Seeing Things Differently

Memoir of an Unlikely Scientist

Peter Foukal

Nahant, Massachusetts

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To Lizzie,

our children Maika, Andrew and Nicholas,

and

grandchildren Carrington, May, Mason, Davis, Will and Finley.

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Preface

In the popular imagination, as in the hit TV series “Big Bang Theory”, physicists are viewed as virtuosos of the deductive reasoning emphasized in our schools and universities. Yet, advances in branches of the field with rich phenomenologies, like astronomy or geophysics, often require talent for noticing patterns in complex data – i.e. inductive reasoning. A knack for spotting that which is in plain sight but has gone unnoticed can also be key. These less heralded talents lie outside the scope of conventional education, perhaps because they are difficult to teach.

This path less travelled has led me, despite my limited deductive gifts, to some interesting discoveries. I was able to explain why the Sun *brightens* when dark sunspots are numerous, and how their magnetic fields are able to modulate the brightness of the Sun and similar stars. Also, to explain why the Sun’s magnetic field rotates faster than its photosphere.

Along the way, a penchant for “seeing things differently”, as described by a Harvard colleague, also led to some unusual life decisions which luckily, turned out better than anyone had a right to expect. I am grateful for much good fortune for these happy outcomes, and to family and friends for their patience and understanding.

No career is an island and I have been fortunate in several mentors who provided encouragement and support. My high school English teacher, Miss Chivers, glimpsed promise and named me Editor of the Year Book, thus giving me a taste for the fast track. Professor Bill Martin, a kindly experimental physicist at McGill, saw potential in my persistent questions and wrote me a recommendation to graduate school despite my lackluster performance in the Physics Department’s offerings. The good fortune that Zdenek Kopal, the Chairman of the Manchester University Astronomy Department had three daughters of marriageable age, probably helped my admission to graduate studies at that institution.

As a post-doctoral fellow at CalTech, I benefited from Hal Zirin’s encouragement to use whatever available facilities to pursue my interests. Later, I was fortunate that Jack Eddy, one of the most recognizable names in astronomy in the 1970’s, spent a year at Harvard during my time there. His influence gained me membership in several committees of the NSF, NASA and the NAS where I met many of the most important figures of that time, in astrophysics and geophysics.

My unusual decision to leave academia to pursue entrepreneurial research left me reliant on the pragmatism of Program Directors at the Federal funding agencies. I am grateful to Dennis Peacock at the NSF, Bill Wagner at NASA and Dick Donnelly at NOAA, for their willingness to take a chance on funding pure research at a newly formed profit-making company

The beauty of the night sky has brought me many hours of satisfaction and calm in stressful times. I am grateful to my childhood mentor in astronomy, our family friend the late Ivo Kudrnac, for fostering my childhood interest in this fascinating pursuit.

Childhood and Youth of an Entrepreneur

Our family emigrated from Czechoslovakia in 1948, when I was three years old. The shoe manufacturing business that my father co-founded during WW II had been nationalized after the Communist coup in that year and he was informed, as a capitalist, that it would be wiser to leave the country.

When we founded Cambridge Research and Instrumentation (CRI, Inc.) many years later, we were advised that, to succeed, a start-up needed an "unfair advantage". That is, a key patent or other boost over the competition. In my father's case, his partner had been placed in charge of leather distribution by the wartime Czech government, and the firm expanded rapidly. Our advantage at CRI lay in the then–novel idea of using Federal research funding for pure science to help defray indirect costs while also building commercial instrumentation.

Entrepreneurship in the family ran back two generations earlier. My maternal great - grandfather founded an oil business in the late 19th century which rose from modest beginnings to become BZ Petroleum, the largest Czech – owned oil company after WW I. My grandfather's contribution was to spend the WW I years in Cleveland, where he secured valuable contracts to supply high grade American oil to BZ.

As a child my greatest gift, if I had one, was in art. Other children in my elementary school art class used to cluster around my desk to watch me draw. Even my father, who did not waste compliments, admitted that some of my pictures were quite good. I, however, knew that my talent didn't hold a candle to the remarkable cartoon figures produced by my friend Steven Slutsky. I wonder whether he followed that talent into a career with Disney.

Czechs have an often-detrimental tradition of thinking differently. Already in the early fifteenth century, Bohemia and Moravia formed the first durable Protestant state and held out against their Catholic neighbors for two centuries. Our family life also differed from the Canadian norms of the time. Much of our socializing took place on long walks with European family friends. Moreover, we swam in the Ottawa river that ran along the shore of our property outside of Montreal. Such behavior was viewed with suspicion by our neighbors.

This contrarian background and a fertile imagination combined to produce a lively life of the mind through voracious reading. My interest in astronomy probably dates to absorbing Fred Hoyle's *Frontiers of Astronomy* when I was ten years old. The theory of stellar evolution and the Hertzsprung-Russell diagram were fascinating even at that early age. I also read and re-read Jules Verne's *Twenty Thousand Leagues Under the Sea* and *Mysterious Island* in Czech at about that age. The adventures of *Horatio Hornblower* and Arthur Ransome's series *Swallows and Amazons* made a great impression and fostered my love of sailing.

My parents' decision to enroll me in the French Lycée when I was 10 years old was unheard of amongst Montreal's English-speaking population. The long trip to school alone across the city, on four buses and street cars in winter blizzards, combined with the rigors of a 1950's French classical education were challenging. But learning French opened the door to different sensibilities that have since enriched my life. I acquired the new language rapidly and soon moved into the top tier of a class that produced several prominent doctors and lawyers and a future Premier of the Quebec Province.

My interest in science prompted me to found the International Scientific Society at age eleven. The headquarters were in my bedroom where the fossils we had gathered from a nearby quarry were on display together with a nice microscope that another ISS member had recently received for his birthday, but I decided it was best kept at our HQ. Notes on the ISS meetings record strife amongst the rank and file. My assertiveness seems to have been annoying to some already at that early age.

First telescope at age 11.

My high school career in the English-speaking suburbs was undistinguished until my senior year, when, buoyed by my new eminence as year book editor, I narrowly missed placing first in my graduating class and earned a prestigious full scholarship to McGill University. My academic successes so far had been based on broad competence in most subjects except music and gym and I was an avid amateur astronomer. But overall there was little in my background to single me out as a future scientist.

In the advanced freshman math, physics and chemistry courses at McGill I found myself amongst classmates who may not have had my broad facility in the humanities but were more adept at math. I barely passed my freshman year, lost my scholarship and was denied admission to the rigorous honors physics curriculum. This was a big disappointment and my parents wondered whether I shouldn't change focus to a career in, perhaps, the diplomatic service; my mother was an admirer of the dapper British diplomat Sir Anthony Eden.

But I continued to take as much physics and math as I could fit in, while also minoring in Russian. I did better in my final year and was accepted to graduate studies in astrophysics at the University of Manchester, in England. Based on my marks alone I would not have been accepted to a US program of comparable reputation but the department chair in Manchester shared my Czech background and was pleased that I wrote to him in my mother tongue. As mentioned above, he also had three daughters of marriageable age.

So I followed my interests instead of my strengths, much as a high school friend who achieved a rare, untutored, near-perfect 799 score in her math SAT in 1962. She had no interest in the subject and went on to a successful career in dolphin training. I've never regretted my decision to choose astronomy over the diplomatic service and the world is the better for it.

Graduate studies in Manchester.

Manchester in 1966 was still recovering from the devastation of WW II bombings. A British friend of my mother wondered whether "Peter had lost his senses" in going there. But the giant Jodrell Bank radio telescope, the world's largest, stood nearby in the lovely Cheshire countryside and I happily joined two other new students in a garret-like space reached by a ladder from an office that had been occupied before WW I by the legendary Danish physicist Niels Bohr. The blackboard on which Bohr had first drawn his historic model of the atom in 1913 still stood below. Bohr had come to work with Ernest Rutherford who had been attracted from McGill shortly before, and discovered the atomic nucleus while in Manchester.

Inspired by these two Nobel-winning giants of physics, I set to work learning all I had missed by failing to qualify for the honors physics course at McGill. My fellow graduate students were an eccentric lot of mainly Cambridge graduates who had missed obtaining first class honors in the redoubtable Tripos Exams. In the British system, students specialized in a subject like math and physics at an early age so my comperes were, once again, ahead of me in these subjects.

Attempting to catch up, I immersed myself in a variety of books ranging from classical and quantum mechanics to stellar evolution and optics. My graduate advisor, John Meaburn, was a young lecturer who had made his reputation in observations of gas velocities in faint supernova remnants using Fabry Perot interferometers of his own design. He was rightfully skeptical of this green Colonial.

My first project was a disaster. John had assigned me the design of a scanning spectrometer to measure the passbands of interference filters. It should have been a straightforward task giving me a chance to work with the department machine shop. Embarrassingly, I misunderstood the interpretation of the basic grating equation that determined the design and John had to correct my error. It was not a good start but I soldiered on.

I would have had difficulty in passing the examinations in the first-year courses that covered topics of interest to the department faculty, namely the theoretical aspects of stellar evolution, binary star dynamics and fluid mechanics of interstellar gas. So Zdenek Kopal, the energetic Czech – born department chairman decided with John that I should instead do a Master's thesis involving observational research on the possibility of luminescence of the Moon's surface. Some evidence for luminescence had been reported in a Ph.D. study by an immediately previous graduate student.

This project involved travelling together to the Pic du Midi Observatory in the French Pyrenees and learning how to use the photometric scanner that he had built for his observations. I looked forward to this opportunity although the scanner looked complicated and challenging to master in the short time available.

We flew from Manchester to Paris and took the night train to Tarbes in southwestern France, continuing on next morning by local train and then observatory van to the mountain resort below the Pic du Midi, an imposing mountain of about 9500 ft altitude. The last leg was, first, a ski cable car and then a little metal cabin that whisked us across a breathtaking precipice to the mountain top observatory. It was an amazing if somewhat daunting experience!

Pic du Midi (observatory on top)

My introduction to the photometer was even more daunting. Its opto-mechanical parts mounted on the 1-meter aperture Cassegrain telescope consisted of motorized mirrors that scanned the lunar image in three color bands fed onto photomultiplier tubes. The power supplies for the PMT's and the signal amplifiers were located in a room below the telescope dome. It was a lot to assimilate, including a warning against touching the kilovolt power supplies with anything but the back of one's hand.


