became the property of the Yaldhurst Museum of Transport and Science, Christchurch to be displayed as an example of Victorian-era engineering. As of 2007, it was not yet erected for display and potential use. The four meridian and zenith instruments had not been used since about 1920 but only the universal instrument was in such condition that it might be repaired and sold.

#Blitzstein remembered the hardware of the Wharton Instrument lying on the lawn as a pile of junk in the 1940s. He had no admiration for Olivier's and Barton's unconcern about instrumental maintenance.#
At demolition, there survived 3 mire mark lenses with focal lengths of about 175 feet, the objective of the Wharton Instrument (and its bottled mercury), the 4-inch Ross/Fecker camera, an elaborate solar eyepiece, an 1888 portable transit by Stackpole & Brothers, the Queen & Co., Philadelphia mean solar clock, the wedge photometer, the filar micrometer, an old sextant and even the instructional spectroscope but without its grating, and a 4½-inch refractor that was a gift of George W. Hewitt in 1922. It is possible that the portable transit was the device used by Charles in his temporary station on the campus before the Observatory was established or possibly it was used to lay out the first meridian at the FO. It is also possible that it was never used at all.

The next Flower Professor was B. S. P. Shen (1931-), who held the position from 1972 to 1996, long after the closing of the FO. By then the endowment was far from sufficient to pay a full salary. The Tobias Wagner Lecture Fund would still support lecturer's visits. In 1975 The American Meteor Society materials were handed over to David D. Meisel, a long-time Society member, so as to continue its mission at SUNY-Genesco. Meisel continues as President, and the Society has been incorporated with a mission greatly expanded in detection techniques. It no longer concerns itself with meteorites and craters.

Seeler is buried in West Laurel Hill Cemetery, Lower Merion Township, PA. Near the northeast edge of the same cemetery there is the grave of the amateur astronomer and TV personality Dave Garroway. His stone also bears the expectant name of Sarah Lee Lippincott; Garroway was her first husband. Two plots away is another more than twice the size of the Garroway/Lippincott one and indeed of most of those in this part of the cemetery. It contains only one burial – Reese Wall Flower. His gravestone, shown in Fig. 30, is the only tangible remnant of him on Earth.

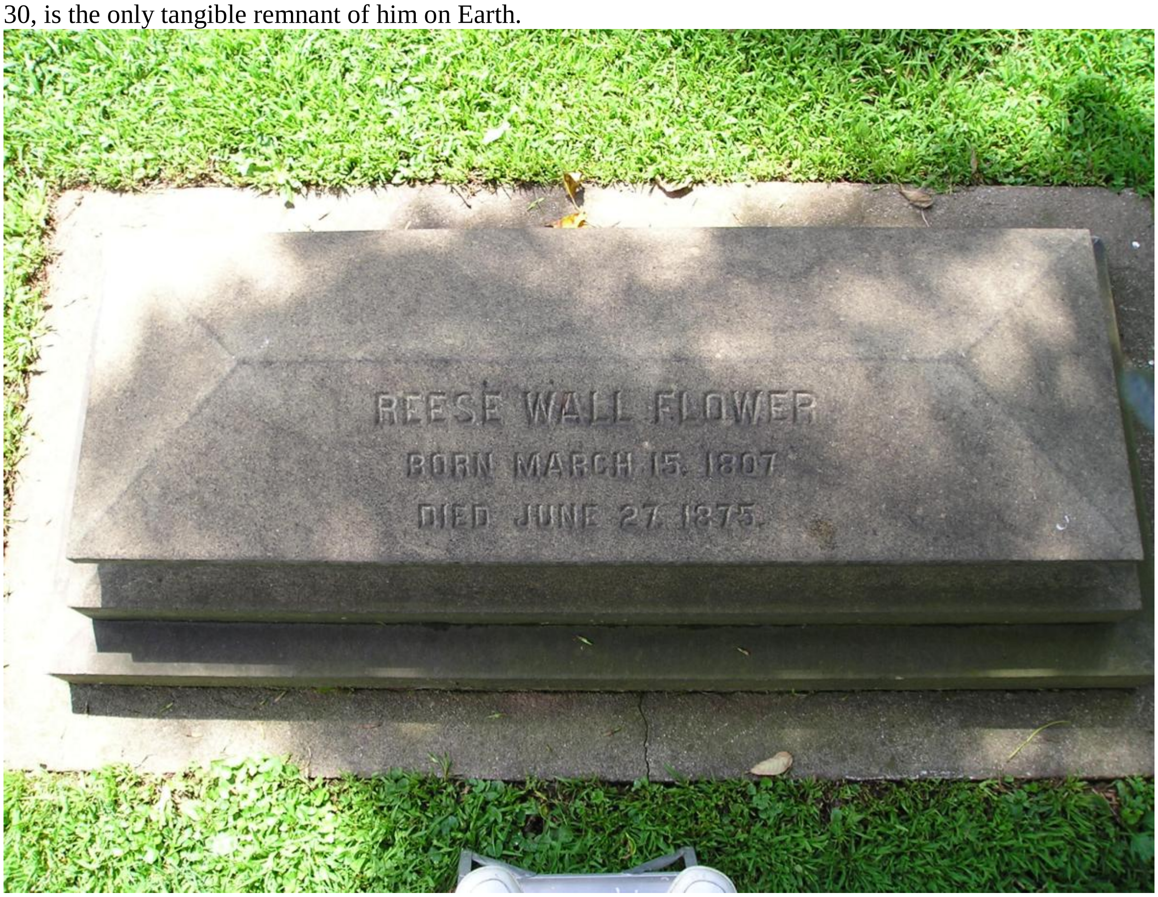


Fig. 30. Flower's stepped and capped but discolored gravestone in West Laurel Hill Cemetery, Lower Merion Township, PA. At ground level its approximate dimensions are (7½ x 4) feet and it is about 1½ feet high. There is no other burial in the large plot and Flower's grave occupies only about 10% of it.

Many meetings of the AAS were memorialized by group photos and identifications of the attending people and these were published first in *Popular Astronomy* and afterwards in *Sky and Telescope*. The faculty whose photos appear in this text show up rather frequently in these shots, none more so than Barton who apparently went to every meeting he could and sometimes smiled for the camera. In departmental files, there are no photos of the other local people who populate this history. Many of them do, however, appear repeatedly in the Society group images beginning when Yerkes was dedicated in 1897 – aging from one year to a later one and passing from one fashion statement to a succeeding one.

The Flower site now contains a mix of commercial and residential enterprises. The land surface shows no visible scar of any old structure. A memory survives in the name of The Observatory Recreational Area, an expansive and well-maintained playground for the neighborhood and nearby school children. The Flower name was incorporated into the next-generation Observatory. Some institutions have more staying power than others. At the southeast corner of the Flower property an enterprising citizen opened Busty's Bar in 1936. #For that generation of observers and grad students this was the location for midnight lunch of a sandwich, a mug of beer and a game of darts before going back to work.# The bar is thriving still, run by the son of the original owner. He remembers being one of the urchins run off the FO grounds by the caretaker.

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THE ROSLYN HOUSE OBSERVATORY (RHO) OF G. W. COOK

INTRODUCTION

In 1937 G. W. Cook and Charles Olivier had a few conversations. This was not unusual since the men were on socially familiar terms and both were active members of the Episcopal Church. In addition, they shared a common interest in astronomy with Olivier being then Chairman of that department at the University and Director of the FO and with Cook still furnishing his personal observatory on the grounds of his Main Line estate, Roslyn House. They were talking of Cook's tentative decision to bequeath his equipment to the University and how this was to be accomplished. Initially, Olivier intended to continue to run both observatories. However, their eventual shared opinion was that the hardware of the two establishments would best be amalgamated, as far as possible, into a new observing station more remote from the light and particulate pollutions of their current locations. In a situation such as this, Olivier's proper posture would be to encourage the concept because the end result was very desirable from his and the institutional point of view (at least he hoped so) and to acquaint the University's administrative officers with the momentary and prospective situations. He did both of these things. Cook's attitude was driven by his age and by awareness of health problems and by the certainty that no one in his family would sustain his level of interest in astronomical science. By the following year, the familiar vigor of his mature life began to decline.

Cook must have been aware that other east coast schools were wondering how he would dispose of his hardware. Earlier in the 1930s he had created an arrangement with the Director of the Sproul Observatory of Swarthmore College that would permit that Observatory staff very generous use of the Cook apparatus. A letter from a former Swarthmore faculty member to William Blitzstein dated September 13, 1988 comments that the Sproul Director was a good friend of the Cooks and visited them in their home but that during 1937-1938 the two men apparently were not in touch with each other. Any implication that Swarthmore was ever in the running for the Roslyn House instruments is probably wide of the mark. The College was already well furnished with its refractor and there would have been very substantial costs incurred indefinitely had it wished to exploit so much new hardware. The 1939 codicil to his 1926 will carried out the intention that Cook had expressed to Olivier.

MORE DOCUMENTATION

I have used Observatory Reports in *Popular Astronomy* and *The Astronomical Journal* and the observing logs as prime sources of information. Research papers from the staff and visitors are readily available in the conventional literature for the most part. Letters in departmental files have been used as well and I have examined a considerable number of direct images and spectra in both plate and print formats. My characterizations of personalities follow the same pattern as in the history of the FO. If a person appears in both these histories, I do not repeat here what was already written about him. Regarding Cook himself, I owe something to the unpublished monograph of Williams (1991a), whose sources included Richard C. and Alfred W. Putnam, Cook's grandsons, and to a copy of Williams's interview notes that are in departmental files.

In order to avoid annoying repetition of the surname, I've called Cook's establishment The Roslyn House Observatory(RHO) in this essay although both designations were used verbally and in print in his lifetime and thereafter.

GUSTAVUS WYNNE COOK (1867-1940) was …

… the son of Richard Y. Cook, a very successful banker, and Lavinia Borden Cook. He was given the equivalent of a prep school education but no special favors thereafter. There was no university exposure

GUSTAVUS WYNNE COOK

for him and the young man's first jobs were in a medical lab and an engraving business. He must have had some ready money because at age 33 with two partners he founded the South Chester Tube Company. In years to come he also headed the South Chester Terminal and Warehouse Company and served as a Director of five other companies including his father's bank. He was a talented, insightful and hardworking man with interests and accomplishments in music, drawing, painting, photography, wood working, metal working, cultivation of orchids, valuable scientific books, and radio. Despite his austere appearance in the portrait, he was a gregarious and welcoming man. A couple of personal consultations with Marconi were apparently very useful to him. He held a patent for an early loop antenna that could be used as a directional locater. Williams is the source for the story by Richard C. Putnam that Cook was instrumental in the capture of a World War I potential saboteur who had landed on the Jersey shore from a U-boat. He is said to have used his antenna as a direction finder, presumably to home in on transmissions that the agent was making. I have been unable to verify this story from any other source. One may be skeptical of the anecdote but it is apparently true that Cook made a lot of money by selling his patent rights to RCA and then, before the Wall Street crash, cashing in the stock that he had received. Cook also played around with early TV and in the 1930s he could receive test patterns on a set that he had made.

Cook became interested in the sky as a child and soon had a rooftop telescope that came along when the family moved in 1927 from Philadelphia to Wynnewood, Montgomery County, PA. A larger telescope was acquired and located in a building about 100 yards away from the family mansion which appears in the distance in Fig. 31.

Fig. 31. The reflector is aimed almost at the zenith in the right foreground while the Roslyn House mansion is seen up the hill and to the left.

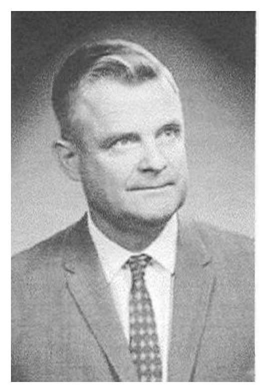
Cook appears in a group photo taken at the Atlantic City meeting of the AAS in 1932 but not in one taken in 1926 in Philadelphia. Mrs. Cook shows up in photos of the Sproul Observatory solar eclipse expedition to Conway, NH on August 31, 1932 so presumably her husband was with her or maybe he was across the state line with another group in Vermont. He financed both of these expeditions and loaned his 40-foot Lundin lens to the Conway team. The turning point to serious commitment of money and time must have come in the late 1920s when he purchased his first instrument of potential research quality. Somewhat later, Cook ordered a 24-inch reflector but was presented with a 28.5-inch disk instead. He bought it of course and continued buying other instruments from the same company as well, it is said, as turning his own hand to telescope construction.

Naturally, as the instruments increased in number, he needed workers. The process had started with his making his reflector available to Swarthmore College if they would create a new faculty and observatory position for which he would pay the salary. Not only gregarious but generous as well, Cook encouraged other astronomers to find ways to use his telescopes and eventually added two more staff members. Honors came to him: Fellow of the AAAS (1932), Member of The APS (1934), and ScD *honoris causa* from the University of Pennsylvania (1936). The honorary degree was said to be the first ever given by the University to a person without college or university credentials but Cook was unable to attend the commencement ceremony. There can be no doubt that the degree was the stimulus for or consequence ofthe terms of his will. He was a valued friend to many people, and the newspaper and astronomical obituaries all testify to his admirable business and personal qualities. There was also a brief notice in *TIME*.

OTHER PEOPLE

The careers of Leendert Binnendijk, William Blitzstein, I. M. Levitt, Philip H. Taylor and Frank Bradshaw Wood have already been sketched in the FO history up to 1954. Some additional remarks about them will appear in context below but now several more people should be introduced.

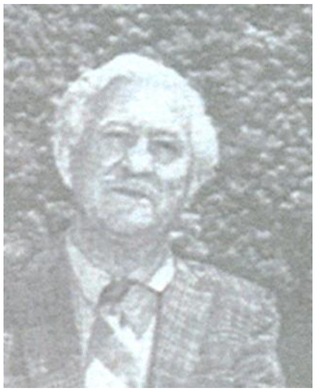
James W. Fecker (1891-1945) came from parents who were technical people – his father was the chief designer and instrument manager for Warner & Swasey. It was the father who started J. W. in the optical profession by taking his son into his team at the shop. His responsibilities there appear to have been in the preparation of drawings and prints for numerous telescopes that were being fabricated at that time. In 1921 he left the company to form his own firm manufacturing small optical items. In 1926 he took over the shop of John A. Brashear in Pittsburgh and there continued to pull in significant contracts. The company made, either entirely or in part, telescopes for Harvard, Ohio Wesleyan, Princeton, Lick, Yale, McCormick, the USNO, Indiana, Illinois, New Mexico, Wisconsin, Yerkes, Mt. Wilson, many smaller educational institutions and government organizations, and several foreign establishments. Telescopic instruments were also made. In the company of the FI expedition to Derby, VT in 1932, Fecker was attempting color photography of the solar eclipse that is noted above. He had also several avocations away from the technical life. The company did not continue to prosper after his death and in 1957 became a subsidiary of American Optical Co. Advertisements for Fecker products continued to appear through 1960.



John S. Hall (1908-1991) would be considered a pioneer in North American photoelectric photometry by any standard. At a time when there were probably fewer than 25 astronomical photometers in the entire world, he had designed and built a number of them. It was Hall who told Blitzstein that Cook and Miller had not mingled socially during 1937. Hall's credentials are very numerous. He may have been the first person to diminish the dark current of a photocell by refrigeration and he appears also to have priority in photoelectric scanning of spectra. Observationally, his peak achievement was the independent discovery (*cf.* Hall & Mikesell 1950) of the polarization of starlight caused by foreground interstellar dust. His administrative accomplishments are also many. He was a successful Director of both the USNO and Lowell, advancing both establishments in scientific breadth and staff strength.

Gordon L. Locher (1904-?) finished his academic work at Rice and then was appointed a National Research Fellow at Bartol. Bartol was located on the Swarthmore College campus at that time in order to achieve the ambiance of an academic research environment while the staff built up expertise in cosmic ray research among other topics. Locher's contribution, beginning at Mt. Wilson and continuing at Rice, was in developing detectors. Some of these, working in mostly visual bandpasses, are described in Locher (1931). He and Mohler must have met at many events and Mohler decided to see if these detectors had an astronomical application to his own work. In the late 1930s Locher convinced himself that other Bartol people were stealing his scientific ideas and actually menaced the Bartol Director with a gun. He was terminated quickly. After his time at Bartol, Lochel held a few other technical positions, principally with biological applications of radiation dosages. In 1949 he formed his own company Western Radiation Laboratory in California and died sometime between 1961 and 1966.

Roy K. Marshall (1907-1972) had received a PhD from Michigan for a spectroscopic dissertation. While at Wilson College, Chambersburg, PA he wrote a considerable number of articles on assorted topics for *Popular Astronomy*. It can be no surprise that not all of these have worn well. He then became more and more interested in a career of public education and decided to try to exploit his experience with planetarium lecturing and shows. This led to appointments at the Fels Planetarium. He had some exposure in early TV, principally on the Monday evening show sponsored by Ford Motor Company, and then became Director at the Morehead Planetarium, Chapel Hill, NC. This was at least partly on the enthusiastic recommendation of Harlow Shapley who considered him the best planetarium lecturer in the country. Marshall had considerable manual capability: he and an assistant once disassembled and then reassembled all the optical components of Morehead's Zeiss planetarium projector in 17 hours. Observing manuals and a few books were published and he wrote feature articles for *The Sky* and *Sky and Telescope* for a number of years. He worked at the RHO from the late 1930s into the mid-1940s. In 1945 he led a solar eclipse expedition to Wolsley, SK, Canada using the 4-inch Ross/Fecker camera that Cook had given to the FO as well as other photographic apparatus. Mohler, by then at McMath-Hulburt, and R. M. Sutton of Haverford College made up the rest of the team. The effort was quite successful, not least in the PR effort of essentially real-time broadcasting of the eclipse phenomena, but I could find no scientific results from it. Some scandalous indiscretions led to the end of the public part of Marshall's career but thereafter he continued to consult on a variety of planetarium projects.



Walter M. Mitchell (1879-1947) published his first paper (on the 1901 Perseids) as a University undergraduate. His graduate training at Princeton resulted in a thesis on spectra of sunspots and a few subsequent publications (*e.g*., Mitchell 1904). For brief times he was at Allegheny and Detroit (really in Ann Arbor) Observatories and on the faculty of Haverford College. It is recorded that he, an RAS member, accompanied the 1932 FI solar eclipse expedition funded by Cook to the NH/VT state line and that he was associated with the University. The latter is certainly incorrect for, after about 1910, he spent his entire career as a mechanical and metallurgical engineer in numerous impressive positions. I have found no information about him re-treading himself. At his death he was Director of Research for Mack Trucks and the *Times* and *Herald Tribune* printed obituaries of him. It is for none of these reasons that he appears in this text but rather because he left behind a few artifacts. Four lantern slides bear the label, WALTER M. MITCHELL, Ph. D. Two of them show different views of a spectrograph from the Detroit Observatory. A third slide is a schematic of the Mt. Wilson spectroheliograph and the last one is a picture of Henry Rowland looking at a ruling engine, presumably one of his own. The originals are all apparently from published sources and the slides appear to have been prepared professionally. This may be consistent with Mitchell's reputation as an accomplished photographer. He had a patent for a now-forgotten chemical developing device, the Photorix. The supposition that Mitchell is best noted in this chapter (and not in the one about the FO) is founded on the certainty that only the RHO had a functioning spectrograph and spectroheliograph. It might be supposed that the slides were made for Mohler or for Levitt, possibly for use at the FI during one of his lectures, or they might even have been made for Cook before the instruments were built.

There is some additional and confusing information. A label, CHARLES L. MITCHELL, M. D., appears on 3 other lantern slides: 2 photographic views of the Mt. Wilson 5-foot spectrohelioscope and one of the

Potsdam spectroheliograph and these identifications are written on the slide frames in ink in a well-controlled hand – the same one which labeled the 4 slides noted above. These 3 slides are also labeled PHILADELPHIA, PA. and they too are not homemade. The identity of Charles Mitchell is unknown to me but it would be an extraordinary coincidence for two unrelated Mitchells independently to have made slides concerned with the same subject matter. There is another confusion factor as well in the very earliest photos of the reflector. The prints of these photos bear a somewhat decorated ink stamp:

E. N. FOUGHT, M. D.
Official Photographer
JEFFERSON MEDICAL COLLEGE HOSPITAL

This Dr. Fought, also an RAS member, took pictures of the outside of the establishment that were actually published with his name. He might also be imagined to be the source of the slides bearing the names of the Mitchells but this seems like stretching evidence. I have found no connection between Cook and Jeff so it might be presumed that he and Dr. Fought were personal friends and that the images were made as a favor. I also do not know if Charles Mitchell was affiliated with the same hospital.

John A. Miller (1859-1946) had a career that jogged across the country from Indiana to Stanford to Indiana to Chicago to Swarthmore. Six solar eclipse expeditions offered opportunity to study the corona and he also published parallax work using the Sproul refractor, the purchase of which was his price to come to Swarthmore. The question of whether he could, in good conscience, smoke a pipe at a Quaker institution was decided in his favor at the same time. He was Chairman of Mathematics and Astronomy for 20 years and gave 21 years of his life to the APS as its Secretary. His was a direct but charitable personality and he and Cook surely had many sympathies in common. Miller was the person with whom Cook dealt for the new astronomy appointment at the College.

Orrin C. Mohler (1908-1985) came to the RHO from an entry-level position at McMath-Hulburt but he really came to Swarthmore with Cook paying his academic salary. He had the warmest feelings toward Cook, commenting, for instance, how the latter would fix a midnight snack for both of them in his home and how Cook would use the reflector for star gazing but eventually found a smaller instrument more observer-friendly. Even while holding the Swarthmore appointment, Mohler maintained his contacts with the Michigan group and published with them. In 1940 he returned to McMath-Hulburt, became Director in 1961 and then Chairman of the Michigan department in 1970. His entire career was directed toward eking out the last bit of information from observations of Sun by the most searching scrutiny of instrumental design and operation. Mohler was a most gentlemanly man and Minor Planet 2528 honors his career.

Albert M.. Skellett (1901-1991) had an Astronomy PhD from Princeton and became a Research Physicist at Bell Labs where his duties and interests concentrated on the solar and interplanetary environments of Earth. One finds him concerned with effects that ionization levels in meteor trails have on short-wave communication and with the impact of solar storms on electrical activity on Earth. He was attracted to the RHO because the siderostat telescope could serve as a test bed for an instrument that he was trying to perfect in order to examine solar effects on radio transmission. It is recorded (Jansky 1933, Millman 1983) that he was the person who told Karl Jansky that a recurrence interval of $23^h\ 56^m$ had to refer to extraterrestrial phenomena. After World War II, Skellett became Research Director of at least the company now known as Florida Power Corp. In 1965 he joined the University of Florida at Gainesville as Professor of EE with the challenge of developing a program in microelectronics. He retired from this position in 1969.

James Stokley (1900-1989) was the first Assistant Director of the FI and first Director of the Fels Planetarium and generally filled a conspicuous place in science on the Philadelphia scene. He too participated in the Institute solar eclipse expedition of August 31, 1932. In addition, he and John Q. Stewart of Princeton went off to the Pacific Ocean on a solar eclipse expedition sponsored by Cook and the FI. At a later time, he was Director of the Buhl Planetarium, Pittsburgh. Stokley also wrote a column for every issue of *Science News* for 51 years. It was he who introduced Cook to Fecker.

Sotirios N. Svolopoulos (1920-) arrived at the University and the FO as a Research Fellow after an interval in England where he had been at the Norman Lockyer Observatory and had published (Svolopoulos 1953) extensive photographic photometry of open clusters. #A very taciturn man, he was believed by Binnendijk to have had some unfortunate experiences during the German occupation of Greece but Wood suggested that maybe he was just shy about speaking English. Despite his retiring nature, he was a very energetic observer who would walk half a mile from the last public transportation stop to the RHO and, if necessary, shovel snow from the siderostat building's slanted and flat roofs in order to observe. He picked up new observing techniques very readily. # Partly due to a recommendation by Wood to C. D. Shane, he was invited to a position at Lick and later moved on to other U.S. and European postdocs where he had the good fortune to work with such people as Harold Johnson, Wilhelm Becker and Hans Haffner. He then returned to Greece where he spent the rest of his lengthy career at Ioannina and Athens publishing on a considerable breadth of topics and serving in numerous administrative positions as well. He was a founding member of The Hellenic Astronomy Society.

Lewis P. Tabor (1900-1974) started with a ChE degree from MIT and then took a position as Head of the Science Department at The Episcopal Academy, Lower Merion Township, PA where he served for nearly

20 years. After the U.S. was driven into World War II, he became a Research Associate at the Rad Lab where he was part of the team developing high-power-level S-band radar with a potential application to control of fighter planes. After receiving a U.S. Navy commission, he helped outfit the new carrier *Lexington, CV 16* with height-finding radar. He then became Project Officer for the guided glide-bomb missile program. Following his naval discharge, Tabor first became Research Assistant Professor in the Moore School of Electrical Engineering at the University where he (Tabor 1947) helped in the design and construction of the first EDVAC machine. In 1948 he moved to the FI Laboratories quickly becoming Technical Director of the EE Division. Nominally retiring in 1960, he instead went to work on radar instrumentation at the RCA facility in Moorestown, NJ. After 5 years there Tabor retired again but really started working for Spitz Laboratories on optical systems for assorted planetarium models. He retired a third time but un-retired once more to work for Response Systems, Inc. until his death. At the FI he was Blitzstein's boss for a time. Tabor was a very congenial man who worked effectively on numerous committees and he was also a gifted gardener and photographer. #Blitzstein had fond memories of Tabor but he never indicated if he knew how Cook and Tabor became acquainted.# It is possible that this happened very early when Tabor taught at Episcopal because the Academy and Cook's estate were quite close to each other. It was Tabor's photographic interests and physical endurance that were exploited at the RHO. His service there continued after Cook's death in order to complete the project that he had begun.

Not all of these people are noted in the **PRIMER** since some of them had no connection with the University.

THE HARDWARE INSTALLATION

However much Cook indulged himself with hobbies of model ships, metalworking, painting, or book collecting, these avocations led to no significant depletion of his fortune. There was plenty of money to build and furnish a modern observatory.

The best way to express this situation is to enumerate the large and small instruments with which he furnished his facility. It is unknown how long he had been pondering a very substantial observatory but, as was noted above, in the late 1920s he made up his mind that adding to his current equipment was worth doing. There are, to the best of my knowledge, no cancelled checks and no dated requisitions or purchase orders and there is no indication that he handled any transactions through the purchasing departments of his companies. He was his own entity, responsible to no oversight. There is a bit of correspondence with Fecker that isn't very informative. The most that can be done now is to indicate when each system was purchased and assembled if that information is known or can be estimated from the date on a layout blueprint or from the brass plate on a telescope or from a telescope log. Mohler (1935a) is a very useful source of information as is the inventory appended to the will. Values in parentheses are taken from a 1953-1954 insurance report.

A 1934 REFLECTOR SYSTEM CONSISTING OF

> a Fecker 28.5-inch reflector with an 11-inch Newtonian flat yielding f/5 and an 8-in Cassegrain hyperboloid yielding f/14.7 beams, a Fecker 6.625-in, f/18 finder, a 4-inch Ross-Lundin camera lens with a Fecker camera body, eyepieces, eyepiece diagonals, a double-slide plateholder, a Fecker 2-prism spectrograph with its supporting cage frame and constant temperature case, a Mohler projection measuring engine, and a Seth-Thomas 30-day, wall-mounted mean solar clock. The telescope was mounted in the east room of the RHO. ($30,800) [$416,000]

A 1932 REFLECTOR/REFRACTOR SYSTEM CONSISTING OF

> a Fecker 15-inch horizontal siderostat telescope with a 25-in Fecker reflecting flat in an alt-azimuth mounting slaved to an equatorial drive, a Brashear 15-inch, f/11.7 visual doublet objective bought from the Philadelphia School Board and Central High School and a 14-inch, f/14 singlet photographic corrector lens with the same history, eyepieces, diagonals, extension tubes, plate holders, an 8.625-inch flat for a 6.5-inch finder, and Frank Hope-Jones, Synchronome Co. table sidereal and mean solar clocks. This system was mounted at the west end of the main building. ($40,100) [$526,000]

A 1932 TELESCOPE TIMING SYSTEM CONSISTING OF

> a Cook/South Chester Tube 2.5-inch broken transit, an E. Howard & Co., Boston sidereal clock with installed electrical relays for operating a chronograph in a maple and glass case, and a time signal receiver. Fig. 38 shows the transit telescope just after installation in a small room off the passageway between the two aforesaid telescopes. ($1,400) [$18,400]

A 1935 WIDE-FIELD IMAGING SYSTEM CONSISTING OF

> Fecker 10.25-inch (for 20 x 24 inch plates), 6.5-inch (for the same size plates), 5-inch (for 14 x 17 inch plates) and 4-inch (for 8 x 10 inch plates) Ross-type astrograph cameras with a normal wire objective grating for the 4-inch camera, and plate holders. Cook is leaning on the mount for the astrograph system as he talks to Fecker in Fig. 39. ($2,400) [$31,600]

A 1939 WIDE-FIELD IMAGING SYSTEM CONSISTING OF

> a (possibly FI) 12-inch spherical primary and a Fecker 8-inch correcting plate, f/2.5 Schmidt with plate holders. ($800) [$10,400]

A 1930 SOLAR OBSERVING SYSTEM CONSISTING OF

> a direct imaging arrangement mounting an unmetallized flat feeding a 6-inch, 40-foot focal length Lundin photographic lens, and a Howell & Sherburne, Pasadena, CA 3-inch, 20-foot focal length spectrohelioscope and spectroheliograph fed by glass or stainless steel coelostat flats. (NOT VALUED)

It can readily be imagined that all necessary darkroom furniture and supplies and a substantial amount of electrical and electronic hardware had been acquired as well. In addition, there was some demonstration, laboratory and instructional equipment, many books, about 200 lantern slides and prints of astronomical subjects, and a meteorite. There also were stored away some small telescopes and a few substantial optical components such as a 12.25 inch stellite flat. This flat had no use that can be documented at present. For insurance purposes in 1954, the net value of the establishment, exclusive of the buildings, was estimated at $88,000 [$587,000] but it must be remembered that, almost 20 years after the Observatory was founded, the installed equipment had depreciated in value severely. To get an approximate idea of how much the original investment would have been in 2002 purchasing power, the last sum should be doubled.

One particular item is worth a few comments. #In 1954 there was found in a bathtub on the second floor of a shed – sometimes called a coach house – on the property# an 8-inch refracting telescope. It has a clear aperture of 8.19-inch, works at about f/12, and is the instrument sized at 9 inches by Williams. There is a general belief that it is the second telescope that Cook owned. Possibly he used it from the building in which it was found but the location of its mount was not recorded at the time it was found. The mount was found a brief time later at a different location on the property. There can be no doubt that the item is a part of the Cook estate for it appears in a 1935 photo mounted on the same support as the 28.5-inch reflector and collimated with it. It is also listed in his inventory after the codicil to his will. It appears too on the assembly blueprint for the reflector system drawn by Fecker himself on June 3, 1932. There is no good reason for this arrangement for the 8-inch has a shorter focal length than the prime focus of the reflector so it would not be useful as an auxiliary guide telescope. Besides this limitation, there was always the capability to guide directly at the Cassegrain focal plane anyhow. Cook's own inventory says that the telescope is a Clark one but one may be skeptical of this entry because the inventory contains a number of mistakes. For example, in that listing the Geiger-Muller tube is attributed by Cook to R. Mohler. He has Mohler's initial wrong and Mohler himself correctly assigns the tube to Locher. In the 1953-1954 inventory the instrument is said to be a telescope by Alvan Clark. Stokley (1934), on the other hand, says the lens is by C. A. R. Lundin with a Clark mount but it isn't clear if he means the lens cell is by Clark or the equatorial mount for the telescope. Mohler's (1935a) description is of a Clark objective with a Fecker mount. The last inventory, by Blitzstein around 1990, says that the mount is labeled Alvan Clark & Sons but only speculates about the lens. The reality is that the lens cell has no information on it, the eyepiece end of the tube is engraved with the legend *Alvan Clark and Sons Co. Cambridge Mass.*, and the equatorial mount and pier have no information at all. All of these are distinctions that make a difference to people interested in the provenance of telescopes. Lundin, who died in 1915, was a long-time optician with the Clark firm and eventually manager of the works. My personal reconciliation of the confusing information is the following. Lundin, and not any of the Clarks, ground the very fine lens whether or not it was new when Cook bought it. This conclusion is grounded in the oldest opinion of all, that of Stokley, who was Cook's first scientific acquaintance. The tube is undoubtedly from the Clark works and presumably so are the clamps and slow motion controls. The pier and equatorial mount are possibly due to Fecker, as Mohler says. Some support for this idea appears in the illustration on the inside cover of the October 1959 issue of *Sky and Telescope*. This is an ad for a Fecker mount and it somewhat resembles the structure of the 8-inch pier.

One device was never realized. The warm, humid climate lasting more than half of each year meant that the reflecting coats on the mirrors had relatively short lives, principally if they were silver. An exciting and potentially explosive silvering event convinced Cook that this was not something he wanted to do frequently. He intended to buy from Fecker a 40-inch aluminizing vacuum chamber so that he could strip the old coats and deposit new ones on-site. This never happened and it isn't known who did the then-new aluminizing procedure for him.

The impression is of an observing facility that was well furnished by any standard. Things were not a dollar short and a day late and there would be no reason for failing to fulfill a clever observing program. Problems should arise only from incorrect instrumental design or fabrication or from neglected maintenance.

THE PHYSICAL INSTALLATION

The Cook mansion of many rooms sat on a high point of the property but, as seen from the telescopes, the house occulted only a portion of the sky below the pole. The grounds of the estate were extensive with numerous trees of assorted kinds in various stages of growth. Since the only telescope that worked at large hour angles was the reflector, the tree cover was not a problem in general. Even for that instrument, sky coverage was limited only to the west. The landscaping was rather formal with box planted at intervals along the alleys that threaded through the flower gardens. There must have been at least one gardener. A small portion of the property and the main observing building appear in Fig. 32. In front of what came to be used as the main entrance to the RHO, although it really led directly into the reflector room, was a shallow artificial pond about 10 feet in diameter; the carp seemed to survive the winters.

Fig. 32. A 1930s photo of the main observing building and its surroundings made from a color lantern slide. The color scheme was slightly inflected in software but closely follows a large photo of that time. The reflector is looking at the sky through the open roof that has rolled off to the west. The upper-level banister in the background edges the roof above the siderostat observer's room. The other observing buildings were to the northwest invisibly behind the main building.

Presumably Cook designed the structures and chose their unobtrusive siting on the estate and the green-trim-on-white color scheme but the contractor is unknown to me. Township zoning may have prohibited dome structures although there were, and still are, silos in the community. The roofs, certainly cheaper than a dome, either hinged opened without undue effort or rolled back by a windlass arrangement cranked easily by a ship's wheel. The buildings were thin-walled so as to diminish accumulation of heat during hot days and only the central and western portions of the largest building were heated. #When I knew the building, the interior was always cluttered with defunct photographic apparatus, what appeared to be old-fashioned decorations and electronic lab equipment. I believe that there was a janitor but apparently he didn't dare to throw out anything. Mention should also be made of the hundreds of wasps that lived in the main building, principally in the reflector room. As morning twilight caused the observer to stop work and close the roof, these insects were ready to go about the day's foraging. They didn't like the vibration and noise of the motion of the roof and let the observer know it.#

THE REFLECTOR PROGRAMS

The reflector, which appears in Fig. 33, had problems that always made an impact on what could be done
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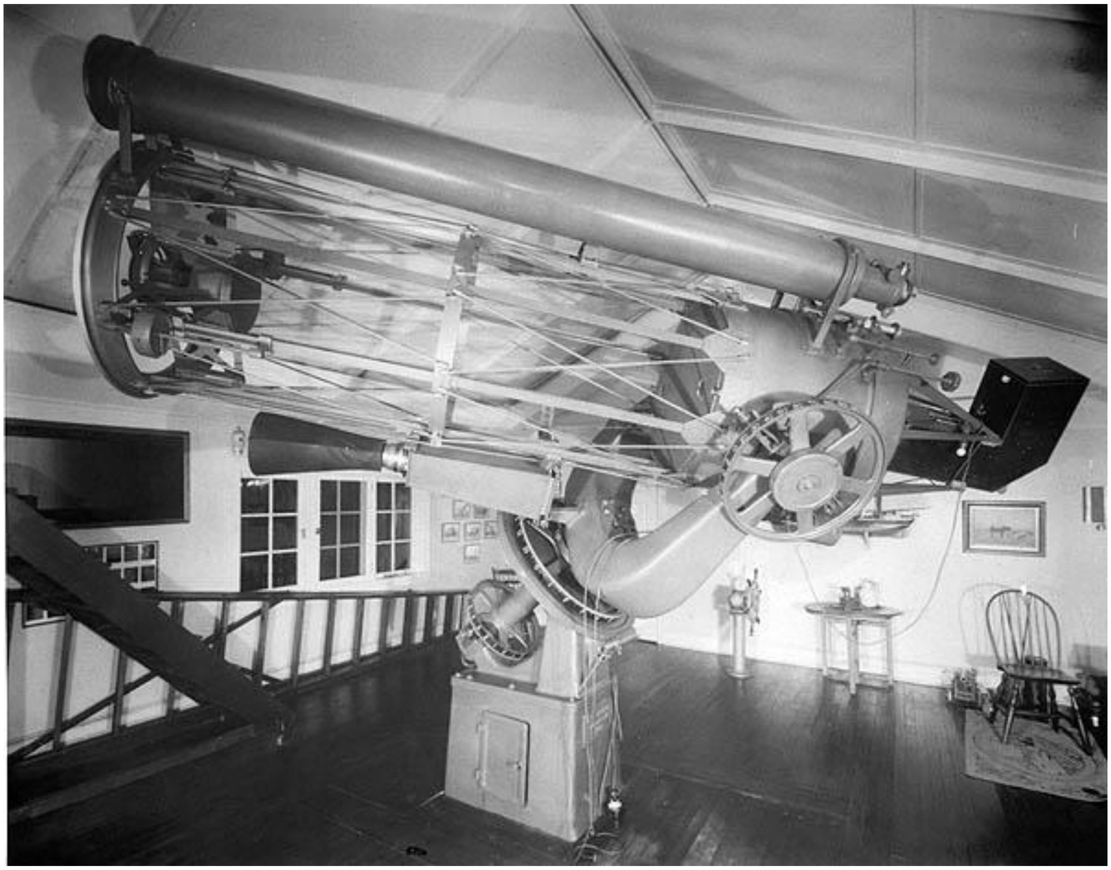

Fig. 33. The 28.5-inch reflector stowed in its observing room at Roslyn House. The finish of the telescope was a very dark and unhandsome green. By the time of this photo the cross braces had been installed and the first finder and the 4-inch camera were still mounted on the tube. At the Cassegrain focus the spectrograph is inside its constant-temperature case. The wheel to open the roof is visible (beneath the declination circle) as well as one each of Cook's paintings and ship models. It's not known what was the function of the toy cannon by the chair. Right-to-left distortion is severe in this close view. Beyond the railing a passage led westward into the rest of the building, passing first the transit, then a workroom leading into the observer's room for the siderostat.

