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Whither The Sun[1]
1946 – 1994

SCIENCE DURING the war was devoted to winning. And, so, scientists, including some astronomers, dropped what they were doing, and went to work in the giant military labs devoted to creating the atomic bomb, jet engines, radar and military electronics. For example, John Hall, who was later to direct the Lowell Observatory during its great expansion at the beginning of the Space Age, worked on radar at MIT's Lincoln Labs.[2]

At the leading American observatories—Lick and Mount Wilson in California, Yerkes in Wisconsin, Lowell in Arizona, McDonald in Texas (there was not as yet a big telescope on Palomar, and Mauna Kea wasn't even a gleam in Gerard Kuiper's eye)—astronomy was temporarily asleep.[3] Except for the famous story of how Mt. Wilson astronomer Walter Baade (himself perhaps suspect as a representative, however unwittingly, of the master race) utilized the temporarily darkened skies above blacked-out Los Angeles to probe ever deeper into the universe, not much was happening. These famed institutions, all privately operated, were living off their investments and biding their time.

In the late forties, the flow of federal funds that was to propel Ameri-

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[1] This chapter was written by George Wesley Lockwood, a valued staff member of Lowell Observatory.

[2] While so employed, Hall wrote the book: *Radar Aids to Navigation*, 1947; McGraw-Hill & Co.; New York.

[3] Kuiper (1905-1973) was born in the Netherlands but became a leading figure of American astronomy.

can science to the Moon and beyond did not yet exist. There was no NASA, only its predecessor, the National Advisory Committee for Aeronautics, which operated a few small aeronautical research centers like Langley in Hampton, Virginia, Lewis in Cleveland, Ames in California, at all of which the studies concentrated on aircraft and immediate military applications, mainly using windtunnels and other laboratory techniques. There was no space science. Except for the German V2 and the rocket studies of Robert Hutchins Goddard,[4] there were no plans or research—most of this came as booty from Germany, captured hardware and brainpower like Wernher von Braun—and when they came, they belonged to the Army at the Redstone Arsenal in Alabama or at White Sands, New Mexico, not NACA. Rockets started as weapons, and after the war, like their creators, they had to be first demobilized and tamed for civilian purposes.

Eventually, of course, there would be civilian money. Enabling legislation for the new National Science Foundation was passed in 1950, and slowly the pace of scientific research began to pick up in the universities and even in the peripheral smaller research institutions like Lowell.

At the end of World War II Lowell was a scientific dwarf. The intense excitement that had brought Lowell into the nation's limelight in 1930 with the discovery of Pluto was definitely over, and its fortuitous discoverer, the photographic technician Tombaugh, was no longer an employee. He was working for the military in New Mexico and would never return. There were only three scientists. The Slipher brothers were prominent, but they were both aging and slowing down. V.M. was still director but after thirty years in the saddle, was largely preoccupied with his private business activities. His younger brother Earl, onetime mayor of Flagstaff, was continuing his careful photographic studies of the planets for which he was to become justly famous. Lampland, quiet, secretive, and obsessively careful to the point of almost complete invisibility, puttered away, leaving ultimately hardly a trace of his activities either in publications or in notoriety.

Meanwhile, the exploration of the universe was being conducted elsewhere. Credit for the recognition of the expanding universe of receding galaxies officially belonged to Slipher, but the fame and glory passed to Hubble for his continuing work on the classification of galaxies. The planets remained the province mainly of the Lowell observers, perhaps because no one else thought the solar system worthy of serious study. In

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[4] A professor at Clark College in Worcester, MA, he wrote Smithsonian Publication #2450 entitled: *A Method of Reaching Extreme Altitude*, in 1919 at age 37.

astrophysics, Lowell simply wasn't in the game, although this was to change with the arrival of Albert Wilson and Harold Johnson. Planetary science did not exist, per se, though before long an entire division of the American Astronomical Society was to be devoted to it. Kuiper, directing the Yerkes Observatory and observing at McDonald, had discovered the existence of a gaseous methane atmosphere on Saturn's moon, Titan, in 1944, but attracted little notice among mainstream astronomers. Then, as now, fashion ruled the roost, and it was the big telescopes like the new behemoth on Palomar Mountain that made the headlines.