The lunar photometer

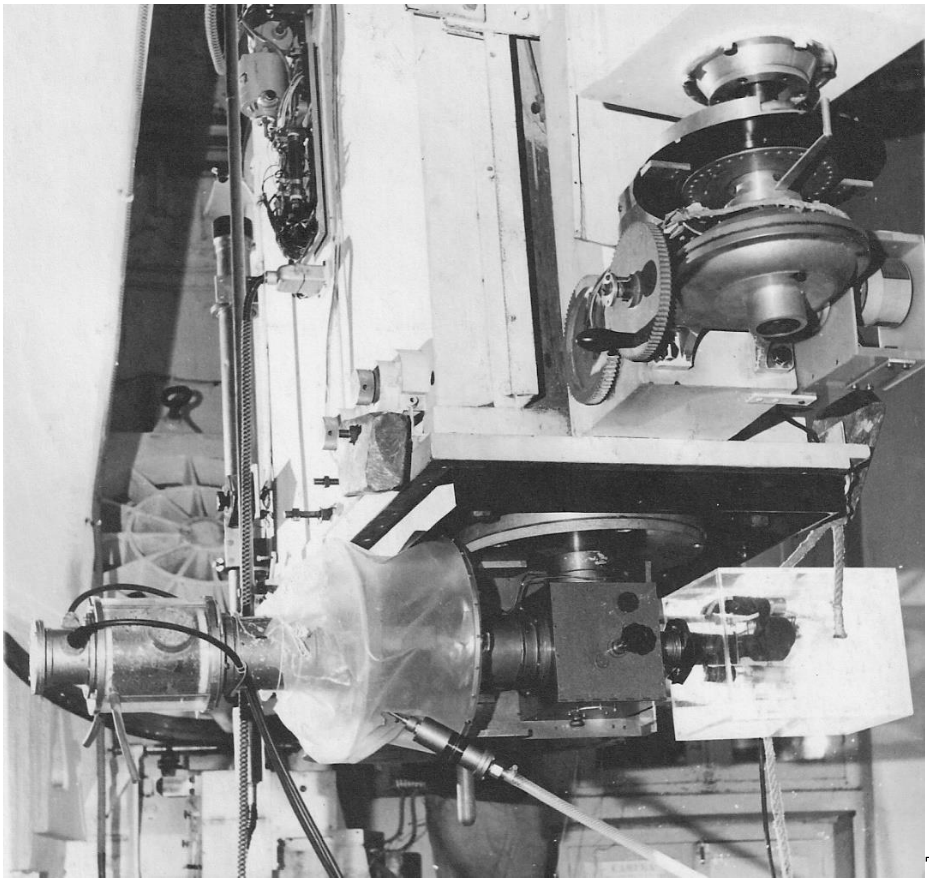
I managed to familiarize myself with this quirky set up during this first trip but viewed with apprehension my next trip out to operate it myself. I managed to muddle through, as the British liked to say, and discovered on this solo trip that the "lunar luminescence" reported by the instrument's designer was in fact a spurious color signal caused by the scanning mirrors, not the Moon. This news caused some consternation back in Manchester but it was difficult to argue with the tests I had performed.

This development caused some embarrassment to Prof. Kopal who had supervised the Ph. D thesis in which the luminescence findings were reported. But to his credit, he congratulated me on my good work and informed me with a broad smile that I had been awarded a handsome Science Research Council graduate fellowship to purse my Ph.D. I was on my way!

I decided to switch topics for my Ph.D. from the lunar studies in which Kopal was a world authority, to observations of the motions in interstellar nebulae that John was pursuing. My decision was inspired in part by the interesting theoretical work on the dynamics of these objects carried out by Professor Franz Kahn, one of Britain's most respected astrophysicists, whose office was down the hall.

John's previous student had built a sophisticated Fabry Perot interferometer and used it to carry out some preliminary observations. I was to continue that work, observing a larger sample of the brightest nebulae in our galactic neighborhood. Again, this required a first trip to the Pic, this time with John, to learn how to operate this ingenious spectrometer that scanned the spectral profiles of nebular lines by carefully introducing compressed air between the finely aligned plates of the interferometer.

The first observation John tried yielded almost no signal so I suggested that we try Messier 8, the so-called Lagoon Nebula which I knew to be the brightest in the summer sky from my amateur days. Sure enough, the signal was so strong that the pen recorder almost jumped off its rails! My astronomy hobby had come to the rescue!



The F-P spectrometer

My trips to the Pic over the next two years were at one point held up by the take-over of the Observatory by Maoist protestors during the infamous French "évenements de 68". But otherwise my Ph.D. work proceeded apace and I passed my oral exam in the spring of 1969 - before turning twenty four that summer.

I knew that I would never have my friend Eric Graham's facility with computers that enabled him to produce the first simulation of compressible convection, nor the mathematical ability of Michael Raadu and Asoka Mendis, who went on to distinguished careers in plasma physics and comet physics. I even doubted my ability to emulate John's talent for designing and coaxing results from complex opto-mechanical instruments.

Yet, I had developed confidence in some ineffable ability to identify problems missed by others and to see relatively simple solutions. I was encouraged that such a talent had marked Ernest Rutherford, considered the greatest experimental physicist of his time. No mathematician himself, he had asked his brother-in-law to perform the elementary calculus integration required to describe the law of particle scattering that earned him a Nobel Prize. I wasn't shooting that high but I had acquired the self-confidence to move ahead.

Lagoon nebula, M 8.

I hoped to extend my work on nebulae into study of their role in star formation; infrared observations focused on this topic were in their infancy and I was interested in joining a group taking advantage of that new technology. I was happy to be offered post-doctoral positions from Groningen, Milan, and Paris – the three European institutions where I had interviewed. But I was turned down by CalTech in Pasadaena, California, where the strongest IR group had been formed by Gerry Neugebauer.

Luckily, though, before I made a decision, Professor Kopal informed that Professor Harold Zirin at CalTech had asked him to recommend a candidate for a position in his solar physics group. My decision was complicated by another offer at the same time, from the Naval Research Laboratory in Washington D.C., to join their ultraviolet astrophysics group. But the attraction of CalTech - the pinnacle of the astronomical research pantheon, and life in sunny California, won out.

Caltech: plasma electric fields, fibril alignment and sub-photospheric solar rotation.

Hal Zirin had recently opened his impressive Big Bear Solar Observatory high in the coastal mountains a couple of hours from Pasadena. When I first arrived at BBSO, he was trying water skiing on Big Bear Lake where the observatory was located. Its placement on a spit of land extending into the lake reduced local heating that interfered with image quality. This arrangement enabled BBSO to obtain unprecedentedly sharp movies of plasma motions in the Sun's atmosphere.


BBSO

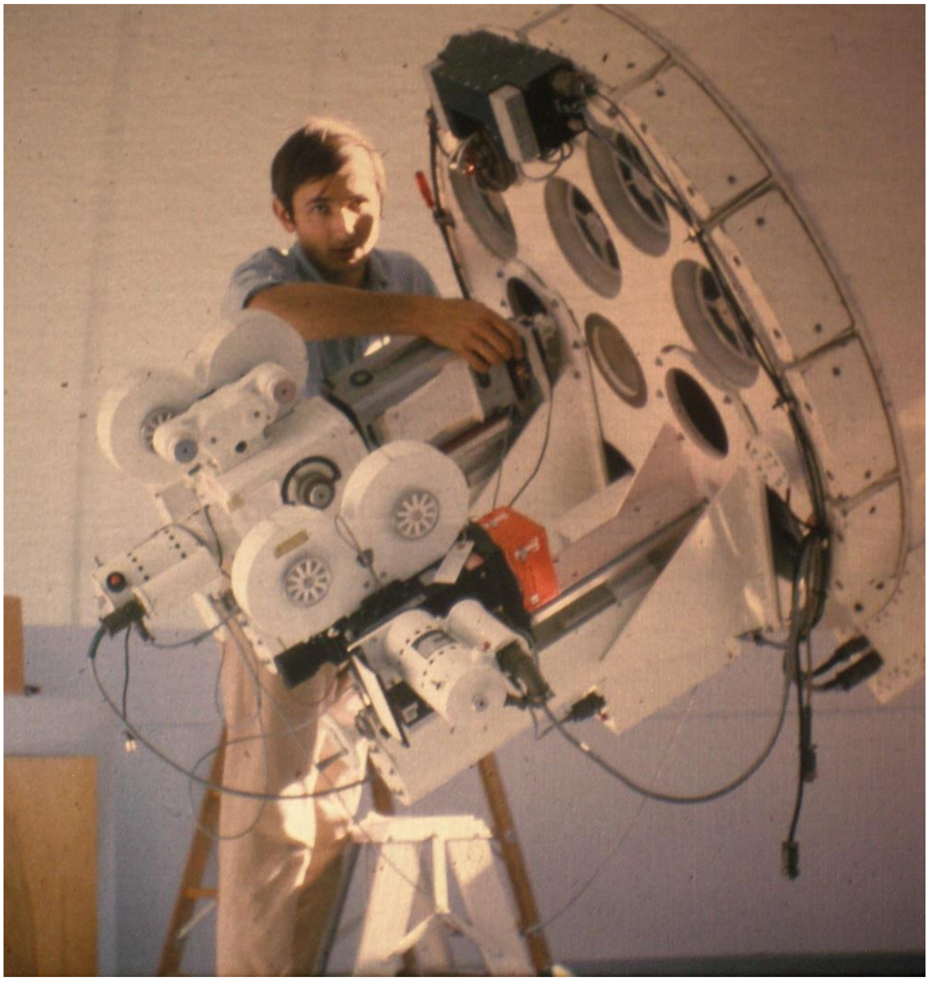
Hal was very supportive of anything I wanted to do that used his powerful new telescope. I knew nothing about solar physics and he was happy to let me spend time reading his textbook and back copies of Solar Physics journal to learn the subject and come up with novel observations.

a) Solar plasma electric fields

In Manchester I had become interested in magnetohydrodynamics – the study of conducting fluids in magnetic fields, so I was pleased that motions in solar structures were a prime example of such mhd effects. The intense magnetic fields of dark sunspots and bright faculae had been measured since the beginning of the 20th century, but I was surprised that no one seemed to have tried to measure the electric fields that were implied by magnetic field reconnection and by the acceleration of high energy particles in transient phenomena like flares.