#The drive that was delivered was a mean solar one that had to be rectified by Mohler.# The original skeleton tube of duralumin was of longitudinal struts passing through more massive annular rings. That tube was floppy so aluminum cross braces were installed but these were of too small a diameter to prevent residual torsional distortion and flexure. In addition, the braces were not machined to be of a unique length. Rather, their ends were threaded so that they had to be tightened by ordinary nuts. It was not really possible to make the complementary braces of identical length to a tolerance of ±0.1 inch. This meant that the tube was not a right circular cylinder but some slightly irregular shape, and centering the secondary mirror in its section did not assure that the mirror would be centered in the sections below it. The mechanism for mounting the secondary was crude but not so bad as the mount for the paraboloid – that mirror simply sat in its casting with no adjustments or restraints built into the sides or back. There was not even a pretense at a float support system for it. The declination bearings and brake shoes were loose and never served well. This was particularly annoying when the paddle containing the slow motion switches was used. The observer at the Cassegrain focus could hold the paddle in his hand as he centered or guided on a star but, if he then let the paddle dangle under gravity, the telescope bounced to a different elevation.

It was enough to move a star off the slit or out of the diaphragm of a photometer or off the wires of the guider of the plate holder. The only way to deal with this annoyance was never to let the paddle fall under its own weight or else to lash it so that it always hung so. The polar axle, mounted in ball bearings rather than roller bearings and with no thrust bearings at all, was insufficiently massive for the torques imposed by the fork and tube in many positions. There were no electrified slew motions. At some point, the original finder was replaced by a 6-inch, f/10 alternate and the 4-inch camera was removed. These changes made it easier to find fields and diminished the weight and moment on the mount. The Newtonian focus was useless because there were no safe movable ladders. All these problems sound overwhelming when they are summarized in this way but, of course, all telescopes have idiosyncrasies and you repair some of them, as Mohler and Blitzstein did, and learn to accommodate or avoid the others. Some, but not all, of the foregoing arises from personal experience. With the optics mounted as poorly as they were, it may be expected that imaging would be bad. This is not borne out by the few guided Cassegrain photos that survive although they typically show only on-axis targets.

By 1934, Hall had considerable experience designing, making and using astronomical photometers with near IR- and red-sensitive photocells. The results of the measures were magnitudes, color indices, and spectral energy distributions for selected cool stars and a good presentation of his results appears in Hall (1936). The work at Yale had been followed by measures at Columbia and then Miller induced him to come to Sproul in order to continue his work on the 24-inch refractor. Miller's invitation letter of May 28, 1934 mentions that the Swarthmore staff also had almost full use of the Fecker reflector at the RHO and that it would certainly be available to Hall. Before receiving Miller's letter, he had actually anticipated the invitation and had moved to temporary lodgings near the Cook home. Hall did use the reflector for a fraction of May 1934 but a letter from him to Blitzstein dated February 4, 1990 says that the results were not worth publishing.

Mohler eventually accumulated more than 90 blue spectra of 36 stars that could be used as spectroscopic references for more interesting variables of all kinds. The emulsion was Imperial Eclipse, quite fast for the time and not too grainy. The dispersion at $H\gamma$ is about 47 Å/mm and typically the widening is satisfactorily uniform. The spectrograph, moreover, was quite successful with its temperature controlled to ±0.1ºF even with a large temperature difference between the interior of the case and the observing room. There is no decker array and, with one exception, only one spectrum appears on per plate. Many of the spectra could have been exposed more deeply. Mohler's first opportunity came in March 1934 when he was able to begin accumulating plates for the velocities of TX Leo, which had recently come to be known as a bright spectroscopic and eclipsing binary. He measured the plates at the RHO and then re-measured a few of them with an engine at the Sproul Observatory and on photographic traces made with a Koch-Göos densitometer at Bartol. The orbit is given very cryptically in Mohler (1935b) and more amply in Mohler (1936), which cannot have had broad visibility. The treatment was quite acceptable for its time and his very small value of orbital eccentricity was subsequently confirmed by Chamberlin & McNamara (1957) using better material.

A second spectroscopic opportunity appeared with Nova Her 1934, now DQ Her. Beginning on December 27, Mohler followed the spectral evolution into the following season. There were many plates of this object and a fraction of them was measured with the densitometer at Bartol. These form the foundation for a talk at the RAS, which was published only as Mohler (1935c). Mohler's is a treatment representative of nova phenomena for the time before they were recognized as interacting close binaries but other workers published much more abundantly on this object than he did. The two notes (Mohler 1938a, 1940) are simply summaries of incidental work with this telescope or of programs begun but never finished. In all, there are 14 other assorted variables with only 70 spectra distributed among them. Not much could be gleaned from the poor phase coverage for these objects.

Much more scientifically significant and also more indicative of Mohler's (1938b) ultimate interests and capabilities is his beautiful work measuring solar UV radiation below 3000Å. For this purpose he lashed a Geiger-Müller counter tube with a gold photocathode (similar to the one shown in Fig. 34) to the frame of the reflector. This tube and others had been made by Locher on the basis of his experiments earlier in the

Fig. 34. This Geiger-Müller tube is somewhat more complex than the one used by Mohler to measure far-UV solar flux but is basically similar to his device. The square quartz window to the left is about 1.25-inch on a side; the black deposits are just sealant. The photocathode is presumably gold and possibly the coloration of the inside of the glass is also sputtered gold. All the electrical connections are intact and the electrical leads show clear signs of attachment to other circuit elements. The stopcock is held closed by a 1930s bandage.

decade; Locher also designed and built the downstream electronics. After numerous tests for electronic and photoemissive stability, Mohler took measurements during August 1936, directing the tube toward the sky and then toward (Sun plus sky); the field of view of the counter was about 11º. Certain detection was achieved for the interval 2000Å – 2300Å even though the observatory is at an altitude above sea level of only 96 meters. Despite reservations about his absolute calibration, he then calculated a sea-level solar flux of 7.2×10^{-7} ergs s^{-1} cm^{-2} over the bandpass interval. A modern average outside-atmosphere value would be of the order of 10^3 ergs s^{-1} cm^{-2} according to Allen (1973a). The two values lead to a (scattering plus absorption) value of $\tau = 21$ for Earth's atmosphere at sea level whereas Allen's (1973b) tabulation gives a value of the order of $\tau = 25$. A discrepancy of 4 in optical depth is a major problem translating into a factor of 55 in attenuation in the atmosphere but a few details must be remembered: Allen's values are for average conditions and Mohler had to contend with variable attenuation every day. Of course, Mohler's observations were primitive with almost an order of magnitude peak-to-peak scatter but that's the same situation for all pioneering efforts. From stations at Arosa at 1,860 m and on the Jungfraujoch at 3,460 m Meyer, Schein & Stoll (1934) had already detected Sun near 2100Å. Also, Pettit (1935) had achieved repeated success from 3200Å to 3800Å but Mohler's results moved the near-sea level limit of detection considerably toward shorter wavelengths.

Mohler was also thinking ahead when he ordered photoelectric cells from Joel Stebbins. A letter from Stebbins advises him to start at low voltage and increase it gradually on the two Kunz cells that he had shipped. These KOH-cathode tubes would be supplanted by the 1P21 after World War II but they were the best blue-sensitive photoelectric devices available when Mohler ordered them in 1939. It isn't clear what Mohler's hopes were, but possibly there were shared by other people. For instance, a year later Olivier employed Taylor full time, even though he was a grad student at the University, to develop a photometry program at the RHO. Taylor was adequately experienced in visual photometry at the FO but it is unknown if he ever did any work at the other observatory. Somewhat later in the mid-1940s, General Electric and Western Electric electrometer tubes were acquired and Thompson was taken on to build an amplifier for a photoelectric photometer. Olivier, Levitt and Blitzstein all agreed that this man wasn't up to the job and never finished it.

When Mohler left in 1940, Olivier assigned Marshall the task of continuing the accumulation of red spectra of B-stars down to m = +6.2. As the FI took on more and more defense contracts, Marshall's time diminished and, as Olivier's teaching burden increased, he abandoned the effort and in effect shuttered this telescope in 1944. Only when Binnendijk came in 1953 was the instrument put to sustained use again. That became possible because the University purchased from Carleton College the photometer that Binnendijk had built and used there. This instrument was brass, very heavy, and too long to go through the reflector's fork but it served for a number of years. A grant from the APS permitted buying a Brown Recorder. The first publication to result from this was Binnendijk's (1955) work on 44 Boo, the beginning of his long series on photometry of close binaries at Pennsylvania.

THE SIDEROSTAT PROGRAMS

The history of this type of optical/mechanical arrangement is actually a long one but it is not necessary to repeat it here. It is sufficient to note that the design of Cook's device is essentially the same as the sketch shown in Fig. 1A of Pettit (1940). Whereas Pettit used his instrument for solar work, Cook intended his to be used for nighttime observing.

Cook's interest lay in the fact that the observer could remain comfortable in a warm room while the flat looked at the sky and fed the reflected beam to the stationary objective lens whose focus was in the observer's room. The optics of the Cook siderostat are built around a very good flat and a visual objective, that is superior at least close to the optical axis. At about 1.25º off-axis, coma is noticeable. The photographic corrector lens was used only long enough to realize that the dark-adapted eye could not recognize star fields with its very large and very blue images. A picture of the telescope assembly, except for the control panel and eyepiece, is shown in Fig. 35. Fecker built a significant flaw into the instrument by mounting an 8.625-inch finder flat outboard to the west of the large flat so that, as the latter rotated in its altazimuth mount to track a celestial object, in general the finder flat could not pass its beam to the finder objective. As a result, the field of the main objective has to be used to find celestial objects. The altazimuth mount is coupled to the equatorial drive by a sliding bearing that moves along a right circular cylinder 2.75 inch in diameter and 43 in long. Around 1956 the cylinder was refinished with some kind of metal spray process but that procedure did not overcome driving problems for very long. The cylinder is unfortunately not stainless steel so there was always a light or heavy patina of patchy rust depending on how frequently the telescope was used. Inevitably, stick/slip friction beset both slew and slow motions and nasty backlash resulted. The sliding bearing also loses mechanical advantage as it rides up the cylinder, and a mechanical limit exists in declination so that a considerable area around the North Celestial Pole cannot be seen. The southern limit of viewing is set by making sure that the sliding bearing does not hit the pier of the equatorial mount. On the meridian of the RHO, one could see over the range $+60º > \delta > -30º$ without serious vignetting of the objective but hour angle coverage diminished swiftly with declination off the meridian. It is in the nature of the optical design that the field of view rotates about the optical axis at a rate that depends on hour angle and declination. Guiding for long exposures therefore is correct only on-axis unless the plate holder is mounted on a rotating bearing at the focal plane. In the RHO installation all the mechanical parts and optics were in a building separated from the room where the observer stood or sat. The converging beam passed through a 17-inch diameter horizontal tube connecting the buildings. In the optics building a slit in the conical roof permitted the flat to see the sky and the orientation of that slit was fed mechanically to the control panel where the observer could know when it was time to rotate the roof. It was inconvenient.

Fig. 35. This is a composite image from two photos of the siderostat system. Because the shots were taken from two different positions, there is a discontinuity of perspective from right to left across the illustration. The "stationary" 15-inch objective and the finder objective appear at right. The large objective is stationary only in east-west and top-to-bottom senses; it can be moved on rails in the north-south direction for focus. The pier for the equatorial mount is at the left edge of the picture and the polar axle can be seen clearly. The sliding-bearing linkage of the equatorial mount to the altazimuth one is invisible behind a 5-inch camera and the 25-inch flat appears very nearly edge-on and cannot be seen. The finder objective and its feeder flat were removed at a later time. The paint finish was the same as that on the reflector. In this view of the hardware room, the roof shutters are open.

The first user of the instrument was surely Cook himself. He was fascinated by telescopic views of Solar System objects and had designed an arrangement whereby the visual eyepiece could be hinged out of the optical plane and replaced by a plate holder in a matter of seconds. There exists a photo of him seeming to enjoy just such a moment and he took a certain number of plates that survive.

According to Mohler (1935a) Hall was to develop an IR-sensitive photometer to be used by Cook on this telescope. Hall himself makes no mention of such an intention and it certainly never eventuated.

After more than 5 years of development at Bell Labs and after being encouraged by Stokley and approved by Cook himself, Skellett started using the siderostat to test a device that he called the "coronaviser" in 1939. This design used TV detection with a spiral scanning spot to image activity of the inner solar corona and is described in Skellett (1940a, b, and c). At the beginning, the occulting disk was poorly formed and atmospheric and telescopic scattered light was a major problem, as a surviving observatory photo shows. The low resolution of TV imagery of that time was also a difficulty but Skellett eventually published good images of what are probably prominences, rather than coronal structures. The concept was a good one but its realization was far in the future. Bernard Lyot's still imagery of almost 10 years earlier and then his and

M. Waldmeier's motion picture refinements would remain the benchmark for years. The work ceased at Roslyn House shortly after its beginning but there are statements that the device was to be tested further at McDonald by C. T. Elvey and E. T. Rogers. I have found no indication that those developments ever took place. Skellett's was a concept that didn't really mature, but the idea that large and cumbersome equipment could do useful work at the stationary focal plane with its massive mounting plate found an enduring application with Blitzstein's photometers. It is worth noting that Skellett was not a free lance. Bell Labs was his employer and they were interested in the real-time activity and predictability of solar disturbances and their effects on terrestrial communications.

In 1947 Blitzstein was transferring his prototype single-channel, pulse-counting photometer from the FO to the RHO. Some mechanical and optical adaptations were necessary and additional electronics were constructed and tested resulting in the increasingly cluttered array that is shown in Fig. 36. By 1948 he was

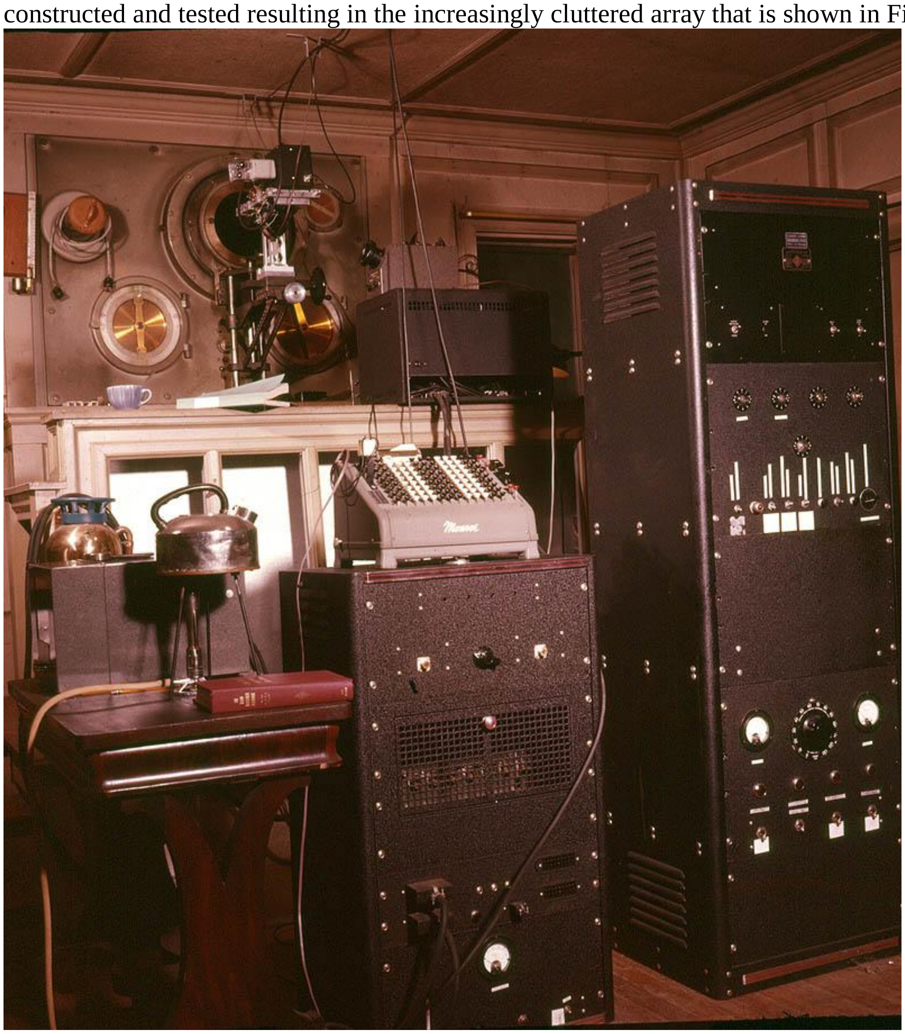

Fig. 36. A view of part of Blitzstein's single-channel, pulse-counting photometer system at the focal plane of the siderostat. The brass circles display the equatorial coordinate and hour angle settings of the telescope and the photometer itself is mounted above and between them. The tall rack on the right contains a temperature-controlled tuning fork frequency standard, an array of dekatron counter tubes and a power supply. The smaller rack houses the amplifier and discriminator circuits surmounted by an adding machine that was used to record source and filter codes and star counts. The high voltage power supply and the pulse counter display are not visible. It is useful to compare this view with Fig. 25 in the chapter about the Flower Observatory taken when Blitzstein and Levitt were tacking together an experimental pulse-counting system.

able to begin measures and the first result was his (Blitzstein 1954) excellent unfiltered light curve of XZ And, appearing in Fig. 37. This is a remarkable Algol-type close binary with geometrically very deep eclipses so that significant color changes occur during the eclipse. Blitzstein's light curve showed none of this richness of information that is known only from later filtered measures but his effort was the first

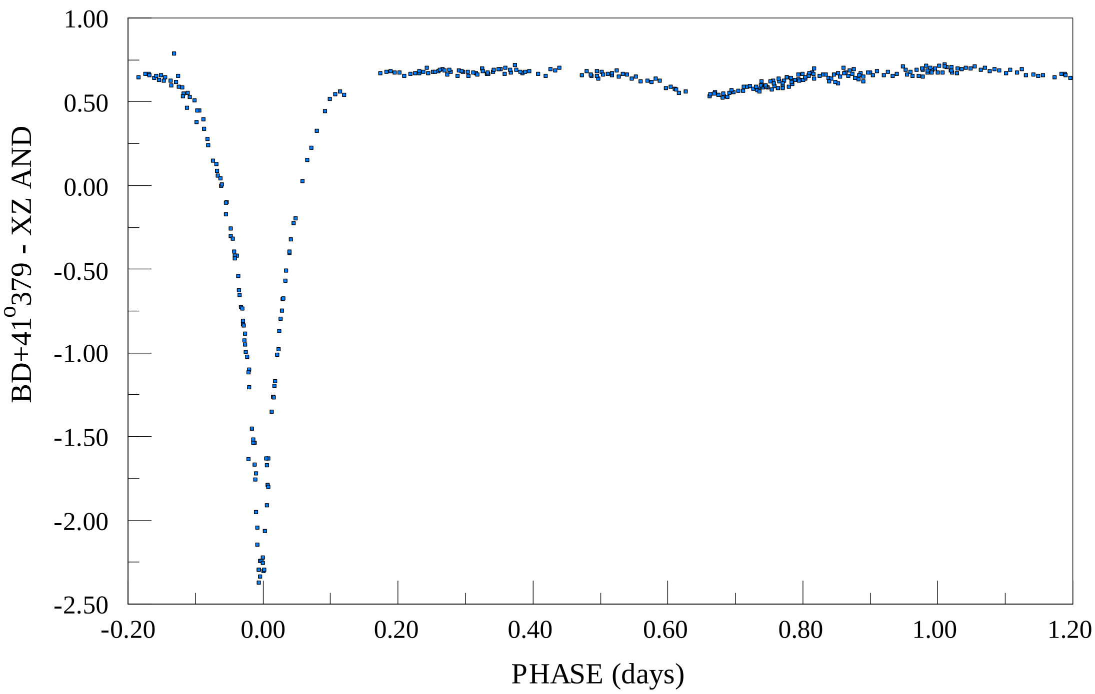

Fig. 37. Blitzstein's light curve of the interacting binary XZ And. The period is 1.357 days and the two component stars are very different in photospheric temperature. It is possible that each star shows an individual type of instability.

extensive application of pulse-counting methods to an astronomical program. The record fails to show any further sustained photoelectric observing until Svolopoulos (1957) developed the 2-color light curves of the contact binary BX And. These light curves are quite satisfactory but the eclipses are so shallow that no modeling of them could be successful at that time. Some of the lack of use of the telescope was due to the rather poor health of Blitzstein, and Wood had other duties that permitted him to observe only intermittently.

THE TRANSIT PROGRAM

Cook conceived of his transit telescope according to conventional models existing in the 1930s, had the finished drawings made at South Chester Tube and #supposedly built it himself either in those shops or in his own machine shop on the third floor of his home.# (It's unknown why he wouldn't install his personal shop in the cellar.) The machining tolerance is good, typically about ±0.005-inch. The drawings say that the base and supports are entirely of gunmetal (bronze now in North America) with an aluminum tube. The fact that the base itself weighs more than 145 pounds makes one disbelieve the possibility that Cook was physically able to handle this casting in his home shop. Also, he is not known to have had a lathe or mill with large enough dimensions to provide a 1-foot radial clearance. It isn't clear why he wanted such an instrument, which is shown in Fig. 38, or even how much it was used. Perhaps with the duplexed altitude/declination circle on the axis of rotation, he used it to determine the coordinates of the station. Certainly none of the staff is known to have used it but there remains a curious puzzle about the device. When around 1970 it was put back into service for instructional use, it was found that the V-blocks were badly galled although the pivots were not worn at all and were as circular and collinear as when new.

There is no understanding of this minor mystery. The striding level, also homemade, has a sealed bubble chamber. With a level trier built around 1955, the nominal equivalent of 1 scale division of the level was found to be 1.57". While the top surface of the chamber seems quite uniform, the bubble length is really too long and the device too sensitive for use in the dark and in an inconvenient posture. The axis of rotation must already be very, very close to horizontal for the level to be useful.

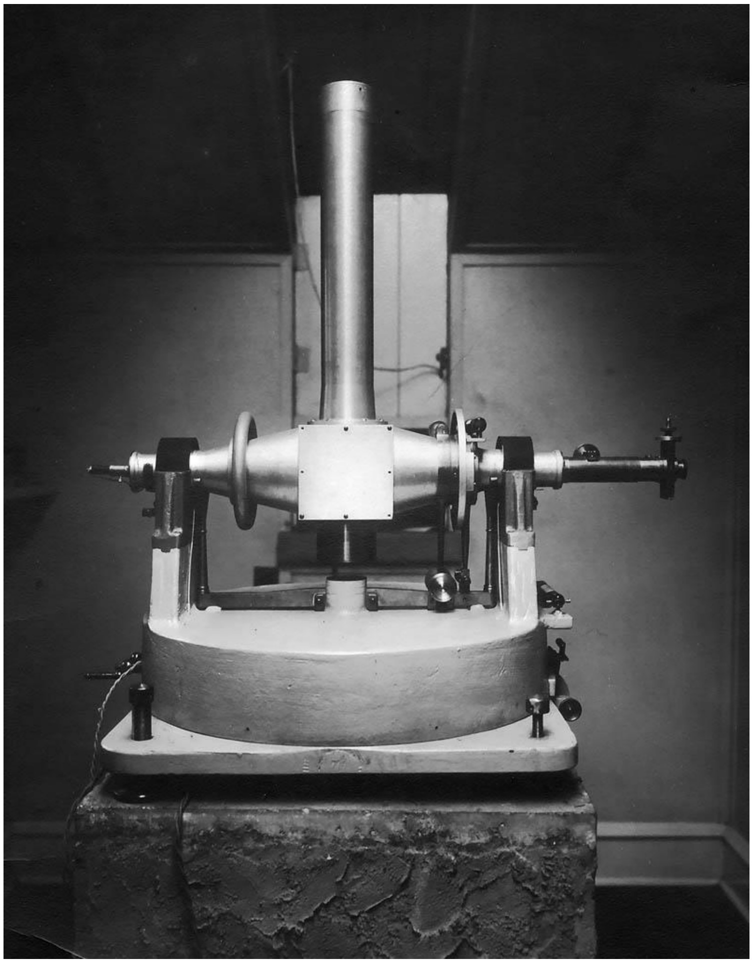

Fig. 38. The just-installed broken-transit telescope. East is to the right and the shutter leaves for the roof are open. The telescope was eventually mounted and used for instruction in the Students Observatory. It is now dismounted but survives.

THE WIDE-FIELD ASTROGRAPH AND SCHMIDT PROGRAMS

The optical throughput of the 6-inch lens was so low that it was replaced by the 10.25-inch lens. The final three astrograph instruments (weighing about 2 tons) were set on a single massive mount (weighing nearly 8 tons) and were mutually collimated. The array was very stable and most of the structures can be seen in Fig.39. The transmission curves of the lenses peak in the blue-green and the secondary spectrum of the cameras is not extreme. A major problem lay in the fact that Fecker had delivered a drive system that operated at the mean solar rate with provision for manual intervention for driving correction about twice a minute. Cook intended that at least one of these cameras be used for very long exposures – up to 15 hours on a winter night– and it was necessary to improve the guiding in order both to minimize fatigue for the observer and to make the imaging as elegant as possible. Tabor (1939), capitalizing on his own background and noting the success of an electronic drive corrector at McMath-Hulburt, designed and built an electronically controlled clock circuit so that manual correction was infrequent over long exposures. Since the only technology at that time was vacuum tubes, his circuitry was bulky and heavy containing more than a dozen tubes and almost as many transformers. It must have dissipated a considerable amount of heat but it worked well. There is no information that the Schmidt was ever used for research.

Before Tabor refined the drive system, Mohler (1938c) had essentially completed a very good effort using the 4-inch lens. By constructing a normal wire objective grating (the grating "1" shown in Fig. 5 of the **PRIMER**), he could easily obtain first-order spectra flanking each zeroth-order image of all sufficiently bright stars over a (20 x 24)-in plate. Were these plates inscribed in a circle, they would subtend an angular diameter of 26º on a great circle. Mohler measured the locations of the first-order density centroids of the spectra and found, as expected, that they correlated well with spectral type from A through M. A substantial effort to calibrate imaging imperfections and departures from the ideal case of exactly axial focusing resulted in the prediction that spectral classification to about ±5 spectral subclasses could be obtained routinely for the hundreds of stars on one plate. Of course, this measure of precision ignores differential atmospheric attenuation across a plate and interstellar reddening.

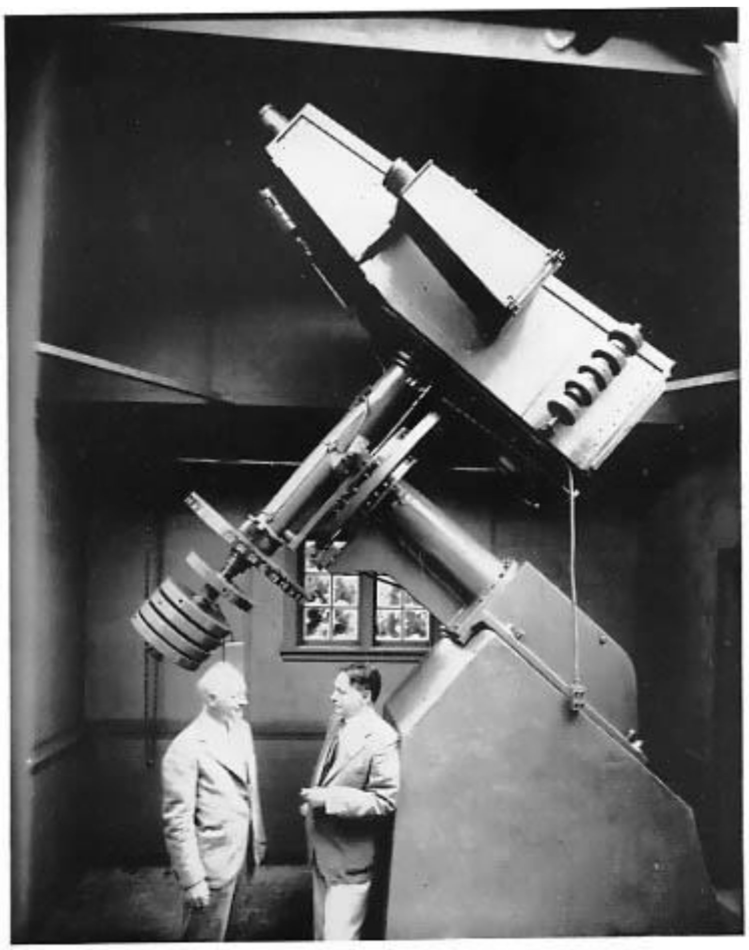

Fig. 39. The astrograph system before the 6-inch lens was replaced by the 10.25-inch one. The 5-inch camera and a finder are not visible in this view. It isn't known who took this photo showing Cook talking to Fecker but it is possible that Mitchell was the photographer.

It is known why the astrograph cameras were obtained. Cook was concerned that he would leave behind no enduring accomplishment so he conceived the idea of a photographic Milky Way atlas, a copy of which would be presented to every astronomer in the world. That language was just extemporaneous rhetoric but he did intend that an atlas be distributed to every observatory. He never enunciated a scientific value for the project, which was intended to be triply redundant with each camera creating its individual atlas. Perhaps he was motivated by the earlier Ross-Calvert (1934) atlas and wanted to improve on it with his larger aperture. By 1941, Cook was dead a year but Tabor continued to work until he completed every 10-inch field that could be reached from the station. Exposures were up to 4.25 hours long and almost every field was imaged more than once, typically on different nights. Eastman 40 emulsion was used for some plates but it's unclear if that was always the case. For the 10-inch camera, limiting magnitude is of the order of +16.0; since no filtering was used, it is not obvious how to interpret this magnitude limit. Typically, exposures were also made with at least one of the other astrographs as well as the 10-inch. I have inspected 4 of the 10-inch plates very carefully. These cover the declination range from the M81/M82 field to the star clouds of Sgr/Sco. The images are remarkably good. In a corner the images are sometimes radial streaks but this is not conventional coma. Furthermore, some plates show round images to the very edge. There are no halation rings for bright stars so the plates must have been backed even though there is no indication of that in the log or files. Contact glass and paper copies, both positive and negative, were made for many of the fields. It is not clear how this project would have culminated. By 1948 Olivier had an agreement with G. P. Kuiper that the entire astrograph system would be loaned to McDonald and Kuiper (1948) records that this did happen. The ostensible reason for the transfer was that the wide fields should be very useful for the McDonald minor planet search. Strömgren (1953, 1956) notes that a new plate measuring engine had been made and that the 10-inch camera had been remounted in a better cell for the minor planet program. There is also language that the instruments were valuable for Milky Way photography. #The agreement had a *quid pro quo:* McDonald staff would add Milky Way fields to the south of those that had been obtained at the RHO and they would have the use of the cameras for their own programs for five years.# Even with these additional fields there would not be complete Milky Way coverage. The real and projected coverages are shown in Fig. 40. Presumably, the best of the several

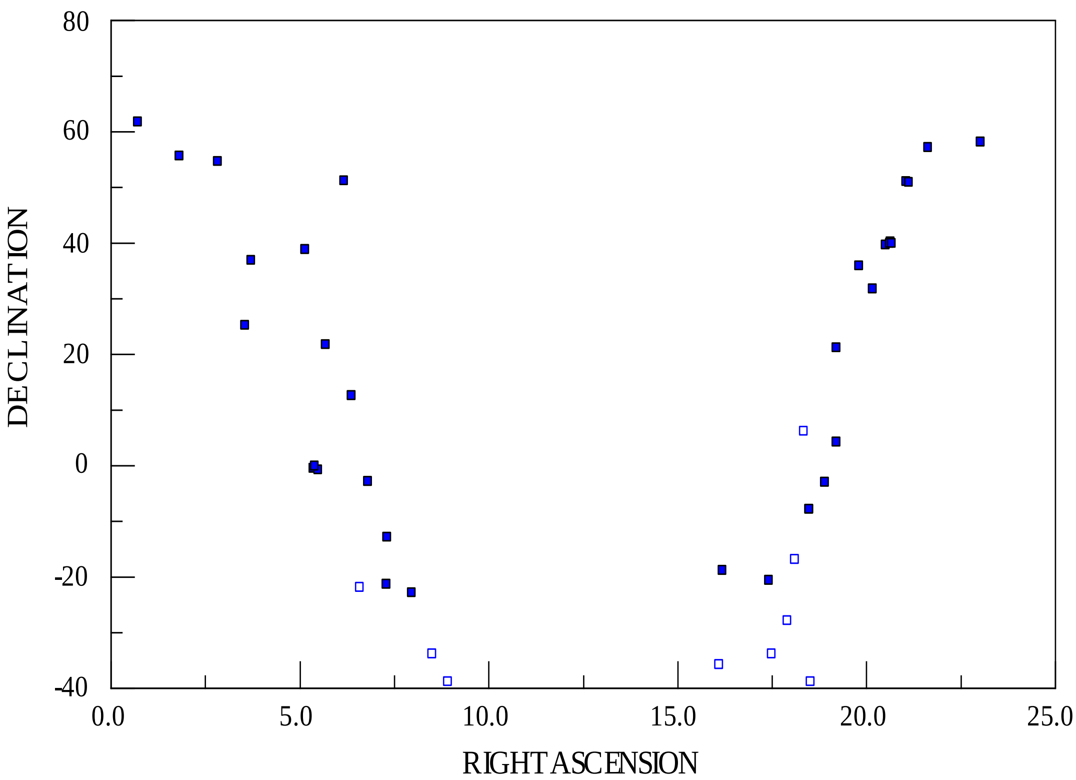

Fig. 40. The field centers photographed by Tabor are shown by the filled symbols and those to be completed at McDonald by the open symbols. Each 10-inch plate would actually cover about 250 square degrees on the equator so coverage overlaps from one plate to its neighbors. Note that Right Ascension increases to the right on this diagram.

images of a field would have been selected for printing in a bound atlas but I am unaware if the additional southern plates were ever taken. They aren't found among the stored collection. Numerous other fields were imaged with each camera but there is no reason apparent in the log for most of them although the plates survive in good condition. The plate jackets are deteriorating badly and have only ever contained minimal information so eventually knowledge will be lost unless something is done to archive these records.

Numerous glass and paper (mounted on heavy cardboard) positive and negative contact prints of the original negatives were made by Tabor and are stored with the originals. There is some reason to believe that others existed once but have walked away.

THE SOLAR PROGRAMS

Olivier wrote that Levitt actually designed the entire solar facility but that seems unlikely. Most likely, Williams's (1991b) comment that Levitt modified the feed prisms is closer to the mark. There is no other independent evidence of this claim. Levitt has written that from 1935 to 1942 he worked every clear day from 11 AM to 1 PM stopping the lens down to 2 inches in order to make direct images of the solar disk and to record positions of prominences. He also made some $H\alpha$ images of prominences. His recollection is that this work was part of an international cooperative campaign fostered by the IAU and that the data were reported to a central agency. There is some evidence of part of this claim in St. John (1932) wherein the FI is named as one of the collaborating institutions in a global monitoring of sunspots and chromospheric phenomena. Neither in that report nor in the same reference for 1935 and 1938 is there any indication that the results of Levitt's observing were ever received at Greenwich or Zurich and the Institute's name never reappears. Against this supposition is a typewritten statement by Barton that the spectrohelioscope results were indeed sent to Zurich. It isn't clear how to interpret this conflict of testimony and possibly Cook, who paid the Institute $600[~$7,700] a year to defray Levitt's expenses, got little or nothing for his open-handed support.

The fact that the plates survive at the University surely means that they were never inspected by anyone at the FI. In all, there are 1,191 plates with a disk diameter of 11.2 cm. There survives no log for this effort and maybe there never was one. The number of plates is large, and is consistent with the conclusion that the noon atmosphere was clear about every other day – at least clear enough to make a solar record. Since there does exist a collection of Stoneyhurst disks, it is possible that Levitt reduced the spot details himself and that they were never reported. There is no record at all of prominence coordinates and descriptions that I have been able to find and only several prints of prominences exist in the files. An image of one of these appears in Fig. 41. The glass plates either have not survived or have not been located.