Although no one knew it, wartime technology had brought on the demise of a mainstay of astronomical data recording. The photographic process, by which starlight was captured as blackened grains of silver iodide in a gelatin substrate, was on its way out, although it would not disappear completely for another forty years. Starlight was beginning to be recorded, not by chemistry, but by electronics, indicated first on the galvanometers of the 1920s and then after the war as a wiggly line on a moving scroll of graph paper.

This was difficult technology, but compared with photographic plates it offered 10-100 times the sensitivity and 10 times the accuracy for measuring the brightnesses of point sources like stars. The photocells of the 1920s and 1930s were works of laboratory art—handmade glass envelopes, cathode materials deposited under hard vacuum by techniques that are still mysterious, minuscule currents that could be rendered visible only with the most sensitive galvanometers. There were few practitioners and they were justly famed: Joel Stebbins[5] of the Washburn Observatory, using cells made by his colleague, Jacob Kunz;[6] A. E. Whitford[7] and G. E. Kron[8] at Lick Observatory; and John Hall at the U.S. Naval Observatory. Stebbins advanced the techniques of photoelectric photometry from the primitive and practically useless selenium photocells to the more practical photomultiplier tubes of the postwar era and was the first to measure starlight accurately through colored glass filters.

Astronomical photometry was changed forever, after the war, by the availability of the photomultiplier tube. Unlike the finicky, handmade photocells that produced, at best, a measly current barely detectable with

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[5] Stebbins (1878-1966), professor of astronomy at the University of Wisconsin, received the 1950 Gold Medal from the Royal Astronomical Society for his work in photoelectric photometry.

[6] Kunz (1874-1939) was a Swiss-born physicist.

[7] Albert Edward Whitford, born in 1905, was a specialist in photoelectric instrumentation.

[8] Gerald Edward Kron, born in 1913, later became director of the U. S. Naval Observatory's Flagstaff station.

the most sensitive equipment, the photomultiplier produced a current a million or more times bigger. The workhorse of the astronomer, the RCA type 1P21 photomultiplier and its generic stepsister, the cheaper 931A, were basically unchanged after 1941 and remained in common use at observatories well into the 1970s. Their applications were at first mainly commercial (motion picture sound decoding) and military (noise sources for radar jamming). Astronomers and physicists used them for photometry and spectroscopy. After the war, thousands were sold in a slightly different format for automatic headlight dimmers for Cadillacs, a commercially exotic idea that never really caught on.

The weak current produced by photomultiplier tubes was nevertheless too small to be utilized directly and, so, required amplification. Astronomers of the day built their own DC amplifiers, a source of additional instrumental grief as these units were every bit as finicky as the phototubes. Doing photometry required knowing the conversion between a given amount of light, as from a star imaged by a telescope, and a corresponding amount of current. The equipment had to be stable with time, and to achieve this was an iffy proposition. The gain calibration had to be verified several times a night, and if anything went wrong, the data were useless.

After amplification, the current was sent to the ubiquitous "Brown Recorder," a clanking monster that produced a jittery inked trace on a scrolling chart of graph paper. (Photons do not arrive in neat rows like marching soldiers, but rather in random clumps more like a street mob. Hence, a noisy record.) This was the one piece of universal commercial hardware that all astronomers utilized; it was robust and reliable but it weighed a ton, being about the size of a big microwave oven. Except for the chronic headache of inkfed systems that hated to work in the cold of observatory domes, they were basically reliable. The usual chart speed was an inch every two minutes, so a night's work produced a chart 25 or more feet long filled with an analog record of jittery squiggles and scribbled notes added by the astronomer to note the time, the star identification, the filters used, the gain settings and all the minutiae of observation.

Now what? From a roll of paper to the *Astrophysical Journal* is a long trip. The term "data reduction" is no misnomer—the night's data literally had to be condensed and translated into numbers, yielding results that filled perhaps only a single page of tabulation. Fifty to a hundred stars was a good night's work. The astronomer, or more typically some low-paid flunky, had to "read" the chart, which meant measuring the heights of the various squiggles on the paper and writing down the numbers.

“High tech” in those days meant measuring the chart with a special ruler calibrated in stellar magnitudes rather than in inches. This saved one laborious step, that of looking up every reading in a table of logarithms, multiplying by 2.5 to produce the astronomer’s goofy scale of stellar “magnitudes” that engineers to this day find incomprehensible, applying various corrections for the gain of the amplifier, and so on.