It was generally assumed that the high electrical conductivity of the solar plasma would short out any electric fields, but even Hannes Alfven, who had won a Nobel prize for inventing mhd in the 1940's warned against neglect of electric fields and proposed solar scenarios in which they would be expected to occur.

I decided to investigate this odd gap, or lacuna, as I liked to call it, in solar research. The BBSO solar films were taken in the light of the Balmer-alpha hydrogen line which was slightly broadened by the Stark effect if an electric field were present in the chromosphere, the solar atmospheric layer where this radiation originated.

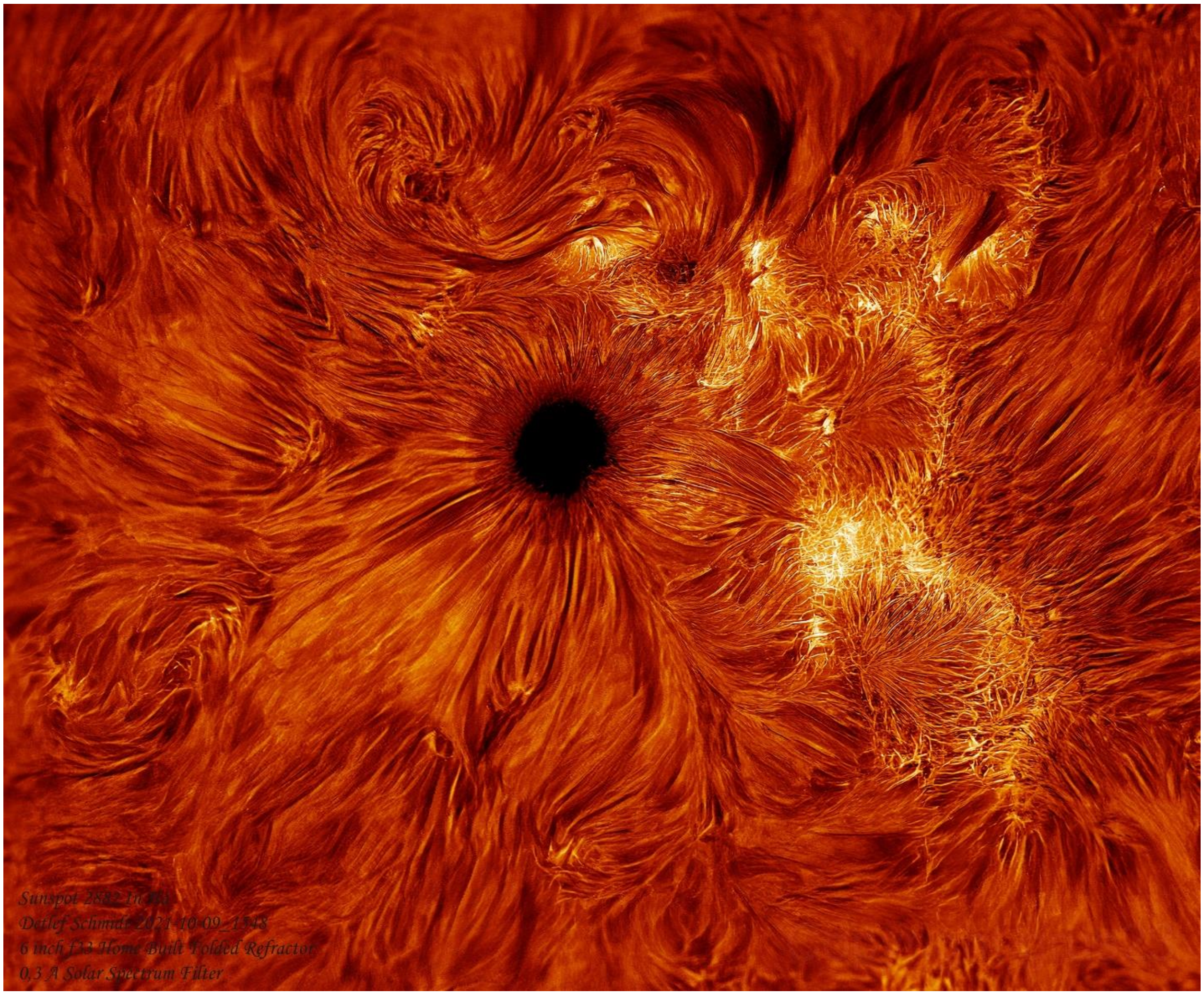

PVF observing at BBSO

The kindly BBSO machinist built me a device overnight that would rotate a waveplate in front of the polarizing filter between successive frames of the time lapse movie. The idea was that, with the H-alpha filter tuned slightly off the center of the line, the Stark broadening would cause a flickering of the brightness as the movie was projected.

I was aware that the Stark sensitivity of H-alpha was low and the likelihood of seeing an effect was small, but it was worth a try. The movie did show a flickering, mainly due to variable transmission and reflection in the optics as the wave plate rotated. But I remained interested in this topic and later developed a more sensitive electrograph that we installed at Sacramento Peak Observatory in New Mexico. But more on that later.

b) Alignment of chromospheric fibrils

I had more success with simple examination of the intricate chromospheric structures revealed in the BBSO movies. Watching them in Hal's projection room in the Robinson Lab at CalTech one day, I noticed that the elongated features called fibrils observed around solar active regions switched their orientation by 180 degrees on the opposite sides of the magnetic neutral line – the line at which the line-of-sight magnetic field switched polarity.



H-alpha image of chromospheric fibrils aligned with the magnetic fields around a sunspot.

Hal was excited to have this phenomenon spotted for the first time in his movies and a paper I published attracted the attention of the renowned German solar astronomer K.O. Kiepenheuer, who invited me soon afterwards to present my result at a small meeting at his observatory on the island of Capri. There I enjoyed minor celebrity status as the discoverer of an effect that had been in plain view for decades but simply hadn't been noticed before.

This anti-parallel orientation of fibrils implied that the magnetic field lines at such a neutral line had a sheared geometry which had interesting implications for the support of the extended columns of cool dark gas called filaments. Decades later it was also shown by my colleague Sara Martin to have implications for the global geometry of the Sun's magnetic field.

c) The sub-photospheric shear layer

The solar group consisting of Hal and his post docs used to have a weekly lunch together at the Athenaeum, a lovely Spanish style building at one end of the CalTech campus. We were usually joined by Bob Howard, the Director of the solar program at Mt Wilson and Palomar Observatories whose headquarters was a few blocks away.

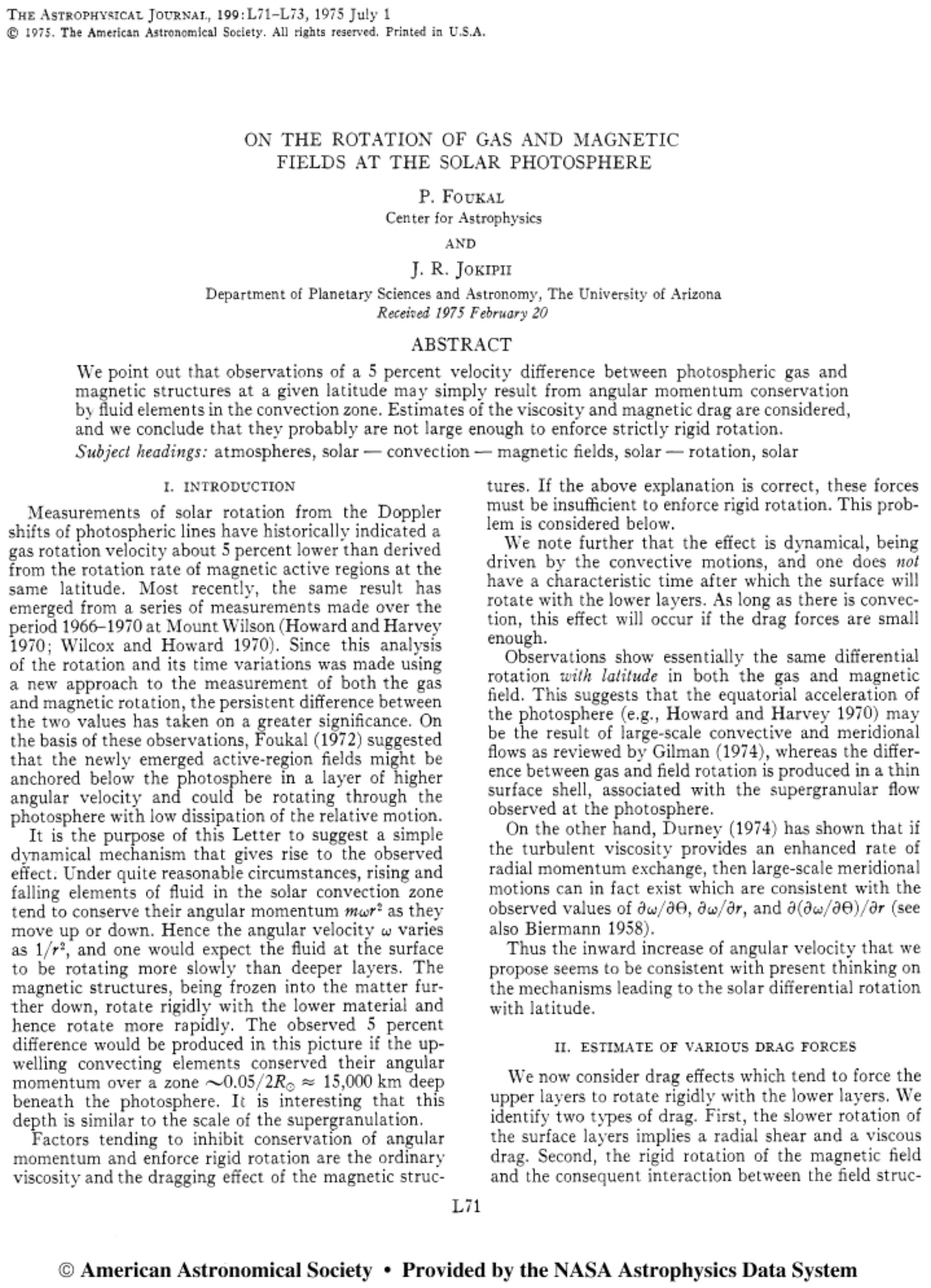
Sometimes also Professor Robert Leighton lunched with us. Bob was considered, by the world - famous theoretician Richard Feynmann, to be America's best experimental physicist. His achievements included discovery of the Sun's 5-minute pulsation and of the supergranulation, a mysterious pattern of polygonal velocity structures covering the photospheric surface. These discoveries were among the most important advances in understanding of the Sun in the past 50 years.