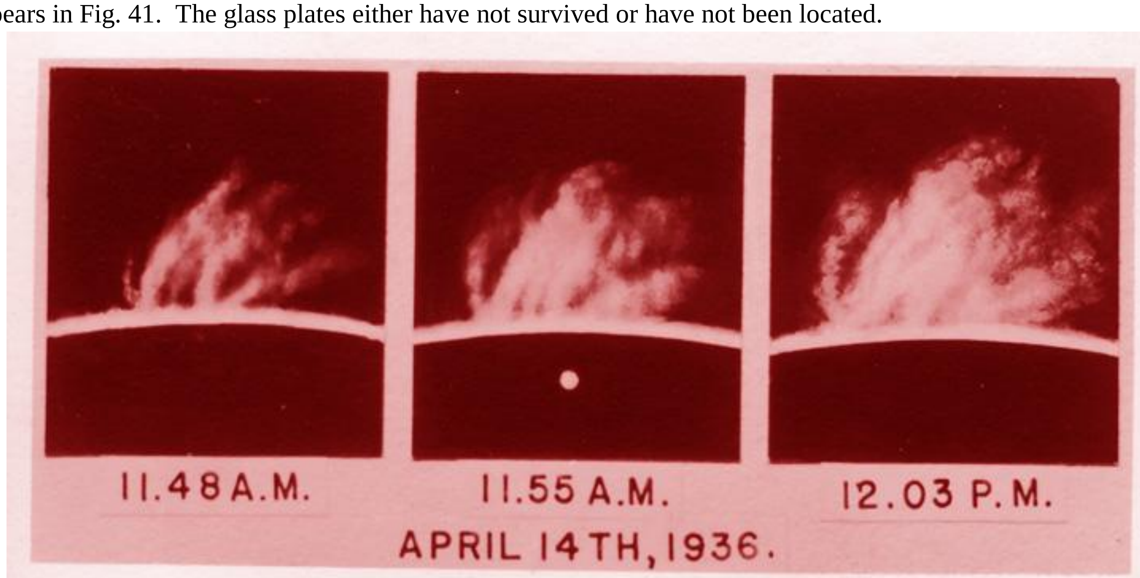

Fig. 41. Spectroheliograph images of an active prominence obtained by Levitt on April 14, 1936. The circle on the middle panel is the size of Earth and the red tint on the original print has been changed slightly in software. As far as I know, the print is the only surviving record of the spectroheliograph program.

MY APPRECIATION

A certain amount has been written about the question of whether Cook was really an amateur astronomer. This inevitably emphasizes the careers of Percival Lowell and Robert S. McMath, the only other American astronomers before the present day who had considerable wealth. My belief is that Mohler offered the most balanced understanding: Cook was really a lover of astronomy who worked at it when he could and was an amateur in that sense as well as in the obvious sense that he never had to take a salary for his enjoyment. He had no fixation on a unique astronomical target as did the two other men. Blitzstein's opinion that Cook was not an astronomer at all but rather a benefactor of the science can be discounted on Mohler's testimony.

What did he leave behind? There survives the successor company to South Chester Tube. Southco, privately held and profitable, is a major manufacturer of industrial and marine fasteners. The peak scientific accomplishment at the RHO, without doubt, was Mohler's catching those few far-UV solar photons. After World War II, sounding rockets and then orbiting spacecraft supplanted ground-based UV solar work but Mohler's measures show a first-class scientist at work. The second remarkable and more enduring accomplishment was Blitzstein's siderostat light curve of XZ And, appearing in Fig. 37 and the demonstration that pulse counting was a useful detection technique. Skellett's accomplishments were not trivial but have mostly been forgotten. Nothing at all eventuated from the transit telescope and the astrographs, at least up to now for the camera images. The intention of a Milky Way Atlas would be realized much better in the National Geographic-Palomar Observatory Atlas. The solar programs also led to no novel discovery and the visible-band spectroscopy was nothing extraordinary.

What was done most usefully was the purchase of the reflector and the siderostat. By the indulgence of Mrs. Cook who lived until 1965, University staff and students were permitted to continue to use the RHO facility after her husband's death even though the will specified that the place be vacated promptly. It can be understood that this was impractical during the War when there were no people at all to clean out the place as well as no observers. In 1949 the University Trustees passed a motion thanking her for her indulgence. In fact, Mrs. Cook continued to tolerate use of the facility on her property until 1956 when the successor station was built and the RHO finally closed. Until 1996 the reflector and siderostat were essentially fully scheduled for students and faculty of the University. Student training was a significant minor fraction of this use.

A lesser, but not insignificant, accomplishment lay in Cook's purchase of some of the smaller telescopes. The three largest astrographs were sent directly from McDonald to the Mt. John University Observatory, New Zealand in the 1960s where they were remounted. The 5-inch camera was used to create *The Canterbury Sky Atlas*, a southern extension of the Lick Survey. Covering the interval $-90^\circ < \delta < -45^\circ$, it was published by Doughty, *et al*. (1974). These instruments are used only infrequently now. The transit system and the 4-inch astrograph found continuous use as training instruments and therefore also filled a need for decades. For a number of years, the spectrohelioscope/graph system was loaned to A. P. Galatola of LaSalle College and then was returned to the University where it was remounted for instructional and demo use in the Students Observatory on the campus. In 1956 the 8-inch refractor was remounted in the same locale and for the same purposes. Thousands of people have had opportunity for day and nighttime views of many phenomena. The long-focus solar imaging system was loaned to the USNO and the Schmidt was sold without ever having been used as far as I am aware. Most of the small hardware mentioned in assorted reports was still to be found when the RHO closed. When Cook's estate was settled, his scientific library went to the FI and his collection of model boats and ships to the entity that became The Independence Seaport Museum where they are still on display.

One of the most interesting matters concerns the inferior quality of some of the telescopes that Fecker delivered and the circumstance that he used pictures of some of them in his ads for years. I have myself used his 15-inch reflector at the University of New Mexico and found it not at all difficult to control and I am not aware of numerous complaints about the many other telescopes that he built. Maybe Cook was too easy on Fecker or too inexperienced to make of him demands for professional-level performance.

Cook's greatest limitation was failing to understand that he had built too amply. There is general understanding that a telescope needs 3 or 4 users to make every night productive. So he really needed a staff or a collection of visiting users who would number about 10. He had put so much cash into the apparatus that he may have been unwilling to spend the remaining monies necessary to staff his establishment adequately. Hall's salary at Swarthmore is known to have been about $1,600 [$21,600] in 1934 and presumably Mohler was paid a comparable amount. There is no information whether Tabor was ever paid a salary. It is, therefore, a reasonable extrapolation that Cook would have had to fund an annual payroll of up to $16,000 [$216,000] in order to get the optimum scientific return from his investment. Perhaps this was too large a sum, even for his wealth, during the Depression.

It might be imagined that modern scrutiny and processing of the measures and records could develop some scientific results that were not accomplished during the RHO's existence. I can think of only two retrospective possibilities. These result from the fact that the large-scale plates could now be scanned and digitized. First, if the scans are treated individually, they likely can be studied to detect a considerable number of polars for which patchy light curves could be composed. Those light curves then could yield approximate times of minimum light in pre-discovery time and thus extend considerably backward in time the dynamical, and inferentially, the magnetic histories of these interacting white dwarf systems. Secondly, for the individual digitized plates some slow-slope algorithms could be developed so as to co-add all plates of a given field. Because the images are so good, this might gain another magnitude in plate limit and contribute something to the pre-discovery histories of some bright quasars.

#I offer one personal recollection of the Roslyn House Observatory. In 1955 I was named the Steward Observatory Fellow at the University of Arizona and was expected to develop my own observing program for what became a two-year tenure. To this end, Blitzstein and Wood offered me about an hour of instruction using the single-channel pulse counter and I observed for a few comfortable nights to make sure of what I was doing. Steward, on the other hand, had only a dc photometric system so the pulse counting experience seemed not too useful. Perhaps out of sympathy, Binnendijk said he would show me how a dc system was really used so I should come observe with him. His idea of observing instruction was that he literally did everything never permitting another person to touch the reflector or photometer or even to write on the strip chart of the recorder. My one night of watching and listening as he worked through the Pleiades was at 15º F and it became a question of whether I or the old guy (he was 42 at the time) would be the first to admit that feet were cold. He had the advantage of having observed in Minnesota winters.#
When I returned on a visit in 1956, the RHO was gone and the new observatory was functioning. After Mrs. Cook's death, the property was sold and the inevitable development ensued.

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THE FLOWER AND COOK OBSERVATORY (FCO) AND THE STUDENTS OBSERVATORY (SO)

INTRODUCTION

After it was decided that there would be a new consolidated observatory, realization of the facility was far from immediate. World War II had depleted even the small staff and the University itself recovered only slowly from this interval. By 1948 the decision to create the new observatory was approved by the University as Olivier (1948) reported. What happened thereafter may be surmised from a succession of Observatory Reports. Olivier (1949): "A new site of 31 acres about 13 miles west of Flower Observatory has been acquired, and plans are being made…"; Olivier (1952): "Lack of funds has so far prevented…"; Olivier (1953): "Lack of funds still prevents…"; Olivier (1954): "The plans for combining the two (observatories) at the new site, purchased about 5 years ago, have moved nearer to realization…". How did this finally happen? Funds had come to hand from the sale of the Flower site as described earlier and so Wood (1955) could eventually report: "Final detailed plans were completed for the new Observatory near Paoli, and construction was started. To be called…".

#For some while, there was departmental debate about the name of the successor observatory. Should it have both the Flower and the Cook names or should the Roslyn House designation be used? If both names were used, should the ending noun be singular or plural? Could that detail be avoided by using just the Cook name since the only functioning telescopes were his? Against that possibility, there were the recognitions that the only money would have to come from sale of the Flower property and that the Flower endowment still existed. There was never any passion associated with these semantic quandaries but they did last quite a number of months. Finally, with Olivier's encouragement, Wood settled on The Flower and Cook Observatory.#

STRUCTURE AND EMPHASIS

Materials are easier to find for the interval of the (FCO+SO) than for the times when the FO and RHO were functioning. My memory for people and events is also fresher although there remain few people whom I can consult to confirm my recollections. At the same time, this chapter cannot be of the same character as the two preceding ones. Faculty experimentalists and theoreticians do not appear in the story. Almost all the grad students from this time are still alive and my candid expression of their contributions would be acerbic in some cases. I could write that type of text but the courteous thing to do would be to keep the document closed until, say, 2060 when everyone would be dead or past caring. The alternative that I have chosen is to de-personalize this presentation almost completely by giving a personality sketch of almost no students and, insofar as possible, not even to recognize their individual accomplishments. Rather, work of numerous people is lumped under an appropriate umbrella topic and an understanding of the net result is developed. Apart from some blueprints and photos, which are in the files of the Department of Physics and Astronomy, all other source materials are to be found in the open literature. The cutoff date is 2008, 12 years after I retired and I could have accidentally missed some of the observational people since 1996.

RURAL CHESTER COUNTY

The site of the FCO first appears in University documentation when DuBarry reported to the Trustees on October 24, 1949 that a bit more than 40 acres had been purchased for the FO. (This was also the occasion on which he reported that $107,000 [$811,000] had been offered for the Flower real estate itself.) I have found no indication of the source of the apparently new purchase money.

It is impossible to know now how attention had turned to the eventual site for the FCO but any inquiry would have to consider A. Felix du Pont who died in 1948. This man was a long-term Trustee who lived in

Chester County and must have known of every local event. It is possible that he called to the attention of someone in the administration that land owned by the Atwater Kent Realty Company was going to come on the market in the relatively near future. Different sources give somewhat different accounts of Arthur Atwater Kent (1873-1949), who apparently used his first name only for legal purposes, but here I have referred to Williams & Wolkonowicz (2002) only. Kent, twice a college dropout, had started his first business in 1895 manufacturing small electrical motors and generators. This lasted only briefly and then he had two other jobs for other companies but in 1902 he started a second firm, again dealing in small electrical appliances. Beginning in 1905 he branched out to electrical components for automobiles and this did well until an economic slump after World War I forced him to change his product line to radios. The company became phenomenally successful with almost annual introduction of new models. The first commercial models were mounted visibly on mahogany boards and the company may or may not have brought into currency the term "breadboard" for circuits that are unenclosed in a chassis. Atwater Kent radios were marketed aggressively through all of North America and Kent became even more visible by sponsoring a weekly radio hour of opera selections. The Depression brought bad times, falling sales and pressure to unionize. Kent was opposed to unions and closed the company in 1936. He had lived the good and conspicuous life and wasn't about to stop: significant gifts to Worcester Polytech, the Atwater Kent Museum of the History of Philadelphia, mansions here and there, and many autos. His most singular inhibition appears to have been vegetarianism. "Mr. Host" in Bel Air, CA, he died there after a lingering illness while enjoying the company of Hollywood characters. There is a vigorous nostalgia trade in Atwater Kent radios and components at present.

The FCO property was carved from two unimproved lots that Kent and his wife Mabel L. had jointly bought from private owners for $1 each around 1930. In 1941, they sold these parcels for $1 each to the Realty Company that would manage their land holdings. The Realty Company in turn sold a fraction of them to the University in 1949 for $20,045.90 [$151,000] – not a bad return on $2 in less than 20 years. I have not examined any other real estate transactions of that time and locale to know if the University had a realistic understanding of property values. The sale occurred about 7½ months after Kent's death as the Company was beginning to liquidate his estate. Apparently within a few more months, about 9 acres along the forested north edge of the newly acquired property were sold because it was believed that the closer dense woods provided adequate light protection. Shortly thereafter, the Treasurer picked up the diminished property on his records for $15,544 [$117,000] while ascribing it to the southeast, rather than the correct northwest, corner of a particular country intersection and while also misspelling the name of the township. In 1966 a parcel of 5 open acres to the west was bought from John J. and Alix Rockwell Hill IV for $18,000 [$100,000] for added light protection and more protection against private encroachment. Clearly, land values were rising quickly. To the north and east, enclosing woods protected the site but to the west and south there were only open fields edged by copses. Situated on a named rural road, the property lacked municipal water, township trash collection and storm and sanitary sewers but it did have electrical power, RFD mail delivery and telephone capability. It was actually very nearly at the end of the power line and there were few other users. Well into the 1980s, electrical transients were conspicuous and a regulator was needed to limit the effects of the surges and drops. It was understood from the beginning that a well and septic field would be needed and that heating would be from delivered natural gas.

The surrounding countryside was, however, not empty and the political process functioned through the Commissioners of Willistown Township. Here and there were gentlemen or real farmers, some horse farms and some retirees but the major presence of the neighborhood was the Radnor Hunt Club about 1,500 yards down the road. It was never clear to the astronomers whether this was a blood hunt or just exercise for the fox and the hounds, horses and riders but #initially the Hunt members were not at all enthusiastic about losing open countryside to some odd characters who stood for higher education and science, whatever they might be. Objections by the locals and the Hunt were dropped when the University announced that the FCO would not be fenced and that the Hunt was welcome to ride across those acres just as previously. This was not always an agreeable accommodation as may be understood from one early Saturday morning event. The Wood family usually kept about four cats and fed them outside. On just this morning, Bede

Wood in her night robe was spooning the food into the cats' dishes when the Hunt came enthusiastically onto the property. The hounds instantly lost whatever scent of fox they had and fell upon the cat food creating a mêlée with Bede in the center, the traumatized cats crawling up her skirts, the dogs scrabbling over each other to devour the food and the huntsman whipping away around the perimeter of the mass of dog flesh. It made for an exciting 10 minutes before some discipline was restored and the Hunt went off into the next field not so glamorously as before. Blitzstein, a town mouse if ever there was one, always disliked the FCO site and the Hunt because he was sure that the horses were the source of all the very large flies that walked across the optical surfaces from time to time. In the surrounding woods, the lower story of plants and trees supported luxuriant poison ivy vines and, as it turned out, a burgeoning population of white-tailed deer and ticks. After about 1980, six cases of Lyme's Disease were contracted by observers and the family living in the residence.#

THE PHYSICAL PLANTS

Wood's appointment as Executive Director of the Observatory and Associate Professor dated from 1950 #and a certain number of enticements had been necessary to make him leave Arizona. He would have final say on the Observatory design and installed equipment, a residence would be built for his family, he was permitted to install a swimming pool at his own expense and he could keep a horse on the property.# With no funds in hand to build, the University still went ahead with architect's designs. This did not proceed in a monotonic fashion at all. One early example of the stuttering process is indicated in Fig. 42.

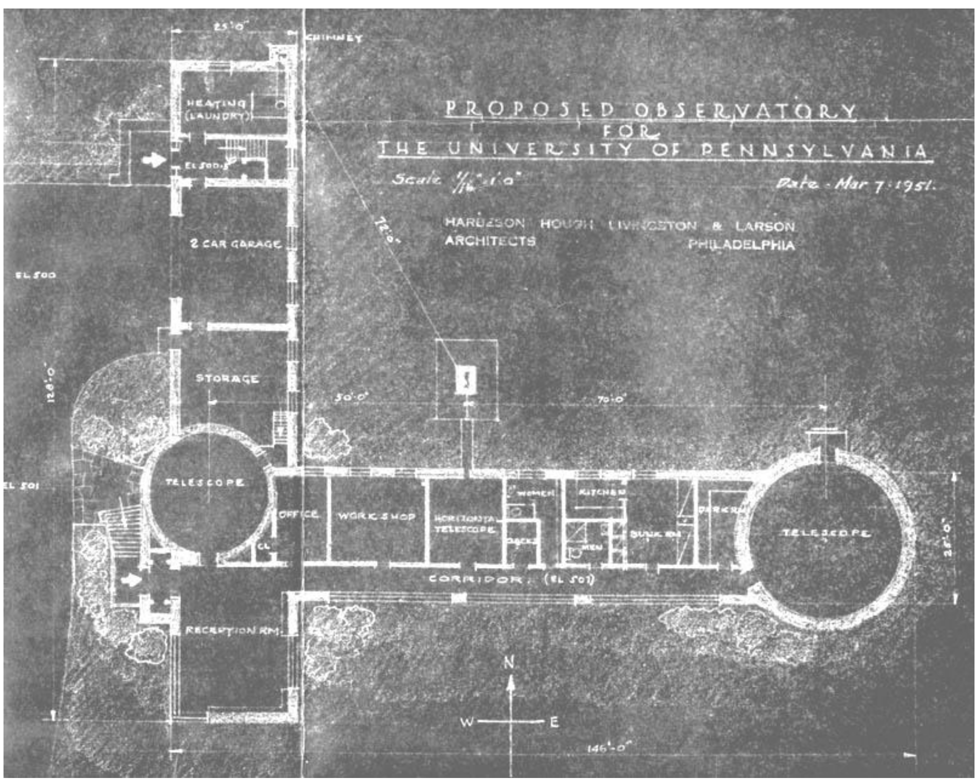


Fig. 42. The ground-level plan portion of an early design for the FCO. In the west wing a utilities room, a 2-car garage and a storage room underlie the living quarters. No thought was given to the exhaust fumes from cars rising into the living room and bedroom. The left dome was intended to house the Cook reflector and the right dome the Flower refractor. The Cook siderostat would have been located about halfway between the domes. Connecting the domes are workrooms, bathrooms, a bunkroom and a kitchen
A second drawing shows that above the west wing there would have been a second-floor with (from north to south) a kitchen and dining room, a living room, a bath and two bedrooms.

The elevation above sea level is, for eastern PA, a non-negligible 500 feet. The reverse side of the sheet shows Blitzstein's comment: "Grandiose plans for Flower & Cook Observatory before Stassen and DuBarry fouled up sale of Flower tract in Highland Park, Upper Darby. Went for $140,000 instead of original offer of over $200,000."

At the time this drawing was submitted, Wood's appointment was 9 months old and yet the architect offered an inoperable concept. The firm's first principle was undoubtedly to conserve earth-moving and maintenance costs so the residence was conceived as the upper floor of the west wing of the scientific building. #Bede Wood vetoed the design on the reasonable grounds that a family with 3 girls and a boy could not be expected to get along with 2 bedrooms but her second emphatic reason was that she was not going to live in an observatory.# The eastern dome was intended to house the Flower refractor, but with almost no clearance to lay the telescope horizontal for maintenance purposes and very little room for an observer to hunch over and look at the zenith.

As I noted above, time passed beyond 1951 and nothing at all happened. Possibly, the University was paying the rent for the Wood's home in Drexel Hill through the interim. By 1954 another architect was associated with the original one and was playing a more conspicuous role. The new man was Alfred A. Bendiner (1899-1964), with his own firm since 1928, and it would be hard to imagine a personality more incompatible with a sober scientific mentality than his. Bendiner had credentials in abundance: a student of Paul Cret and trained in Rome for two years, a long career, associate membership in The National Academy of Design, past-President of the Philadelphia chapter of The American Institute of Architects of which he was a Fellow, and great metropolitan visibility. He had written three books, deplored destruction of established buildings simply because their styles were no longer in vogue, wrote a weekly column for *The Sunday Bulletin Magazine* for years, had lots of talent for caricature of things and people, went along on some University archeological digs and affected a whimsical attitude about all things and events. Perhaps most importantly, Bendiner had two degrees from the University but there is no getting around the circumstance that he definitely lacked a record of conspicuous and successful buildings. A number of his character traits led Wood, Olivier and Blitzstein to label him a *poseur* and it is easy to see why when his writing and drawing styles are examined. A single example of his manner appears in Fig. 43 on the next page. In retrospect, the astronomers' judgment is somewhat off the mark for Bendiner never really presented himself as only an architect. #The three astronomers also considered him a fool and possibly this has more support. He pushed the idea that the south wall of the observatory building should be made of glass bricks so that the public could see the scientists at work. After an exchange of viewpoints, he did accept the recognition that you can't see anything in detail through a glass brick and so would use plate glass. Eventually he was argued out of this idea too.# The only logical understanding of this episode is that Bendiner was indulging himself in an extended joke at the expense of the others. He did get the message about the size of the Wood family and designed a residence that was a single-story box with rooms larger than formerly and with three bedrooms. #The Woods converted part of the basement into a bedroom for their two oldest daughters, one of whom was about to go away to college anyhow.# Bendiner's blueprints dating to 1954 are very clear and show every indication that he could design a satisfactory structure when he set his mind to it and understood the technical requirements to be satisfied.

After 1951 and before 1955 it had been decided that the Flower refractor would not be erected at the new site and so, with most local concerns satisfied, there was a call for contractor's bids to be received no later than May 2, 1955. The bids were not to include electrical work (to be done separately) or the dome because the University had already let a contract to the Pittsburgh Des Moines Steel Co. to fabricate that structure. The company was experienced but the choice was a mistake. The FCO specs actually call for an aluminum dome and shutter but a steel one was the ultimate choice. It was needlessly heavy and required too much power to rotate and maintenance was always a problem. A more serious mistake was made in housing the siderostat in a building with an over-designed rolloff roof. For a reason that no one ever could

UNION LEAGUE

And then there is that one about the visitor and the Philadelphian walking down South Broad street.

"What is that building?" asked the visitor.

"That," said the Philadelphian, "is the Union League."

"What's the crepe on the door for?" asked the visitor.

"I guess one of the members died," answered the Philadelphian.

"Cheers it up some, doesn't it?" remarked the visitor.

The Union League is where all good Republicans go to die. I think they now recognize Roosevelt, if you voted only once. The late Jack Kelly, Democrat, once walked up the front steps, blueing up a lot of tired blood and driving the old seat-holders to an extra hot milk and bourbon.

If you can get a foot inside the door, it is still Victorian Saratoga hotel or Mississippi riverboat, beautiful, and the food is much too good for the common people.

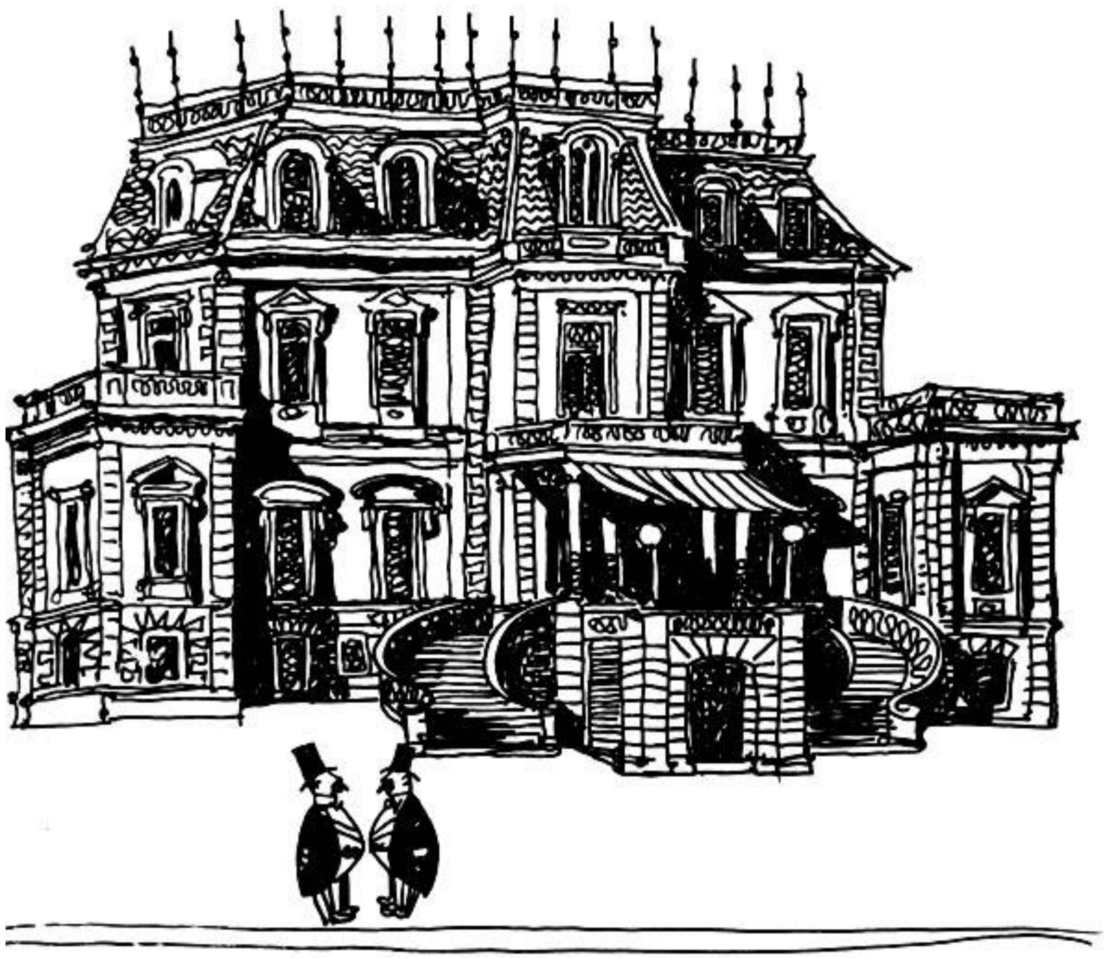


Fig. 43. An example of Bendiner's (1964) amusing columns that appeared weekly for about 4 years in a Philadelphia newspaper. At the time of the drawing's appearance, the League was widely understood as a citadel of 1870s thought processes and values. It has now certainly moved into the 20th century.

explain, this roof drive was not electrified and had to be operated manually by pulling on an endless chain captive on a sprocket; other gearing engaged a second endless chain which looped around a second sprocket captive to two large wheels built into the movable roof. The net effort was to drive that roof segment along horizontal rails. The roof weighed somewhat more than one ton and it took a lot of work to move it, principally if a person were small and tired at the end of a night's work. A final mistake was built into the height of the dome sill. Wood decided that he would preempt any temptation for an observer to observe too far into the eastern sky in the direction of the city. There was no logical reason for this idea since sky brightness was typically not the limiting characteristic of the intended measuring techniques. But he required that the wall be about 2½ feet higher than was mechanically necessary. This resulted, of course, in sky coverage that was more restrictive than necessary.

The general contractor did a reasonably speedy job. A man fell to his death while the dome was being erected but otherwise the work was done without incident. A photo taken when the facility was new appears in Fig. 44. All the walls were of double-laid cinder block 14 inches thick and completely without

Fig. 44. A view of the FCO from the west in the early 1960s. Every window was double hung with storm panes but the ceilings were a needless 10½ feet high. The front pergola of the residence, in the left background, was eventually roofed with unattractive fiberglass panels. The Ford station wagon finally expired around 1980.

insulation; most portions of the roof were not insulated either. After construction was complete, the telescopes were installed and only then was it discovered that the north and central piers of the siderostat were about 1.25 inches higher than the south pier carrying the objective. Consequently, the objective could not receive centrally the horizontal beam fed to it by the flat. This problem was never corrected and the inevitable vignetting remained.

Rather soon after the Woods moved in, Traveller, a very gentle pony, appeared and was ridden by children when they were small. #Blitzstein made no exceptions: he disliked Traveller just as much as the Hunt's horses.#

It remains to describe the Students Observatory built atop the 4th floor roof of the campus building housing the Physics, Mathematics and Astronomy Departments and their Library. These departments had lived for decades in parlous quarters with little internal unity and finally the University decided to rectify the disagreeable situation. I do not propose to tell of the fitful progress in funding and constructing the first stage of the current structure but eventually that wing was done and named the David Rittenhouse Laboratory after the original choice of Franklin's name was discarded. Wood, Blitzstein, Merrill and Protheroe all worked on the choice of the small instruments to be located there and finally chose the 8-inch refractor – a general purpose instrument for easy learning, the Cook broken transit – useful for teaching fundamental astronomy, the 4-inch Ross/Fecker astrographic camera – adequate for the rudiments of astronomical photography, and a pair of clocks.

The Laboratory is four-square with the street corner on which it sits and the telescope piers were installed parallel to the walls of the building. William Penn's grid of streets is, however, off the cardinal directions by about 8º. The architect did not discover this misorientation and the astronomers didn't check it so the contractor poured the piers incorrectly. Steel adapter plates had to be cut, bored and counterbored for mounting each telescope on its pier; an example of this carelessness can be seen in Fig. 45.

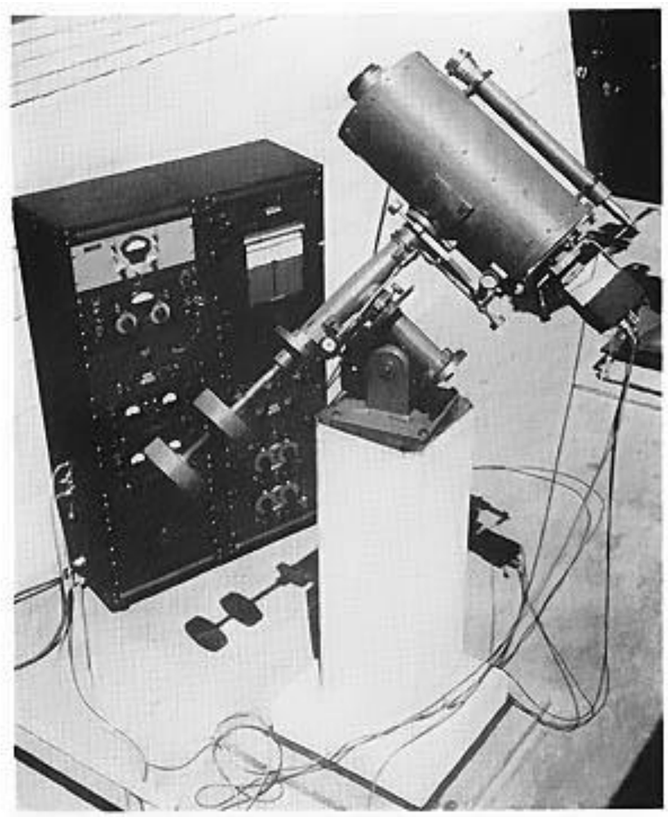


Fig. 45. The real interest in the picture is the necessary misalignment of the adapter plates on the top of the pier of the 4-inch Ross/Fecker camera. Protheroe's first scintillation photometer is mounted on the camera.

For the SO an electrically-driven roof was provided and this opens onto a supporting rooftop steel skeleton. The adapter plate and the German mount of the refractor holds the tube and declination axle quite a distance above the floor and there is only about 1 inch of clearance between the correctly stowed telescope and the moving west edge of the roof when it is opening or closing. #Over the years, numerous accidents occurred requiring new shear pins, or much more substantial repairs, to the telescope. The most annoying incidents were those in which the inattentive party failed to tell of the accident in good time for fear of Blitzstein's scolding. The result would be discovered only on the succeeding day or night when the facility was needed instantly for class or public demos.#

LOTS MORE PEOPLE

Some of the people associated with the later years of the Flower and Roslyn House establishments were still working in the mid 1950s and they simply continued at the FCO but there also emerged other individuals who need be mentioned now. For some of them, I could find no photos.

Charles R. Alcock (1951-) came to the University in 2000 as seventh Flower Professor from UCBerkeley and The MACHO Collaboration. Because there had been so much success identifying and interpreting gravitational lensing of, *e.g*., very distant quasars by closer massive galaxies, it is natural to suppose that similar lensing displays would be picked up if a monitoring program looked at a very dense distant star field through a reasonably dense foreground field of stars. The routine availability of large-format and fast-response CCD cameras beginning about 1985 set in train a few programs doing just that. The team of which Alcock was a member had a nice situational advantage in that the Large and Small Magellanic Clouds, companion galaxies to our own, would be nicely visible from Australia for a certain fraction of a year and, when they became inconveniently low in the sky, the team could look at the central bulge of the Milky Way Galaxy. Lensing candidates were indeed found and fractions of light curves compiled for them. Beyond the dedicated purpose of the program, it was a certainty that many new pulsating or eclipsing variable stars – they would not be lensed − would be picked up by the panoramic cameras and this would create an opportunity to enhance by a very large factor our inventory of these objects. This in turn would create a downstream need to handle timed data in quantities that had never been experienced before. Large teams investing complementary capabilities were needed and inevitably led to inconveniently large gangs of authors on many, many papers. At the University Alcock spent his time trying to create a virtual global observatory and to start up the Taiwan-American Occultation Survey for detecting lensing phenomena by Kuiper Belt objects beyond Neptune. A few consequential results from the MACHO team appeared in 2000 and 2002. In 2001 he was elected to the National Academy of Sciences but after 4 years he was recruited away to become, *inter alia*, Director of the Harvard-Smithsonian Center for Astrophysics.

Frank M. Bateson (1910-2007), a very well known and productive amateur working on variable stars and a 1930s member of the AMS, was appointed Research Associate with responsibility for site testing in both islands of New Zealand (*e.g*., Bateson 1962, 1963) when Wood was first realizing his hope for Southern Hemisphere observing. Eventually he was appointed Astronomer-in-Charge for day-to-day operations at the South Island station near Lake Tekapo. Most of the visiting FCO grad students behaved well enough so that they didn't offend most New Zealanders but, to a man, they uniformly disliked Bateson's starchy, superior and unwelcoming manner and felt that he had numerous reasons to be modest if he would just set himself to thinking about them. Personal conflicts between Wood and Bateson arose after a time but the latter man did not trouble me when I was Director. Bateson continued to work productively long after the collaboration with the University ended.

Gary M. Bernstein (1962-) took his degree at Berkeley on a cosmological topic and then spent some years on postdoc appointments – first at Bell Labs and afterwards as the Bok Fellow at the Steward Observatory. These were followed by academic appointments at Michigan and in 2002 he arrived at the University. His productivity has been high and has attracted numerous collaborators with a double-barreled career. The weak-lensing fraction of it might be exemplified by the rather recent paper of Huterer *et al*. (2006) concerned with systematic effects when surveys looking for such lensing effects are run in the future. The second matter appears disconnected from the first in that it concerns the outermost volume of the Solar System. One somewhat older contribution to this subject appears in Bernstein *et al*. (2004).

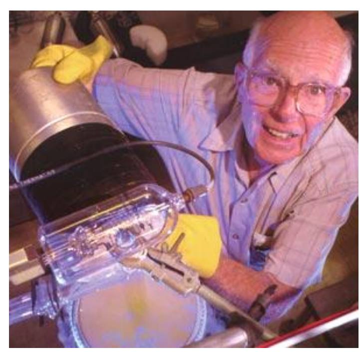
John G. Brainerd (1905-1988) had been Director for the ENIAC Project during World War II and became Dean of The Moore School of Electrical Engineering in the early 1950s. He wrote the first undergraduate text about radar and established the first bioengineering program in the country. Brainerd met Wood socially and they found common hopes for electrical engineering applications in astronomical photometry. It was through their agreement that Blitzstein was given a joint appointment in EE and Astronomy. Brainerd continued to be an outside friend of Astronomy and toward the end of the 1950s was instrumental in the appointment of the first radio astronomer to the faculty.

William Buscombe (1918-2003) spent his entire career in stellar spectroscopy. Another Princeton PhD, he worked many years at Mt. Stromlo and presumably he and Wood became amply acquainted when Wood spent a sabbatical at that observatory in 1957-1958. Buscombe was appointed Visiting Associate Professor for the 1964 academic year but this was not useful to anyone. The rest of the staff evinced little interest in his work and he formed an unfavorable opinion of their research and that of the grad students. Buscombe was an active Friend and might be thought to find a congenial environment at the University with its original input from the Quaker ethic. If that did occur, it was not in the Astronomy Department. He ended his career at Northwestern and his death led to warm testimonials and remembrances from several places in the U.S. and Australia.

Robert E. Davies (1919-1993) was a British biochemist appointed to the School of Veterinary Medicine. He and Langer established a course examining what would have to be credible evidence for accepting the existence of extraterrestrial life, and after a while Davies and I became the team-teachers for the course.