Despite atmospheric and instrumental limitations that limited the precision of photometry to barely 1 percent, and more typically 2 to 3 percent, the detailed computations of data reduction had to be carried out to three and preferably four significant figures throughout. This eliminated the slide rule as an instrument of computation and forced the astronomer to tables of logarithms and mechanical calculators. Hence, the kerchunkety-clunk of the the ten-place Monroematic filled the building from dawn to dusk.

The resulting table of numbers still were far from the final results. These numbers, the so-called “raw” magnitudes still required further processing, all by hand, of course, to account for the peculiarities of the particular equipment used, to correct for the loss of light in the earth’s atmosphere (15 percent at the zenith in visible light, twice that much in

Don Thompson inundated by paper from the Monroematic calculator in 1972

Correction:
The computer shown here was the observatory’s Digital Equipment Corp VAX computer system. The paper tape was generated by a model ASR33 Teletype machine attached to a DEC PDP 11/03 at the telescope. The paper tape era ended in 1983.
--gwl 2/06

the violet). The various sums of products and squares for linear regression were all done with an adding machine, and if a star did not fit, the work had to be done all over again. A night's photometry thus produced a good hard day's work and often more. Today, that task requires only a few minutes to produce a neatly printed summary.

After the war it was clear to the Observatory's sole trustee, Roger Lowell Putnam, that it was high time for new ideas and new blood. Otherwise, the Observatory, which had been quietly dozing for two decades, would simply lapse into a coma as its aging staff withered away.

A 1939 conference held in Flagstaff at the county fairgrounds had caught the attention of the Observatory's director, V. M. Slipher, as well as the trustee, and furnished the genesis of renewed vitality.[9] The focus of the meeting was on agriculture in the plains states but included was a panel on long-range weather forecasting. With civilian aviation gaining momentum, better forecasts were needed.

The chaotic unpredictability of weather had long been a source of frustration for forecasters. If only a Rosetta Stone could be found! One theme that had run like a thread throughout the twentieth century was the possibility, indeed the perceived strong probability, that one key to weather might lie in small changes in the output of the sun. This idea had been vigorously promoted first by S. P. Langley[10] and then by C. G. Abbott, a successor as director of the Smithsonian Institution and its various outposts in North and South America.

Since the turn of the century, Abbott and a small band of dedicated observers had been monitoring the total irradiance of the sun from isolated mountain tops in California, Chile (and for a time from Harquahala Peak in Arizona) using an instrument called a pyrheliometer. These observations had to be adjusted for the amount of radiation lost in the earth's atmosphere, and in particular, for varying amounts of water vapor. The resulting number, the so-called "solar constant" represented the amount of sunlight falling on the earth's atmosphere, expressed, as was the custom in those days, in calories per square centimeter per minute.

It is irresistibly tempting to believe that the solar constant would be

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[9] For several years there had been a small amount of U. S. Weather Bureau money available for studying atmospheric circulation on other planets, but Slipher had not cared to look into this matter. The funding would largely have gone to support the work of Adel - a prospect the trustee cared more about than the director.

[10] Samuel Pierpont Langley (1834-1906) was well-known for his experiments in heavier-than-air flight, contemporaneous with those of the Wright brothers. However, he was also director of the Allegheny Observatory and Secretary of the Smithsonian after 1887.

temporarily lowered by the passage of sunspots across the sun's disk, because, after all, sunspots represent regions of lower temperature on the sun. That is why they appear dark. Abbott was convinced that the solar constant fluctuated slightly, and further, that these fluctuations could be associated with specific weather events. The acceptance of sun-weather connections may not have been universal, but the meteorological literature of the 1920s was pervaded by example after example of correlated time series showing various relations between the solar constant, the sunspot number, and various weather and climate phenomena on time scales from days to decades. Abbott was a tireless promoter of anecdotal solar-weather connections and never minded that few were subject to the rigors of statistical tests invented during his tenure at the Smithsonian.

Almost forgotten in the faddish acceptance of solar relations to weather is the notorious counterexample of the "Lake Victoria effect," well noted in meteorological folklore. Early in the twentieth century, the respected British climatologist Charles Ernest Pelham Brooks[11] reported a correlation between the level of Lake Victoria in central Africa (a kind of continental rain gauge that is the source of the White Nile) and the frequency of sunspots. From 1900 until 1923, the two time series tracked perfectly, leading to the conclusion that solar activity predicted precipitation. Unfortunately, after 1923, the two series diverged sharply, never to be joined again.