During one of our lunches Bob Howard showed a plot of the Sun's rotation rate at the photospheric layer that emits most of solar radiation. His plot confirmed how the rate decreased towards the poles but the intriguing feature to me was the roughly 5% higher rate of magnetic structures like spots and faculae relative to the less magnetized plasma. Bob was aware of this difference but had made little attempt to explain it. The result implied that the flux tubes of spots and faculae were plowing through the photosphere at a speed of about 200 mph.

In a short 1972 paper I suggested that this relative motion might imply that the intense magnetic flux tubes were anchored at a deeper, more rapidly rotating, level of the solar interior. Soon afterward, I described this aspect of solar rotation in the weekly astrophysics colloquium at CalTech. In the hall afterwards, Randy Jokipii, an Associate Professor in the Physics Department, approached me asking whether there might be some reason to believe that the depth at which the flux tubes were anchored was about 15,000 km.

Randy's expertise was in the dynamics of the solar wind outflow from the photosphere and he had done a quick calculation during my talk indicating that this 15,000 km depth would yield the observed 5 % higher rotation rate if solar convection were conserving angular momentum. I recognized this 15,000 km as the depth commonly associated with the supergranular overturning pattern that had been discovered by Bob Leighton and his students a few years earlier.

Randy and I were excited by this finding but proceeded cautiously. I calculated that the drag of the intense flux tubes rotating like rigid spokes through the sub-photosperic gas was probably insufficient to spin it up within an estimated overturning time of the convection in the 15,000 deep layer. Whatever impulse given to the super-granular gas by these paddle wheel-like flux tubes would simply be returned to the deeper sun when the gas sank back to the base of this layer.



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ON THE ROTATION OF GAS AND MAGNETIC FIELDS AT THE SOLAR PHOTOSPHERE

P. FOUKAL
Center for Astrophysics
AND
J. R. JOKIPII
Department of Planetary Sciences and Astronomy, The University of Arizona

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ABSTRACT

We point out that observations of a 5 percent velocity difference between photospheric gas and magnetic structures at a given latitude may simply result from angular momentum conservation by fluid elements in the convection zone. Estimates of the viscosity and magnetic drag are considered, and we conclude that they probably are not large enough to enforce strictly rigid rotation.

Subject headings: atmospheres, solar — convection — magnetic fields, solar — rotation, solar

I. INTRODUCTION

Measurements of solar rotation from the Doppler shifts of photospheric lines have historically indicated a gas rotation velocity about 5 percent lower than derived from the rotation rate of magnetic active regions at the same latitude. Most recently, the same result has emerged from a series of measurements made over the period 1966–1970 at Mount Wilson (Howard and Harvey 1970; Wilcox and Howard 1970). Since this analysis of the rotation and its time variations was made using a new approach to the measurement of both the gas and magnetic rotation, the persistent difference between the two values has taken on a greater significance. On the basis of these observations, Foukal (1972) suggested that the newly emerged active-region fields might be anchored below the photosphere in a layer of higher angular velocity and could be rotating through the photosphere with low dissipation of the relative motion.

It is the purpose of this Letter to suggest a simple dynamical mechanism that gives rise to the observed effect. Under quite reasonable circumstances, rising and falling elements of fluid in the solar convection zone tend to conserve their angular momentum $m\omega r^2$ as they move up or down. Hence the angular velocity ω varies as $1/r^2$, and one would expect the fluid at the surface to be rotating more slowly than deeper layers. The magnetic structures, being frozen into the matter further down, rotate rigidly with the lower material and hence rotate more rapidly. The observed 5 percent difference would be produced in this picture if the upwelling convecting elements conserved their angular momentum over a zone $\sim 0.05/2R_\odot \approx 15{,}000$ km deep beneath the photosphere. It is interesting that this depth is similar to the scale of the supergranulation.

Factors tending to inhibit conservation of angular momentum and enforce rigid rotation are the ordinary viscosity and the dragging effect of the magnetic structures. If the above explanation is correct, these forces must be insufficient to enforce rigid rotation. This problem is considered below.

We note further that the effect is dynamical, being driven by the convective motions, and one does *not* have a characteristic time after which the surface will rotate with the lower layers. As long as there is convection, this effect will occur if the drag forces are small enough.

Observations show essentially the same differential rotation *with latitude* in both the gas and magnetic field. This suggests that the equatorial acceleration of the photosphere (e.g., Howard and Harvey 1970) may be the result of large-scale convective and meridional flows as reviewed by Gilman (1974), whereas the difference between gas and field rotation is produced in a thin surface shell, associated with the supergranular flow observed at the photosphere.

On the other hand, Durney (1974) has shown that if the turbulent viscosity provides an enhanced rate of radial momentum exchange, then large-scale meridional motions can in fact exist which are consistent with the observed values of $\partial\omega/\partial\Theta$, $\partial\omega/\partial r$, and $\partial(\partial\omega/\partial\Theta)/\partial r$ (see also Biermann 1958).

Thus the inward increase of angular velocity that we propose seems to be consistent with present thinking on the mechanisms leading to the solar differential rotation with latitude.

II. ESTIMATE OF VARIOUS DRAG FORCES

We now consider drag effects which tend to force the upper layers to rotate rigidly with the lower layers. We identify two types of drag. First, the slower rotation of the surface layers implies a radial shear and a viscous drag. Second, the rigid rotation of the magnetic field and the consequent interaction between the field struc-

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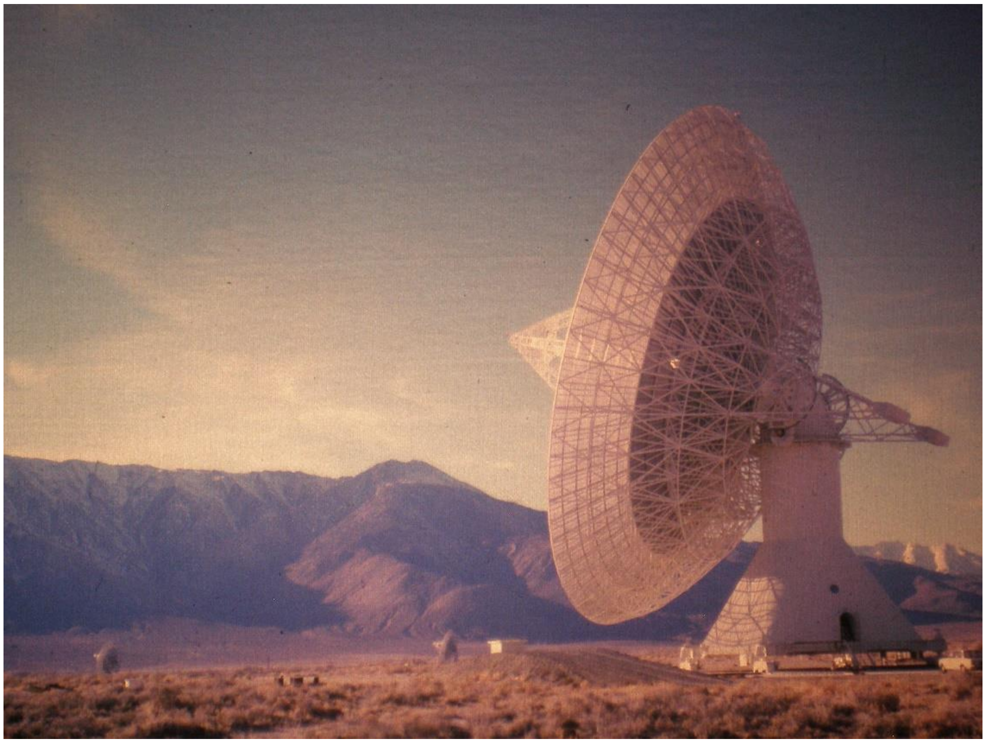
We tried out our result on a few colleagues more experienced in fluid mechanics, but no-one could see any flaws in our reasoning so we finally published our paper in the Astrophysical Journal Letters in 1975. Fifty years later, our mechanism still provides the best explanation of Bob Howard's observational finding that solar magnetic structures rotate faster than the general photosphere.

This deceleration of photospheric gas in a thin "shear layer" below the photosphere was later confirmed by analysis of the solar oscillations that Leighton had discovered. Interestingly, our prediction and explanation of this shear layer still seems to be the most successful example of agreement between the dynamical theory of solar rotation and observations.

d) Other research

I observed at BBSO regularly and Hal also assigned me the task of researching high performance video cameras for the solar space telescope that he was planning with NASA. In addition, I used the historic 100 inch reflector at Mt Wilson Observatory overlooking Pasadena, to pursue my interest in diffuse nebulae. Hal had acquired a near – infrared sensitive image tube and he let me use it on the 100 inch to produce the first image of a diffuse nebula in the intense emission of S III, which I published in the Ap.J. I was lucky to escape electrocution when the image tube's kilovolt power shorted out while I was focusing the device.

The accuracy of some work I was doing on the microwave brightness of small solar flares was limited by the low angular resolution of available solar microwave observations. Hal suggested that I try using the big 130 ft aperture radio telescope at Owens Valley Radio Observatory to get much higher resolution. It had never been used for solar observations but my application was accepted and with fellow post doc Michael Prata we produced the better data I needed. We had a fright when a software glitch in the control program sent the several-hundred-ton dish hurtling downward, narrowly missing Michael's new BMW 2002 sedan parked nearby. Doing science can be as exciting as the most important results!



Owens Valley Radio Observatory 130 ft telescope

After three productive years at CalTech I began to look around for a position back East. Hal spoke of a future for me at CalTech but I wanted to return to a more familiar environment closer to my family. He had asked me to fill in for him at a meeting of NASA group directors in Washington DC and there I met Professor Bob Noyes from Harvard University and Ed Reeves, the chief of the Harvard Smithsonian Solar Satellite Project. They invited me to visit Harvard and soon after, I accepted their offer of a research fellowship in astronomy.