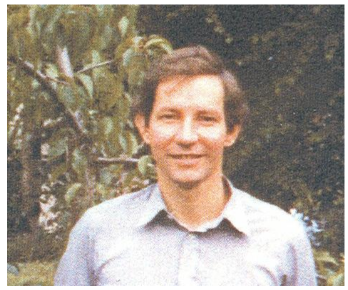
Bob's interest in astronomy started when he had been captivated by the BBC radio talks of Fred Hoyle. He had had an eminent career beginning as a student of Hans Krebs and, as he frequently said, then became the world's expert on the musculature of the stomach of the frog. His was both a warm and sardonic personality with particular sensitivity to attempted incursions by The Administration – #that ignorant lot − on faculty perks. He once bet me that a particular officer of the school would not know any scientific word I would choose. I really didn't believe this accusation and chose *isotope* because it was then in the news for a couple reasons. I lost the bet.# He had been a member of the committee convened in 1966 by the Space Science Board to examine the dangers of the pure O_2 atmosphere inside the *Apollo* spacecraft. The committee's negative recommendation was published only after the fatal fire. From him I picked up a lot of chemistry and we wrote a couple of papers that united our interests. A brief note (*cf*. Koch & Davies 1984) disputed the claim by Karim *et al*. (1983) that *IUE* spectrograms provided evidence of interstellar proteins. By Bob's standards, we were the most visible scientists in the world for one week (or maybe one day) when the Editor of *Nature* took notice of another of our products (*cf*. Davies & Koch 1991) and put a spin on the paper that we had never intended. Bob was a man of amazing vitality. Had the 1940 Olympics been held, he would have been a member of the U.K. pole vault team. In preparation for his annual mountaineering trips he practiced walking unsecured on the parapet of his 4-story office building until this came to the notice of the Dean. He was also an experienced spelunker with a feature of some sort in Wookey Hole in Somerset, U.K. named after him. On a walking trip in Scotland he finished supper, left the lodge and fell dead within a few steps.

Raymond Davis, Jr. (1912-2006), along with Masatohi Koshiba and Riccardo Giaconne, were the 2002 Noble Laureates for Physics. That specific division of the award froze out the major contributions of John Bahcall to neutrino studies. After World War II Davis had made a formidable record and reputation for his neutrino experiments with the Savannah River reactor and at Brookhaven National Lab and had started his attempts at detecting the solar neutrino flux in 1965. By 1967 he was taking data at what eventually became the Solar Neutrino Observatory in the Homestake Mine, Lead, SD at a depth of 4,850 feet. Although his data have no information about solar neutrino variability, they made it clear that the then-standard solar model predicted a neutrino flux that was not observed. It was an inspired idea for Shen to appoint him Adjunct Professor of Astronomy in 1974. Ray's reputation was towering and it would be difficult to find a more congenial colleague than he. It is reasonable to say that Ray was an inspiration for everyone around him.



Mark J. Devlin (1966-) had been at The Center for Particle Astrophysics, Berkeley when he was recruited to the University, where he is the incumbent Flower Professor. He had abundant hardware design and fabricating experience as a team member working on the CMB. Just as for teams concentrating on gravitational lensing, these groups are very, very large so that literature citations either use some shorthand

or have to display many lines of authors' names. It's inconvenient to cite specific papers here but it must be understood that the CPA team concentrated not only on mapping the CMB signal itself but also on publishing abundantly about possible biasing effects such as the microwave emission from warm interstellar dust and similar emission from compact sources that would be foreground objects. The concept of a months-long balloon flight circumnavigating Antarctica with a dedicated microwave detecting package was realized in the BOOMERanG experiment. Devlin was a part of all these efforts. Currently, his activity is spread across additional balloon flights, a ground-based monitoring program in the dry *altiplano* of Chile and the design of a radiometer for the 100-m radio telescope at Green Bank. These assorted efforts are dedicated to work on the Milky Way Galaxy, distant galaxy clusters and the CMB.



J. David Dorren (1945-) served as Lecturer during two disjoint appointments. Trained at Oxford as a theoretical particle physicist, he had had several postdocs and came to astronomy through his association with the Physics Department at Pahlavi University, Iran. Blitzstein and I had been instrumental in establishing the observing facility at that school and building its first photometer and Guinan attracted Dorren to observational work there. After the Iranian Revolution, Dorren came to North America as a staff member for several years at the University and at Villanova University before returning to the UK. He was a superb undergraduate teacher and an excellent photometric observer.

Paul B. Eskridge (1960-) was a staff member for only the 1990-1991 year, having come from RPI. For a young man, he had already a fairly long record in galactic and extragalactic studies when he arrived and he extended it during the year, not by local or even contemporaneous observing, but by exploiting data already in hand for himself and some collaborators. The work with Pogge (Eskridge & Pogge 1991) concerned the correlation between the HI content of S0 galaxies and the emission of these objects in the far-IR. In Hodge *et al.* (1991) the authors examined star formation in NGC 6822, a relatively nearby dwarf irregular galaxy and by himself (Eskridge 1991) Paul presented a catalog of the member galaxies in the more distant Dorado Cloud. He left to go to Harvard SAO and has since taken another position in Minnesota.

Theodore D. Fay (?-) had taken his PhD at Indiana and then became a junior colleague of Wyller on a solar spectroscopy program. When that postdoc ended, he returned to Indiana as a Research Associate continuing his broad spectroscopic interests. Fay subsequently held an academic position at the University of Alabama but, despite an abundant publication record, moved away from astronomy toward an engineering career around 1985. I have lost track of him.

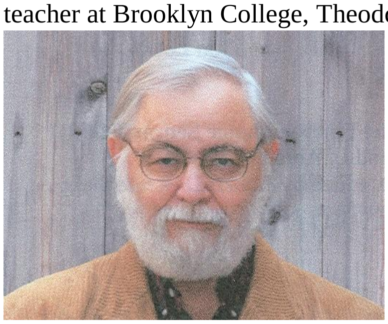
Mario G. Fracastoro (1913-1994) spent the fall, 1961 semester as a visiting faculty member and observer. He had taken his degree in 1942, #passing his exams with artillery fire in the neighborhood,# and was still on the staff at Firenze in 1961. Fracastoro had a gift for working with promising younger people and he was able to indulge this capability by observing with the FCO students as well as with his Italian students. He built on his beginnings at Firenze to establish the breadth of the modern Catania Observatory and later moved to Torino where he continued some already functioning programs while starting a modern astrometric effort. A Etna field station of the Osservatorio di Catania is named after him. He was an ebullient and most likeable scientist.

Herbert Friedman (1916-2000) was also named Adjunct Professor at the instance of Shen – another very successful appointment. Friedman had been a very productive X-ray laboratory scientist and around 1950

moved into space astronomy with sounding rocket experiments concentrating on the ionosphere and with many additional flights quantifying solar X-ray flux levels and solar X-ray images. His team was the first to detect both diffuse and point-source X-ray fluxes in the Milky Way Galaxy and he was ultimately able to show that the Crab Nebula X-source was occulted by Moon and therefore had to be intrinsic to the nebula. The present and eventual understanding of the Crab pulsar and all its extraordinary detail can be traced to this discovery by Friedman's team.

Christ Ftaclas (1944-) came to the University as a postdoc to work with a Physics faculty member but after a year moved to Astronomy when a teaching vacancy opened. In the classroom he was superior. He had finished grad school as a theoretical cosmologist but by 1982 had changed his emphasis so as to collaborate with Struble on a variety of topics associated with clusters of galaxies. In Struble & Ftaclas (1982) they are looking for systematic effects in images of galaxies between the original and second printing of the POSS. Two years later and with an undergraduate student, the same authors (Ftaclas *et al*. 1984) are claiming that the Local Supercluster affects the velocity dispersion of the member galaxies of the Virgo Cluster itself. Between these two contributions they (Ftaclas & Struble 1983) developed evidence that the ellipticity of well-exposed and accurately-photometered images can distinguish prolate from oblate cD galaxies. When his appointment terminated, Ftaclas went to work for Hughes Danbury but quickly thereafter found his way back to academic life. He is now in Hawaii.

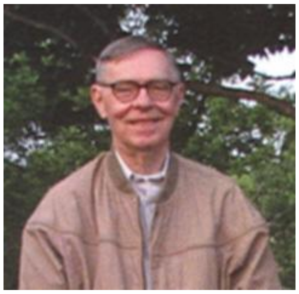
Anthony P. Galatola (1935-) was impelled toward an astronomical career by an outgoing and gifted teacher at Brooklyn College, Theodore A. Smits (1899-1964). Smits had himself been a Physics grad



student and Instructor at the University but he left without a degree and married one of his students (or maybe the order of these events should be reversed). After taking his own degree, Galatola spent 9 years on the Physics faculties at LaSalle and Union Colleges. He then moved into an industrial career with General Electric (later Martin Marietta and ultimately Lockheed Martin) at Valley Forge, PA. He became the corporate specialist in very fundamental astronomy concerned with satellite coordinate systems and attitude control. Galatola retired in 1998 and then spent more than a little time at the FCO with his own photometric programs. By accident, he set in train the process toward the ultimate disposition of the establishment.

Bruce D. Holenstein (1960-), a Physics undergraduate at Bucknell, worked with Blitzstein and me as a grad student. His dissertation monitored the Stokes parameters of cool giant and supergiant stars and was

quite successful. Holenstein, however, had no intention of looking for an academic career or even a full-time research one. Rather, he and his brother Paul already had a successful software company and it was to commercial life that he returned. Over the years, the company has prospered both domestically and internationally, been re-organized and re-named Gravic, Inc. and has about 35 employees (Richard Mitchell among them) at present. The Holensteins and Bill Highleyman are the authors of the 3-volume *Breaking the Availability Barrier*, a presentation, defense and illustration of "active-active" software/hardware systems with downtimes much less than 0.00001% of running time. When it became clear that the University was going to divest itself of the FCO, Holenstein pulled together a loose confederation of local amateurs, neighbors and University people to try to alter the mission of the facility and to preserve and develop it further *in situ*. He is currently developing a floppy-mirror facility for a private observing station..



Robert H. Koch (1929-) had been a grad student at the University and at the University of Arizona and thereafter a faculty member at the Four College Department and the University of New Mexico. I was unprepared for Wood to leave within a year of my arrival at the University and also unprepared to become Acting Chairman, a job for which I did not have the necessary force and subtlety of character and personality. I refused to become Chairman because of the foolish hope that the University would recruit a senior scientist from outside to fill the full position. A happy day occurred when I finished this administrative chore. In principle, this new free time should have led to more insightful research but I can't make that claim. An extremum of my career occurred in 2004 when Robert Mkrtichian and I were introduced. His evident astonishment was due to the belief that someone who had made photoelectric measures 49 years ago should reasonably be dead. Either this says something about longevity in the Former Soviet Union or it really is time to go.

David W. Koerner (1955-), arriving from Caltech, was a staff member from 1998 into 2002 after which he moved to Northern Arizona University. His interests in astronomically small masses has led to assorted near- and far-IR flux-measuring and imaging observing programs from ground and space. During his time at the University he touched on immense dusty disks around stars (*e.g.,* Padgett *et al.* 1999, Marsh *et al*. 2002) and binaries in the coolest stars and brown dwarfs (Reid *et al*. 2001). Since moving to the west, his career has broadened to examine interstellar clouds as well as continuing work on stellar disks and very small stars with the *Spitzer* spacecraft.

Yoji Kondo (1965-) was born in Yokohama and, after a brief interval in Brazil, took a business position in NYC where Isadore Epstein of Columbia offered him some advice about graduate training. Thereafter, Kondo took his PhD at the University and immediately began his career-long sequence of appointments with NASA facilities. At Johnson Manned Spacecraft Center he was head of the Astrophysics Lab during the *Apollo* and *Skylab* intervals and then moved to Goddard where eventually he became leader of the geosynchronous satellite observing programs for about 15 years. Most notably, this covered the lengthy success of the *IUE* vehicle followed by *FUSE*. A lengthy series of Colloquium and Symposium credits appear on his *cv* and he became a successful international bureaucrat participating in the re-organization of the lower structures of the IAU. Shen appointed him Adjunct Professor in 1978 and he served two years. In fact, he has had several such appointments at assorted universities and also has a series of science fiction stories published under a *nom de plume*. In these, there appears a character, Tiger Kondo. Currently, he is President of the Aikido American International Organization and has had a minor planet named after him.

Serge A. Korff (1906-1989) was brought to the U.S. from Russian Finland as a child and eventually took his Physics PhD at Princeton. His interests led to appointments at the Carnegie Institute and at Bartol during the 1930s and thereafter he spent the rest of his career at NYU. That career was distinguished and will be remembered for the strenuous field efforts on ground and with balloons and aircraft to map the cosmic-ray-generated neutron flux in space and time. In this case, "ground" typically does not mean sea level. The role of the atmospheric neutrons in generating C^{14} laid the foundation for Willard Libby's calibration of carbon dating. Korff was a gentlemanly and kindly man and Shen was able to appoint him Senior Lecturer in 1975; he served for a few years.

Kenneth Lande (1932-) emerged from Columbia as a high-energy physicist and joined the Physics Department at the University in 1959. His interests moved toward cosmological effects that could conceivably be evidenced by low-mass or "massless" particles and, thus naturally for the times, to neutrinos and anti-neutrinos. He became Acting Chairman in 1984 and served conscientiously in a difficult position for he was still carried on the Physics budget. Lande was a fine colleague and friend. By the 1970s he had been using medium size underground water detectors to search for neutrino and anti-neutrino emission as a result of stellar collapses. None was ever detected. When the University assumed responsibility for the neutrino detector in the Homestake mine in Lead, SD in 1985, it simply formalized Lande's association with the Davis experiment. He had a great affection for Ray and frequently assisted him personally during his last years. In subsequent years he has capitalized on his radio-chemical experience to begin dating some geological salt deposits that have never been dated previously.

William D. Langer (1942-) was appointed Assistant Professor in 1976 but stayed only two years. He was an inventive teacher, creating and developing with Bob Davies a stringent interdisciplinary course examining the possibilities of and constraints on extra-terrestrial life. The FCO and the University offered no colleagues for a radio astronomer so Bill left for Bell Labs and subsequent academic and administrative positions and has enjoyed a satisfying career. While his observational work is emphasized in the following text, the critical synthesis of the cooling/heating balance in interstellar clouds by Goldsmith and Langer (1978) is also a very significant accomplishment.

T. K. Menon (1928-), having finished his degree at Harvard, came to the University with junior rank in both Astronomy and Electrical Engineering in 1958. After two years he departed for NRAO in order to enjoy more radio astronomers in his daily associations but the evidence is that he had made a good impact on some grad students before moving on. His career after the NRAO years has been very creditable.

John E. Merrill (1902-1991) was really an applied mathematician. The almost intuitive way by which Russell analyzed eclipsing binary light curves was formalized in Russell (1912) and Russell & Shapley (1912). Not everyone possessed this insight, however, and light curves were becoming more precise and more amply covered as the years passed so Merrill codified the procedure in an early application of mainframe computing and graphics with the products that became known as Merrill's (1950, 1953) Tables and Nomographs. Although not an observer, he had been a student of Russell's and had supported Pierce in his design for a simultaneous two-channel photometer. He exercised great influence in the intellectual life of the FCO while he was an Adjunct Professor at the University while still retaining his research appointment at the FI. With Wood he shared an unstinting admiration for Russell and Raymond S. Dugan. He, Blitzstein, Wood and Protheroe installed the telescopes at the SO and the FCO. Merrill was both a sardonic and kindly man. #He (and Wood too) could become very angry if it were suggested that Russell had any flaws or limitations.#

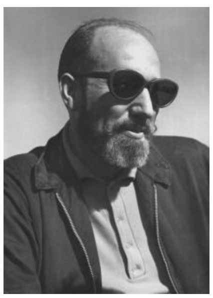
Steven T. Myers (1962-) already had a substantial publication record when he arrived at the University from Caltech. With notable co-authors, his microwave, radio and visible-band work concerned such matters as the Sunyaev-Zeldovich effect in a couple of Abell clusters and lensing displays discovered during the Cosmic Lens All-Sky Survey. During his time at the University there appeared the study of anisotropy on 7'-20' scale in the CMB by Leitch *et al*. (1996) and then a very lengthy cosmogonical modeling by Myers and Bond (1996a, b, c), but after 3 years he departed for the VLA at Socorro, NM where his career continues to flourish.

<u>Eva C. Novotny</u> (1932-) began as an observational scientist with summer experience on the Sproul astrometric program and a dissertation from Columbia (under Esptein) concerned with an atmospheric

analysis of the visual and spectroscopic binary μ Cas. For a few reasons, this work was not published. She participated in analysis of the motion picture film spectra from one of the solar eclipse expeditions but this also was unpublished. During her time at the University she concentrated on preparation of the volume *Introduction to Stellar Atmospheres and Interiors* published by the OUP but this did not appear until 1973 and was, so to speak, an ineffective career move. While the testimony of a published book is a necessary prerequisite for retention on an arts faculty, that kind of effort is so long running that science departments long ago decided that they wanted to see a sequence of refereed journal articles, and in Novotny's case that wasn't available. At the time, her compendium was actually a fine assemblage of the basics of her two topics and was loaded with useful reference citations at a very good price, but it did not find numerous niches within North American undergraduate and graduate curricula. In 1968 and 1969 she put in a considerable effort accumulating photometric measures of contact binaries but these too were not published as far as I know. A few short-term NASA appointments followed and then she was able to move first to Manchester and afterwards to Cardiff University from which place she eventually retired. Novotny was a person of considerable breadth of interest with concerns about music and good cooking of the best foods and in the UK she has recently been publicly opposing genetically-modified foodstuffs.

<u>Mirek Plavec</u> (1925-2008) had developed a reputation for insightful and inventive theoretical and observational studies of close binary evolution in Czechoslovakia by the time he was granted a leave in 1969. The first months were to be spent in spectroscopic observation at DAO Victoria and the last portion as my Research Associate. The grant funds paid a derisory stipend for a family but he came anyway so that we could collaborate on an evaluation of eclipsing light curve results. Toward the end of that year, he announced that the he was not going back home because of the oppressive Czech regime. I would have done anything to keep him on the staff but stagflation had clamped down on the University and it was politically impossible to open a new faculty appointment at that time. Through the interest of Protheroe and Arne Slettebak, Mirek was able to spend a short time at OSU and then fortunately was recruited to UCLA by Daniel Popper and George Abell. After he was settled in California, we were able to collaborate a few times thereafter. There he spent the rest of an admirable academic and research career.

<u>Howard L. Poss</u> (1925-1999) finished his PhD at MIT in 1948 and then spent decades in high-energy research positions. He eventually became Professor in the Physics Department at Temple with interests in many details of stellar astronomy and was repeatedly a guest observer at the FCO attempting lunar occultation determinations of radii of cool giants and supergiants with his own instrumentation mounted on the reflector. His solar eclipse work shows that he could make modern instrumentation work in the field as well as in the lab.



<u>William M. Protheroe</u> (1925-) came to the University in 1955 from Ohio State University where he had been a student of Geoffrey Keller. A seagoing U.S. Navy engineering officer in World War II, he was very capable with electro-optical-mechanical systems. Bill's personality was more outgoing than those of the rest of the FCO staff members as might be testified by one evening of bagpipe music after dinner at the Protheroe home. He was capable of getting work out of a machinist more quickly than Blitzstein could. The grad students were very impressed by his command of messy analytical details of stellar astrophysics delivered from his notes or even extemporaneously. He had discovered Ambartsumian's text before they had.

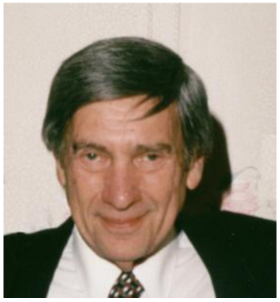
Bill's research career endured a long gap when he moved to a succession of administrative positions in the University office of the Dean of the Graduate School of Arts and Sciences. This was a good match for his energies and talents and he returned to OSU to continue that type of career there. After several years in the position, however, he moved entirely back to the OSU Astronomy Department. #Our most memorable social event in common was not the bagpipe music but the trip to the Grenoble IAU General Assembly. The plane from JFK enroute to Paris made a stop at Keflavik, Iceland where there boarded about 50 French teenagers who had been camping in the countryside of the island for two weeks without bathing. They added to the redolence of the cabin and the composure of many other passengers by smoking through the rest of the flight.#

I. Neill Reid (1957-) took his final degree at Edinburgh in 1957 and eventually, after an appointment at CalTech heading the second Palomar Sky Survey, came to the University. Although on the staff from 1999 into 2005, his position was always that of a research appointment and for 4 years was held concurrently with STScI. With respect to stellar studies, there is almost no specialty which Reid has not explored for both the Milky Way Galaxy and the Large Magellanic Cloud and with essentially every observational technique. It suffices to indicate that more than 40 refereed papers bear his name from 2001 through 2005 and this rate of publication started back in the 1980s. A very fine volume *New Light on Dark Stars: Red Dwarfs, Low-Mass Stars and Brown Dwarfs*, published by Springer with Suzanne L. Hawley, indicates one of the pronounced concentrations of his research. After joining STScI full time, Reid continued work on all stages of stellar evolution with the Hubble Space Telescope and also moved a bit into the field of astrobiology.

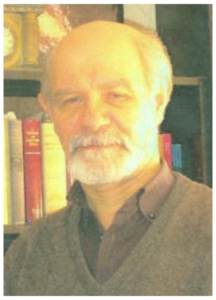
Arthur R. Rivolo (1944-) was briefly a staff member after his training at SUNY-Stony Brook and service at STScI. Before his grad student days, he had been a carrier-based combat pilot during the Vietnam War. Rivolo was a gregarious colleague and a fine teacher with great breadth of research interest and follow-on accomplishments and lots of common sense. A portion of his private life is revealed in the photo. Rivolo used to put on a flight suit and bring a USAF helicopter to a Ronald McDonald House Summer Camp near Philadelphia. The unfortunate children loved the whole show and him. After leaving academic life, he became a rather conspicuous staff member of the Institute for Defense Analysis and continues to be a significant collector of print art. We knew him as Rex but lately he has been using his given first name.

Benjamin S. P. Shen (1931-) graduated from a French *lycée* in Shanghai and took his D.Sc.d'Etat in cosmic-ray physics under Pierre Auger at the University of Paris. Working in the laboratory of Raymond Davis, Jr. at Brookhaven, Ben was among the first to apply particle accelerators to cosmic-ray and high-energy astrophysical research, especially in the study of cosmic-ray-associated nuclear cascades, spallation nuclear reactions, and shielding (e.g., Shen 1963, Shen 1967, Shen & Merker 1976). Later, he turned to the study of the optical variability of Seyfert and other quasar-like galaxies. Ben became the sixth Flower Professor in 1972 and Chairman and Director of the Observatory in 1973. He was also Chairman of the Council of Graduate Deans and served, in 1980-81, as the University's acting Provost. A retired member of the National Science Board, Ben became emeritus professor in 1996 after 30 years on the faculty.

Robert K. Soberman (1930-) spent his career concentrating on solid particulates in the Solar System partly at AFCRL and then at the FI where he was Director of the Franklin Research Center for a decade. During



this time he had also been PI for the Asteroid Meteoroid Experiment on *Pioneers 10* and *11*. Although his active space observing was behind him in 1988 when he became a Lecturer at the University, he spent his remaining service on the concept of a solar aerostat as an energy source and in trying to develop the idea that impacting solids were a significant source of solar and stellar energy. These "cosmoids", as he and Murray Dubin called them, were imagined to be remnants, still existing, of the star-formation process but the observational evidence and calculations failed to convince many people. Bob was the type of scientist who tries to make the broadest application of a concept.



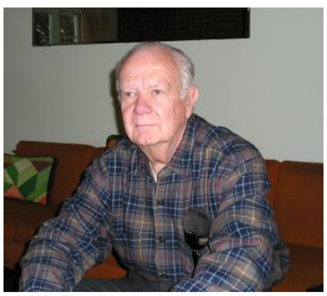
Mitchell F. Struble (1945-) had two simultaneous careers. To put food on the table, he was a long-time staff member of the Space Science Division (and its later re-namings) of General Electric Co. While much of Struble's contract work remains classified, it can be remarked that it concerned timing, with corrections from General Relativity, of communications from many different spacecraft in assorted orbits. Throughout this 21-year interval and even subsequently he has held Lecturer, Adjunct or Visiting Scholar appointments at the University and this has resulted in a long suite of publications concerning structure and evolution of clusters of galaxies. A significant fraction of this productivity has been done with extramural collaborators, principally from Princeton. A recent and interesting departure from this concentration (Struble *et al*. 2006) concerned a patchy disk of dust particles and gas orbiting a single star in the Large Magellanic Cloud, an object that first came to attention during the MACHO Collaboration survey of that companion galaxy.

Peter van de Kamp (1901-1995) had started his astronomical studies in Holland and came to the U.S. in 1923. After working at Virginia and following his PhD degree at Berkeley, he returned to Virginia and McCormick working on that astrometric program for 12 years. When Miller retired at Swarthmore, van de Kamp succeeded him and formalized the observing and measuring program to fulfill the principles that Schlesinger had propounded earlier in the century. It all worked very well with the Sproul refractor and a numerous series of papers explored many stars in the vicinity of the Solar System. Van de Kamp pulled in good staff members such as Lippincott and L. W. Fredrick, took on summer workers such as Novotny and me, and galvanized undergraduates to make a sensible observational contribution. Van de Kamp was an accomplished musician and at the Sussex General Assembly found a cinema which played silent films and extemporized piano accompaniment to the plot. Shen appointed him Senior Lecturer in 1974 and he served five years. During this time, his career suffered a substantial bump because of a hitherto undetected systematic telescopic error. After many years of observational concentration on Barnard's Star, he (1982) had announced the star to be accompanied by two planets - the first extra-solar system to be discovered. Amid more than a little acrimony, it was shown that these results, among others, are spurious and caused by an inadvertent change in the alignment of the two components of the refractor's objective. Of course, such planets are now known in abundance but not yet around Barnard's Star.

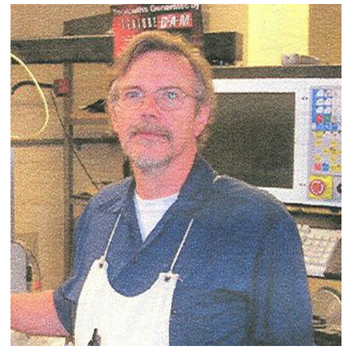
Arne A. Wyller (1927-2001) came first to Swarthmore College as a Research Associate and Instructor and moved from there to a position as Atmospheric Scientist at the Thermal Radiation Laboratory. He was a most skillful, broad and earnest scientist with PhDs from Harvard and Oslo. Around 1970 he became a staff member at Bartol and simultaneously a Visiting Professor again at Swarthmore. After leaving the U.S., he was named successively Director of the Royal Swedish Academy of Science's Solar Observatories on Capri and in the Canaries. At the time of his death he had been a member of the Academy for more than 20 years. His most surprising attainment came in retirement with Wyller (1999). This book develops the idea that there has not been adequate geological time for natural selection to operate in biological evolution so as to arrive at the level and density of information content that exists in the genomes of even simple organisms. There must, therefore, be an external guiding intelligence and the human species inadvertently has cooperated with that intelligence. I feel certain that anyone who knew Wyller in his 30s and 40s would never have expected him to associate himself with this variant of active pantheism. The book, poorly edited though it is, is interesting reading since it represents another expression by a reputable physical scientist of dissatisfaction with a fraction of the evolutionary party line of life scientists. Arne was a very outgoing scientist, ready and willing to talk about his work at any time. It was necessary to have a background and knowledge at levels comparable to his in order to follow his quick line of exposition and argument.

Hong-Sik Yun (1937-) was a postdoc at Bartol after finishing his degree at Indiana University. He worked with Wyller providing model solar atmosphere and sunspot calculations that incorporated the local magnetic field strengths. After this project was finished in 1973, he returned to Korea and was appointed at Seoul National University. Thereafter, a frequent Visiting Scientist in the U.S. and Europe, he eventually served two terms as Chairman at Seoul National and one term as President of the Korean Astronomical Society. He retired in 2003 after an eminent career as a solar astrophysicist.

Some words must be said about the technical staff for there would have been no observational program without them. Only a few instrumental designs were contracted out, but the first of these set the standard for what came afterward. This was a second photometer meticulously designed by Alan F. Petty (to the left), an engineer at the nearby GE facility, with fabrication turned over to the first machinist James K. Thorpe, who was known to Blitzstein from time together at the FI. #Bud (shown adjusting an early near-IR photometer on the 8-inch refractor) was a very good worker as long as he didn't drink his lunch but his alcoholism eventually made him a danger to himself in the shop. It took Blitzstein almost a year to persuade soft-hearted Wood to fire him.# Petty had a much happier career. Some years later, he showed up unannounced with a theoretical study of stellar interiors and its application to binary stars; this had been done without supervision and was easily turned into a dissertation. A few years later still, he moved to the Naval Research Lab where he spent the rest of his career. It was Petty who introduced Shen to Wood and recommended his appointment. Thorpe was replaced by William Barrie (shown with Shen at a tea) who, for family reasons, had relocated to Philadelphia from his job at the OSU Radio Astronomy Observatory. With Blitzstein's support, Bill demanded drawings of the quality established years earlier by Petty and woe did betide the grad student who turned inferior drawings over to him. #Barrie was a dapper, feisty man with a fund of uncommon ethnic slurs that he enjoyed telling in a genial manner: *e.g.*, there's really only one difference between the Scots and the Irish!# (I leave the reader to imagine a suitably denigrating punch line to this one.) With it all, he turned out polished work, albeit a bit slowly, until he retired. George Reahm was the last machinist and he was the epitome of the profession. He worked to tolerances that had not been demanded before, he was quick and kept a fastidious shop, he could draw, and he could improve the inventions and conceptions of the staff and students graciously. #In a private shop, he had been the machinist who actually perfected the Slinky™ toy but he made no money from it.# George had a lengthy combat record in World War II and then was a POW in Germany. Some of his hardships are summarized in Roughan (2000). #He was far from an optimistic man but believed in the reality of UFO phenomena and felt rather comfortable with the idea that they were U.S. government stunts or investigations. He and his wife were childless and, when his dog died, he lost heart for the long daily commute and retired.# George's retirement did not leave the

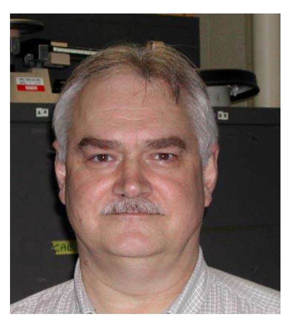


observatory bereft of technical capability for Sam Seeleman (at left) had been around for more than 5 years. During World War II Sam had been sent on detached duty for a short time to the Royal Navy to see how



their air surveillance radar was used and after the war became a CIA employee in Europe. When he retired from government service, he came to the Engineering School of the University as a contracts administrator. It may be Sam who coined the now-familiar joke: "Once I thought I had made a mistake but I was wrong." In any case, he was the first person whom I ever heard say it. At the FCO he was a volunteer machinist, optician and general jack-of-all-trades who would also run the telescopes for public nights. He was an invaluable, hearty man who tiredly sat down one Sunday and never got up from the chair. At his death, Lande and I were trying to get a balloon flight off the ground against a very near deadline. We needed machine shop help instantly and Bob Hee (center) and Buddy Borders (at right) of the Physics Shop volunteered to take over. It was a generous offer efficiently fulfilled. Buddy was also responsible for the Students' Shop and he attempted to teach proper machining technique to hundreds of grad students and other people over many years. After retirement, I was one of those people.

Although there were a few part-time software assistants before 1960, the first real electronics technician was Robert E. Smith, who implemented the card-punching capability for data taking during the early 1960s under Blitzsein's supervision. At that time, there was only mainframe computing hardware available anywhere in the University so decks of cards were typically reduced the following day on the campus systems. Almost no manual intervention had to be exerted after the observing interval other than to avoid dropping the cards. If that happened, they could be sorted by punched time and the hapless individual could pretend that he had not been careless. Eventually Bob moved to Physics, supervising undergraduate student labs. In the early 1980s a pair of Ohio Scientific mini-computers were contributed to the FCO by Edward J. Devinney and they were immediately implemented by Richard J. Mitchell who had succeeded Smith as the electronics technician some years earlier. The real value of these devices, which were not so reliable as they might have been, was that they opened eyes to the possibility of complete online computer control of the telescopes and of data taking and reduction with commercial PCs. The realization of all these advances, entirely with UNIX capability after a brief flirtation with APL, was accomplished by Mitchell who oversaw all further instrumental and software improvement. He also took over the maintenance of both observatories as well as actually doing some observing when I was out of town. Rich's talents were many and he was a dogged worker who never left a task unfinished. In 1997 he became chief of the Physics Demo Lab apparatus and operations, including classroom computer functions. From this position, he moved to Gravic, Inc. in a position much more responsible and rewarding than was ever possible at the University and the observatories.



TELESCOPIC INSTRUMENTATION

Grant and University money existed for support of outside fabrication but almost all of the telescopic instruments were built in-house under Blitzstein's supervision. He was most comfortable with essentially hourly supervision of work. Two very well equipped shops were staffed by the machinist and the electronics technician, who later doubled at developing software/hardware interfaces. The facilities of the shop of the Physics Department were also available if very large machines were needed.

Over the years, several small visible-band and near-IR photometers were built for specific purposes and a few instruments, such as the double-slide photographic tailpiece, were rehabilitated from their uses in the FO and RHO. Only one device was purchased and that was a fine blue/red-grating spectrograph built in the Yerkes shop. On the reflector this device was uncomfortably long in an axial sense and was disassembled and reconfigured so that the telescopic beam was bent 90º before the position of the slit.

Binnendijk's cumbersome brass photometer that he brought with him from Carleton College was too long to pass through the fork of the reflector. Accordingly, Petty designed and Blitzstein and Protheroe had built an Elbow Photometer, as it was called, which turned the Cassegrain beam 90º before the photometer's wide-field eyepiece. The cell was not refrigerated because Blitzstein was convinced that thermal noise would never be the limiting condition at the FCO. This photometer was handy to use and very stable and lasted more than a decade. It was never pushed to its shot-noise- limited performance.

Simultaneously, Blitzstein was perfecting his two-channel, pulse-counting photometric system also built around un-refrigerated 1P21 photocells. This machine was much more challenging to use than the Elbow Photometer because two stars had to be kept centered in two off-axis diaphragms during the counting interval while the focal plane rotated slowly about the optical axis and the equatorial-altazimuth drive coupling stuttered along. The original photometry circuitry was not simple, with discriminator levels and gains having to be chosen appropriately for each source. In an electronic sense, its early versions were also not so stable as the Elbow Photometer.

Fig. 46. The two-channel Pierce-Blitzstein photometric system around 1985. The old vacuum tube hardware had occupied about 40 ft^3 of the room but it was gone by then. A cardpunch had been off the right edge of the picture but it too was gone. By 1980 there were primitive OSI computers on-line and the system became progressively more compact as more processes were moved from hardware to software and as minification progressed. By 1990 the total volume of the system, other than the photometer heads and the online computer, was about half of a small rack.

Progressively more automation and minification, better photocells and solid state components were continually introduced until in the 1990s the system ran very reliably with minimal human intervention. The photometer eventually rotated under software control so as to keep pace with the rotation of the field of view. Even so, some observers were defeated by the device, which was christened the Pierce-Blitzstein Photometer and was called the PBPHOT. A late version of the system is shown in Fig. 46.

In theory, the limits of the PBPHOT for simultaneous observing of two bright sources should be due to scintillation noise uncorrelated between the two channels. This is certainly not the case for faint sources for which shot noise was the dominant component. Blitzstein put an immense effort into comprehending the theory of the entire system and the channel-calibration method. A description of his understanding of it is presented in Blitzstein (1988). In essence, in the 1950s and 1960s he had invented and perfected a poor-man's 2-source radiometer and his calibration method is the same as a portion of that used for modern electronic cameras.

Blitzstein's concern about the sources of observational noise was very likely the reason that he supported Protheroe's joining the FCO staff. Protheroe's dissertation had been concerned with measuring the atmosphere-imposed scintillation noise pattern and power at ground level. Very quickly after his arrival, he was able to start measures in the SO with the 4-inch Ross/Fecker camera feeding the rotatable photometer that appears in Fig. 45. He elaborated this device to the double telescope shown in Fig. 47 on the roof outside the SO and built still a third such instrument for use on the reflector at the FCO. Protheroe had Air

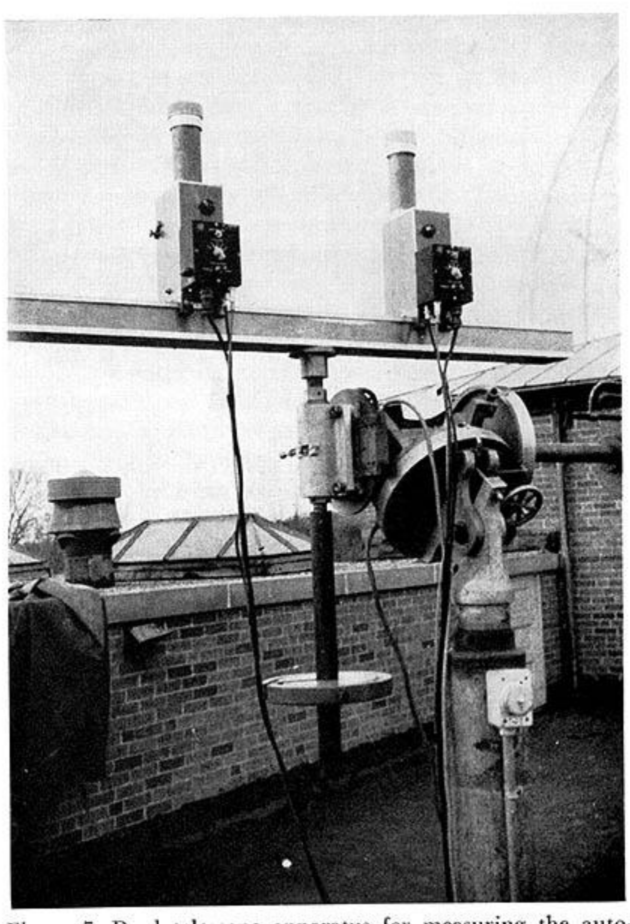


Figure 7. Dual telescope apparatus for measuring the auto-correlation function of stellar shadow-band patterns.