A recent publication provides yet another sense of deja vu. Two Danish scientists, writing in the journal *Science*,[12] recently presented an almost-perfect correlation over the past hundred years between the length of the sunspot cycle (that is, the time between successive minima) and global temperature. This result, whether yet another "Lake Victoria effect" or a real relationship, is nevertheless applying heat to the currently fashionable and "politically correct" view that it is man, and man alone, which is responsible for the global temperature rise of the last century. Who knows?

Meanwhile, the idea of solar weather connections was still very much alive, and part of the proceedings of the Flagstaff conference turned to the question of Abbott's measurements and how they might be improved or validated. The astronomers at the conference put forth an idea, not a new one exactly—credit for this dated from the turn of the century—to monitor total sunlight by tracking the brightness of reflected light from the outer planets, that is, to study the sun at night!

[11] Author of the widely respected, *Climate Through the Ages*.

[12] "Length of the Solar Cycle as an Indicator of Solar Activity closely Associated with Climate" by E. Friis-Christensen and K. Lassen; *Science* 254, p. 698 (1 November, 1991).

The fallout from the 1939 conference proved to be a fruitful turning point for many aspects of the Lowell Observatory. From it came the first-ever government money to arrive at the Lowell Observatory, a program entitled "The Project for the Study of Planetary Atmospheres" funded at first by the U. S. Weather Bureau and later by the Air Force Cambridge Research Laboratories (now at Hanscom Air Force Base, Massachusetts) in the summer of 1949. Much of the work was accomplished in summer sessions by a small team of visiting meteorologists, several of whom have had long and distinguished careers—Ralph Shapiro, a 1943 graduate of Bridgewater College and later project scientist at the Air Force Cambridge Research Center; Seymour Lester Hess, then of the University of Chicago and later Florida State University; Hans Arnold Panofsky, New York University and later Penn State; and Edward Norton Lorenz from MIT. Each of these young participants in Lowell's Planetary Atmospheres program achieved later distinction as professors of meteorology.

The planetary atmospheres project, which entailed studies based mainly on Lowell's photographic collection led to a new observational sub-program that was to endure for forty years with hardly a break that began under the direction of a newly-hired young astronomer, Harold Lester Johnson (1921-1980). Johnson was brilliant, mercurial, impatient and stayed with Lowell Observatory for only seven years, until 1959. His specialty was instrumentation and techniques of photometry, and together with his slightly older contemporary, William Wilson Morgan of the Yerkes Observatory, he devised a system of multi-color photometry, the so-called UBV system. If practical astronomical photoelectric photometry is said to have a father, then that father is surely Harold Johnson, who after two stints at Lowell, worked for a while at the McDonald Observatory in Texas, and went on to the newly-created Lunar and Planetary Laboratory at the University of Arizona in Tucson and then to Mexico.

Actually, the first such observations of Uranus and Neptune were made by Henry Giclas during 1949 using the 1909 40-inch telescope and a primitive photometer. There were lots of problems brought on by an amplifier designed by Johnson that never worked well, and a disconcerting tendency of the telescope to drift away from the star being observed. Nevertheless, Giclas's report, included in the 1950 report to the U.S. Air Force, is a classic presentation of the methods of photoelectric photometry.

Why is it better to monitor the sun indirectly using light reflected from planets, rather than by the more seemingly straightforward direct methods of Langley and Abbott? What was sought was not an actual value of the solar constant—that had already been determined to better than 1 percent—but evidence of small changes of a percent or less. For this purpose, direct measurements were unsuitable owing to the uncertain but necessary corrections that had to be made to account for losses in the Earth's atmosphere. Indeed, a possible interpretation of Abbott's claimed solar-weather connections could well have been changes in the atmospheric transparency, an idea still not completely ruled out.

Observations of the planets neatly skirt this issue, at least to a first approximation, because they can be made differentially with respect to so-called comparison stars lying near the planets' path along the solar ecliptic. When comparing two celestial objects in this way, the amount of light lost in the atmosphere is immaterial, provided that it doesn't change between the two measurements, often only a minute or two apart.

Of course, the planets move with respect to the celestial background, although for Uranus and Neptune, the motions are quite small, only 4 degrees per year for Uranus and half that for Neptune. This meant that each year, new comparison stars had to be selected, and their brightness values tied into a growing network of prior years' stars, slowly creeping along the ecliptic.