So, in June 1972 I set out in my 1966 Morgan Plus 4 two-seater, crossing the desert and then the Rocky mountains and the Great Plains. I stopped in Madison, Wisconsin to give a talk on my S III observations of nebulae, invited by Professor Don Osterbrock, a renowned expert on the interstellar medium. Then I crossed into Canada and followed the Ottawa River to Montreal for my sister Jane's lovely wedding.

Hal and his post docs in 1972. From left: PVF, Jay Pasachoff, Katsuo Tanaka, Dainis Dravins, Hal, Arvind Bhatnagar.

The discovery of the fibril alignment switch, and especially of the sub-photspheric shear layer, were my most important achievements at Caltech. Neither required any deductive wizardry. They were both the product of simply noticing and pursuing lacunae - something that lay in plain view but had been overlooked by others.

Harvard: Cool loops, *Solar Astrophysics*, Astro 10 and first steps on solar brightness variation.

The Center for Astrophysics was more formal than CalTech, where Nobel prize winners like Richard Feynmann, Murray Gell-Mann and Max Delbruck wandered around the campus in shorts and Hawaiian style shirts. The solar group was focused on preparations for the flight of an extreme ultraviolet spectrophotometer on Skylab, the first space station. I was involved in preparation of the intricate observing sequences and in training the astronauts on how to carry them out, which required frequent trips to Johnson Space Center near Houston.

After the launch of the first Skylab mission in 1973, most of the solar group moved to the Clear Lake City location of JSC for the expected roughly year-long duration of the three planned missions. Our photometer was one of several solar telescopes on Skylab and their co-ordinated 24-hour operation was quite stressful. Tensions between the various research teams like ours and the Naval Research Lab group sometimes rose to un-scientific levels. As I soon discovered, there were tensions within groups as well.

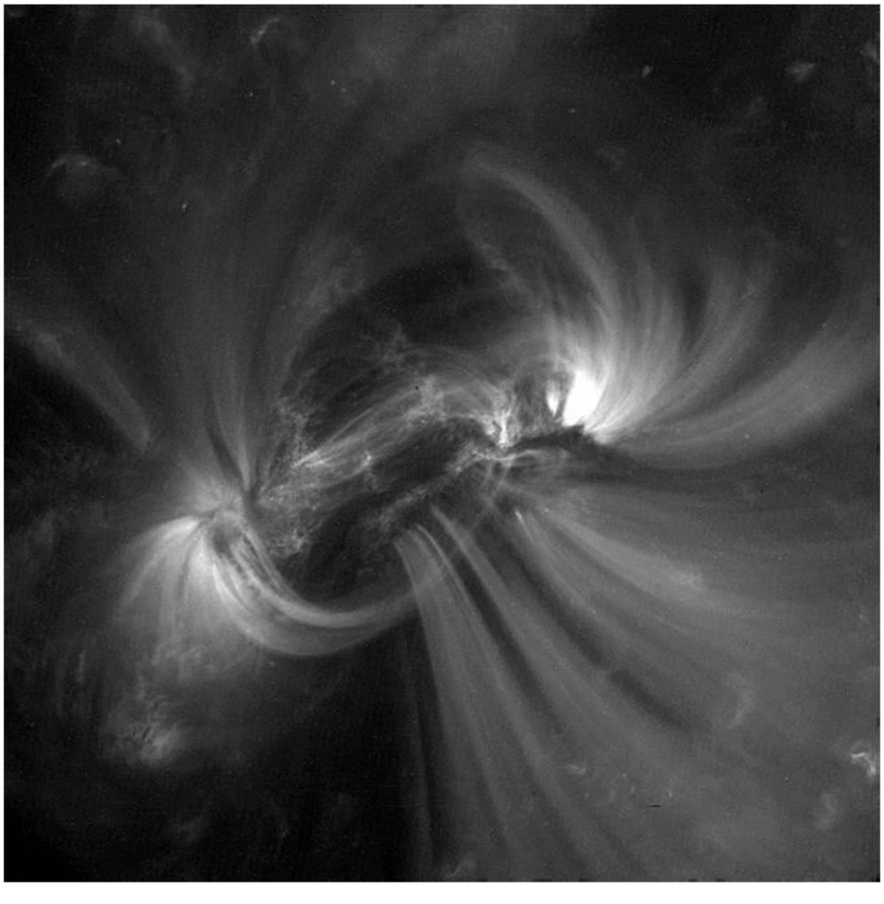
Spirited discussion in the Harvard solar group in Clear Lake City. Left to right: Jorge Vernazza, Ed Schmahl, George Withbroe and Bob Noyes.

Reading over our group's previous publications, I had noticed that the Harvard models of the Sun's EUV emitting layers were based on observations at relatively low angular resolution. In my first foray into research in this area I pointed out that their accuracy was questionable because the magnetic structures being observed were much smaller than the resolution of the data; I had been observing those structures at much higher resolution in visible light at BBSO. So I was aware that the modelled plasma densities, for example, were under-estimated by over an order of magnitude.

My attempts to discuss this with group members were not well received, in part probably because the competing NRL group was reporting such higher densities in their data obtained using a different diagnostic less sensitive to angular resolution. I believed that the issue needed to be aired so I published it anyway. The disagreement did not endear me to the more senior members of the group and thereafter I was viewed as something of a renegade.

a) Cool plasma in the hot corona

I noticed in our Skylab images that the brightest solar EUV emissions originated in plume-like magnetic structures over sunspots. I was new to EUV plasma diagnostics so Bob Noyes helped me with calculations showing that the plume plasma was no denser than its coronal surroundings. Subsequently I was able to show that the bright emissions were due to the unexpected presence of large volumes of cool plasma in the plume and loop structures connected to umbrae.



Cool sunspot loops

This raised the question of why the corona over intensely magnetized sunspots is so cool if coronae in the sun and stars are supposed to be heated by magnetic fields. My finding attracted attention and I was invited to give several talks at the Max Planck Institute in Garching, the ETH in Zurich and the Institut d'Astrophysique in Liege. I was even hailed by a kind Florentine colleague as the "Father of cool coronal loops.

The plasma diagnostics expert Carol Jordan from Oxford University spent some time at CFA at that time studying our EUV data. She used to enjoy a cigar in my office while we discussed a species of curious dark EUV structures we had noticed. I later published a paper showing that they contained plasma that was even much cooler than the spot plumes, immersed in the hot corona.

We collaborated on another paper with Andrea Dupree, CFA's expert on stellar UV emission. I knew little about the topic and my function was mainly as moderator between these two competitive women. Carole went on to become an Oxford professor of theoretical physics and was knighted by Queen Elizabeth. Andrea was later elected President of the American Astronomical Society. I've occasionally cited them as examples of successful female scientists whose careers do not seem to have been hampered much by discrimination.

b) *Solar Astrophysics*

Another notable visitor to CFA was Professor Tom Gold of Cornell who had become famous in the 1960's for providing the explanation of pulsars as rotating neutron stars, and for his warnings that NASA missions to the Moon would sink deep into a thick layer of dust. Tom was a renegade on a higher plane than I; it was rumored that he didn't know enough math to integrate sin θ, but he explained enthusiastically how it was he who had pointed out the importance of feedback in explaining the broad frequency sensitivity of the ear. He often came by to discuss various solar topics and proposed that we write a book together about the Sun.

That never happened, but instead I wrote my text *Solar Astrophysics* which appeared in 1990 and is in its 3rd edition. The large effort would not have been possible without my wife, Elisabeth's, support in handling the three young children in the morning. Writing it required rising unnaturally early for five years which probably contributed to the atrial fibrillation I developed about that time. But I am glad to have provided a comprehensive and balanced resource that is used world-wide by readers with a technical background.

c) Astro 10

I had enjoyed substitute teaching a few classes at Caltech for Hal and for Professor Jesse Greenstein, the Astronomy Department chairman so I was happy to volunteer when the opportunity arose to teach the astrophysics course for juniors and senior astro majors at Harvard. It was a full year survey of astrophysics split with Professor Charles Whitney, who had, many years ago, explained the puzzling brightness changes of Cepheid variables. Chuck advised me a bit at first in how to organize my half of the course but then let me go my own way.

Preparing the course from scratch required a great deal of effort but I was proud of some improvements I instituted like inventing some interesting labs for the students to perform at Harvard's little-used Agassiz Observatory about an hour's drive out of Cambridge. Several of the students were talented and probably the best, James Kasting, worked with me one summer and went on to a career as a prominent expert on astrobiology. Harvard rewarded me with a Lectureship and two thousand dollars.

d) First pass at solar irradiance variation

The EUV data were interesting but I wanted to find a project of my own, separate from the Skylab group research. One day while browsing in the CFA library I came across the proceedings of a conference honoring Charles Greeley Abbot, the first director of the Smithsonian Astrophysical Observatory. Abbot had become famous for a 30-year series of daily measurements of the Sun's total irradiance (TSI), or "solar constant" as it was then known. His value of this parameter provided, for decades, the best measure of the Sun's total light and heat output.

But Abbot became convinced that small fluctuations in the TSI influenced climate and even local weather. A couple of independent statistical analyses had reported that the variations he linked to climate were more likely to be instrumental calibration drifts or changes in atmospheric transmission in his ground based pyrheliometry. This tarnished his reputation and, since the end of his measurements in the 1950's, cast a pall over the study of Sun-climate influences.

I noticed that the statistical analyses had focused on variations over time scales of years to decades of interest to climate studies and I wondered whether his data might reveal fluctuations on the few-day time scale expected from evolution of sunspots and faculae. Such variations were less prone to instrumental drifts. To investigate this I undertook a study of the *differences* of Abbot's values between days of high and low sunspot or facular area, but separated by only a few days; essentially high-pass filtering.

This first study, carried out with my Argentinian colleague and friend Jorge Vernazza, and Pamela Mack, a Radcliffe undergraduate research assistant, showed a tendency for the Sun to dim when spot area was high and brighten when large faculae were present. In our paper we showed that the small amplitude of the variation agreed roughly with that expected from the photometric contrasts and projected areas of these structures on the Sun's disk.

A more sophisticated follow-up study of the entire daily time series with Jorge confirmed these findings and attracted considerable attention. Our result complemented the widely heralded previous finding by Jack Eddy that the 17th century hiatus of solar activity known as the Maunder Minimum corresponded suggestively with the so-called Little Ice Age. Our findings raised hopes that the connecting mechanism might be TSI variation.