Fig. 47. Protheroe's double scintillation photometer was movable on its I-beam in order to attempt to map more fully the schlieren pattern due to the atmospheric inhomogeneities and winds.

Air Force contracts to fund all this because the DOD saw the work as a way to learn something about upper atmosphere wind velocities and their turbulence spectrum. In principle, there were both solar and stellar applications of the work as well but these were never pursued.

In the very late 1960s G. W. Wolf and Blitzstein decided that there was a possibility of detecting each of the three components of the Stokes radiation column vector due to interstellar and stellar polarization with a 2-channel device. Accordingly, they designed and built a first-generation polarimeter that was not too convenient for a tiring, uncomfortable observer. Despite this limitation, Wolf (1972) used it for a general survey at KPNO and obtained some marginal detections of the circular *V*-component. Thereafter, it was redesigned and rebuilt numerous times with a number of changes of analyzers and detectors and upgrades of the software to which it was slaved. Never quite fully automated, it was nonetheless a fine instrument and gave reduced results within 20 seconds of completing the measures. The appearance of the polarimeter in the mid-1990s is shown in Fig. 48.

Fig. 48. The device known as the PEMP mounted on the reflector. This was its appearance after moving to a Kemp-type photoelastic modulator that would yield simultaneously the *Q*, *U* and *V* components of the net polarization vector. All electronics are in the floor rack and the monitor would show the data string in real time. The archiving computer is in the adjoining warmed room. The yellow object in the background is a forklift used to remove and re-install the paraboloid and its cell. Obviously, the walls need pointing and painting at this time.

Blitzstein was the driver behind an early effort to detect low-amplitude, coherent oscillations from a variety of astronomical sources as is described briefly in Blitzstein, Wood & Svihel (1951). The basis for the initial work was the very short time constant of the 1P21 multiplier photocell which permitted searches up to about 100 MHz, *i.e.*, the limiting frequency imposed by the transit time between dynodes. This effort, apparently done with his single-channel pulse counter at RHO, led nowhere but more than 30 years later he looked again at the same problem in a more bounded way. By this time he had concluded that he might be able to detect high-frequency oscillations from solar photospheric plasma. Around 1980 he began to stop down the siderostat objective to 6 inches in order to diminish local heating and to feed a small fraction of the beam directly into a photocell; the pulses were counted at assortments of brief and long intervals. He also set in train a brief reconnaissance of the problem with the McMath solar telescope by Holenstein.

Around 1967 Wyller started a program to learn more about the sunspot environment and the flux ratios between spots and the photosphere. He designed and had built on contract a device that he called URSIES (for Ultravariable Resolution Single Interferometer Echelle Scanner). This is first described in Wyller (1969, 1970) and was a very inventive and complex device containing a tunable Fabry-Perot spectrometer in a variably-pressurized Freon environment. It was not small (an aluminum cylinder about 2½ feet in diameter by 4 feet in length) but was mounted on large wheels so it could be moved rather easily and positioned reliably to receive the incoming beam. The intention was to have the instrument isolate a small (2"-4" diameter) area of the photosphere or of a sunspot and measure flux at a specific continuum wavelength or throughout the profile of a temperature-sensitive absorption line. It would then be possible to face observed flux ratios and line profiles against solar models. The solar flux was collected by the siderostat and imaged into the spectrometer on many days. Because the URSIES was not fixed, it was moved away at night so that stellar photometry with the PBPHOT could continue as usual and then would be wheeled back into position on the next clear day. It was a very efficient use of the telescope but by 1973 Wyller had a dedicated and larger reflecting solar telescope at Bartol and moved the observational effort there.

No optical surface ever stayed clean enough for Blitzstein. He quickly instituted a routine of semi-annual optical washing assisted first by Smith and then by Mitchell. The flat and objective of the siderostat could be cleaned in their cells without dismounting anything and the same easy effort would also work for the hyperboloid of the reflector. The paraboloid was another matter because 3-foot jack screws had to be threaded and unthreaded in order to isolate the cell and mirror from the rest of the telescope. Then the mirror was removed manually from its cell for cleaning. This typically took at least three people and usually four were recruited. In 1976 I dropped my arc of the edge of the mirror. That edge struck the cell and a significant concoidal fracture happened. Characteristically, Blitzstein insisted that it was his fault for not inventing a safer procedure and he did just that promptly.

For the 40 years of the FCO's productivity, weekly mechanical and optical maintenance and electrical calibration were sustained by Blitzstein, Smith, Mitchell and me sometimes helped by the momentary machinist. Typically, this paid off in very few nights lost to instrumental failure after the vacuum tubes for the PBPHOT were replaced by solid-state components. The single most enduring concern was the imaging quality of both telescopes. With the PBPHOT mounted on the siderostat, the images of both program and comparison stars were off-axis and astigmatism was insuperable. This, plus the vagaries of the telescope drive, meant using focal –plane diaphragms larger than one really wanted. The uncontrolled mounting of the reflector's paraboloid led to constantly changing image character even on-axis and the telescope had an alignment error which caused a conspicuous drift in declination. Finally, Blitzstein designed a simple radial restraint for the paraboloid that alleviated some of these problems.

THE STUDENT TRAINING MISSION

Until about 1967 inexperienced grad students were taken to the FCO by other students who were already working on some program and shown and told the current practice. Input from Wood, Blitzstein and Binnendijk was after the fact and usually took the form of telling how to correct or avoid mistakes that had already happened. These three men were so over-extended that they did not have the night hours to commit to observational training and, in any case, Wood and Blitzstein were no longer observing. My appointment meant that another experienced observer was on hand and I decided to change this situation. As Director, Blitzstein was very willing that I start the change and in about 2 years observing routine had been stabilized and efficiency and productivity greatly improved.

My aim was to make sure that everything possible had been squeezed from the FCO capabilities and then to move the students to complete their personal programs with the more powerful capabilities of KPNO or CTIO or the different skies at MJUO. I remembered well my own first run at KPNO when, although very experienced with Steward and other facilities, I was still grateful for the first afternoon's advice from the staff observers. I was also concerned that students not choose photoelectric programs by default but for a credible reason. This attitude led to the purchase of the spectrograph and shortly thereafter to a 1-year course in observational techniques with the hardware of both the FCO and the SO: the basics of visual observing with the Cook broken transit emphasizing instrumental errors and timing precision; photoelectric and polarimetric practice with the Elbow and Pierce-Blitzstein photometers and with the current version of the 2-, 3- or 4-channel polarimeter; spectral line identification in the blue and red and MK classification; and photographic imaging with fast and slow beams and darkroom experience. Very rarely, there would be a visit to NASA at GSFC or to a radio observatory. A few simple CCD arrays were used for demonstration when the first ones came on the market. There were individual and team observing projects and possibly the best one determined the 1986 moonless zenith sky brightness at the FCO to be the equivalent of 1 V = +18.1 A0 V star/square arcsec. All this sounds dated, rudimentary and paternalistic now but I believe that it served a purpose into the early 1990s.

From time to time, there would be some noteworthy celestial event for which the SO was opened to anyone who wished to see the phenomenon and to hear its scientific importance described. A photo taken on such a day is shown in Fig. 49.

Fig. 49. The (probably Lundin/certainly Clark/possibly Fecker) composite 8-inch refractor in 1973 when a group of people was viewing a projected transit of Mercury in the SO on campus. For another purpose, the finder had come from Villanova University and never went back. Among the onlookers are Bruce Hrivnak now at Valparaiso University (second from left), C. Sean Sutton currently of the Mt. Holyoke Physics Department (the hairiest one), and Tony Hull retired from JPL (behind the fedora). Ernie Robson in the hat and polished shoes was obviously not a student; dead now, he was a conspicuous amateur astronomer in southeastern PA. It may be noticed that the architect mistakenly believed that the Philadelphia streets were laid out exactly east-west/north-south and so poured the concrete pier to be parallel to the street orientation. At the right edge of the background the broken-transit is shrouded.

Undergraduate training was not neglected although there were few students. Peter Shelus has been established a long time and Peter Eisenhardt and Douglas Leonard have made very promising beginnings to their careers. C. R. Shanus was an excellent prospective scientist but unfortunately decided to become a lawyer.

THE FCO OBSERVATIONAL RECORD

Stellar Radiometry

This program began with the establishment of the FCO. One way to demonstrate the productivity of the Elbow and Pierce-Blitzstein photometers is to compile an HRD for the eclipsing binaries whose light curves were observed with the instruments and then analyzed or synthesized. Values of T_{eff} and luminosities have been taken from Lang (1992) and Allen (1994) as functions of the spectral type and luminosity class of the hot star and the light and the surface brightness ratios from the published results. For a small number of cases, the light curves were neither analyzed nor synthesized when they were published and I have made approximate interpretations based on analogy and experience. The net accomplishment is shown in Fig. 50. The workers who contributed to the data ensemble are: C. W. Ambruster, R. Arquilla, R. W. Avery, J. A. Bangert, Binnendijk, Blitzstein, B. B. Bookmyer, D. H. Bradstreet, C. Brown, C. R. Chambliss, K.-Y. Chen, K. C. Chou, M. F. Corcoran, M. S. Dekayne, E. J. Devinney, J. D. Dorren, Eisenhardt, N. M. Elias, C. Fang, H. F. Fliegel, L. W. Fredrick, Galatola, B. J. Geldzahler, J. K. Gleim, E. F. Guinan, A. J. Harris, D. H. Hough, B. J. Hrivnak, A. B. Hull, M. W. Johnson, I. Jurkevich, G. C. Kilambi, D. J. Kjer, Koch, Y. Kondo, C. A. Koegler, Leonard, K.-C. Leung, G. E. McCluskey, T. A. Nagy, R. J. Nemiroff, Nha, P. M. Perry, R. J. Pfeiffer, G. F. Reed, E. G. Reuning, P. V. Rigterink, R. E. Ruland, Shanus, J. S. Shaw, J. M. Siah, S. Sobieski, G. G. Spear, C. S. Sutton J. F. Wanner, R. E. Wilson, T. Wickramasinghe, L. Winkler and Wolf.

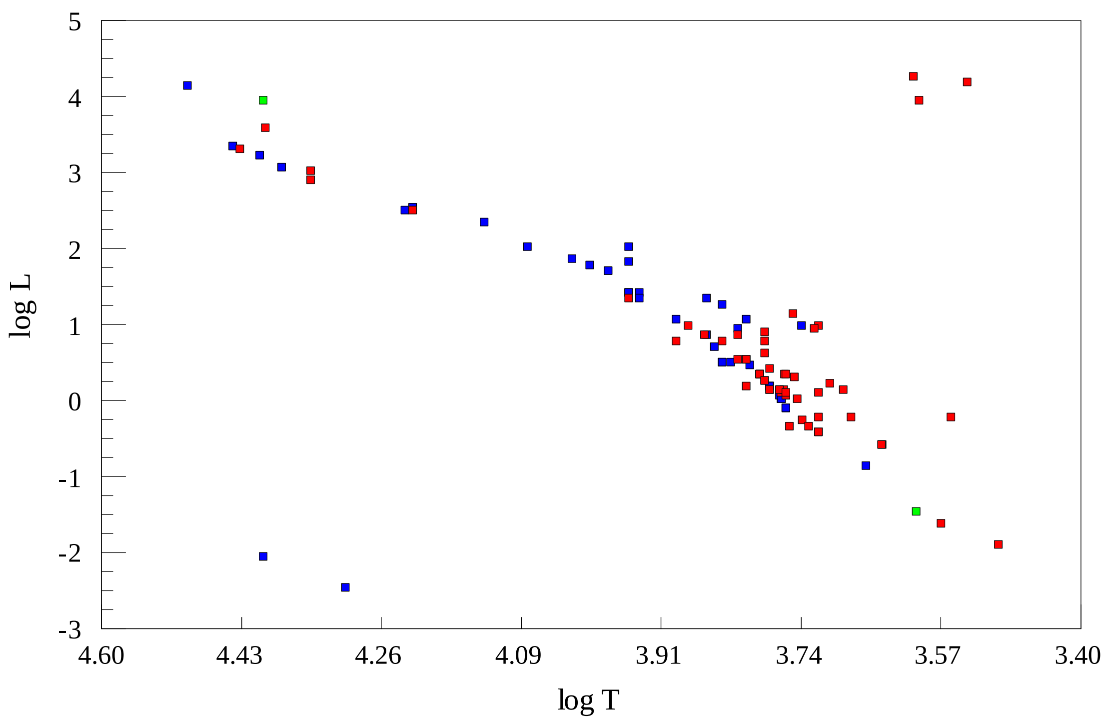


Fig. 50. A theoretical HRD for close binaries observed photometrically at the FCO. This is only an approximation to reality since distances for most of the objects are not well known and many of them are single-line binaries. The main concentrated of points is neither a ZAMS nor a TAMS since the stars are in a variety of evolutionary stages. The blue and red squares represent the hotter and cooler binary members, respectively, and the green symbols the infrequent third companions to a few binaries.

The illustration shows clear evidence of observational selection and also an obvious emphasis on cool contact binaries (Binnendijk's specialty) and short-period Algol-type systems. All but one of the cool supergiant systems are the result of the early enthusiasms of the 1950s and 1960s and just represent observational effort and not fitted light curves. By and large, high precision and adequate phase coverage attest to the standard demanded of light curve work at that time. The scatter among cooler stars in the figure is largely due to the W-type configuration for contact binaries and to the inadequacies of the Russell Model in dealing with these binaries and the short-period Algols. By a ratio of about 2:1, the majority of the light curves were observed with the reflector. My interpretation of this work is favorable. Almost all of the studies that underpin Fig. 50 were published after using the most modern models of the day and they made a significant contribution to close binary radiometry. If one adds the additional results from other stations and the invited reviews that were built on the binary work, it all forms a considerable body of very good science for its time.

Another consequence of the reliable photometric instrumentation and dedication of time is the inevitable accumulation of timings of minimum light and studies of Keplerian period variability or stability. While this may seem a low-level accomplishment, it remains the most reliable photometric entry into stellar and orbital mechanical and magnetic dynamics and also makes a contribution to the understanding of hierarchical star formation. A particular example of this accomplishment is furnished by XY Leo, for which the (O-C)-diagram is shown in Fig. 51. This contact binary of approximately 7-hours eclipsing

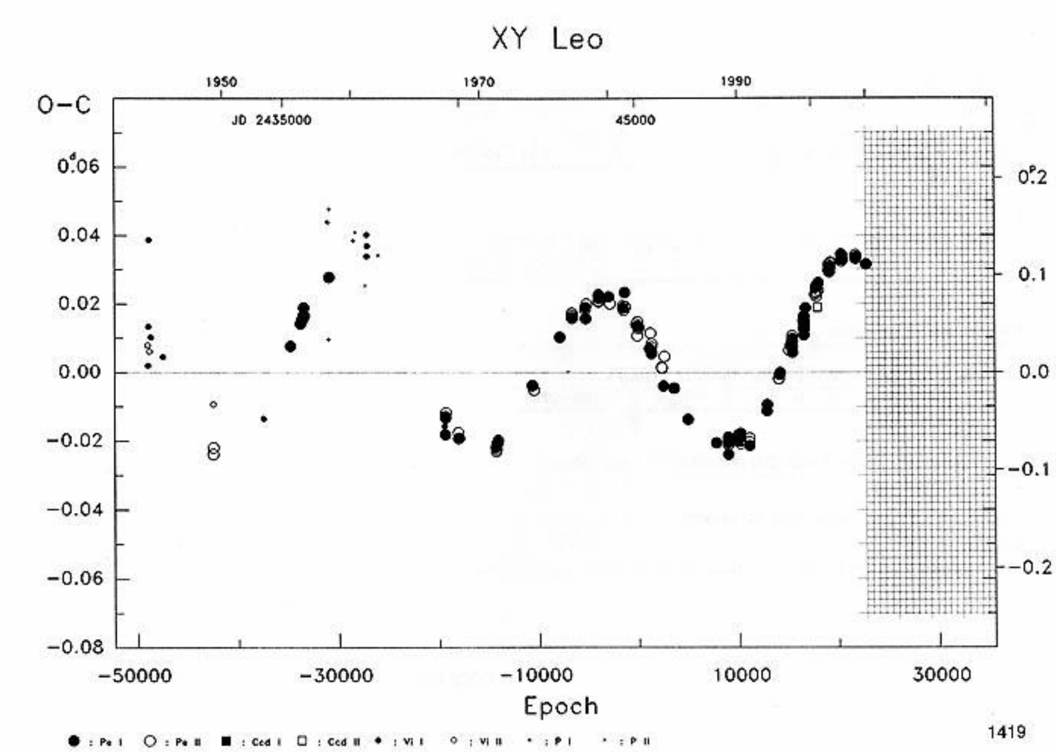


Fig. 51. The (O-C)-diagram of the cool contact binary XY Leo taken from Kreiner, Kim & Nha (2001). In addition to the evidence for a hierarchical system, the diagram is significant for showing only a modest secular period change, presumably from mass transfer or loss.

period is accompanied by an unresolved, wider, cooler pair with a period of about 0.8 days and the additional nearly 20-year cycle is thought to represent the motion of the two close pairs about their common barycenter. Kim (private communication) also finds a secular period change, indicating possible mass transfer from the less massive eclipsing star to its companion. It is particularly satisfying that Blitzstein himself began the photoelectric study of this star, that Shanus and I added to the data partway through its history and that Galatola continued to observe it after 1996.

It cannot be known directly from Fig. 50 but Blitzstein continued to push observers to accumulate near-IR and narrow-band light curves of interesting stars and had some success. Chen & Reuning (1966) made the first IR light curve of Algol and for the same target Cristaldi, *et al*. (1966) developed the first $H\alpha$ light curve (65Å half-width) that they supplemented with similar, later data from Catania. There were either or both of two aims of such programs: (1) to quantify more confidently the contribution to systemic light from unresolved cool companions to an eclipsing pair and (2) to establish more accurately the contribution to systemic light from the cool component of Algol-like binaries. Both purposes were successful for a few cases.

A small amount of photographic radiometry was completed by Kilambi and Perry. In support of extensive *V*-monitoring of NGC 2264 at the KPNO, Kilambi (*cf.*, Koch & Perry 1974) took one *R* and numerous *V* plates at the FCO. These went as faint as $R = +14.2$ and it took real dedication to accumulate them because of the poor tracking of the reflector. The other and final effort of this kind was Perry's (unpublished) dissertation, a *BVR* photographic map of NGC 1976 in the Orion Nebula. His was an effort of the most meticulous care in order to overcome the variable astigmatism at the Cassegrain focus of the reflector and he also designed and built a very satisfactory spot sensitometer. The plates were scanned with a densitometer at the U.S. Department of Agriculture to a smoothed spatial resolution of 1" and via an FFT reduction showed emissivity contour levels very nicely. Using assumed gas/dust densities, Perry calculated limiting masses for some of the condensations in the nebula. This work really showed that local guided photographic imaging was just too troublesome for the level of the scientific results that resulted from the effort.

It is not irrelevant that this era of concentration on close binary observations also resulted in some very large editorial tasks by the observatory staff. They include the last three editions of *A Finding List for Observers of Eclipsing Binaries* initiated by R. S. Dugan: Wood (1953), Koch, Sobieski & Wood (1963) and Wood, Oliver, Florkowski & Koch (1980). The basis of the *Finding Lists* was a handwritten Card Catalog on 5x8 cards of the discovery and literature references supported by cryptic interpretations for every known eclipsing binary. Originated also by Dugan, then continued by Pierce, the Card Catalog came to the University at Pierce's death with the understanding that Wood would continue it in its historical form. This he did but also enlisted a sequence of grad students as assistants. Some of these people were less than able. Over the years, the compilers of the Card Catalog answered innumerable requests for information about specific binary systems and the entire work demanded about 20% of the research careers of the three men. When Wood left for Florida, he arranged that a copy of the entire Catalog remain behind. For a time, Binnendijk and I kept it up to date but eventually this seemed useless since Wood was continuing the original at its new home.

In the days before machine-readable data files and catalogues, the *Finding Lists* were very useful guides to work that had not yet been attempted or had actually been done on a given object or an entire class of systems and the *Lists* were marred by remarkably few errors. They were distributed broadly and their contents were very frequently cited as the justifications for new observational programs. The initiation of the *Bibliography for Eclipsing Binaries* in 1957 and the succeeding *Bibliography and Program Notes on Eclipsing Binaries* in 1963 did not materially diminish the impact of the *Lists*. By 1974, however, the *Bibliography and Program Notes on Close Binaries* were becoming so careful and complete under maturing editorial care that only one further edition of *A Finding List* was justified.

The publication by Koch, Plavec & Wood (1970) was viewed by its editors to have two complementary purposes. Firstly, it would guide an observer interested in typically bright close binaries to the ones that had poor photometric data or an inferior analysis (or both) for reasons that were detailed in the volume and a new observer could decide if a new effort were going to be useful. Secondly, the book would be a Catalogue parallel to those published from the DAO, Victoria wherein spectroscopic orbits were weighed and interpreted. Although these were worthy aims, the volume did not achieve its expected impact. In major part, this was due to the old modalities of analysis being supplanted just at that time by physically more realistic models, most significantly by the gravitational model built into the Wilson-Devinney (1971) code.

In the early days of the FCO Binnendijk (1960) published a text with the definitely misleading title, *Properties of Double Stars - A Survey of Parallaxes and Orbits*. Its organization and emphasis were the consequence of his Leiden training and he had not yet made the realm of cool contact binaries his own. One could do only so much with his Dutch English to make this more agreeable for North Americans. At the time, there was no comparable text in English providing an observational background for binary stars but the volume could have had a more emphatic emphasis on photoelectric detection and also have de-emphasized the outmoded European methods of analyzing data sets. More than two decades afterwards, Binnendijk (1984) was trying to foster interest in syntheses of light curves by using a cylindrical coordinate system but this could not compete with the bi-spherical-coordinate codes that had already been in use more than 12 years.

Stellar Polarimetry

After Wolf completed his survey, the Mark I polarimeter was not used for a few years. In mid-1971, Pfeiffer decided to start a new program and the instrument was upgraded and the data handling streamlined. At that time, the scheduling of the reflector changed because the polarimeter could be used as a photometer, which meant that the less efficient Elbow Photometer could be retired. Binnendijk didn't wish to use the newer instrument and transferred much of his program to KPNO. In addition, there was at that time a call to resurrect the Cassegrain photographic capability of the reflector so the dark of the moon went to that program and the bright half-month to polarimetry. After the photographic programs ended, the reflector was scheduled 100% of the time for polarimetry for many years.

Wolf's survey had not shown promise of finding the circular polarization component for a wide variety of objects. Had he observed a few polars, the conclusion would have been different. Approximately 33% of the nights after 1972 were invested in finding and observing polarization standard stars, a body of information that was not too well documented and was very scattered at that time. More than a little judgment was exercised in weighing older standard values against newer ones. Eventually, a degree of confidence was achieved in the precision of repeated data as is illustrated in Fig. 52. Contributors to both

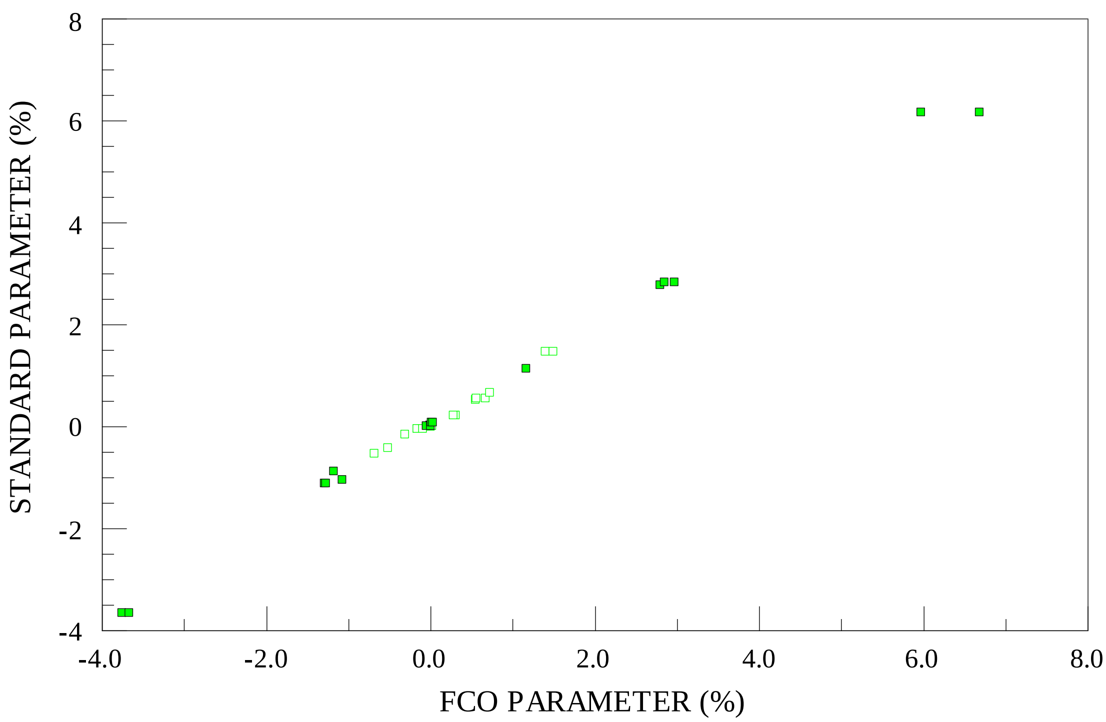


Fig. 52. The calibration of the FCO values of the green *Q* and *U* polarization parameters over the interval, July 1983 through May 1984. Filled symbols refer to *Q* and open ones to *U*. Linear regressions lead to: Q(std) = +0.0301 + 0.9668Q(FCO), U(std) = +0.0176 + 0.9293U(FCO).

the standard stars and program stars were Corcoran, Elias, Holenstein, Hull, Koch, Koegler, I. Pachoulakis and Pfeiffer.

Originally, it was decided to concentrate on close binaries because the FCO already had such a depth of knowledge about them. In the manner of Fig. 50 the productivity of the polarimetry is indicated in Fig. 53.

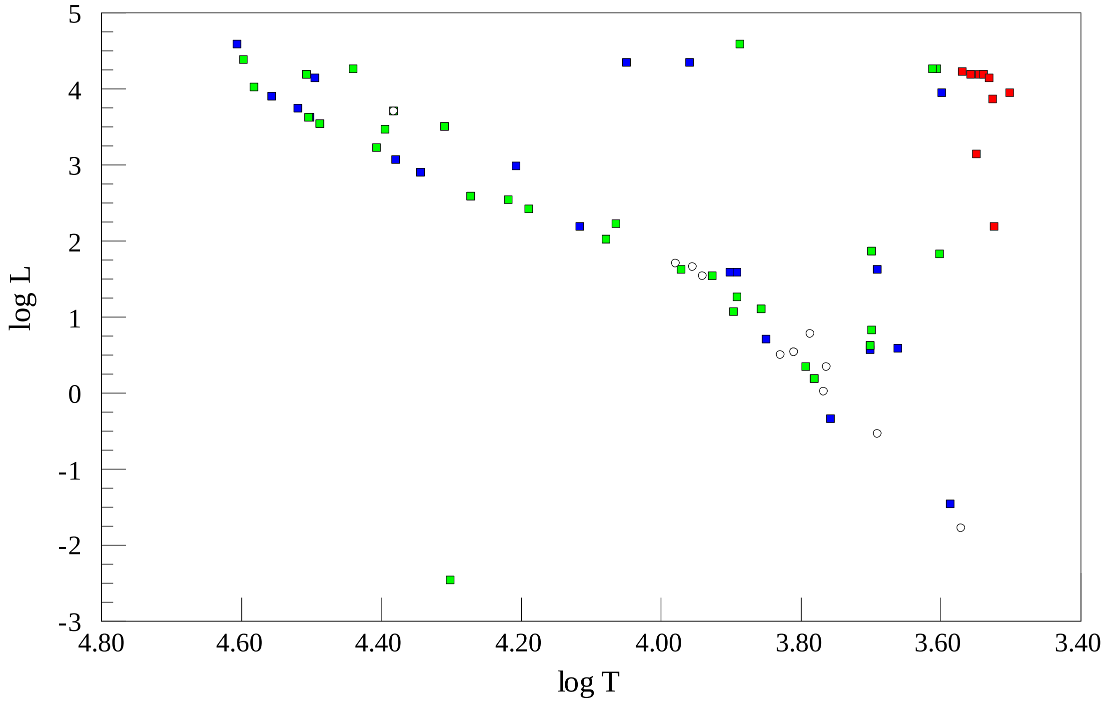


Fig. 53. The theoretical HRD for the bright components of polarization targets (mostly close binaries) at the FCO: intrinsically polarized stars as filled blue squares, detected polarization objects as filled green squares, a polarized intrinsic variable as an open square, and zero polarization targets as black open circles. The red supergiants, which are not close binaries, appear as filled (indicating intrinsically polarized stars) red squares. As before, the main locus is neither a ZAMS nor a TAMS.

Our initial criterion required that intrinsic stellar polarization (which may really be circum-binary and not photospheric in origin) had been detected only if the net signal were phase-locked to the Keplerian period or varied monotonically with time. This was eventually seen as a criterion too stringent and was relaxed for it ignored the possibility that fast transients could occur in the stellar winds or gas streams and it also gave no weight to polarization spectra. The contents of Fig. 53 require detailed comment. “Detected” polarization implies 3σ validation of a non-zero result but the signal may either be stellar or interstellar or a combination of the two sources. Data of this description usually lack polarization spectra or sufficient phase coverage to establish the source or sources decisively. “Zero” polarization also implies 3σ certainty but there could still exist the nasty conspiracy of nature from an interstellar signal being exactly nulled by a stellar one. The “zero” polarization sets cannot deny this possibility. It is also necessary to avoid over-interpretation of Fig. 53. Whereas the intrinsic and zero polarizations are unambiguous attributions and will withstand the test of future observing, the detected polarizations could, with more data and better time and phase coverage, identify sources that are intrinsically polarized. Conservatively, detached MS binaries fainter than about M_V = -1 appear unlikely sources of photospheric or supra-photospheric scattering while many of the detections brighter than that limit will likely, with more data, transform into intrinsic polarizations.

In the early-1980s when the PEMP (Photoelastic Modulating Polarimeter) device was developed from the three earlier versions of the instrument with a piezoelectric device working at 50 KHz and 100 KHz, another student joined the team and concentrated on red giants and supergiants. His decision is easily defended since one of the earliest detections of intrinsic stellar polarization was for *μ* Cep, a star in just such an evolutionary stage. The variability results (Holenstein 1991) for these stars, half of which are binaries of one sort or another, are incorporated into Fig. 53. A model was developed to explain the polarization patterns of these stars based on bright and dark photospheric spots and a variable circumstellar dust cloud. The most impressive data set is for Betelgeuse, which is the brightest object in the sample, and Holenstein's data are incorporated in Fig. 54, which shows all known measures for the star.

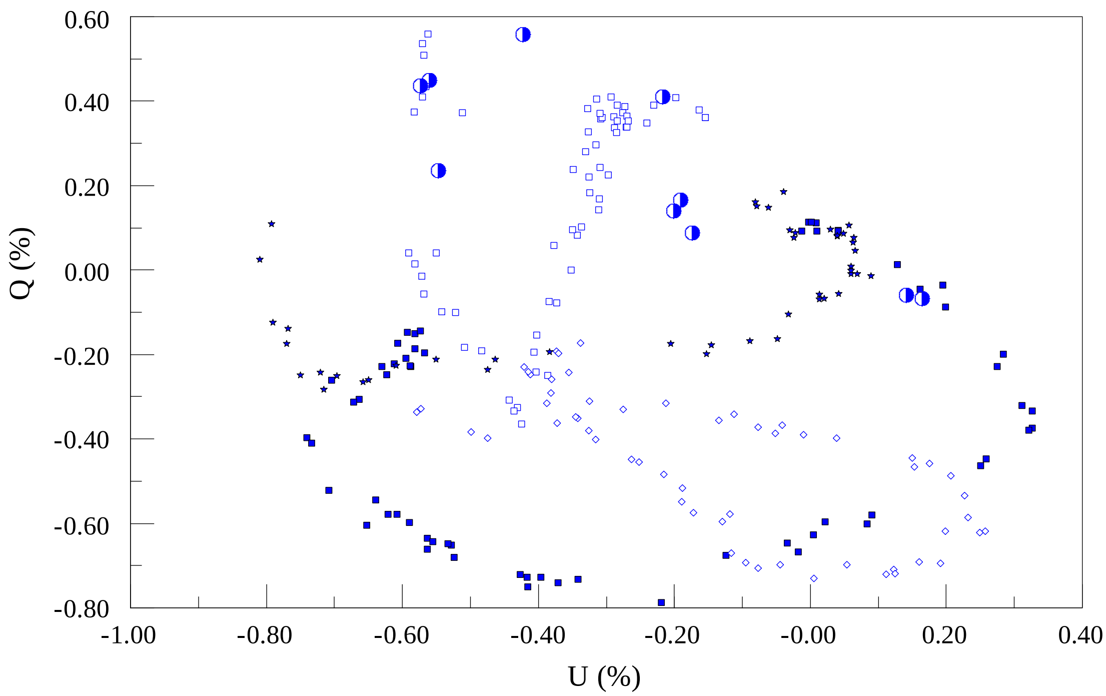


Fig. 54. The history of all of the linear blue polarization of α Ori. Different symbols code for different seasons: open squares, 1979-1980; filled squares, 1980-1981; open diamonds, 1981-1982; and stars, 1982-1983 - all from Hayes (1984). The half-filled symbols are from Holenstein (1991) and show that the modulation had sped up recently so that in 1986-1987, for only the second time, a polarization "petal" could be observed completely in one season.

Two other polarimetric programs were not successful. The first of these, begun in the early 1970s, was a long survey of Am-type stars. Although many of the objects are bright and S/N quite good, no unequivocal detection of stellar polarization emerged, as is implied in Koegler (1976). The second of the additional programs started in 1988 and concentrated on 12 β Cep-type objects. Positive detections resulted for all of them but, with one possible exception, the signals appear to be of interstellar origin.

With the appearance of Koch (2005, 2006, 2007) and Elias *et al*. (2008), almost all of the polarimetric work has been analyzed but some data sets remain only in my files. It is true that the null detections of what were intended to be program objects are of little general interest and therefore not worth publication other than as potential null standards. Some other detections have been found to be entirely interstellar and they may also be useful as non-null standards for other workers at later times. For some binaries with clearly intrinsic signals (*e.g*., XY Leo), there was never enough telescope time to obtain dense phase coverage and so these have just been filed away. There are, in fact, many score of measures for over-contact binaries which have never been worked up, and the Am-star observations remain in Koegler's hands although they probably could be recommended as non-null standards. Even some impressive results, such as for δ Ori where there is a long-term secular change in the parameters that may be traceable to the relative proper motion between the binary and the intervening dust distribution, have not been published for lack of time and manpower. In general, it is a reasonable conclusion at this time that better concentration on fewer targets would have paid off more conclusively. In addition, two workers somewhat diluted their efforts by teaming with a U.K. group for an independent program on massive close binaries using high-resolution *IUE* images. The original motivation for this collaboration was better determination of the stellar masses but it almost instantly became clear that the same spectroscopic images could lead to models of the stellar winds and that these might be correlated with the ground-based polarization results. From the point of view of the mass determinations, this was an effective collaboration but the wind studies progressed much too slowly and the polarization work was diminished as a result. My conclusion: the polarimetry was a program that should have had more impact than it did; it needed more focused direction because observationally it was so successful that analysis always lagged severely.

In the mid-1980s I began to keep a machine-readable, coded file of secondary literature source material to polarization of close binaries. Only an early version (Koch 1990) was ever published. This file has been kept current and by now lists more than 1,100 targets with secondary citations to the literature back to Öhman (1934). It is available to anyone requesting a copy.

Spectroscopy
In the mid-1960s R. E. Wilson redesigned the back end of the Cook spectrograph and Thorpe fabricated that design replacing the plateholder with a photocell housing. I can find no record that any telescopic spectrophotometry was ever done.

Atmospheric Extinction
Blitzstein fixated on atmospheric attenuation and scattering for decades. The underlying reason for this attraction was the necessity to document his conviction that credible radiometry could be done at less than perfect sites. All of the photometry and polarimetry were underpinned by his applications of atmospheric models from the AFCRL and spectral responses for a wide range of celestial sources that he was continuously culling from the literature. There seemed to be almost nothing which he didn't know about atmospheric physics and its effects on astronomical measures but he published very little of his accumulated knowledge. One piece of work does appears as Blitzstein, *et al.* (1970). Eventually, he immersed himself in isolating contributions from individual atmospheric pollutants and with S. R. Negley developed a narrow-band photometric test system that could quantify the H_2O and O_3 contents at the FCO and SO as a function of time. This too was never published.