The program required careful repeated measurements and a consistent plan of work. The results, prior to Johnson's return in 1952, were not very precise, owing mainly to growing pains in the art of photoelectric photometry. Nevertheless, the first refereed publication on the subject, a study of the brightness variations of Uranus by Henry Giclas appeared in the *Astronomical Journal* in 1954. This paper showed less than a 1 percent variation in the brightness of Uranus since 1950 and less than a 10 percent discrepancy with measurements made in 1927 by Stebbins.

Johnson, perhaps more than coincidentally, used Lowell's 42-inch telescope to redetermine the magnitude of the faintest star that had been observed photoelectrically with the 200-inch telescope on Mount Palomar. He found that it was in error by 0.4 magnitude. It has been a characteristic of Lowell's astronomers to push their instruments and observations to the ultimate limit in accuracy and sensivity. Those with easier access to larger instruments are often complacent in what they do, perhaps because they really do not have to try as hard and feel that others just cannot compete.

Robert Howie Hardie, who together with Giclas published a retrospective analysis of the Lowell photometry of Uranus and Neptune from 1949 to 1954, was only briefly on the staff of the Lowell Observatory. He went on to a distinguished career as a pioneer of photoelectric photometry at the Dyer Observatory at Vanderbilt University.[13] Surprisingly, only these two refereed publications appeared in the standard astronomical literature of the time. Perhaps because of this, the Lowell program attracted little notice, with its most significant findings buried in the far more obscure Lowell Observatory *Bulletin*.

The "solar variations" program continued without interruption through 1966, with a succession of observers and improvements in equipment. In 1961 a Lowell *Bulletin* titled "The Sun as a Variable Star" appeared, summarizing the results of the program from 1953 onward.[14] The author was Krzysztof Serkowski, a young Polish astronomer later to become famous at the University of Arizona for his pioneering work on astronomical polarimetry. The centerpiece and main conclusion of this publication was an eight-year time series of annual mean brightness values for the planets Uranus and Neptune that showed only small fluctuations from year to year.[15]

By observing sun-like comparison stars over this same eight-year interval, however, the Lowell observers had inadvertently produced a data set of great astrophysical interest. Little was known of the intrinsic variability of solar twins. These common dwarf stars lying on the so-called "main sequence" in a diagram of absolute stellar brightness as a function of stellar temperature were, for the most part, thought to be absolutely stable in their light output. Indeed, many were selected in the early 1950s as photometric "standard stars" used to calibrate the colors and magnitudes of other ordinary or variable stars. What Serkowski and Jerzykiewicz (another Polish astronomer recruited into the program) showed was that these ordinary solar dwarf stars, if they varied at all, did not fluctuate by more than a percent or so over eight years.

This program was continued through the thirteenth season, after which it suffered a five-year hiatus until, with some gentle prodding from the meteorologist and paleoclimatologist, John Murray Mitchell, Jr., the Observatory's new director John Hall, reinstated it in 1971, this time assisted by a five-year grant from the National Science Foundation. The

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Hardie's 1962 chapter on photoelectric reductions is a classic reference and still in use; see *Stars and Stellar Systems*, Vol. II, U. of Chicago Press. Harold Johnson also contributed to this volume - a chapter on design and construction of photoelectric photometers and amplifiers.

The earlier photometry - 1950-53 - was of inferior quality and was not included in this report.

There were three important contributions to this *Bulletin* series, each entitled; "The Sun as a Variable Star." The first, authored by Johnson appeared in 1959 (LOB # 96). Subsequent papers bearing the same title appeared in 1961 (K. Serkowski, LOB # 106) and in 1966 (M. Jerzykiewicz & K. Serkowski, LOB # 137).

program has continued, with uninterrupted funding from the Division of Atmospheric Sciences until the present time and has produced a unique time series of photoelectric measurements of a number of solar system objects: Saturn's moons Titan and Rhea, Jupiter's moons Io, Europa, and Callisto, Uranus and Neptune.

In an addition, funded for a while again by the Air Force Cambridge Research Laboratories, an eight-year series of precision measurements of several dozen solar type stars produced valuable new data concerning the variability of sunlike stars. This last project again addresses the question of solar variability from a stellar perspective and seems to be showing that the sun's present variability, finally detected decisively from space radiometry, is perhaps abnormally quiescent at the present time.