Sun-climate studies that had languished since the 1950's suddenly became the New Big Thing and I was invited to give reviews on solar constant variation at several meetings in the US, Europe and even India. When the CFA recently made the sad decision to close its venerable library, I wrote to the departing librarian how important browsing its stacks had once been in determining the focus on my research.

My most important achievement

But these developments were eclipsed by the most important event in my life – meeting Elisabeth Evans and persuading her to marry me. Lizzie was the younger daughter of a school girl friend of my mother in Prague, who had married an American journalist after the war. I visited them at their home in New York soon after Elisabeth's father, the editorial page editor of the Wall Street Journal, died unexpectedly at the age of fifty-two. Lizzie was only fifteen but I fell hopelessly in love with her dark eyed beauty and cheerful personality. I sent her some funny postcards at first, we began dating two years later and were married in 1975 when Elisabeth was nineteen.

Honeymooner

The eleven-year age difference between me and Lizzie, a sophomore at Barnard College, caused some consternation amongst well-meaning friends. My parents, on the other hand, were very happy that I had had the good fortune to find a lovely, intelligent and kind girl who spoke Czech. I never doubted that Elisabeth's self-knowledge, energy and level headed approach to life provided a strong basis for a happy marriage. We had the courage to go our own way and now, after three wonderful children and six energetic grandchildren, we look forward to our 50th wedding anniversary next year.

A nice stay in Nice and a lesson learned.

In 1978 I was awarded a NATO Senior Fellowship and opted to spend six months at the Nice Observatory. Lizzie was able to pursue her love of ballet at Princess Grace's dance academy in Monaco a few miles down the coast, and we travelled to France, Italy, Switzerland, Czechoslovakia and Germany where I was invited to give talks. The opportunity to discuss some lingering questions I had about the explanation of higher solar field rotation with the fluid dynamicist Douglas Gough visiting from Cambridge University, was a corollary pleasure.

Shortly after arriving in Nice I happened on an article by an Australian shock wave expert reporting that their damping was expected to decrease when they propagated through an expanding atmosphere. It occurred to me that this might explain how shock waves might accelerate spicules, the tall gas jets observed at the solar limb. Past studies had found that shock waves strong enough to propel these massive spicules well above their ballistic trajectory would be heavily damped by radiation and conduction.

The published equations indicated that, in the Sun's expanding atmosphere, the transformation of a simple wave driving such a spicule into a dissipative shock would be delayed by the stretching caused by the increase of expansion velocity with height. The numbers indicated that I may have happened on the solution to the classic problem of spicule acceleration and I presented the results in a colloquium delivered in French before leaving Nice. It was encouraging that the well--known French theorists Evry Schatzman and Paul Delache in the audience agreed that the effect seemed reasonable.

Luckily, the paper I submitted when we returned home was sent to Roger Kopp, a congenial colleague at the High Altitude Observatory in Boulder Colorado, to referee. He had tried using the same equations in his Ph.D. work but unlike me, he had taken the time to re-derive them and found an algebra error. In their correct form the equations showed much less of an effect. That saved me the embarrassment of a retraction and taught me to at least try to re-derive theoretical results before adopting them.

Entrepreneurship

After returning from France in 1978, I began making preparations to leave Harvard and join ., Atmospheric and Environmental Research, Inc., a start-up company founded a few months earlier by a few post docs and faculty in the Harvard Atmospheric Sciences Department. Bob

Noyes had assured me that the CFA would find me a position if I stayed on. But after three years as a post doc at CalTech and six as a Research Fellow and Lecturer at Harvard I was more intrigued by entrepreneurship.

I had submitted a proposal to the NSF and the Program Director had confirmed that he could fund it through AER if I moved there. He stressed that supporting myself on soft money was risky but NSF regulations did not prevent him from funding a profit-making firm like AER. My colleague Jorge Vernazza joined me at first but accepted a position at the Lawrence Livermore Lab in California soon afterwards. He had less confidence in entrepreneurial experiments.

I had achieved some prominence through my work on the solar rotation, the cool loops, and especially on TSI variation, so I had confidence that I could make a go of it. Sun-climate influences were attracting more attention with the availability beginning of the first continuous TSI measurements from above the atmosphere obtained by John Hickey's group at Eppley Labs. The improved data obtained from Richard Willson's ACRIM radiometer beginning in 1981 first showed clear dips in TSI associated with the rotation of sunspots across the Sun's disk.

How do sunspots decrease the TSI?

Attention turned to the mechanism by which spots decreased the TSI. Eugene Parker, the most celebrated solar theorist of the past generation, calculated that, if the spots simply blocked the Sun's convective heat flow as thermal plugs, the heat leaking around the plug should create a bright rim that was not observed. He proposed instead that the heat was converted to Alfven waves that dissipated in the overlying corona whose EUV and X ray radiations were not detected by the TSI radiometers. But observations of velocities in spot umbrae showed no sign of the required enormous wave flux which, in any case, would have far exceeded the radiation from the entire corona.

Others suggested that the missing spot heat flux was routed through sub-photospheric magnetic connections to the bright faculae accompanying spots. But faculae were known to far outlive the associated spots and no convincing mechanism to route the spot flux to them was identified. So the problem remained unsolved.

I had noticed a paper by the young Dutch solar physicist Henk Spruit, in which he showed that the bright rings could be avoided if a more realistic depth profile of eddy thermal conductivity (instead of a constant value) were used. Spruit's model, like Parker's, assumed that the spot already existed. I wondered how the heat flux around the spot would behave in a time dependent calculation which followed the evolution of the heat flux after a spot is inserted.

I raised this question with Henk when I ran into him during a visit to HAO, where he was working. He agreed that the idea was interesting and could be checked with a relatively straightforward analytical model. Back at AER, I gave the problem to a post doc I had recently hired from MIT. He addressed the problem using numerical, rather than analytical, integration.

To my surprise, his model showed no sign of any perturbation to the heat flux around the spot, even when run to simulate behavior for over a month after insertion of spots represented as thermal plugs. I was convinced that there must be an error in his integration and sought the advice of Michael Livshits, an applied mathematician recently arrived from Russia. Michael had experience with heat flow problems and was not surprised at the result. Repeating the calculation for shallower plugs and longer run times he was able to show that the isotherms around the spot did slowly change, in response to heat storage in the sub-photospheric layers around the spot.

Henk sent me a copy of the paper he was submitting which confirmed the efficient heat storage we saw but by using an analytical approach, he was able to show that the storage time of the blocked heat flux throughout the Sun's convection zone was of order 100,000 years. It also showed that the expansion of the Sun caused by storage of the blocked heat flux was un-observably small. Our numerical calculation offered complementary information on the deformation of the isotherms around the spot and the dependence of the storage on the spot size and depth. Henk's two papers appeared in 1981 and ours appeared a few months later.

One solar physicist renowned for his calculations of radiative transfer in the Sun's atmosphere protested that the model couldn't be correct because "heat didn't flow backward'. I countered with the analogy that the water level behind a new dam rises although the water never stops flowing downhill.

More knowledgeable criticism came from colleagues who had developed sophisticated numerical simulations of turbulent convection, saying that our long storage time was caused by using a simplistic mixing length model of solar convection. It took some time for them to understand that the long storage times were the result mainly of our use of a radiative upper boundary and were insensitive to the convection model used.

Even now, almost a half century later, most solar and stellar astronomers are unaware that the presence of luminosity fluctuations on the Sun and similar stars is due to the rather subtle combination of very high thermal diffusivity and relatively low radiative efficiency of a star's convection zone.

I put the converse problem of explaining the TSI *increase* caused by bright faculae to W. Chiang, another post-doc, and we published a paper soon afterward on this solar luminosity increase when faculae are modelled as thermal short circuits, instead of thermal plugs.

A few years later, one of Gene Parker's grad students at the University of Chicago sent me a copy of the Ph.D. thesis in which he had duplicated Henk's analytical calculation, not realizing that it had already been published. Gene apologized for not being aware of Henk's and our papers but it was nice to see our somewhat anti-intuitive results confirmed by a colleague celebrated for his once-iconoclastic prediction of the supersonic solar wind.

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A THERMAL MODEL OF SUNSPOT INFLUENCE ON SOLAR LUMINOSITY

P. FOUKAL, L. A. FOWLER, AND M. LIVSHITS
Atmospheric and Environmental Research, Inc., Cambridge, Mass.
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ABSTRACT

Recent measurements of the solar irradiance have confirmed that sunspots block energy flow to the photosphere in rough proportion to their area and photometric contrast. We have constructed a time-dependent two-dimensional model of heat flow blocking in a turbulent convective layer, to investigate the physical interpretation of the observed irradiance dips. Our numerical model shows how formation of a spot at or below the photosphere leads to heating of surrounding convective layers over a diffusive time scale $\tau_D \sim 10^4$ s. This heating rapidly propagates outward, storing the blocked heat throughout the convection zone. The stored thermal (and potential) energy is only released very slowly by radiation through a gradually increasing photospheric temperature. The very long radiative time scale $\tau_R > 10^{10}$ s for this release is quite insensitive to reasonable uncertainties in the model parameters or the diffusion approximation we have used. We point out that this very efficient storage implies a sunspot contribution to the modulation of $L_\odot$ over the 11 year cycle, at a level somewhat below 0.1%. Our study indicates that the amplitude, duration, shape, and phase of the observed spot-correlated irradiance dips can be easily explained by extending a conventional thermal blocking model of spots to include time dependence. We find no reason to expect that the contribution of faculae to S and $L_\odot$ cancels that of spots, on any time scale.

Subject headings: radiative transfer — Sun: activity — Sun: general — Sun: sunspots

I. INTRODUCTION

Radiometers on the *Solar Maximum Mission* and *Nimbus 7* satellites show clear depressions in the solar irradiance S, whose timing correlates well with disk passage of large sunspot groups (Willson *et al.* 1981; Hickey *et al.* 1980). The amplitude (0.2%–0.4%), duration (10–20 days), and shape of many of the larger dips agree well with the irradiance decrease calculated (Foukal, Mack, and Vernazza 1977; Willson *et al.* 1981), on the assumption that spots block photospheric heat flow in proportion to their area and bolometric contrast.