Scintillation Results
Protheroe showed that his measures could really determine upper tropospheric wind velocities but his move out of science and into administration effectively stopped the local development of this specialty. His last local results appear in Protheroe (1961, 1964). Other teams at many other places were working on this subject at the time and they continued to develop it over the following three decades.

Lunar Occultations
Poss's (1971) paper is the only instance of using the reflector for determining stellar angular dimensions. I do not know what caused Blitzstein to initiate a somewhat similar program much later. Possibly it was his continuing interest in geophysical matters or possibly he was just casting around for another application of the PBPHOT and the siderostat. At the beginning of every lunation he would calculate the occultations that would be visible above the walls of the telescope room and then noncommittally pass the predicted times to Mitchell and me. If we could observe the events on a clear night when the telescope was not otherwise scheduled, he would reduce the data and report the results to ILOC. Starting in 1992 and continuing into 1998, this became a routine among the three of us. Bright-limb immersions were abandoned almost as soon as they were tried because scattered light led to very low S/N values, and emersions were never successful because it was impossible to orient the photometer carefully enough so as to anticipate the location of the reappearing star. The dark-limb immersion procedure, however, was simple: isolate the star in the smallest possible diaphragm of one channel of the photometer and, at a defined time, command the system to move into sequential mode taking 2000 counts of 10 msec each. Each count had to be followed by an archiving interval of 5 msec. For a few bright stars it was possible to use shorter counting intervals. If the predicted time were even close to correct, a good occultation record resulted and it was sometimes possible to detect the star as an occultation binary as is indicated in Fig. 55. This 1998 record leads to a separation of 0.445" – consistent with data of 0.413" and 0.440" from 1991 and 1992, respectively,

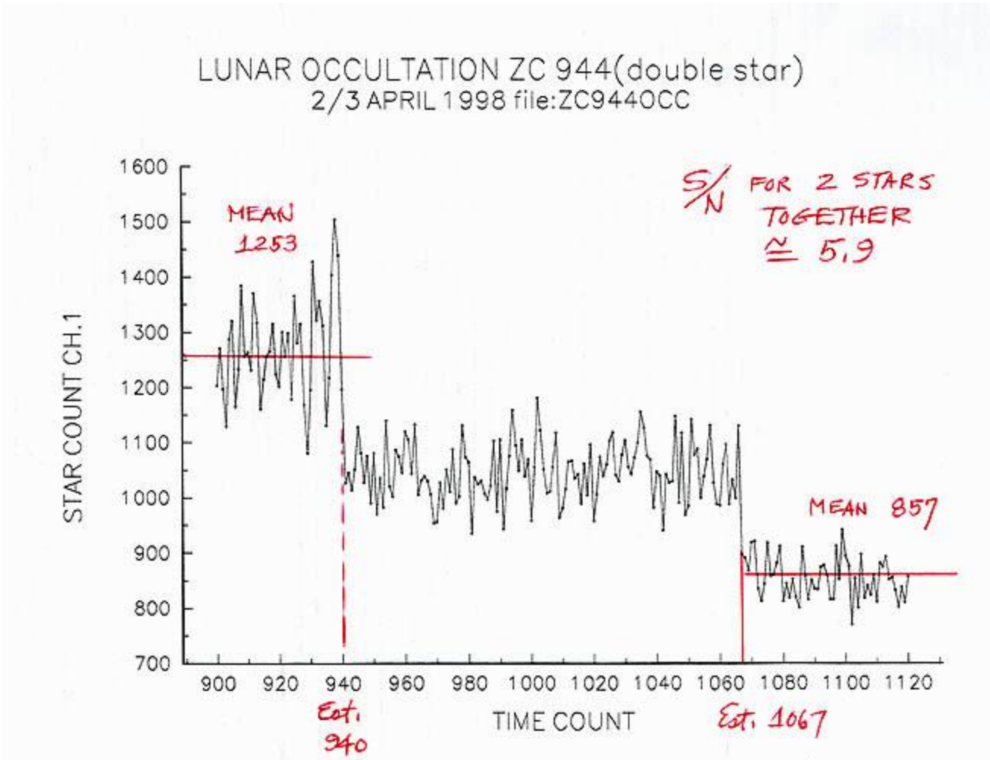


Fig. 55. The worksheet for the *V*-band observation of the occultation binary, ZC 944 = HD 42954. The star is classified as an A6m object at $V = +5.88$. The angular separation is about 0.45".

obtained by other teams. We eventually observed 37 of these events mostly in 1995 and 1996 but much more could have been done to enhance the results. For instance, it would easily have been possible to observe a reference star simultaneously in the other channel of the photometer and reduce the data as if it were a magnitude difference, but this was never pursued past the demonstration stage. Similarly, we never corrected for sky brightness. Only one of the occultations, that of α Tau in 1998, would have been useful for determining an angular diameter. Shown in Fig. 56, it has been only approximately reduced to an

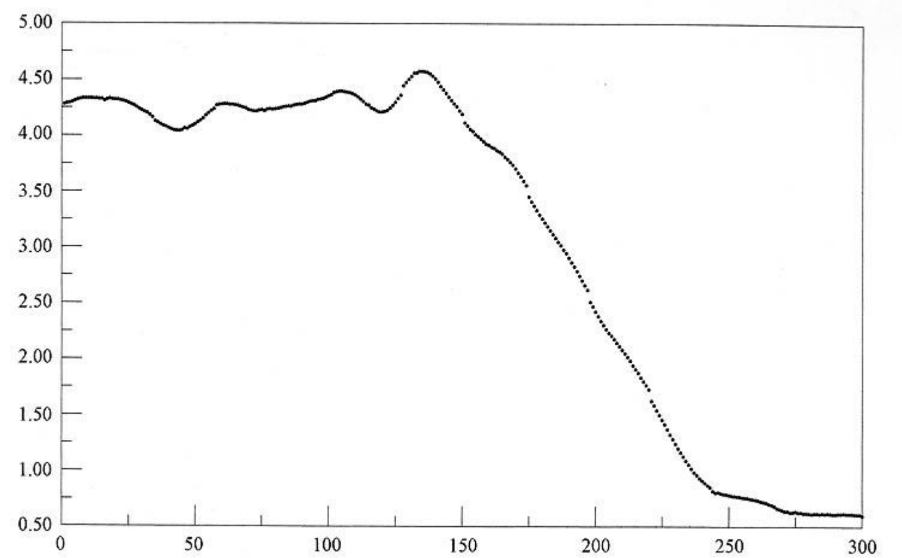


Fig. 56. The occultation record for Aldebaran in 1998 with a counting time of 0.59 ms. Part of the expected diffraction pattern is delineated cleanly and the low-amplitude scintillation can be seen as well. The small discontinuities in flux and time occur at the breaks for archiving in the sampling routine and prevent a good determination of an angular diameter.

angular diameter of 0.024". Within formal errors, this is not consistent enough with the accepted value of about 0.020" and perhaps the discrepancy can be traced to an imprecise occulted-light level because the observations ended too soon. This program, which could have been much more productive as may be understood from Richichi (2004), was a lot of fun but basically a time-filler.

Solar Studies

Two solar eclipse expeditions – to Jackman, ME on July 20, 1963 and to Manaue in the Society Islands on May 30, 1965 − resulted in nothing. A considerable number of flash spectra were accumulated at the earlier eclipse despite some interruption by clouds but analysis of them was never completed, and the three experiments contemplated for the second event were lost entirely to clouds.

Very quickly, first results from URSIES were in hand as reported by Wyller, Fay & Yun (1970) wherein they compare observed profiles of strong lines with Yun's calculated ones. The final report concerning their FCO results appears in Fay, Wyller & Yun (1972), which shows, first of all, how hard they had worked to diminish and understand all the sources of scattered light. They argue convincingly for the individuality of umbral flux levels compared to photospheric flux levels and present a variety of comparisons of the observed *Na* D_2, *Hα* and *He II* λ10830 profiles against their own models and those of others. The promise of careful stellar observing with URSIES is clear in Fig. 57. Even with the small 15-

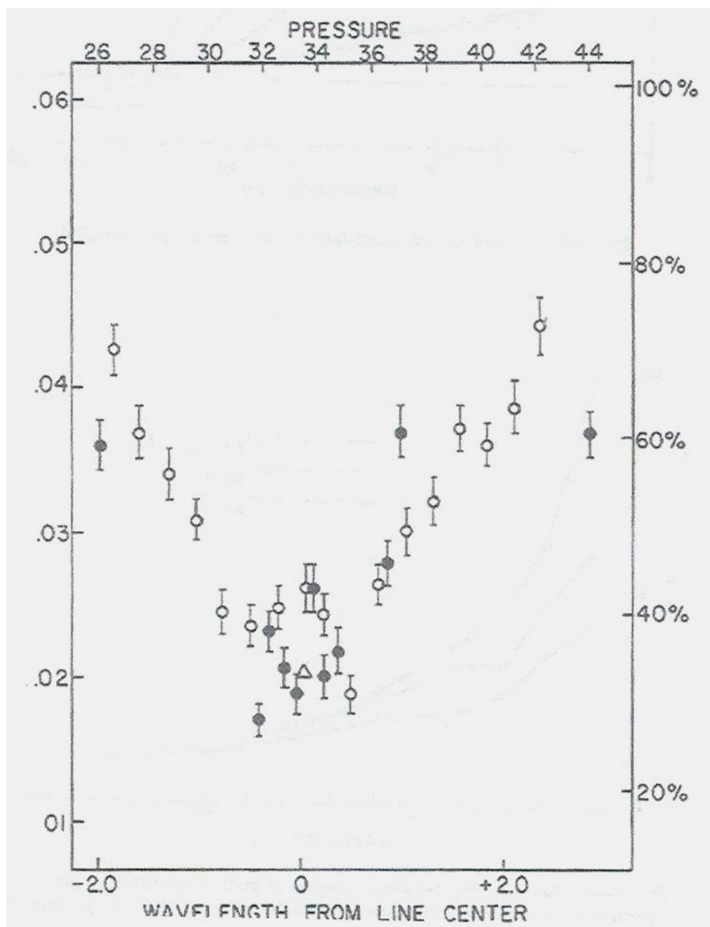


Fig. 57. The *Hβ* profile of α Lyr observed with URSIES coupled to the siderostat. This illustration is published as Fig. 9 in Wyller & Fay (1972).

inch siderostat, the profile is delineated quite well. From the point of view of the Bartol team, their work at the FCO was a success. From the point of view of the observatory staff, it has to be seen as an opportunity missed. Had at least some of the staff associated themselves with Wyller in a timely way, they could have participated in some very fine fundamental stellar and solar atmospheric work.

Blitzstein and Holenstein projected a solar image from the siderostat so that it was about 40 inches in diameter and then sampled it locally with paired Si photodiodes. The outputs were fed into FET op-amps and then to lock-in amplifiers. One detector looked at an area of interest and the other at a control area in order to measure the scintillation power law in the solar plasma. Five bandpasses were sampled and diaphragms sequentially isolated circles on the solar photosphere of about 700, 1,000 and 1,300 km diameter. Tests for claiming a positive detection of an intrinsic solar signal were three: (1) the smallest sampled area should show a more decisive signal than the progressively larger ones because a larger number of individual cells should lead to a less coherent signal; (2) there should be greater scintillation noise in the umbra of a spot than on the photosphere and (3) scintillation noise should be greater at the limb than at disk center because more uncoupled cells are sampled near the limb. The frequency range that was sampled was much higher than the typical frequencies associated with the many modes of helioseismology. FCO results were somewhat encouraging but local seeing problems masked any decisive results. The two men then applied for KPNO time to make confirming measures and were granted two days at the McMath-East focus. No conclusive results were obtained there either and the project was ended.

THE EXTRAMURAL OBSERVING RECORD

To overcome isolation and broaden contacts for grad students Wood started looking to alliances with other institutions beginning in 1954 and I include a description of these affiliations because they were commonly founded on experience with ongoing programs at the FCO. These collaborations were of two kinds: real-time accomplishments and first-time or retrospective interpretations of historical data that were not taken by the FCO people. After Wood's departure numerous other collaborations were set up either *ad hoc* or programmatically in part to take advantage of space opportunities. I describe the efforts approximately in order of increasing photon energy.

A bit of radar work happened early. In 1959 a Lincoln Labs group (Price, *et al*. 1959) had shown that technology advances then permitted detection of radar echoes at 440 MHz from Venus. At that time and later, Blitzstein was a consultant at the RCA facility in Moorestown, NJ where there was an 84-ft dish functioning in the general *BMEWS* system. He convinced the engineering group at the station to try to detect the planet at the next near-Earth passage in 1961. Transmission and reception times were each approximately 1 hour and the team made successful detections on 4 of 8 experimental runs. Their results appear in Maron, *et al*. (1961), which is a rather nice example of an engineering stunt turned into a good piece of science. As far as Blitzstein was concerned, it was the calculated solar parallax that was going to be the important result and the article reports a value of 149,560,000 ± 200 km for the Astronomical Unit. This compares very favorably with results from MIT (149,597,700 ± 1,400 km) and Jodrell Bank (149,601,000 ± 5,000 km) at the same planetary encounter. Rittenhouse, Smith and Ewing would have been proud of the work. #A darkly humorous event happened a few years later when Blitzstein was still a consultant at RCA. At supper one evening his wife called him to the phone; it was the Duty Officer from the *BMEWS* dish. They had just picked up the strongest echo they had ever detected, it was in the northeast and the man was going to call SAC to recommend a scramble of all available aircraft and missiles. Blitzstein asked him for the Doppler and the man came back with a ready answer. Blitzstein's response was to tell him to go out of the building and look low toward the northeast horizon. Of course, it was Moon in a sidelobe of the antenna pattern.#

Some radio efforts also started quickly and continued intermittently for about 15 years. Menon (1960) published a comprehensive survey of the electron distribution in the Orion Nebula from scans with the 84-foot telescope at NRAO. Close to the Trapezium, correlation with optically bright features was good but this correlation diminished as one approached the perimeter of the nebula. Summer student grants at NRAO were awarded to McCluskey, Chen (who worked on the calibration of the Little Big Horn), Sobieski and Wanner. Wanner's (1961) map of the SNR IC443 followed from this experience. Sobieski profited by his opportunity to work with C. R. Lynds (Lynds & Sobieski 1961) in order to create a 3,000 MHz map of the Perseus Cluster, which detected the PerA source and NGC 1265. Langer and his colleagues (*cf*., Guélin, *et al*. 1977) capitalized on the Ft. Davis Millimeter Wave Observatory to observe DCO^+ and HCO^+ in several galactic emission regions. Although the papers are not observational ones, Goldsmith & Langer (1978) and Langer (1978) are important for investigating the general stability of interstellar clouds. In more recent time, Mason's dissertation work, supervised by Myers, added a cosmological result to all the previous galactic results.

With his thesis adviser, Rivolo (*cf*. Solomon & Rivolo 1989) made a very nice extraction from the *Massachusetts-Stony Brook Galactic Plane CO Survey* to show the distribution of GMCs in the first quadrant of the inner Milky Way Galaxy and to note the differences between the warm and cool clouds as they traced out parts of two spiral arms. It may reasonably be expected that Devlin's surveys will continue their productivity.

With the VLA and under the supervision of K. J. Johnston of the NRL, Migenes surveyed a variety of numerous OH and NH_3 masers in the direction to the Orion-KL region of the Orion Nebula. This work. measuring intensities and proper motions, and a bit more was published in Migenes *et al*. (1989).

Two fine efforts were completed later though the good offices of Michael J. Mumma at NASA GSFC. By using both the NASA IRTF and the KPNO McMath telescope, Jeffrey J. Goldstein (1990, 1991) was able to map the winds of Venus between altitudes of 70 and 200 km. The observations covered 82% of the planet's synodic period and exploited the $^{12}C^{16}O_2$ R(8) feature at 10.33μm in order to model the velocity profiles of the Cytherian winds. Mumma also supervised the retrospective study by X. Xie of the NMS measures from the *Giotto* spacecraft of Comet Halley in March 1986. This work, appearing in Xie & Mumma (1996a, b), assumed a model for the comet's nucleus and then mapped the velocity field of the H_2O molecule from about 10^3 km to about $3x10^4$ km from the nucleus. The analysis showed that rotational cooling of the molecule was significant. Both of these accomplishments were impressive, at least to a visible-band worker.

Next a summary of miscellaneous photographic enterprises. A cooperative effort with Dr. Remeis Sternwarte was agreed to by Wood and W. Strohmeier but this resulted only in Avery and John Sievers spending a summer in Bamberg, West Germany. Eventually a brief publication by Shaw & Sievers (1970) announced 4 suspected variables. A certain number of the photographically-discovered Bamberg Variables were eventually observed photoelectrically by other FCO grad students. At Basle, U. Steinlin and A. Tamman had accumulated 6 each of RGU-filtered plates of a particular star field with the Palomar Schmidt and C. Fang spent a year working with Wilhelm Becker on their reduction. The field was somewhat anomalous in that it had a high star density despite being in the galactic anti-center direction. The magnitude errors were reasonably small and the work appears in Becker & Fang (1973). The same plate material gave Fang (1970) a chance to study the poor open clusters NGC 1605 and NGC 1664. C. W. Ambruster (1978) worked at Sproul one summer and published an analysis of the astrometry of ADS 8887. The retrospective monitoring of the activity of "compact galaxies" from the HCO plate collection by Shen and his collaborators appears in a number of papers in the 1960s and 1970s (*e.g*., Usher, *et al.* 1969; Shen, *et al*. 1972, Pollock, *et al*. 1974). This was work representative of the interest in these objects at that time when so little was convincingly known of them.

While still a grad student, F. Giovane became a Co-PI on the T025 Skylab 3 mission to observe Comet Kohutek 1973. His participation involved taking a number of narrow-band images isolating several emission features of the comet at an altitude of 3,045 m above sea level from the Zodiacal Light Station on Haleakala. Giovane's images were meant to calibrate the similar ones taken from the spacecraft but, because of mistakes by the astronauts, their effort was a disaster with essentially no results. So the 20 nights of mountain images were put to use as prime material. Giovane did an excellent job of extracting from them every bit of scientific information which they contained and also avoided the pitfall of over-discussion of the material. His images compared favorably with those of others and he was able to extract some information of the production rate of the molecules that were the parents of those seen in his bandpasses. These results, however, did not add substantially to the understanding of cometary phenomena. The reduction techniques, on the other hand, were used subsequently in the treatments of zodiacal light and galactic light experiments from *Pioneers 10* and *11* and in the data stream from the NRL LASCO coronagraph onboard the *SOHO* spacecraft.

Early and later examples of mining of the NGS-Palomar Sky Atlas for detailed examination of clusters of galaxies can be found in Struble & Rood (1984, 1999). Many other examples of examination of such clusters were also published. Another widely-cast survey, expressed in Rusin's dissertation, exploited gravitational lensing.

The collaboration with Canterbury University, Christchurch, NZ could have been much more substantive than was eventually the case. Startup was made possible by an outlay of the equivalent of about $84,000 [$500,000] by the New Zealand government. Initially, a few of the Cook and Flower telescopes and subsequently a new 24-inch reflector were sent to this site and the Mt. John University Observatory was developed. Unfortunately, the new telescope had to be extensively rebuilt in the Canterbury Physics shop because workmanship by The Optical Craftsman was poor. The NZ collaboration resulted in FCO faculty and students gaining access to southern objects for a number of years and was a reasonable success. It also provided an opportunity for U. Köhler of Dr. Remeis Sternwarte, Bamberg to survey southern fields with I. M. Paterson working as the resident photographic observer. More ambitiously, C. D. Shane wanted to extend the Lick Photographic Atlas to fields that could not be reached from Mt. Hamilton and this was set in train and finished by him, Wood and Noel Doughty (*cf*., Doughty, *et al*. 1974) of the University of Canterbury with A. J. Thomas actually doing the observing and photographic processing. Relations with the Canterbury faculty and administration were consistently good and it was not a happy affair for anyone when University financial problems required me to end the institutional subvention to the MJUO. For a number of years thereafter my NSF grants were sufficient to support one or two observers at the station. The photoelectric variable program was undertaken by Pamela M. Kilmartin and resulted in a small number of publications (*e.g*., Kilmartin, *et al.* 1987) that pushed the limits of the reflector. Some of her data remain unanalyzed and unpublished. Alan Gilmore obtained many hundreds of plates and films of LMC fields selected by me but neither the standard-field nor program-fields have ever been reduced.

In the 1960s there began a collaboration between the University Medical School and a teaching hospital and medical school at Pahlavi University in Shiraz, Iran. This was to be a two-way street: Iranian health and medical people, historically mostly trained in Germany, would come to Philadelphia for advanced training in a different *milieu* and visiting Pennsylvania staff would be able to learn about diseases and health conditions that didn't exist in the States. Pahlavi, bearing the dynasty's name and set up so that instruction was in English, had the active support of the Shah for exposure to U.S. methods and knowledge. Wood got the idea that astronomical collaboration could also be developed with a station at such a different longitude and this thought was supported by the University Provost. The Pahlavi Physics Department was the local structure with which Wood dealt but he left before any concrete result emerged. I made a couple visits to Shiraz to foster the collaboration and found no lack of good intentions. In short order, I arranged for Pahlavi to buy a small AstroMechanics, Inc. reflector, a somewhat over-designed observatory structure was constructed north of the campus, and Blitzstein built a photoelectric system to be fed by the telescope. From 1975 into 1978, Guinan (by then a faculty member at Villanova University) spent time at Shiraz installing the telescope and dome, breaking in the hardware and fostering the training of local observers. The station was named after the medieval Persian polymath, Abu Rayhan Muhammed ibn Ahmad al-Biruni, and was dedicated by Empress Farah. Guinan's efforts convinced Dorren, who was on the Physics staff, to re-tread himself into an observational astronomer and some local students started work as well. Beginnings were promising, but even as early as my last visit the nearby hillside had large signs with DEATH TO THE KING (in Farsi) visible to all on campus. The Islamic Revolution caused Dorren to leave and the uncertainty that followed made it seem as if there were no long-term future for observational work at Biruni. This turned out to be too pessimistic as Blitzstein's photometer, now refurbished, and a CCD camera have continued stellar radiometry at the renamed Shiraz University. However, no collaboration was sustained with the FCO.

Partly through the efforts of Nha there was set in train a brief observational collaboration with Korean observers on the light curve of η Ori, which in the 1990s was the brightest naked-eye eclipsing binary in the entire sky that had no usefully modern light curve. With the PBPHOT several of us had been working on the object for years but we needed data from a station at another longitude to complete phase coverage. The collaborative work was reported on at a conference in Seoul. After this initial effort, I was approached by Chun-Hwey Kim to collaborate with the eclipsing binary photometry program at Chungbuk National University in Cheongu about 2 hours south of Seoul. The local group had plenty of assigned observing time at Korean facilities and eventually on Mt. Lemmon and was tightly trained and guided by Kim. Even so, they were somewhat short of experienced manpower and at the time felt that their written English was not so fluent as they wished. Quite a number of collaborations, not restricted to tuning up language, have resulted since that beginning and Kim and a grad student have come to the U.S. more than once to profit from the "free time" of my retirement.

Based on experience at the FCO, several ground-based programs were undertaken while staff and students were guests at stations other than MJUO. For photometric and radial velocity work mostly on close binaries, extended or intermittent stays at Cordoba, CTIO, DAO Victoria, KPNO, LaPlata, Lowell, Mt. Stromlo and Steward were all very useful. Faculty and students who profited from these and MJUO guest privileges were Avery, Binnendijk, Bradstreet, Chambliss, Fliegel, Giovane, Guinan, Kilambi, Koch, Leung, Protheroe, Shaw, Spear, Wolf, and Wood but not all of their work was published. There were also useful collaborations initiated by Guinan, Kilambi, Perry, Pfeiffer, M. S. Snowden, P. D. Usher and E. J. Woodward, none of whom was associated with the University at the times of the collaborations. The content of Fig. 58 will permit the reader to judge the productivity of these programs against the record of the in-house ones shown in Fig. 50. Completely different efforts, but not specifically based on

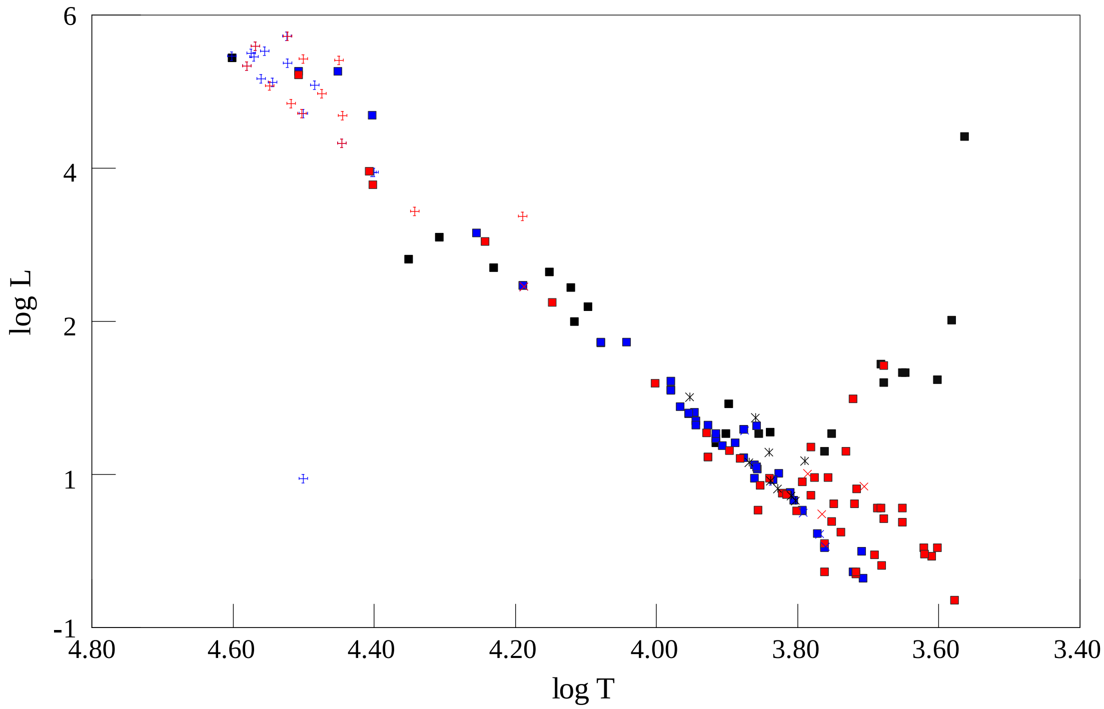


Fig. 58. The theoretical HRD of targets not observed at the FCO coded as follows: solid black squares, *Einstein* targets; plus signs, *IUE* targets; solid blue and red squares, close binaries; crosses, radial velocity measures; and black stars, stellar envelope analyses. Blue and red symbols refer to hotter and cooler binary component stars, respectively, and, as in Figs. 50 and 53, neither a ZAMS nor a TAMS is specifically indicated.

observational experience at the FCO, show up in the remotely-observed dissertations of Cruz and Wahlaj, concerned, respectively, with very low-mass stars and debris disks around other stars.

Over the interval 1994-1998 Lande and I tried two off-ground pieces of work. The earlier enterprise arose from a politically correct ingredient in a NASA program whereby money would be available to integrate undergraduates into observing efforts that had at least some potential for space applications. We got the money and some University funds as well and recruited three students who knew almost nothing about science when they began. Nonetheless, they applied themselves and, we thought, learned more than a little.

Partway through the construction phase Sam Seeleman died but Hee and Borders of the Physics Department Shop pitched it voluntarily and completed the heavy machining and welding. In a matter of 2 months we built a skeletal telescope out of honeycomb and carbon-composite materials around a 20-inch Zerodur mirror feeding dichroic-filtered split beams to two CCDs. Four outboard GPS receivers were to be used for tracking and a stripped-down CPU for control and archiving data. Of course, time was always urgent and weather and wind at Wallops Island repeatedly unsuitable but eventually the 140-pound payload was tethered to a balloon and off the launch truck went. It was a debacle: the computer failed as the truck accelerated down the runway and couldn't be recalled. Although the payloads, mirror and detectors were recovered in good condition after flight, we never attempted to repeat this experience. At about the same time, I became interested in the concept of pneumatic mirrors that had been demonstrated to us in a local colloquium by Peter Waddell of the University of Strathclyde. Over the next several months I built a few of these with diameters between 30 cm and 1 m trying out different metallized films and sealing techniques. These looked only at lab targets or ground-level scenes but they were cheap and light and promising enough so that I imagined that we could put them to work on celestial fields. This idea led later to a kind of fitful collaboration with Aurora Corp. in Manassas, VA for which I built a CCD imaging system fed by an 8-inch fast-beam siderostat. The instrument looked down toward ground from a company plane on experimental flights but Lande's eventual hope was to mount floppy mirrors in robot aircraft that could fly in high altitude, polar-latitude, small-circle flight paths to image the sky. The work was reported in Koch, *et al.* (1998) but we really didn't command the engineering experience and manpower to do more than the test flights with conventional mirrors and this effort too was abandoned. Although the company had structural problems with at least one plane, they still exist as Aurora Flight Services and are apparently prospering.

Near the start of the 1980s, there began a long-term collaboration with D. J. Stickland and his colleagues using the *IUE* spacecraft. The original aim of the effort was focused on determining very precise masses and orbits for binary components by requesting scheduling for back-to-back shifts with Stickland's 8 hours followed by my 8 or 16 hours or my U.S. shift 2 succeeded by the U.K. one. When this was achieved (and scheduling did commonly permit these arrangements), very good data density resulted over a satisfyingly short time for many of the targets so noise from streaming gas transients was minified. In the first year of this program, we understood that the same spectral images could yield information on the expanding and colliding stellar winds from the binaries as well. All of the numerous mass determinations have been published but for only 5 systems have wind analyses been completed. Partway through the *IUE* effort, Plavec, Guinan, and I pursued an *Einstein* program that was largely unproductive because it detected only 2 of 6 shell stars and none of the 6 eclipsing targets. Rivolo and colleagues (*cf*. Kinney, *et al.* 1990) used the *IUE* spacecraft in archival mode to look into what was called the Baldwin Effect – the correlation of the strengths of the red-shifted *Lyα* and *C IV* λ1550 features in the spectra of quasars and Seyfert galaxies with the absolute luminosities of these objects.

The appointment of Friedman created opportunity for grad students to develop dissertations in space astronomical topics, although not necessarily by taking real-time observations. The first to do so was Ambruster who profited by the completion of the *HEAO* A-1 Sky Survey. The spacecraft had actually stopped functioning before she joined Friedman's team but she and others (Ambruster, *et al*. 1983) were first able to locate an X-ray source H0547-14 that seemed to be emitting the low-energy tail of a γ-ray burst and then to define repeated intense X-ray flares from a small variety of active stars (Ambruster, *et al*. 1984, 1986). These discoveries were significant advances at the time. M. W. Johnson also finished his dissertation as a member of Friedman's group and again one source of his work was the Sky Survey. In Johnson, *et al*. (1983) the group examined the Abell (1958) Catalog of Clusters of Galaxies and was able to make X-ray detections of 128 of them, 91 of these being new discoveries. Detection was correlated only with Abell's richness criterion and with no structural indicator. An earlier paper (Johnson, *et al*. 1979) was the result of a rocket flight from Australia and had reported on the diffuse X-ray emission from the Coma cluster.

A constant theme in all of Dorren's collaborations with the Villanova astronomers was the search for solar-type activity and markers on other stars. The motivation for this is easily understood: these stars can, in principle, tell one much about the past and future of Sun. The culmination of this work appears in Dorren, *et al*. (1995) where the PSPC instrument on the *ROSAT* spacecraft was used to establish the X-ray levels for solar surrogates - EK Dra as an antecedent for the youthful Sun and β Hyi for its prospective old age.

There is no need to repeat the well-known history of Davis's development of the theory, hardware and experimental techniques employed in his chamber mounted in the Homestake mine. Many people felt that his Nobel award was more than a little belated. The deficiency of the neutrino flux compared to the predictions of the Standard Solar Model set the stage for a credible theory of neutrino oscillations although the data sets never showed statistically-valid evidence of neutrino variability for Sun. A good summary of the Homestake effort appears in Cleveland, *et al*. (1998). The detector was also too small to detect the flux from SN 1987A.

THE ACADEMIC ENTERPRISE

Some basic observational training was possible for undergraduates at the SO and the year-long course in observatory practice was offered every other year. Since equipment was maintained methodically, limitations were due to the small telescope apertures, the metropolitan weather which always seemed worse than at the FCO, and the adjoining floodlit tennis courts and football stadium which were never darkened. From time to time, a dedicated undergraduate pursued a short photometric or polarimetric program at the FCO. Peter Eisenhardt and Doug Leonard picked up some observational experience there and it would be nice to think that this kind of exposure contributed to their careers.

From time to time, a Masters degree was awarded but the concentration was naturally toward the PhD program. There follows an alphabetical list of dissertations founded on observational material.

Allen, P. R. 2005, *From the Cradle to Limbo: A Bayesian Study of the Substellar Mass Function*
Ambruster, C. W. 1984, *A Survey of Fast X-ray Transients using the HEAO A-1 Sky Survey Experiment*
Avery, R. W. 1971, *Element Abundances in A and F Stars HR6917 and HR 7061*
Bookmyer, B. B. 1964, A *Study of Photoelectric Observations of SW Lacertae*
Bradstreet, D. H. 1983, *K-type Overcontact Binaries*
Chambliss, C. R. 1968, *Photometric Studies of Four Southern Hemisphere Variable Stars*
Chen, K.-Y. 1963, *Infrared Photometry of β Persei*
Chou, K. C. 1962, *Photoelectric Photometry of the Eclipsing Variables β Persei, RZ Draconis and BX Pegasi*
Corbato, S. C. 1989, A *Search for Anisotropies in the Underground Cosmic Ray Muon Flux*
Corcoran, M. F. 1988, *Polarimetry and Spectrophotometry of the Massive Close Binary DH Cephei*
Cruz, K. L. 2004, *The Luminosity Function of Low-mass Stars and Brown Dwarfs*
Devinney E. J. 1968, *The Eclipsing System Z Draconis and W Ursae Minoris*
Elias, N. M. 1990, *Elliptical Polarimetry of the Eclipsing "Serpentid" Binary Stars SX Cassiopeiae and V367 Cygni*
Fang, C. C.-Y. 1968, *Photographic Photometry of a Galactic Star Field near NGC 1664*
Fliegel, H. F. 1963, *The Eclipsing Systems AG Virginis and SX Aurigae*
Fredrick, L. W. 1959, *The System of VV Cephei*
Geldzahler, B. H. 1980, *The Relation between the Galactic Compact Radio Sources and the Extra-galactic Compact Radio Sources*
Giovane F. 1977, *Photographic Radiometry of Comet Kohoutek (1973f)*
Gleim, J. K. 1965, *Photometric Studies of the Two Eclipsing Binary Systems BV 382 and SW Lyncis*
Goldstein, J. J. 1989, *Absolute Wind Measurements in the Lower Atmosphere of Venus using Infrared Heterodyne Spectroscopy*
Guinan, E. F. 1970, *U, B, V, Narrow-band u(3580Å), b(4550Å), v(5050Å), org(5905Å) and Hβ Photometry of the Eclipsing Systems R CMa and V1010 Oph*
Harris, A. J. 1966, *Photometric Studies of the Eclipsing Binary Systems BV 412, BV 332 and V836 Cygni*
Holenstein, B. D. 1991, *Elliptical Polarimetry of Eleven Luminous Late-type Variables*
Hrivnak, B. J. 1980, *A Photometric Study of Active, Short-period Binary Systems XY Leo, AW UMa and ER Vul*
Johnson, M. W. 1990, *HEAO A-1 Observations of X-ray-emitting Clusters of Galaxies*

Kilambi, G. C. 1975, *Photometric Investigations of the Southern Galactic Clusters, NGC 6025 and NGC 6530*

Koch, R. H. 1959, *Photoelectric Photometry of R CMa, AO Cas, AS Eri and XY Leo*

Kondo, Y. 1965, *The Eclipsing Variable Systems BV 342 and BV 267*

Leung, K.-C. 19667, *A Photoelectric Study of the Variables BV 544, V701 Sco and HD 199757*

Mason, B. S. 1999, *An Improved Measurement of the Hubble Constant Using the Sunyaev-Zeldovich Effect*

McCluskey, G. E. 1965, *The Eclipsing Systems RX Arietis and UV Leonis and a Discussion of the Mass-Luminosity Law*

McCook, G. P. 1968, *An Integrating Digital Electronic Acquisition System and a Photometric Study of TW Cassiopeiae*

Migenes, V. 1989, *A Three-year Study of the OH Masers in Orion KL and a Study of the (3,2) Transition of Ammonia*

Nagy, T. A. *1974, Synthetic Light Curves of Four Contact Binaries*

Nha, I.-S. 1971, *Photoelectric Photometry of CW Cephei and RU Ursae Minoris*

Pachoulakis, I. 1996, *A Systematic Study of the Structure and Interactions of Winds in Selected Hot Close Binary Stars*

Perry, P. M. 1974, *Two-dimensional Spectral Analysis of the Optical Features of NGC 1976*

Pfeiffer, R. J. 1975, *Intrinsic Linear Polarization in Eclipsing and Spectroscopic Binary Stars*

Reed, G. F. 1970, *Observations of RZ Cassiopeiae and SU Cassiopeiae* (This paper was paired with a pedagogical dissertation in partial support for a D.E. degree by the School of Education.)

Reuning, E. G. 1961, *Astronomical Photometry in the 1 to 3 Micron Region*

Rigterink, P. V. 1971, *A Numerical Analysis of the Variations of the Light Curves of the Close Binary Systems W Ursae Majoris and U Pegasi*

Rusin, D. 2001, *Studies of Gravitational Lens Systems Discovered in the Cosmic Lens All-sky Survey*

Shaw, J. S. 1970, *Photoelectric Photometry of HO Telescopii and KZ Pavonis*

Spear, G. G. 1973, *Properties of Two Common Photometric Systems and Photometric Observations of Selected Eclipsing Binary Systems*

Wahlaj, Z. 2005, *Planetary Signatures in Circumstellar Debris Disks*

Wilson, R. E. 1963, *Photoelectric Photometry and Orbital Solutions of the Eclipsing Variable Stars W Ursae Majoris and XZ Canis Majoris*

Winkler, L. 1964, *Selected Topics Concerning Photoelectric Photometry and Eclipsing Variables*

Wolf, G. W. 1970, *Elliptical Polarization of Starlight*

Xie, X. 1994, Cometary Atmospheres*: Monte Carlo Simulation and Its Application to OH Radio Observations*

I believe this list is complete and the total has been accumulated at a rate not too different from one degree annually. It is sobering to realize that a few of these people, who inevitably remain in my mind as young adults, have been dead for quite a number of years.

A CRITIQUE OF SCIENTIFIC POSITIVES

Some things were done well or in a novel fashion during the 40 years of the functioning of the FCO and SO. These matters have had enduring scientific value and it is worth enumerating them.

(1) The observational training program was successful. The subsequent careers of many of the grad students validate this judgment because they have built on what they experienced at the observatories.

(2) Binnendijk's concentration on cool contact binaries was just what was needed at the time since it provided the 1960s and 1970s theoreticians with excellent light curves to test the concepts of mass and energy exchange. Many other people worldwide have extended his samples. His work is by no means passé and his emphasis on A-type and W-type contact and over-contact binaries is perhaps the most inventive product of the FCO.

(3) Just as the visual binary micrometric measures from the FO will endure, so the light and polarimetry curves from the FCO will last. The value of historical light curves grows with every passing year as they preserve the evidence of the momentarily active states of the stars. More

than 125 papers were eventually published based on single- and dual-channel pulse-counting, single-channel dc photometry, and dual- and triple-channel polarimetry. Most, but not all, of these programs concentrated on close, interacting binary stars. Almost none of this could have been accomplished without the immense investment of time by Blitzstein and his technicians and machinists as they constantly modified and modernized the telescopes and developed new instrumentation for them.

(4) The numerous timings of minimum light for all the variables will continue to be used indefinitely by workers interested in close binary dynamics.

(5) Resurrecting the bright variables δ and η Ori (Koch & Hrivnak 1981; Lee, *et al*. 1993) from more than 60 years of photometric neglect will continue to pay off. These objects show radiometric and dynamical curiosities that are far from understood, but these effects should not be unique to them so they are exemplars of what may be expected in many more systems. Because they are so bright, they offer opportunities for study without need for very large telescopes.

(6) The discovery of V641 and V684 Mon (Koch, *et al*. 1985; Koch, *et al*. 1986) in the very young cluster NGC 2264 moved pre-ZAMS processes slightly onto the conventional path to better understanding. Russell's dictum that you can only know stars intimately and correctly if they are visual or close binary components remains almost as true today as when he wrote his famous essay *The Royal Road to Eclipses*. Before the Monoceros stars were discovered, there were only two pre-ZAMS binaries known and they were field stars whose interpretations depended on no external evidence. The testimony of cluster membership is so persuasive that concentration on objects such as these was inevitable and is now in full swing.

(7) The more than three decades of polarimetry contain very high-weight information on the stability of null and non-null polarization standards.

(8) The polarimetric variability of the luminous cool variables is very complex with the linear and circular components seemingly independent of each other. More than one mechanism is necessary to explain the polarization patterns of these stars and these are not yet worked out.

(9) A considerable amount of inventive instrumental design underpins all of these accomplishments.

(10) The consequence of collaborating with good scientists shows conspicuously in some of the spacecraft results.

For a few decades the FCO had presented an example of an eastern U.S. observatory that was able to capitalize on the 1 night in 3 that was photometrically useful. That statistic emerges from the reflector logs from 1966 to 1996 (the earlier one is not accounted for at present) and refers to the intervals when neither Binnendijk nor I was on leave. Although I haven't examined the log for the siderostat in this detail, it probably presents the same evidence. By the 1990s, improved instrumental versatility and the availability of electronic cameras permitted exploitation of less-than-photometric nights and greater productivity on fine ones.

More than a few successful professionals and excellent scientists emerged from the observational training and practical programs. Nicholas Igantuk has been the long-term Superintendent of Schools for the Ridley Township School District and John Bangert moved from the Army Map Service to the USNO staff years ago. Larry Fredrick had a fine astrometric career and served 10 years as AAS Secretary. He also was Chairman of Virginia's Astronomy Department and Director of the Leander McCormick Observatory although none of these attainments can be credited to the FCO itself. Ed Sion put in lengthy service as an ApJ Associate Editor. Beginning in 1970, Bob Wilson and Ed Devinney created the normalized gravitational model for light, velocity and polarization curves of close binaries, and this remains the dominant code world-wide. Ed Guinan has been an inventive worker on solar-type stars and their activity and on extragalactic binaries as well as a conspicuous scientific bureaucrat serving in administrative positions for both the AAS and IAU. He has been President of both IAU Commission 42 and Division V. Il-Seong Nha trained the entire first generation of photometrists in Korea, became an authority on Far Eastern medieval astronomical hardware and textual materials, and served as President of IAU Commission 41. It should not be imagined that this record of achievement ended with my retirement in 1996. For instance, Kelle Cruz gives every indication of having started a fine career under newer faculty. The few choices that I have cited here are not meant to be invidious but only to indicate some breadth of attainment.

There is also the diverting information that Minor Planets 3625, 6076, 8072 and 8895 have been named in honor of Fracastoro, Plavec, Kondo and Nha, respectively, but no credit should accrue to the FCO for these recognitions.

THE BALANCING NEGATIVES

There are significant negative elements that oppose the optimistic assessment.

(1) The results enumerated in the previous section contain no fundamental discovery except for Binnendijk's work on contact binaries.
(2) Although Wood offered direction for the observing program, Blitzstein, Shen and I did not. We were Directors in name only. Our attitude was that opportunity existed to try whatever came to a person's mind and we would give him a chance to do just that. Morale might have been high as a result of our policy but some telescope assignments and time were wasted due to inefficiency.
(3) I suffered from scientific attention deficit. As soon as I understood what was likely to be the result of one program, I would be moving toward another one. This gadfly behavior meant that I, and the people associated with me, did not probe many things deeply enough. It's exactly the same criticism that I made of Olivier's photometric program at the FO.
(4) No person associated with the FCO recognized that there was a very valuable scientific opportunity in associating with Wyller's science.
(5) Blitzstein had two fundamental problems. He did not believe that it was essential for him to publish and so his work did not become known as widely as it merited. Of course, this did not help the institutional appreciation of the FCO. He also loathed travel and went to almost no AAS meetings where others would have learned of his work and where he could have profited from the interchanges that would have been inevitable.
(6) If one believes that only extragalactic research was worth doing from, say, 1960 into the present, the entire FCO observing program was beside any point.
(7) The staff was really too small. Had there been a larger number of responsible people, some of the extramural accomplishment could have been done more aptly in-house and credit accrued to benefit the entire program. The number of nights actually used became fewer as time passed. By 1990 I was the only staff observer left and there were many fewer interested students. In addition, the global scientific trend toward fainter and fainter sources for extragalactic studies requires ever larger objective sizes and this could not be justified locally.

"FOR OF ALL SAD WORDS OF TONGUE OR PEN,"

The dedication of the FCO and SO was the occasion for the first demonstration of Blitzstein's new photometer that appears in Fig. 59.

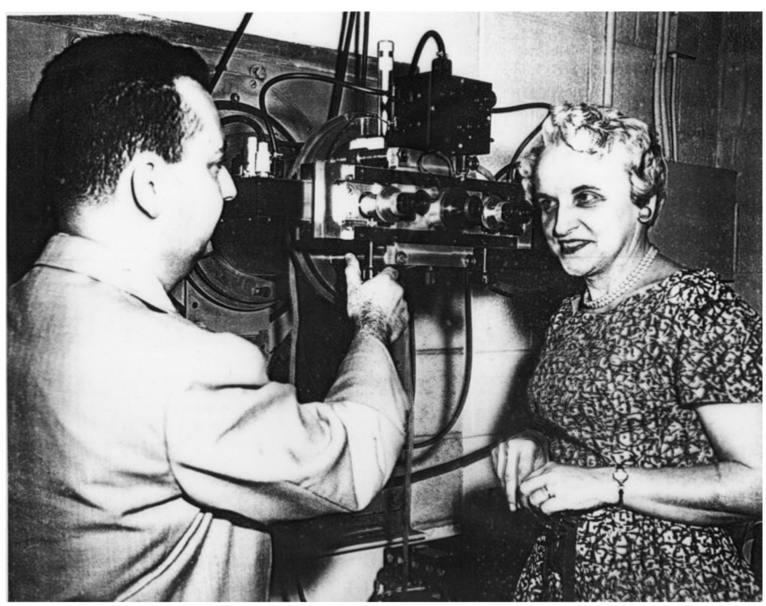


Fig. 59. Blitzstein is showing Mrs. Pierce (then a widow) the realization of the 2-channel, pulse-counting photometer head that he and her husband had envisioned almost 10 years earlier.

The event was also marked by a colloquium that resulted in a volume *The Present and Future of the Telescope of Moderate Size* that Wood (1958) edited quickly thereafter. Its Table of Contents appears in Fig. 60.

Contents

Fig. 60. The Table of Contents for the symposium volume compiled at the dedication of the FCO.

It can be noticed that most of the participants of the meeting were of more than trifling consequence and that a considerable range of interests is represented. What is also conspicuous is the prominence given to multiplier photocell applications with the implicit expectation that the quantum efficiency of the devices would ameliorate limitations of moderate objective size. It can also be seen that some early imaging devices were given attention. These compliments having been paid, it has also to be said that the event, which was supported by the Tobias Wagner Fund, was really not a forward-looking one. It was a citation of the success of the *status quo*.

At exactly this time AURA was being organized and the field testing to establish KPNO was already done. Had the years after Cook's death not been lost to the war and to dithering, the University might have been approached to join the consortium. There is no evidence that such an approach ever happened and it is reasonable that it didn't. Although Olivier had more than enough friends and colleagues among the founding institutions, he was on his way out and, although Wood had tight Princeton and Arizona ties, he was just feeling his way toward stabilizing the local situation. Even had there been an invitation to join AURA, it seems likely to me that the conservative administrators and trustees of the University would not have decided on that option. Had that actually happened, Blitzstein would have been a very valuable instrumental resource for the national observatory and Binnendijk's productivity would have been even greater than it was. The missed chances and poor judgments that I detailed in a previous paragraph and this lack of opportunity to affiliate with the AURA institutions lead me to the second and rhyming line of Whittier's (1900) couplet: "*The saddest are these: "It might have been!"* Of course, the poet wasn't thinking of anything as mundane as episodes in the existence of an observatory.

LIGHTS OUT!

As the population of Philadelphia declined for various reasons, the surrounding counties grew at different rates and on different time scales. By 1980 Chester County was beginning to show signs of out-migration with a few tract developments and starter castles spotted here and there. In the vicinity of the FCO, development was slow to start and was measured rather than a rampage. What did happen was a psychological response to something unexpected. Once many people realize the fantasy of living on country acreage, they discover that they are afraid of the dark. They are intellectually clear that the local population of lions and tigers is imaginary and that there might be one bear only every 50 years but still it is dark after evening twilight. Who knows what or who might be lurking out there to assault them or make off with their personal testimonies to capitalistic success? It doesn't matter that the typical crime is that of teenage boys riding around in the dark bashing mailboxes with baseball bats; many residents are sure that individually and personally they are marked. The obvious thing to do is to light up their premises and this became common, driven in part by the real and ruthless menace of escalating homeowner's insurance premiums if they didn't illuminate their holdings. Consequently, the night sky became somewhat brighter from residential development over about 15 years but not disablingly so for a very large number of stellar programs. The fact that there is no street or industrial lighting for many miles kept the environment from runaway illumination.

In 1985 Lande discovered that Physical Plant had been skimming the FCO budget by requiring observatory funds for routine maintenance although they actually had their own budget item for exactly that purpose. They were putting their own money to other uses. After Wood left, the residence was rented to grad students and then to Mitchell for a nominal fee. It was also discovered that those small sums were not coming back to the proper budget but vanishing into some general fund that the observatory did not control. Belatedly these malfeasances were stopped. In the mid-1990s, when I was the only observer left, a considerable upgrading inside and outside the FCO was done for no obvious reason – you wouldn't imagine that it was institutional guilt for the financial peccadilloes that the observatory had endured.

By the mid-1990s, the University looked at the FCO as potential revenue in that part of the 36 acres could be turned into cash while still leaving a diminished buffer zone around the buildings. At that time, it was generally supposed that the local area of the county was going to continue to be developed enthusiastically. Some of the occupants of the neighborhood were, however, not pleased at the possibility of new neighbors and mobilized an open-space-minded nonprofit, The Delchester Group, whose writ was controlled growth rather than endless suburban sprawl. In mid-1994, Mitchell, Holenstein and I proposed to buy the building and installed hardware from the University for $1 and run the place as a public science education center for the population of Chester County with guaranteed access to the University family. One might imagine the University financial people rolling on the floor at such a preposterous idea as failing to realize capital from real estate. Delchester and the University then established The Observatory Woods Land Associates L.P., and the University sold 31.262 acres to this limited partnership for $1 in September 1995. The Associates, in turn, achieved a sale of the same parcel to Philip J. Harvey, Jr. and Elizabeth A. Harvey for $1,400,000 [$1,610,000] in April 1996. Unfortunately, even earlier than this year the Treasurer's reports came to resemble corporate annual reports and lack detail so it is impossible to discover easily how much of the sale price was Delchester's fraction, but it is worth noting that the University had originally put out a net of [$217,000] for the land. A number of encumbrances – the materials of roofs, walls and foundations of any future structures, the height and spacing of outdoor lighting fixtures, the widths of easements – were written into the transaction and were intended to control the future of the now-private property and its environs for a number of years. Within seven months, the new owners subdivided their property, selling off 21.199 acres to another party for $700,000 [$803,000] and each of the two property owners built a sizable mansion. No further development would be foreseen for these parcels but this is of no consequence as could have been predicted. Since the University had already made a programmatic decision not to sustain ground-based work on stars, or really on any type of target, no faculty appointments were made in these specialties after about 1990. Use of the station for research purposes diminished to very little after my retirement in 1996. The observatory logs show that the last polarimetric and photometric observations were made on October 17, 1996 (by me) and on June 4, 2004 (by Galatola), respectively. The last electronic-camera image was taken on May 22, 2002 (also by Galatola). There had actually been accumulated a considerable number of images of Arp Peculiar Galaxies in 1998 but I have been unable to discover if anything was ever done with them.

The University looked for a way to disencumber itself of the remaining property and hardware. In mid-2004, the remaining 4.125 acres, structures and installed equipment were sold to the Harveys for $420,000 [$402,000]. From the beginning, their intention was apparently to raze the residence and replace it with a guest house for their personal use, temporarily maintain the grounds and observatory building in a presentable manner, and eventually decide what to do with the observatory building and telescopes. It is presumed that the money from the sale was deposited in the Flower Fund but this is not known publicly. After the sale to the Harveys, a small group from the Chester County Amateurs Astronomers, a few University people living in Chester County, and Holenstein tried to rent the property, the observatory and its apparatus so as to run it as non-profit public education facility. Although the Harvey's attorney eventually presented a lease text actually signed by the Harveys and incorporating a nominal rent of $1/ per annum for a 15-year interval, a mistake and an omission in the terms were not rectified during the 90-day grace interval. It was eventually decided that the lease terms were too uncertain to be useful for the potential lessees.

The final sale having been accomplished, Holenstein decided to challenge it on the grounds that it did not respect the terms of the Flower endowment. This naturally found no support in the University administration and he pursued the matter to the state Attorney General's office. The AG found no merit in the contention. On June 21, 2004 an interview with the University President was broadcast by WHYY, a local PBS station. She emphasized the good-faith intentions of the University in the disposition of the property and the circumstance that the institution had not accepted the highest bid because of concern about impact on the countryside. This situation shows rather nicely conflicts that cannot be foreseen. Open space preservation and a green conscience are not to be ignored, but there can also be the recognition that Flower gave his money for science and, capricious though he might have been, he likely would have wanted the highest return for translating his endowment into new capital for continuing that science. It may well have been that the differential between the highest bid and that finally accepted was of trifling consequence but, since nothing numerical is known of the transaction, there is no way to judge the matter. There is also no indication whether the then-current scientific staff was consulted about the choice of bid.

Despite a promise by a University attorney of informed communication to all interested parties at the time of any prospective final sale, Mitchell was given less than a day notice to remove from the FCO anything that he considered of value. What could be scavenged by one person after the end of a workday was, of course, meager. (This same product of the humane legal system gave the Mitchells the remaining fraction of the month of January to vacate the residence despite a Pennsylvania law requiring landlords to provide 90 days notice during winter months.) The facility sat vacant for months and in June, 2006 Galatola entered the building to look for a telescope log which he wanted to inspect. The logs weren't there because I was using them for documentation of observations in a prospective paper but he did discover ample evidence of vandalism with vestiges of fires in the work room, destruction of almost all the computers, some mechanical and optical damage to the PBPHOT, and theft of the 6-in finder of the reflector and of the photographic corrector to the siderostat objective. The dome slit had been left open for some unknowable length of time. A call to the Harvey residence elicited the fact that they were unaware of any intrusion and had actually taken no measures to secure the place – an impressive dereliction for someone in the insurance business. The township police found no suspects but concluded that the vandalism couldn't all have been done at one time. I called the few pawn shops in southeastern PA and eastern MD thinking that the vandals had disposed on the finder through such a contact but turned up nothing. Holenstein continues to hope that the same object would turn up on eBay. Parts of the dome and interior are shown in Fig. 61a and b.

Fig. 61a, b. Some free-form and some presumably political graffiti disguise the fact that the finder had gone missing from its bracket mounts. In the opposite direction of the workroom the floor was littered with color and interference filter stock and machined and polished filters.

Not all hardware derelictions can be counted as vandalism. Sometime between 1950 and the end of 1953 and for no reason known now, Cook's Seth-Thomas clock and an original drawing of the interior of the

RHO by Russell W. Porter were dropped from inventory. By the 1990s, the whereabouts of the Hewitt refractor and the FO filar micrometer and wedge photometer were unknown. #Blitzstein believed that he had loaned the latter to someone at the FI but he never pursued the matter.# The ancient sextant was misguidedly given away as well. Presumably there are still other equipment items which can no longer be traced.

The Harveys now wanted to get rid of all responsibility for the derelict station and contact was made with people associated with The Antique Telescope Society. An agreement was struck for the Society people to remove everything that they wished from the property and this was accomplished in September, 2007. Apart from the reflector and siderostat, items that had survived the vandalism and were removed are not known to me in detail but, when the demolition company was partway through its task (shown in Fig. 62), I

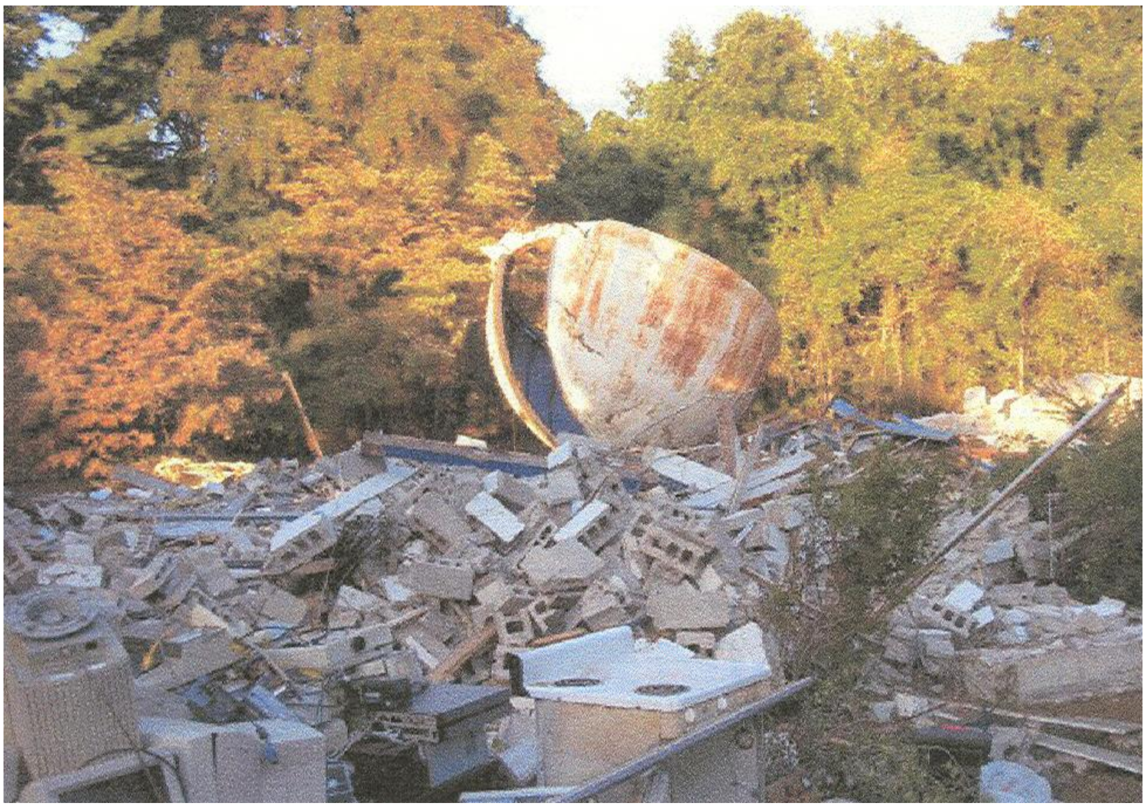


Fig. 62. What goes up can come down. The contractor told me that he would make $5,000 from the copper that he had pulled out of the ground and the buildings.

found only a power supply. One may presume that the Ross/Fecker camera, Cook's transit, the grating spectrograph, and a certain amount of electronic and computer hardware were all salvaged by the ATS people. #On February 5, 2008 I wrote to Philip Harvey asking if he wished to offer perhaps 150 words about his ownership of the facility but there was no response to my note.#

The reflector is and always was a commonplace device but the siderostat is the only machine of its design which ever achieved a record of observational productivity. It, therefore, has some historical significance and is potentially to be re-erected and used at Florida Community College in Jacksonville.

The SO was still used for infrequent public demonstrations but its training program stopped when the Cook transit and the Ross/Fecker camera were dismantled. As go-to telescopes and computed-controlled instrumentation became more common, the observational training mission also became less and less significant. In mid-2007, the spectrohelioscope was given to a local amateur who plans to put it back in service on his own property.

A SCARY NIGHT AND AN OUT-OF-THE-ORDINARY WORKDAY

After the foregoing text about the FCO and the SO was written, it appeared boring with no colorful personalities. There really weren't any such people. It seemed a shame, therefore, not to recollect two unique events which have lingered longer in the mind than almost all of the matters that have been described above. No research was necessary to write the following.

Throughout the existence of the SO, one or two nights per week were routinely open to the University family and the public to see what might be picked up with the 8-in refractor. These were typically supervised by grad students as part of their Teaching Assistant duties. One night a boy and girl walked in while Edward Sion was in charge of things. After a few minutes, the girl let it be known surreptitiously that she was frightened and not of the dark. With his wits and composure about him, Ed invited them to his office to see something of fascinating interest. He and the girl pushed inside slamming the door and he called the campus police. Of course, the possible perpetrator was never caught since he took off instantly, but it was a sobering recognition that the astronomers were actually agents inviting who-knew-whom into the DRL and that there was no control over these visitors outside the SO room itself. No visitor was ever checked in or out. This practice continued unchanged but fortunately without further incident.

#In the very early FCO days, staff, faculty and students would appear for the day with more or less enthusiasm according to the expectation of classes, committee meetings, arguments about errors of measurement, re-drafting of drawings, cloudiness of the previous night, and the like. At that time there was a clerk X. Perhaps in her early 20s, she had adequate typing skills and could find what she had filed away. For her salary, this was all that could be asked of her. Of course, she would make mistakes from time to time and, when these were pointed out for correction, her winsome smile would forgive the other party for the crass way by which this additional workload was laid on her. A particular day came when an arresting spectacle caught the eye – X beaming her morning welcome in a transparent blouse with no undergarment. One after the other, everyone strode instantly to his desk staying there as long as possible (for different reasons). If an errand required one to go past X's desk, eyes were to the front as if on military parade. On that day, no one asked her to do any task whatever and less work was probably done by everybody than had been intended. It was very quiet as the collection of a stolid machinist, straight-arrow engineers and workaday astronomers wondered what message X was giving us. Day's end came and everyone went home or observing and the next morning everything was as it had always been. Wood let it be known that he had admonished X most sternly on her duty as a representative of her exalted institutional employer and her obligations to herself The first assertion was unanimously disbelieved because he could never have done it with a straight face. The second claim was more credible but it was likely that he stood in the doorway about 10 feet away with his eyes on the ceiling as he stammered some platitudes. After some months, X married and departed. Since Blitzstein's death, I am the only remaining witness of this quiet drama.#

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SOME AFTERTHOUGHTS

Anyone looking back to the **PRIMER** will recognize that some specialties (such as the study of nuclear reactions) have been developed within my lifetime and others (telescope design and binary stars, for example) have a much longer lineage. For the latter, that means that we now face more searching questions about their subject matter and that their intrinsic appeal is greater than ever. For me, that has been the most agreeable conclusion of this tale. Science may mutate but it never really ends and there is an application of this adage right here: local ground-based observation may be finished but observational celestial science may confidently be expected to continue with new local people investigating new questions.

Why did I make this effort? The easy reason is that Nha asked me to do part of it and Shen suggested that I complete it. A better reason is that long ago I decided to control my career so as to have as much fun as grief. I was successful; I think I can say that I learned something every day and night of work. This story became fun during the first week of studying for it. I could, of course, have made it much longer but the level of scientific achievement that it describes makes me comfortable with the presentation as it is.

It must be accepted that what I have written is not a complete history of astronomy at the University. There appears in my text nothing about inter-departmental politics nor any recognition of the work of faculty theoreticians or experimentalists – Yousf Sobouti, Milton Merker, Pedro Saizar, Samuel Vila, Mikail Opendak and Paul Wiita. None had been appointed before 1964 so I knew them all but I don't feel comfortable interpreting their science. What I have done is recover something of the careers and lives of the observing scientists who were my predecessors and contemporaries seeing them as individual personalities. I might be accused of having a pollyannaish understanding of their characters but my experience is that people are typically not disagreeable and I am happy with my descriptions. I leave it to someone else to document and judge the careers of the observing students who were products of the last 35 years of the system. It's too bad that I could not find a complete record of years of birth and death and I also regret being unable to find photos of some people. Presumably, these can be recovered from more detailed searches of public records. In addition, it is possible that scrutiny of correspondence from the office of the Dean of Arts and Sciences would change some of my conclusions or even fill in some gaps. It would have been nice to have a complete lineage for Grew and I continue to be puzzled by Jacobs and his motivation. He remains the thread most loosely pulled.

In studying all the material that I collected, I learned many things about the assortment of people who appear in these words. My opinion is that a few of them really possessed both the spark and tenacity of the gifted scientist – Rittenhouse, Charles and Blitzstein and also Mohler, although he was never paid by the University. In a more comprehending world, Rittenhouse and Blitzstein would have been salaried just for their inventiveness and not dunned for anything routine such as serving as Provost or teaching classes or working on committees. Charles seems the most complete academic scientist for he created an observatory and academic department, worked tirelessly at nighttime observing in a self-checking technique of his own creation that was ahead of its time, and sustained a national presence as well. Rittenhouse and Mohler, on the other hand, were the most adaptable to changing times.

Let 1751 be the start of astronomical studies at the University. On behalf of observational astronomical science then, there were three and only three substantial private bequests approximately 125, 150 and 190 years after this beginning. One of these was real money and the other two equipment in kind without any supporting funds. It might be imagined that Philadelphia-area philanthropists could have been more forthcoming but there is ample evidence that the local affluent classes put their money where it would likely have a practical and possibly humanitarian effect. Cook's contribution is all that might have been hoped for from amateur astronomers locally. Barton, Olivier, Wood, Binnendijk and I all had ample grants and contracts but others were less fortunate.

Parts of the fragmentary and somewhat disordered notes by Barton and Blitzstein have been essential for my rendering. These two men made successful efforts to collect and preserve information pertaining to the observatories' pasts. I have not deliberately used their language at any point but I believe that I have preserved much of the sense of their writing and that of other witnesses very closely. It is not evident that

Barton was ever going to do anything with his notes. Blitzstein, however, definitely hoped to compose a history of all three observatories and the academic department up to 1995 and his interpretations might have been more severe than mine on some topics. I have removed nothing, no matter in how many redundant copies, from the materials that I have inherited. These materials are actually not very abundant, at most 5 shelf-feet, plus another 5 shelf feet of technical manuals and commercial catalogues. All documents that I have added show my initials and are dated. The contents of the interpretive sections contain my own opinions and these sometimes do and sometimes don't agree with Blitzstein's conclusions.

NAME INDEX

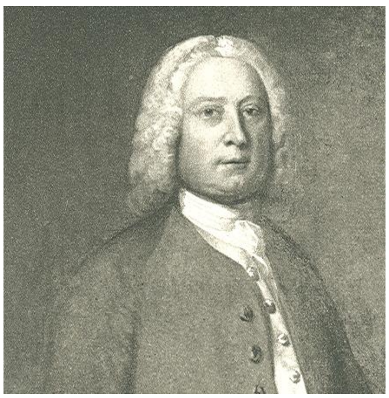
Names in literature citations do not appear in the following list. The many names assembled on p. 136 do not appear individually in this Index. Although only a nickname or a title or a pronoun or a proper adjective may be what appears on a given page, that page number is associated here with the correct given name of the individual.

SUBJECT INDEX

Because this text is unlikely ever to be used as a reference source in which some future worker would have to depend on an exhaustive subject listing, I have made this Index as cryptic as I could contrive. Only very broad omnibus headings appear and these have been interpreted generously.

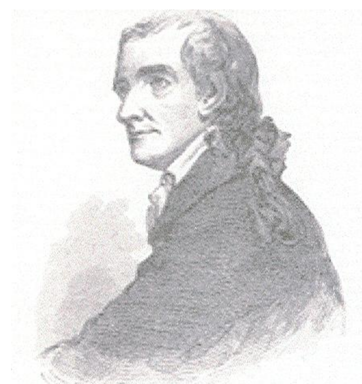
Quiz to accompany <u>Observational Astronomy at the University of Pennsylvania 1751 – 2007</u> by Robert H. Koch

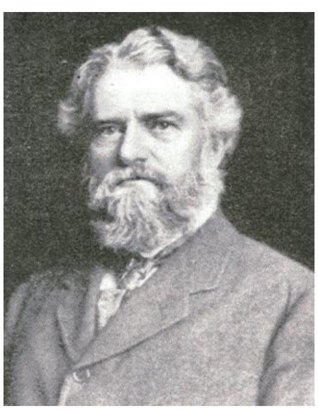
Some men mentioned in the main narrative set in train events of significance for that history but they were not themselves important enough to merit a picture or portrait in it. A selection of these people appears below with précis of why I cite them now. For convenience, the images are mostly copied from *Wikipedia* but the text contents are from print sources or my own knowledge. I've made a game of the identifications.



When my father died, I (1702-1775) was 17 years old and went to London to work as a mercer's assistant. Although raised a Quaker and Friend, my marriage late in life and some political experiences convinced me to adhere to the Established Church. During my time as Proprietor of the Pennsylvania colony, there was annoying friction with the Assembly and the Quaker merchants of Philadelphia who wanted the French and the Indian tribes to be ejected from the western lands that they occupied but didn't want to pay for that effort and didn't want to serve in a militia. When an Anglican divine asked for assistance to buy some astronomical hardware, I was happy to accommodate him. My father and possibly even my one grandfather are better known than I.

You might imagine that my faraway stare is that of an artsy person and I (1737-1791) will say that I have had more than a few consequential attainments in the fine arts. Some of these are not only remembered but even performed in the modern day. I was also an able public servant repeatedly and this led to some grief. I became the first student enrolled at the College of Philadelphia and profited by that opportunity. An open-air effect that I observed accidentally one evening in my neighborhood and communicated to a man much smarter than I led to the first fabrication of an important scientific instrument and to its understanding. Can you guess my identity?





Nova Scotia was where I (1835-1909) was born but I made my scientific career in the United States. One could say that for a few decades I was publicly the most visible astronomer in North America. My word and presence counted for something also in governmental scientific affairs and I had the pleasure of being asked to be the guest speaker at dedications of new establishments. You might think it tedious to have to compose a new talk for each such occasion but I enjoyed it and considered it an opportunity to present a compelling and accurate representation of astronomical science and its future to people who would otherwise not be exposed to such an important matter. About the Solar System I knew almost everything that was known in my time. Most probably, you have heard of me.

My life (1846-1912) fell in an interval when North American astronomy and physical science in general were trying to establish their identity and validity. Very few career opportunities existed and most of these inevitably were filled by men whose families were comfortable enough to send them to a college or university. I was one of those fortunate individuals – a Dartmouth grad – but I actually started to make my name with a multi-year international surveying project. This experience led naturally into fundamental astronomy and I might be considered the inadvertent godfather of geodetic studies at the University. My son, Benjamin, who lived to an immense age, had a considerable career of his own and composed a very nice bio of me. Is it possible that I, the second editor of the *AJ*, am still remembered?

I (1874-1937) was an international personality, perhaps the best known of all my countrymen for most of my life until an arriviste journalist became a politician. I was also rich because of my drive to pursue the applications of my inventions and because still richer people understood that they could profit by supporting me. Lots of honors came my way and eventually I was even ennobled. I may or may not have increased the understanding of Gustavo Cook on matters of transmission and reception of wireless messaging. You have undoubtedly used modernized versions of some of my inventions many times.

For about 3 decades I (1877-1957) was the preeminent astrophysicist in North America and maybe even in the world. I was also a relentless publicist for all of astronomy because, the more the layman and amateur knew of the science, the more the pursuit of knowledge would be supported. I don't mean to suggest that I knew everything, and some routine duties – such as making sure that my colleagues were paid on time – were not always my highest priority. My son-in-law Frank also had an estimable career, not only in academic life but also as a long-serving officer of the AAS. He wore a red tie while giving his annual report in order to signify that the Society budget had again run a deficit. Ed, a son-in-law of Frank, became a fine stellar spectroscopist and photometrist and has now somewhat retired. Every undergrad student of astronomy knows my name and some of my work.

In order of appearance, the names of the men are disguised here by a simple and unique substitution cipher: Nqdacl Fkbb, Mjcbgsl Qdfwsbldb, Lsadb Bktgdae, Ykpsl Edll, Opoyskyad Acjgdbs, and Qkbjx Bdjjsl Jpllkyy.

Addenda and Errata
to
Observational Astronomy at the University of Pennsylvania
1751 – 2007

By Robert H. Koch

2009

People

A certain amount of mostly negative, new information has been uncovered that possibly helps to constrain the background of Theophilus Grew.

Except for the last year, all the following dates are Old Style. On March 3, 1671, there was christened in the Walloon or Strangers Church, Canterbury, Kent, England Theophile le Grou, the son of Ignace le Grou and Susanne Bodele. Nothing more is known of these parents at present other than that the father's surname would have been pronounced (at least by the English) the same as Grew is in English. If either they or their son anglicized the surname for any reason, it would have been easy to turn it into Grew. Another record shows that Theophelus Growe married Rebecca Rogers on December 26, 1696 at Saint Dunstan's in the East, Stepney, London, England. The time interval between 1671 and 1696 permits the baby and the groom to be the same person. On October 18, 1711 Theophilus Grew married Elizabeth Barrine in the same London church. Almost certainly, Rebecca had died and the now Theophilus was marrying again. (In other records, the bride's surname is Barron illustrating how labile spelling was in those days.) On February 9, 1735 Theophilus Grew married Elizabeth Cosins in Christ Church and Saint Peter's, Philadelphia, PA. This Theophilus is the first mathematics professor at Franklin's College of Philadelphia and he had been in town since at least 1729. The same Theophilus married Frances Bowen in the same Philadelphia church on March 5, 1739. It would seem that these incidents cannot all refer to one man since he would have been 64 when he married Elizabeth and 68 when he married Frances and 88 when he died in 1759. This set of intervals could be consistent with the Philadelphia Theophilus being the son or grandson of the French/English baby. That remains a speculation at present.

Lifelines

Life years, previously unknown, have been found for the following people:

R. Stanley Alexander (1909-2004) P. G. Crout (1898-1977) Alan E. Gee (1916-1991) Edith D. Kast (1880-1967) Sam Seeleman (1914-1995) A. M. Skellett (1901-1991) Paul S. Watson (1905-1986) Joseph L. Woods (1890-1963)

2010

People

From 1696 through 1783 the current St. John's College in Annapolis, MD functioned as a prep school, King William's School. The surviving records of this institution give no indication of Theophilus Grew as a matriculated student. Similarly, he does not appear in the records of Marischal and King's Colleges of Aberdeen, Scotland, which eventually amalgamated to become the University of Aberdeen. Of the European and North American English-language institutions of learning open around the turn into the 18th century, these are the last ones to be checked. One must begin to consider that Grew could be an autodidact in celestial phenomena and time matters.