The irradiance dips are too large to be compensated by observed ultraviolet flux variations below 0.18 μm (e.g., Heath 1980; Heath and Thekaekara 1977), to which the radiometers may be less sensitive. Nevertheless, the dips do not directly require changes in solar luminosity, $L_\odot$. For instance, the missing flux might emerge at a large angle to the radiometer's line of sight, in the anisotropic radiation field of bright magnetic faculae (e.g., Chapman 1980; Oster, Schatten, and Sofia 1982). But large facular areas often occur without detectable sunspots on the disk, and their lifetimes are also much longer (e.g., Kiepenheuer 1953). Detailed balance of radiative fluxes between spots and faculae would then require efficient transfer and storage of roughly 10^{36} ergs over months between these magnetic features. A physically convincing mechanism to achieve this detailed balance has yet to be put forward.

We show in this paper that the observed properties of the irradiance dips can be easily explained by the conventional thermal blocking model of sunspots (Biermann 1941; Cowling 1976) extended to include time dependence. Our model does not rule out energy transfer between spots and faculae, but it does not require it. Our analysis in §§ II–III indicates that the heat blocked (in proportion to a spot's area and contrast) is stored very efficiently in the slightly increased thermal (and potential) energy of the solar convection zone. The radiative flux blocked during high sunspot activity periods is only radiated away over many subsequent 11 year cycles. In § IV we point out that this efficient storage implies a contribution to variation of $L_\odot$ and S over the 11 year cycle, at an amplitude that can be computed from the known variation of sunspot areas.

II. A TIME-DEPENDENT THERMAL SUNSPOT MODEL

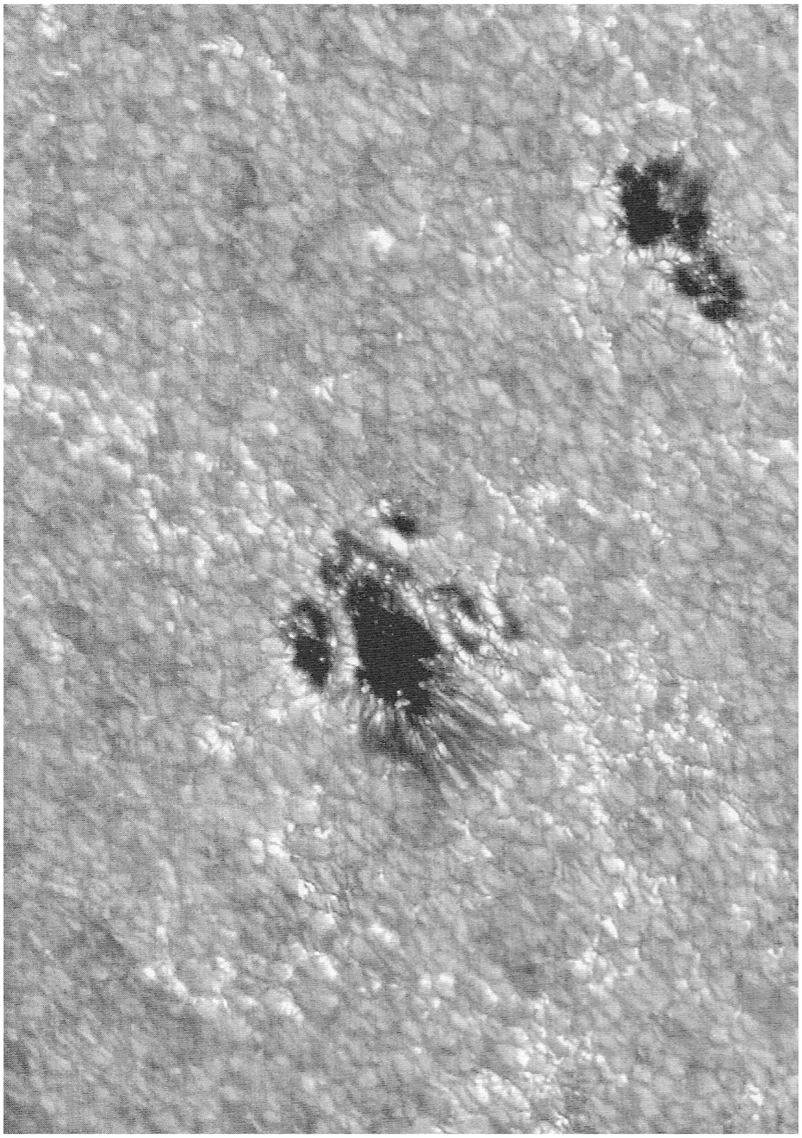
a) *Physical Assumptions*

Recent work on thermal obstacles in the solar convection zone (e.g., Spruit 1977; Clark 1979) shows that the most restrictive photometric features of a spot, namely the sharpness of the umbral edge and faintness of any photospheric bright ring around the penumbra, can be explained in terms of a relatively simple diffusion ap-

Why does TSI increase when dark spots are most numerous?

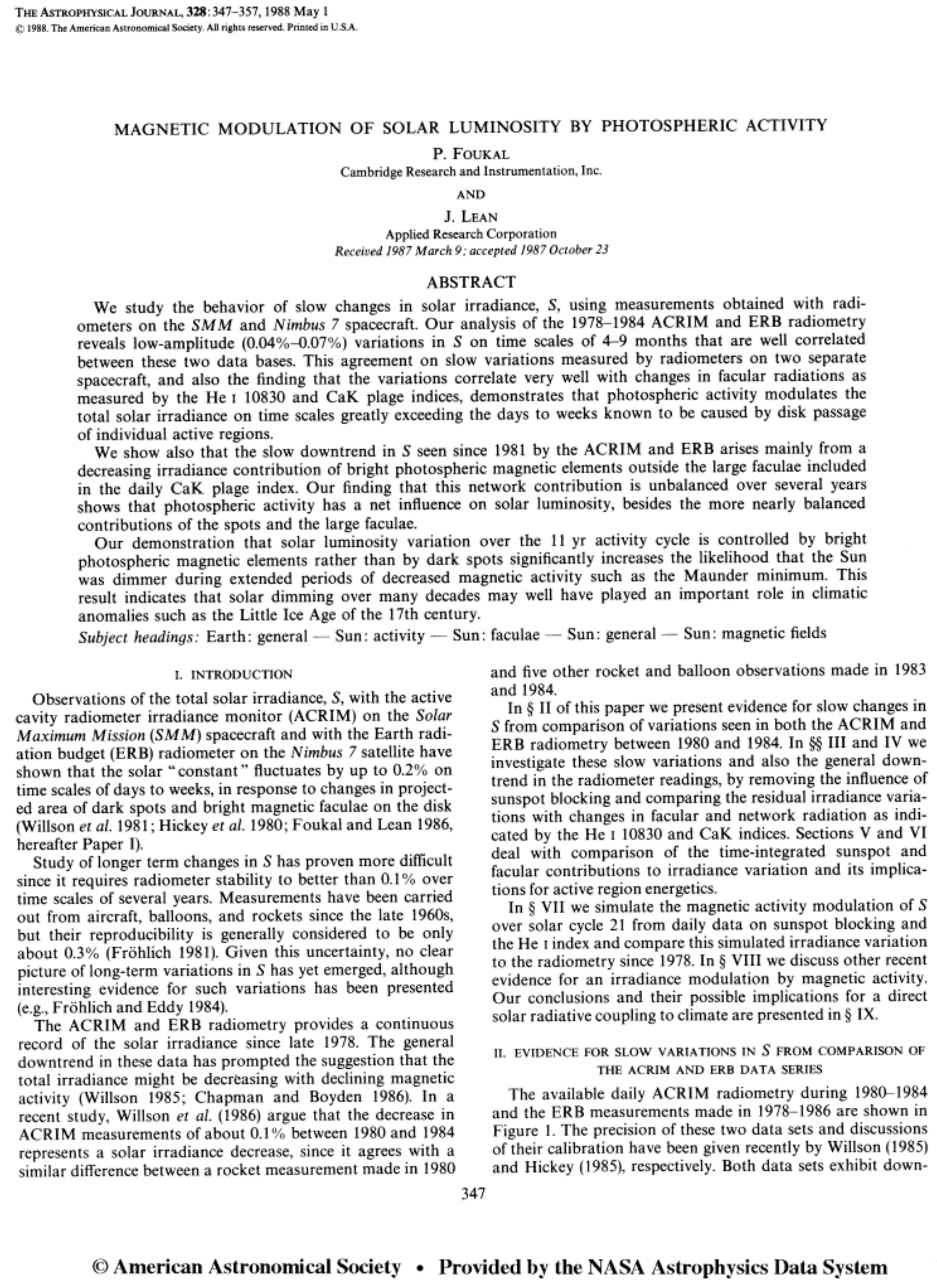
Analysis of the ACRIM radiometry by Dick Willson, Hugh Hudson and their collaborators showed already in 1981 that spots were responsible for the TSI dips on time scales of solar rotation. They also found evidence suggesting that TSI increases discernible in the residuals after the spot signal was removed, were due to bright faculae. But since the brightest faculae occurred together in the same active regions as spots their analysis couldn't rule out that the residuals were caused instead by error in the uncertain sunspot photometric contrasts.

Working with Judith Lean at the Naval Research Laboratory in Washington, D.C., we were able to show, using auto-correlation analysis, that the TSI residuals had lifetimes of several months. This was similar to facular lifetimes, but significantly longer than those of spots. Our finding completed the evidence that the TSI variation observed on the time scale of days to months was caused by the rotation and evolution of magnetic active regions.



Bright faculae around dark spots

It remained to explain why, despite the darkness of spots, TSI *increased* at high sunspot number around the maximum of the 11-year sunspot cycle. Judith and I were able to show that this component of TSI behavior was not captured in correlations with indices of active region structures like spot number or area of the Ca K plages that overlay AR faculae.



THE ASTROPHYSICAL JOURNAL, **328**:347–357, 1988 May 1

MAGNETIC MODULATION OF SOLAR LUMINOSITY BY PHOTOSPHERIC ACTIVITY

P. FOUKAL
Cambridge Research and Instrumentation, Inc.
AND
J. LEAN
Applied Research Corporation

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ABSTRACT

We study the behavior of slow changes in solar irradiance, S, using measurements obtained with radiometers on the *SMM* and *Nimbus 7* spacecraft. Our analysis of the 1978–1984 ACRIM and ERB radiometry reveals low-amplitude (0.04%–0.07%) variations in S on time scales of 4–9 months that are well correlated between these two data bases. This agreement on slow variations measured by radiometers on two separate spacecraft, and also the finding that the variations correlate very well with changes in facular radiations as measured by the He I 10830 and CaK plage indices, demonstrates that photospheric activity modulates the total solar irradiance on time scales greatly exceeding the days to weeks known to be caused by disk passage of individual active regions.