The American Genealogical-Biographical Index **67**, 509 says that Grew was born in 1710 and on the same page that he was born in 1720. The earlier year is suspect because two women with the Grew surname are given the same birth year. The later year is not consistent with his marriage to Elizabeth Cozins at Christ Church, Philadelphia in 1735. Along with Rev. George Thorald and Raphael Neale he was a witness to the will of William Boarman drawn up on 02/26/1728(Julian)1729(Gregorian) in St. Mary's Co. of the Maryland Colony. This association might be taken to imply that he came to PA from that location or that his family had been established there. Neither possibility can be verified and it is certain that no will with the Grew surname was filed in MD between 1634 and 1759. From another source he is recorded as arriving in Philadelphia in 1742. This year is also likely too late for he was already a widower once and had married Frances Bowen in the same church in 1739. The only way to salvage 1742 is to imagine that the couple moved out of Philadelphia after their marriage. The only inferences that can be drawn from the small amount of certain information is that Grew was an Anglican and neither a Catholic nor a Puritan immigrant to MD or PA. His origin remains unknown.

The online Minutes of the Trustees prepared by the University Archives contain the following entry on page 109 pertaining to the meeting of the Trustees on Tuesday the 13th November 1759:
"Then the Trustees returned to the Apparatus Room and considered a petition presented by the Widow of the late Professor Grew deceased setting forth that she was very poor and had a large family and prayed their Charitable assistance. It was agreed that as M^{r} Grew dyed in the middle of a quarter the full quarters Wage should be paid to her agreeable to what had usually been done on the like occasion and that over and above this she should receive a present of Twenty pounds, and the Treasurer was ordered to settle with her accordingly."

The Chairman and the 13 Trustees present passed the motion. Grew was therefore still a faculty member when he died 08/30/1759 during the August/September break of that year. From 1750 through 1759 Trustee Minutes refer to teachers who were sick and temporarily unable to perform for at least a month. Grew is not among them so his final illness must have lasted during the last part of the year's first term that ran from 05/15/1759 to 08/15/1759.

According to William Playfair's *The Noble Families of England*, William, Duke of Normandy, was attended by a baron Hugh in1066. Because of his handsome countenance and his vigor in battle, he was Hugh la Fleur – something like Fanfan la Tulipe of that eponymous movie. Over the centuries this became anglicized to the Flower surname. There seem to be almost no politically or militarily prominent

Flowers in their home counties of Wiltshire, Gloucestershire and Somerset, and it might be that the families were consistently in trade. If interest is restricted to "recent" times, it is not difficult to find Flowers older than those in the main narrative, as is indicated by the partial pedigree on the next line:
Charles (~1652-?) → John (~1678-1738) → Richard (1724-1762) → Richard (1759-1843).
The older Richard had married Hannah Grubb (1728-1810) in 1746 and they had several children: Mary (~1748-?), Rachel (~1751-1822), John (1750-1825), Jemmie Edwards (1752-?) and the younger Richard. All of these generations lived in the vicinity of Chester, PA, which had previously been called Upland and which name still survives slightly to the northwest of the present Chester. Hannah's one grandfather, Emanuel Grubb (1678-1767), was born in a cave on the banks of the Delaware River and is said to have been the first English baby in the colony. It is possible to trace the Grubb line to Emanuel's father John (1652-1707), who was born in Cornwall and brought his wife (Frances Vane (1660-1712)) and family to the New World, and even earlier to John Grubb's father, Henry, who had married a woman with the last name of Wilmot and died in England. The older John Flower may or may not have been born in North America but the likelihood is that Charles Flower had been born in England. The very first settlers along the Delaware River had been Swedes who established several settlements. Civil authority passed to the Dutch and then back to the Swedes before England was able to claim the territory definitively. The earliest Flower in North America may have been motivated to emigrate because the English Civil Wars ended in defeat for the Royalist cause or because his own grass was no longer so green.

The youngest and long-lived Richard was apparently the first Flower to move some distance from the close Chester environs. Living in Ridley about 8 miles NNW of Upland, in1785 he bought a half-interest in the nearby and substantial grist mill *Lapidea* (originally *Phipps's* and sometime *Lapidia*) and its tract along Crum Creek. Richard employed a miller to do the actual work at least part of the time. Then, in 1793 he sold his half-share in the property and business to John Wall, Sr. (~1710-1771), a Philadelphia merchant who became the step-grandfather of Reese Wall Flower, the UP benefactor. John Wall, Sr. and his wife, the widow of the elder Richard, eventually sold their interest in the property for $8,500[$116,000] – an almost unbelievable sum. This may be an indication that John Wall, Sr. was very well-to-do.

This same younger Richard Flower married Henrietta Graham (1768-1841) and they had the following children: Jeremiah E., Zedekiah Wyatt, William Graham, Mary Ann (Hubbell), Reese Wall (hereafter called RWF in this and later paragraphs), Jemima Edwards (Flickwire and Hickson) and Henrietta G. (Ashmead). All these people are familiar from assorted legal documents including the trying of RWF's will. In sum, his lineage is known through 4 and 6 previous generations on his paternal and maternal grandfathers' sides, respectively.

Some inferences about RWF's existence can be pulled together from census records. For instance, when he was 53 in 1860, in his Upper Darby home there also lived Ellen Jones(25, his Housekeeper), Emma Jones(8), Anne Kelly(18), Daniel McClaren(60) and Patrick Morris(28). Ten years later the record (which misspells his given name) shows Ellen Janer(36 now and still his Housekeeper), Emma Janer(18) and Charles Williams(23) at the same residence. In both records he identified himself as a farmer. The two females are the same people and it may have been that Ellen had married a Mr. Janer in the 10-year interval and he was no longer in the picture. It may be noticed that Reese Wall Flower Jr. was not living with his father at either of these times and Ellen Jones was certainly not the son's mother.

The acrimony at the time of settling RWF's estate is inconsistent with the harmonious past history of these families. For instance, the younger Richard and Henrietta lived with stepfather John Wall, Sr. at his Leiper's Mill residence for some time. Later, the younger Richard, his brother John, their half-brother Reese Wall (~1765-<1800) (a son of John Wall, Sr. by Hannah Grubb Flower, his second wife) and a brother-in-law Capt. John McKeever were all amicable 1790s partners in exporting grain and milled products to Europe before losing 3 vessels to seizure by French warships and having their cargoes condemned as war prizes in La Rochelle. (In the 1870s their descendants were still futilely trying to get compensation from the Third Republic for these losses.)

My reconstruction of the family anger with RWF begins with a tragedy: Reese Wall, the child of John Wall, Sr. and Hannah Grubb Flower Wall, drowned in Delaware Bay shortly before 1800. On their mother's behalf, the children of Hannah must have felt this keenly for the boys born to two of her children at the beginning of the new century were named Reese (McKeever) and RWF, memorializing the dead youth. RWF's sisters later complained that John Wall favored their brother over his siblings and this may well have been true two centuries ago when personalized associations were considered important. This John Wall, however, was not the step-grandfather who had died before RWF was born. It was rather his first son by his first marriage in 1740 to Phebe Buffington (1714-?). The second son of this marriage was confusingly Reece Wall (1745-?). So seemingly John Wall, Jr. (1741-1816) showed some favoritism to his step nephew if RWF's sisters are to be believed. A second element in the family friction appears in the summary of the will of John Flower (1753-1825), the only paternal uncle of RWF, who died childless October 14, 1825. The two executors were the brothers William Graham Flower (1794-1865) and RWF and not their father Richard who was very much alive in that year. RWF was only 18 at the time. John Flower's estate included 8 individual properties – not an insignificant estate. When later his own mother and father died, RWF was the executor of those wills too – leading to complaints that he had given short shrift to his sisters. When one remembers that an executor is entitled to a significant minor percentage of the assets of an estate, it is clear that RWF need never have lacked for money even if he hadn't been successful as a lumber merchant and broker. My belief is that the challenge to the will was founded in the sisters' belief that RWF had been too much of a favorite of the previous generation and that he was ungenerous in sharing wealth and possessions that should have been widely distributed among family members. The sisters would, of course, have read *A Christmas Carol* and could have decided that their brother was an incarnation of Ebenezer Scrooge.

It could be imagined that RWF acquired the FO property itself as a result of his share in the real property portion of the estates of his uncle or father. This is not so. The Sheriff's Deed Book of Delaware County shows that he picked up 86 acres of it on November 29, 1831 at a sheriff's sale, and he bought 14.5 more acres from George P. Snyder and his wife on October 5, 1850.

It has always been amusing to confound the name of Reese Wall Flower with the common noun of the same pronunciation indicating a very shy or socially inept person. This association was partly entwined with the uncommon name Reese simply because previously there had been no Flower known with that given name. This present text shows the provenance of the name to have arisen from the Wall relations by marriage and now one can also see that it had still further use. In 2004 Bruce Holenstein found the will of Reese Wall Flower, deceased about 1:45 PM on Thursday July 9, 1891 so there were actually 3 people of the identical, uncommon name alive at the same time in the 1870s in a small area of PA. This youngest man described himself as an edge tool maker from Ashbourne Village in Cheltenham Township and there was a factory making such tools in that locale at that time. The will names his wife (Helen), daughter (Netty) and brother (Richard), and the inventory of his effects is attached to it. He was worth about $119 [$2,300] in material possessions and had some property as well. He was a grandson of Zedekiah Flower and a grand nephew of the subject of this essay. It is not impossible that there is at least one Reese Wall Flower alive today but he is not to be found in southeastern PA. Notes were sent to the 26 Flowers in this area who have listed land phone line numbers to see if any of them had knowledge of 19th century ancestors. The few answers led to no new information.

None of the relationships described above leads one to imagine that RWF learned any astronomical knowledge from a family member and he is not known to have had significant higher education. Either he set up his will to spite family members or he composed it on the basis of some self-gained knowledge. I offer a speculation to show that the latter is not unthinkable. When the man was young, the eastern U.S. was treated to the hitherto unknown spectacle of the Leonid meteor shower of 1833. He lived long enough to know of and possibly witness the somewhat diminished display of November 13, 1866 and to be aware of the association of the shower with Comet Temple-Tuttle. Such spectacular naked-eye events may have impressed him sufficiently to cause him to think that he could advance scientific knowledge with a proper distribution of his assets.

Some mostly incidental (*i.e.*, non-astronomical) information has been uncovered pertaining to other personalities in the main file.

The career of Reese Wall Flower, Jr. has been filled out a bit. At the 1880 census he and his wife Annie E. had 2 daughters (Marion E. and Edith) and a son (Harry E.). The children were minors at the time. His parents are not named but their nationalities are given as U.S. for the father and English for the mother. So RWF, the bachelor benefactor of the FO, had possibly exploited his English servant. As it happened, at the time of the 1880 tally the Flower, Jr. household also included live-in female servants – a Philadelphia girl and a Scottish one.

Dr. Charles A. Young came from an academic family and after 9 years of service at Western Reserve College (including guard duty during the Civil War) was appointed astronomer at Dartmouth College without an advanced degree. Within 4 years he had become an expert on solar phenomena because of his reports of direct imaging and spectroscopy of chromospheric and coronal features. His several books carry his academic honors of LLD and PhD. The former was awarded by Princeton College on his retirement and the latter *honoris causa* by UP at the 1870 commencement. By that year, he was recognized as a solar astrophysicist but was hardly the leading observational astronomer of the country. The possibility of the UP degree was placed before the Trustees at their meeting of December 7, 1869 and was referred to the Faculty of Arts for an opinion. A favorable opinion came back at the January 4, 1870 meeting. At the previous meeting, there had also been considered the possibility of establishing a PhD degree and this too was confirmed subsequently. In fact, then, Young's is the first such degree awarded by the school and therefore G. W. Cook is not the first essentially self-educated person to receive such an award. Perhaps E. Otis Kendall lobbied for Young but the language in the minutes suggests that the resolution was framed by a Trustee rather than by a faculty member. Finally, it was decided at the March 1, 1870 meeting to award a DD to Rev. Beale Melancthon Schmucker (1827-1888) at the ceremony when Young would receive his award. This man was a pastoral clergyman, a scholar of the development of Evangelical Lutheranism in North America and Pennsylvania in particular, and a fervent supporter of recovering and putting into current practice the 17th century Lutheran liturgies, presumably in German. Who knows what prompted these awards at a school with very bounded astronomical competence and with no obvious reason to recognize Lutheran activity.

Dr. Joseph Wharton's title is an honorary ScD from 1902 given by UP. He was a successful metallurgical and chemical industrialist who made his first fortune by smelting zinc and nickel ores and refining the metals to unprecedented purities. He also made his mark in academic and research matters. For instance, he endowed the Wharton School of Finance and Economy (later renamed more than once) and was one of the founders of Swarthmore College. Exactly how he came to fund Doolittle's Reflex Zenith Tube is not known now but perhaps he was intrigued by an unconventional use of a liquid metal.

G. W. Cook's given names are uncommon but have an easy understanding. A bachelor uncle, Gustavus Benson Cook, died less than two years before his birth. The family is in the line of Dr. Thomas Wynne (1627-1691), Welsh physician to William Penn, an original colonist having arrived on the ship *Welcome*, and the first Speaker of the Pennsylvania Assembly. The RHO was just inside a segment of the southeastern perimeter of Wynnewood, the suburban village named for Dr. Wynne.

Dr. Edgar N. Fought (1878-1944) and Dr. Charles L. Mitchell (~1855-?) took numerous panoramic and detailed inside photos of the RHO. They were friends of G. W. Cook, perhaps because he shared their interests in photography. These men had taken their medical degrees at the Thomas Jefferson Medical School, Fought in 1905 and Mitchell in 1880. In their own rights, they had significant careers. Fought was apparently the official photographer for Jeff as well as an accomplished performing organist. Mitchell was a published force in art photography around the turn of the century in the Philadelphia area. He was dead set against any kind of impressionistic darkroom or printing technique that shaded or smeared a line or edge that was sharp in nature.

With the assistance of Nancy Miller, UP Archivist, it has been determined that the following were never matriculated UP students: Robert D. Armstrong, Ralph B. Baldwin, R. A. Binkley, Thomas Finletter, John B. Gest, A. E. Hayes, Harry S. Jacobs, Ross P. Marsteller, Isaac Ray, J. S. Stevenson, James Thompson, Nancy Weber, E. E. Whelder and Doris M. Wills. A few of these are surprising: Ray because one thinks that all physicians practicing in Philadelphia in the 19th century had been students of the UP Medical School and Thompson because Blitzstein had been heard to say that this man was an unreliable student and part-time worker from the EE School. Pauline W. Jacobs could not be checked because her maiden name is unknown.

Martin E. Nason, awarded an MA in 1951, was a BS graduate of Washburn University in Topeka, KS in 1949. This is the institution where R. Stanley Alexander spent his entire academic career. It may have been he who pointed Nason to UP. Nason's middle name was Elinor, perhaps his mother's maiden name but surely a source of anxiety for a growing boy. He appears in the Shawnee County, KS list of World War II army enlistees in 1942 and it is not impossible that he profited from the GI Bill of Rights to finance his education after the war.

William Blitzstein's given name at home was Velvel-a not uncommon Yiddish name meaning "wolf". "William" itself stands for "protector of the kingdom" or something like that. Blitzstein chose it or it was chosen for him by the family after he had been in school a brief time. He believed that his mother was a Ukrainian wetback but his details of her entry into the U.S. changed from time to time. His father, supposedly from the same village that was razed to the ground during World War II, was said to be a "parlor pink". The star of the family, as it were, was a paternal aunt, a woman so far left and so out of control that the Philadelphia branch of the Communist Party barred her from their meetings. Bill's political opinions were those of an observant Democrat but were mostly unemotional. On the other hand, his attitudes toward musical performances and compositions were more fervent. Vladimir Horowitz (1903-1989), for instance, was disdained for his recital showmanship and hand technique and for his manipulations of the works of older composers.

After Benjamin S. P. Shen's French *lycée* training in Shanghai, he passed on to his Paris degree under Pierre V. Auger (1899-1993) in cosmic-ray physics. He was the 6th Flower Professor and departmental chairman from 1973-1979. *[B. D. Holenstein 2010 note: Before his passing, RHK authorized the updating of Shen's biography in the main narrative text. The updated text and additions are indicated in blue.]*

Scientific matters

The UP library contains 29 scientific publications of Theophilus Grew in a total of 68 copies. Publication dates span the interval from 1735 into 1766. He was, therefore, a known scientist before being appointed to the staff of the College of Philadelphia. One publication, *Description and Use of the Globes, celestial and terrestrial* (with still more verbiage in the title) dates from 1753 and is basically a teaching tool. All the rest of the published works are almanacs and are specific for a variety of stations: Barbados, New York, Philadelphia and Virginia. The last two locations are the most numerous and refer to individual years. The publications nominally for Philadelphia are entitled with the latitude and time difference from Greenwich. A supplementary remark "but may without sensible error serve all adjacent places, even from Newfoundland to South-Carolina" shows the real limitations of the calculations. Sometimes the remark is limited to "all the northern colonies". In a way, Grew's career resembles that of E. Otis Kendall who contributed more than 25 years of entries in *The American Ephemeris* for the ephemerides of Jupiter and Neptune and their satellites. Grew died in 1759 so some of his almanacs are posthumous. He was able to calculate the entries ahead of time because essentially no new solar system objects – plants or satellites – were discovered during his lifetime. His was a completely stable universe.

After the failure to mount the 18-in Brashear/Flower refractor in New Zealand for more than 40 years, there is currently a new initiative to install it in a more generalized public astronomical facility near Lake Tekapo at the foot of Mt. John.

Sometime after 2000 UP decided to give the Cook spectroheliosclope system to Matt Considine, a Bucks County amateur astronomer. It was removed from the DRL Students' Observatory and was to be installed on Considine's home property but this never happened. Instead, Considine has now given the hardware to the Springfield Telescope Makers of Stellafane, VT to be installed on the famous property which is the site of the annual ATM conventions. The condition of this donation is that the system be restored, put back into service and be available for visitors to use. In 2010 the 8-in refractor originally from the RHO also passed into private hands by a donation from UP: Bart Fried intends to have it re-erected in the Vanderbilt Museum and Planetarium in Suffolk Co., NY. He and Considine have a testable hypothesis of the origin of this instrument. The FO visual photometer has also passed into Fried's hands for possible rehabbing.

A troubling scientific incident emerged in very early 2010 from some correspondence regarding the lunar occultations of stars observed with the 38-cm siderostat and the Pierce-Blitzstein photometer. The FCO station coordinates are given in the 1964 *AENA* as:

N39 59.95, W75 28.6 (509 feet)

and are footnoted to say that the coordinates refer to the "equatorial reflector". From personal recollection, I know that the station position was measured in 1955 by Blitzstein (then not a full-time faculty member), Frank Bradshaw Wood (then Observatory Director) and Leendert Binnendijk (a UP faculty member) working together, and a surviving handwritten sheet actually shows some of their calculations. Each of these men was well-trained in fundamental astronomy and would have had no doubts about how to lay off a meridian and how to determine latitude although they had never done so.

While searching the old files, I discovered other information. In April, 1976 P. Kenneth Seidelmann, Director of the Nautical Almanac Office at the USNO, had requested that observatories update their entries in the *AENA*, which updating would first appear publicly in the 1981 issue of the *AENA*. This request was apparently unanswered on behalf of the FCO for a follow-up letter from Seidelmann is dated January, 1977. Amid the station property drawings I found a form response to the second request in Blitzstein's (by then Associate Observatory Director) hand dated 6/27/1977. The reflector's coordinates are given thereon as:

N39 59 57, W75 29 37 (509 feet) = **N39 59.95, W75 29.61(509 feet)**.

An asterisk attached to the longitude value leads to a note at the bottom of the page: "PREVIOUS TABULATIONS OF THE LONGITUDE IN THE A.E. + N.A. WERE IN ERROR BY ABOUT 1 MINUTE OF ARC". Other notations on the form say that the coordinates are geodetic (datum 1927 N. American) and that their source is the U.S. Geological Survey, scale 1:24000 of 1966. The *AENA* entries did change in 1981. It has to be accepted, therefore, that the published station coordinates were in error from 1955 through 1980 and that they were corrected not by new observations but by map scaling and interpolation. There are also preserved some calculations in Blitzstein's hand bearing dates in 1991 and 1992 in which he was looking at the coordinates again and these suggest that some of this scrutiny was also done by Howard Poss, a Physics Professor of Temple University who was interested in determining a diameter of Antares from lunar occultations observed with the reflector at the FCO.

There seem to be 3 possible explanations for the original positional error.

(1) There was an unacknowledged clerical error of 1' made in 1955 and discovered in 1977.

(2) The second interpretation is based on pencil calculations in Wood's hand that can be interpreted to mean that the station coordinates were determined by temporarily setting up the 2.5-in Cook broken-transit in the reflector's dome and observing Polaris with it. This is not explicitly stated but there are two notes about reversing and not reversing some instrument. This broken-transit eventually was set up for many years in the Students Observatory on the UP campus as an instructional instrument. It would have been perfectly serviceable for the task if it were properly mounted and aligned but perhaps this wasn't done. There is no indication that observations were made of numerous stars over a large range of declination as would have been necessary to evaluate all the instrumental errors. It is also not impossible that an error of about 4 s in setting a sidereal clock was the fundamental fault. A variant of this possibility would speculate that the instrument used in the reflector's dome was the Cook meridian circle but this instrument was more cumbersome to use than the broken-transit and seems a less likely possibility.

(3) The last interpretation of the 1955 error that can be suggested now is that the erroneous FCO position was determined with the coordinate circles on the Fecker reflector. Although fairly evenly engraved,

these circles were coarse (broad tickmarks every 2.5 minutes in hour angle and every 5° in declination) and it is possible that "large" errors made when reading them would not have been noticed by the observers. Because the collimation of the reflector was always difficult to preserve and because no optical reticle other than unilluminated crosshairs was ever available, it is certain that the three observers could have made a systematic error of about 4 s and been unaware of it until Blitzstein did his map interpolation in 1977. A sidereal clock error could also be implicated in this possible explanation. In view of Wood's notes, this third possibility appears the least likely of the three.

In 2010 and with all the 1955 parties dead, it is impossible to know if any of these explanations is credible. Neither R. J. Mitchell nor I remembers internal observatory or departmental notice being given of the station coordinate change, but this must have been done because Mitchell was responsible for developing and maintaining the data acquisition and reduction codes and I was a frequent user of the photometric system.

During the FCO's existence, the major observational program was differential and "absolute" photometry. It must be true, therefore, that hour angles were systematically in error by about 4 s until 1977 and that there resulted systematic errors in calculated airmasses for all program and reference stars. The downstream errors in extinction corrections for either type of photometry are, however, exceedingly small in the visible and within the errors of the shot and scintillation noises that dominated the measures. Although coordinate errors surely remained until the station's end, they were likely smaller than those which have just been described.

This is not the end of the story, however, for the 1977 coordinates do not agree with those that can be determined from a *GoogleEarth* image. The tools available for analysis of such images permit one to determine geographical coordinates and elevation above sea level for any image. B. D. Holenstein, Mitchell and I did this with the help of the original FCO architect's blueprints that I have preserved. It was possible to locate each telescope accurately on the space image because the 3 people knew the dimensions of the dome and the room housing the instruments and the location of each within its shelter. The results are:
Reflector – N39 59 55.33, W75 29 35.78
Siderostat – N39 59 55.49, W75 29 36.73.
The separation of the two telescopes agrees within 0.05″ with the separation that can be calculated from the 1955 blueprints. It can be seen that neither of these agrees well with the 1977 redetermination, disagreement being obvious in longitude.

To try to resolve this bust, Holenstein used his Garmin GPSMAP 60CSx receiver to make 12 stationary repetitions of the coordinates at the top of the driveway into the FCO property. This location is the only ground feature still surviving on the razed and re-planted property and it is now impossible to enter the rest of the privately-held property. The internal precision of his means is close to ±0.06″ and ±0.3 feet. From the surviving property survey map it was possible to determine the linear separations between the telescopes and Holenstein's position and then to convert these differences into angular separations and finally into geographical coordinates, of course referred to the GPS position. These results are:
Reflector – N39 59 55.54, W75 29 35.80
Siderostat – N39 59 55.66, W75 29 36.72.
There still remains a bust for the longitude of the reflector compared to the 1977 value from Blitzstein's

map interpolation. Comparison of GPS results with the astronomical and map coordinates is problematical for a fundamental reason: the geocentric coordinate frames are not the same. GPS determinations are referred to the WGS (World Geodetic System)84 frame while Blitzstein's calculations refer explicitly to the 1926 NAD (North American Datum). The Royal Observatory Greenwich Learning Team has published more than one memo on the differences among coordinate frames for continents and sub-continental areas as well as their understanding of the reasons for the frame differences. For the present case, their latest memo asserts differences of -8m(-0.34″), +160m(+5.19″) and +176m(+577ft) for 1926NAD against the WGS84 system for longitude, latitude and altitude above sea level, respectively.

The isotope in the Serge Korff entry should be C^{14}.

Lifelines

For context or reference the names of many people appear in the main narrative but many of them were not considered central enough to it to warrant giving therein the years of their lives. I decided to change this somewhat uncharitable attitude in large part because the Name Index is incomplete. Many more than half of these missing names and dates have now been found and appear in the following summary; a fraction of them were contributed by other unnamed third parties. Misspellings and mistakes of names and initials have been corrected but aren't flagged. UP students whose degrees date from 1960 and later are typically not listed unless they appear in the narrative or have died. Because of modern privacy restrictions, institutional files that potentially contain many useful dates are not available. A few notes appear at the end of the listing.

George O. Abell (1927-1983) G. W. Airy (1801-1892) Henrietta G. Ashmead (1809-1879)[a] John N. Bahcall (1935-2005) William Barrie (1905-1986) A. Henri Becquerel (1852-1908) Hans Bethe (1906-2005) Harold H. Borders (1956-) Pierre Bouguer (1698-1758) Nathaniel Bowditch (1773-1838) James Bradley (1693-1762) S. W. Burnham (1838-1921) Charles the Bold (1433-1477) Seth Chandler (1846-1913) Charlie Chaplin (1889-1977) Kyong C. Chou (1929-2010) Alvan G. Clark (1832-1897) Gustavus Wynne Cook (1868-1940)[b] Lavinia Borden Cook (~1849-?) Mrs. G. W. (Nannie M. Bright) Cook (1877-?) Richard Y. Cook (1845-1917) Nicolaus Copernicus (1473-1543) Gen. Charles Cornwallis (1738-1805) George L. Crawford, Esq. (1832-1908) Paul Cret (1876-1945) Sybil Csigi (1950-) Rev. John T. Desaguiliers (1683-1744) Edward J. Devinney (1940-) Raymond S. Dugan (1878-1940) Noel A. Doughty (1939-2001) William H. DuBarry (1894-1958) Maurice Dubin (1926-) A. Felix DuPont (1879-1948) Albert Einstein (1879-1955) Kenneth Edgeworth (1880-1972) W. Lewis Elkin (1855-1933) C. T. Elvey (1899-1970) Isadore Epstein (1919-1995) The Hon. Thomas K. Finletter (1820-1907) Wilmot Fleming (1916-1978) John Flower (1817-?) Thomas B. Flower (>1811-?) Richard Flower (~1815-?) Zedekiah W. Flower (1788-1846) Dave Garroway (1913-1982) Carl Friedrich Gauss (1777-1855) John B. Gest, Esq. (1823-1907) Riccardo Giacconi (1931-) Alan C. Gilmore (1944-) Curvin V. Gingerich (1880-1951) John Goodricke (1764-1786) Abigail Graham (1780-~1845) Albert M. Greenfield (1887-1967) Edward F. Guinan (1942-) Hans Haffner (1912-1977) Edmund Halley (1656-1742) Carl Hammer (1914-2004) Oliver Hardy (1892-1957) Gaylord P. Harnwell (1903-1982) John Harrison (1693-1776) Leon W. Hartman (1876-?) John Hearnshaw (1948-) Robert Hee (1954-) Henry III (1207-1272) William Herschel (1738-1822) Ejnar Hertzsprung (1873-1967) G. W. Hill (1838-1914) Dorrit Hoffleit (1907-2007) Cuno Hoffmeister (1892-1968) Jeremiah Horrocks (1618-1641) Gen. William Howe (1729-1814) Thomas Jefferson (1743-1826) Harold L. Johnson (1921-1980) Kenneth Johnston (1941-) James E. Keeler (1857-

1900) James C. Kemp (1927-1988) Mabel L. Kent (1885-1970) Johannes Kepler (1571-1630) Pamela J. Kilmartin (1949-) Chun-Hwey Kim (1954-) Philander C. Knox (1853-1921) Ulrich Köhler (1939 -) Masatoshi Koshiba (1926-) Hans Krebs (1900-1981) G. P. Kuiper (1905-1973) F. Kustner (1856-1936) Lincoln LaPaz (1897-1985) Daile La (1958-1996) Stan Laurel (1890-1965) Adrien Marie Legendre (1752-1833) David Levy (1948-) Meriwether Lewis (1774-1809) Willard Libby (1908-1980) Gordon L. Locher (1904-1964) Percival Lowell (1855-1916) Carl A. R. Lundin (1880-1962) C. Roger Lynds (1928-) Bernard Lyot (1897-1952) Walter Marsteller (1898-1987) or (1914-1975)[c] Pierre de Maupertuis (1698-1759) Maximilian II (1527-1576) James Clerk Maxwell (1831-1879) Andrew McKellar (1910-1960) Robert R. McMath (1891-1962) David D. Meisel (1940-) Milton Merker (1941-) A. A. Michelson (1852-1931) M. Minnaert (1893-1970) David Mkrtichian (1956-) E. W. Morley (1838-1923) Michael J. Mumma (1941-) Homer Newell (1915-1983) Isaac Newton (1643-1727) Il-Seong Nha (1932-) Sir Theophilus Oglethorpe (1650-1702) Michael Opendak (1953-) Donald Osterbrock (1924-2007) Empress Farah Pahlavi (1938-) Mohammed Reza Pahlavi (1919-1980) Maxfield Parrish (1870-1966) Thomas Penn (1702-1775) William Penn (1644-1718) Edison Pettit (1889-1962) Alan F. Petty (1926-2010) E. C. Pickering (1846-1919) Plutarch (~46-120) Richard W. Pogge (1961-) Daniel M. Popper (1913-1999) Russell W. Porter (1871-1949) Ezra Pound (1885-1972) Mary Proctor (1862-1957) Alfred W. Putnam (1895-1971) Richard C. Putnam (1926-2002) Dr. Isaac Ray (1807-1881) George E. Reahm (1921-2001) Elizabeth Rhoads (1797-1881) Franklin Roach (1905-1993) Hal Roach (1892-1992) Ernest Robson (1902-1988) Judith Rodin (1944-) Wilhelm Röentgen (1845-1923) Benjamin Rush (1745-1813) Henry Norris Russell (1877-1957) Frank Schlesinger (1871-1943) C. D. Shane (1895-1983) Harlow Shapley (1885-1972) Eugene M. Shoemaker (1928-1997) Abe Silverstine (1909-2001) Edward M. Sion (1946-) Arne Slettebak (1925-1999) W. M. Smart (1889-1975) Robert E. Smith (1944-) Willibrord Snell (1580-1626) Stanley J. Sobieski (1937-) Yousef Sobouti (1932-) Lyman Spitzer (1914-1997) Harold E. Stassen (1907-2001) Joel Stebbins (1878-1966) David J. Stickland (1946-) Wolfgang Strohmeier (1913-2004) Bengt Strömgren (1908-1987) Richard M. Sutton (1900-1966) W. F. G. Swann (1884-1962) Alan J. Thomas (1944-) James K. Thorpe (1906-1976) Peter Usher (1935-) Joseph von Fraunhofer (1787-1826) William Thaw, Sr. (1818-1889) Max Waldmeier (1812-2000) Claire F. Weaver (1899-1980) Dr. Joseph Wharton (1826-1909) Fred Whipple (1906-2004) A. E. Whitford (1905-2002) John Greenleaf Whittier (1807-1892) Paul Wiita (1953-) Thomas R. Williams (1934-) Robert E. Wilson (1937-) William Carlos Williams (1883-1963) Elizabeth H. Wood (1917-1998) Edith J. Woodward (1914-1995) Charles A. Young (1834-1908)

[a] If this birth year is correct, Henrietta was a younger sister of Reese Wall Flower, rather than an older one, as is asserted in the main document.
[b] The birth year of G. W. Cook is given incorrectly in the main document and in some other published sources. His parents were married 03/10/1868 and he was born 12/12/1868. The years 1867, 1868 and 1869 can be found in published sources but 1868 is attested in a genealogical document that his father wrote.
[c] I can’t tell which of these men was a FO volunteer photometric observer of variable stars but my guess is that it was the younger one

Neither birth nor death years are presently known for about 40 individuals who appear in the main narrative.

Robert H. Koch
1929 - 2010

Robert H. Koch, emeritus professor of astronomy and astrophysics at the University of Pennsylvania, passed away at his home in Ardmore, Pennsylvania on 11 October 2010 after a brief illness. Bob was 80 years old and remained sharp and intellectually engaged with the astronomical community up until the onset of complications from a brain tumor.

Bob was born in York, Pennsylvania on 19 December 1929, and graduated from York Catholic High School in 1947. He attended the University of Pennsylvania on a senatorial scholarship, graduating in 1951. After two years in the United States Army, he enrolled in graduate school at the University of Pennsylvania, doing his doctoral research on the photoelectric photometry of R CMa, AO Cas, AS Eri, and XY Leo at the Steward Observatory, University of Arizona in Tucson. Bob would continue this exploration of close binary stars, their atmospheres and interactions, for the rest of his career. Bob met his future spouse, Joanne C. Underwood, while in graduate school in 1957 and they were married in 1959. Bob received his PhD in astronomy in 1959 and moved to Amherst, Massachusetts where he taught as a member of the Four College Astronomy Department until 1966.

Following a year at the University of New Mexico in Albuquerque, Bob joined the Astronomy Department at Penn, teaching and doing research there until his retirement in 1996. Bob's main interests were the study of close and eclipsing binary stars, stellar envelopes and winds, intrinsic variables, transits and occultations, and the Milky Way Galaxy, producing well over 100 refereed publications. Bob was partial to photoelectric photometry and polarimetry, conducting most of his observational research at the University of Pennsylvania Flower and Cook Observatory, and at other ground- and space-based observatories. As an international figure in the area of binary stars, Bob had widespread collaborations with scientists at other institutions, in the US and throughout the world, and made significant contributions to the understanding of the process of mass transfer and accretion in close binary star systems and in developing stellar polarization standards. A number of astronomers were the recipients of his inspiration and mentorship as doctoral students at Penn.

Bob was a polymath who was able to expound eloquently on the intricacies of observational polarization measures or the various dealings of notable figures of the High Middle Ages with no advance notice. Along with his friend, biochemist Dr. Robert E. Davies, Bob helped establish at Penn one of the first courses to examine the astrophysical and biological implications for life beyond earth, long before NASA's own focus on the subject took shape. Bob was active in the astronomical community and served as president of IAU Commission 42 (close binaries).

A life-long love of astronomy led Bob to continue pursuing many areas of astronomical research during retirement. As an emeritus professor, he made important contributions to the detection of exoplanets by the eclipse-timing method, and explored the development of large, lightweight telescope mirrors for ground- and space-based observatories.

In his retirement, Bob also researched and wrote a history of observational astronomy at the University of Pennsylvania. He also was an active gardener and a talented musician, and learned to play the mandolin when he was 77. In addition, Bob and Joanne both loved traveling and bird watching, visiting nearly 30 countries during his retirement years. Besides Joanne, Bob's survivors include sons Thomas and James (Dana), daughters Elizabeth (Murray) and Patricia Budlong (Steven), seven grandchildren, a brother and a sister. Bob once wrote that he long ago decided "to control my career so as to have as much fun as grief"; in this he was successful beyond his dreams.

Joanne Koch
Michael Corcoran
Bruce Holenstein
Edward Sion

Additional information
http://www.legacy.com/obituaries/mainlinemedianews/obituary.aspx?n=robert-h-koch&pid=146110910
http://www.upenn.edu/almanac/volumes/v57/n08/obit.html#koch

Facebook page
http://www.facebook.com/pages/Robert-H-Koch-Astronomer/166544633365581?v=wall&_fb_noscript=1

Memorial Conference
A number of Bob's former students and colleagues are planning a conference in his honor entitled "Stars, Companions, and their Interactions: A Memorial to Robert H. Koch" for the summer of 2011. The conference website may be accessed here: http://www.gravic.com/RHKochConference.

Robert H. Koch at Flower and Cook Observatory in December 2000. He is standing by the final generation PEM polarimeter.

R. H. Koch wrote in 2008 the following text about himself for the narrative web site: http://www.gravic.com/about/RHK-Observational-Astronomy-UP/index.html.

Author's Background

I held academic appointments at Amherst and Mt. Holyoke Colleges and the Universities of Massachusetts, New Mexico and Pennsylvania. For essentially all of this time there were grant funds to support my observational research locally, at Kitt Peak, remotely in New Zealand, and in Earth orbit with the *International Ultraviolet Explorer* spacecraft.

I retired from teaching in 1996, none too soon in the opinion of numerous people. This permitted me to continue astronomical research, mostly on close binary stars; travel to Europe, Central America, Korea, Canada and across the U.S.; admire many species of birds; enjoy varieties of foods; compose a family history that goes back to the end of the 18th century; read; attend the Chicago Lyric Opera repeatedly; and work endlessly on our house and property.