We show also that the slow downtrend in S seen since 1981 by the ACRIM and ERB arises mainly from a decreasing irradiance contribution of bright photospheric magnetic elements outside the large faculae included in the daily CaK plage index. Our finding that this network contribution is unbalanced over several years shows that photospheric activity has a net influence on solar luminosity, besides the more nearly balanced contributions of the spots and the large faculae.

Our demonstration that solar luminosity variation over the 11 yr activity cycle is controlled by bright photospheric magnetic elements rather than by dark spots significantly increases the likelihood that the Sun was dimmer during extended periods of decreased magnetic activity such as the Maunder minimum. This result indicates that solar dimming over many decades may well have played an important role in climatic anomalies such as the Little Ice Age of the 17th century.

Subject headings: Earth: general — Sun: activity — Sun: faculae — Sun: general — Sun: magnetic fields

I. INTRODUCTION

Observations of the total solar irradiance, S, with the active cavity radiometer irradiance monitor (ACRIM) on the *Solar Maximum Mission* (*SMM*) spacecraft and with the Earth radiation budget (ERB) radiometer on the *Nimbus 7* satellite have shown that the solar "constant" fluctuates by up to 0.2% on time scales of days to weeks, in response to changes in projected area of dark spots and bright magnetic faculae on the disk (Willson *et al.* 1981; Hickey *et al.* 1980; Foukal and Lean 1986, hereafter Paper I).

Study of longer term changes in S has proven more difficult since it requires radiometer stability to better than 0.1% over time scales of several years. Measurements have been carried out from aircraft, balloons, and rockets since the late 1960s, but their reproducibility is generally considered to be only about 0.3% (Fröhlich 1981). Given this uncertainty, no clear picture of long-term variations in S has yet emerged, although interesting evidence for such variations has been presented (e.g., Fröhlich and Eddy 1984).

The ACRIM and ERB radiometry provides a continuous record of the solar irradiance since late 1978. The general downtrend in these data has prompted the suggestion that the total irradiance might be decreasing with declining magnetic activity (Willson 1985; Chapman and Boyden 1986). In a recent study, Willson *et al.* (1986) argue that the decrease in ACRIM measurements of about 0.1% between 1980 and 1984 represents a solar irradiance decrease, since it agrees with a similar difference between a rocket measurement made in 1980 and five other rocket and balloon observations made in 1983 and 1984.

In § II of this paper we present evidence for slow changes in S from comparison of variations seen in both the ACRIM and ERB radiometry between 1980 and 1984. In §§ III and IV we investigate these slow variations and also the general downtrend in the radiometer readings, by removing the influence of sunspot blocking and comparing the residual irradiance variations with changes in facular and network radiation as indicated by the He I 10830 and CaK indices. Sections V and VI deal with comparison of the time-integrated sunspot and facular contributions to irradiance variation and its implications for active region energetics.

In § VII we simulate the magnetic activity modulation of S over solar cycle 21 from daily data on sunspot blocking and the He I index and compare this simulated irradiance variation to the radiometry since 1978. In § VIII we discuss other recent evidence for an irradiance modulation by magnetic activity. Our conclusions and their possible implications for a direct solar radiative coupling to climate are presented in § IX.

II. EVIDENCE FOR SLOW VARIATIONS IN S FROM COMPARISON OF THE ACRIM AND ERB DATA SERIES

The available daily ACRIM radiometry during 1980–1984 and the ERB measurements made in 1978–1986 are shown in Figure 1. The precision of these two data sets and discussions of their calibration have been given recently by Willson (1985) and Hickey (1985), respectively. Both data sets exhibit down-

It correlated better with whole-disk indices like the microwave flux or the global emission in Lyman alpha, both of which included the additional emission from the photospheric magnetic network. This led us to identify changes in area of enhanced network outside active regions as the 3rd component of TSI variation. Our 1988 paper reporting this result rated a cover in the journal *Science* and remains amongst the most cited in solar- terrestrial research.

Afterward, Judith urged me to join her in extending these analyses backward in time into the Maunder Minimum of greatest interest to climate researchers. I was reluctant to participate, preferring to focus on photometric studies of the photosphere to better understand possible other sources of TSI variation besides the magnetic structures that had already been implicated.

The model of TSI variation Judith developed subsequently proposed sufficient solar dimming to help explain climate cooling during the Maunder Minimum. It was widely adopted by the climate community and gained her several awards including membership in the National Academy of Science. But it was shown to be incorrect by the time of the awards and her failure to retract it in timely fashion unfortunately created a rift between us.

Are there other sources of TSI variation besides photospheric magnetism?

a) Photospheric limb darkening variation.

Together with Larry Petro, a talented post-doc from MIT, we carried out a several-year program of precise photometric observations at the McMath telescope on Kitt Peak, searching for variations in photospheric limb darkening. Changes in the Sun's global structure and luminosity might produce such variations possibly independently of surface magnetic structures. We showed from modeling that this technique was sufficiently sensitive to detect luminosity variations that might be difficult to recognize by radiometry due to instrumental calibration drifts over multi-decadal time scales. But the limb darkening proved to be remarkably stable down to well below the 0.1% level.

b) Giant convective cells.

The scales of solar convection remain unknown, except for the small granules seen at the photosphere. Careful analysis of full-disk photospheric images searching for large scale convection cells with Lee Fowler and W. Chang, two other post docs, also proved fruitless. Variation in their number and contrast might be expected to produce detectable luminosity variations possibly un-correlated with magnetic activity, but no such evidence was found.

c) A new technique: photospheric temperature *gradient* measurements

While obtaining the images for such studies at Kitt Peak with my colleague Tom Duvall, we happened on a new photometric technique. We noticed that, if an image was recorded in the *difference* of two continuum channels spaced far apart on the solar spectrum, facular regions normally detectable only near the limb popped out at high contrast across the whole disk.

After publishing an incorrect paper reporting that this surprising visibility was due to the difference in color temperature of faculae and photosphere, I realized that the real reason was that we were actually measuring the temperature *gradient* between two different levels in the Sun's grey (not black-) body atmosphere. Our observations confirmed that the temperature gradient in the facular atmosphere was lower than in the photosphere, as predicted by Henk Spruit's dynamical models.

d) Infrared imaging: faculae are dark at their deepest levels.

In 1990 I learned that the Air Force Geophysics Lab near Boston had acquired one of the first Pt Si infrared imaging cameras and that they were willing to help me try it out for the first two-dimensional IR imaging of the photosphere. Years before, Pete Worden had found evidence from scans with a linear array at Kitt Peak that the bright magnetic network looked dark when observed around 1.6 microns in the IR where photospheric transmission was highest, enabling a view of their deepest levels.

Using the small coelostat on the roof of the Harvard Science Center, we were able to obtain images showing some network and faculae that were bright in the Ca K chromospheric line that marked magnetic elements, but were dark at 1.6 microns. This was confirmed in better observations we carried out later using an InSb array at the large McMath telescope at Kitt Peak. These observations confirmed the model prediction by Henk and others, that faculae were dark at the deepest observable levels.

Together, these findings showed that TSI variation seemed to arise only from changes in photospheric magnetic structures; there was no evidence that changes in non-magnetic, larger scale structures made a significant contribution. They were summarized in a widely referenced review paper with Henk, Claus and the climate modeler Tom Wigley, that made the cover of *Nature* in 2006.

Nature cover 2006

Cambridge Research and Instrumentation.

Some of the work described above was carried out after I had left AER in 1985 to form our own firm, Cambridge Research and Instrumentation, Inc. Differences over the division of profits from sales of commercial instruments were the main reason for the separation.

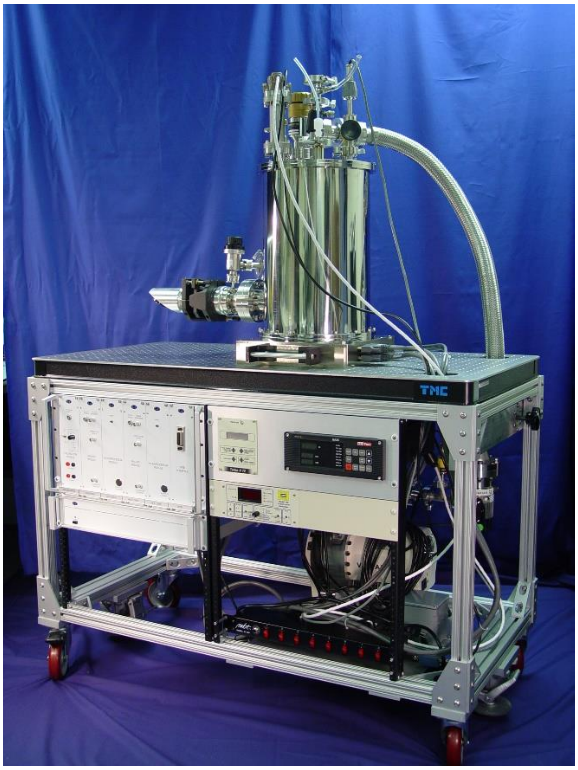
Peter Miller, a very capable electrical engineer who had joined me a few years earlier at AER, also made the move with me. Cliff Hoyt, another capable Williams College graduate working with me promised to join us after he graduated from his MIT Masters course in mechanical engineering in two years.. Elisabeth signed up as CFO while raising two, soon to be three, youngsters. Elisabeth and I had to put our house on the line to secure a line of credit from the bank, but we were adventurous.

Peter and I set up shop in a room at 21 Erie St., a converted brick mill building near MIT. My plan was to continue the mix of solar research and commercial instrument building that we had pursued at AER. We hoped to commercialize a cryogenic radiometer and also a servo system for removing intensity fluctuations in laser beams, that Peter had perfected while at AER.

Our LaseRad radiometer was based on a concept originally pioneered at NIST in Gaithersburg, MD. The National Physical Lab in the UK had shown that their version was able to deliver an order of magnitude better accuracy in light flux measurements than conventional radiometers. Peter had much improved the electronics and I modified the receiver module and we packaged the instrument more compactly than a competitive version sold by Oxford Instruments, a large cryogenic instrumentation manufacturer in the UK.

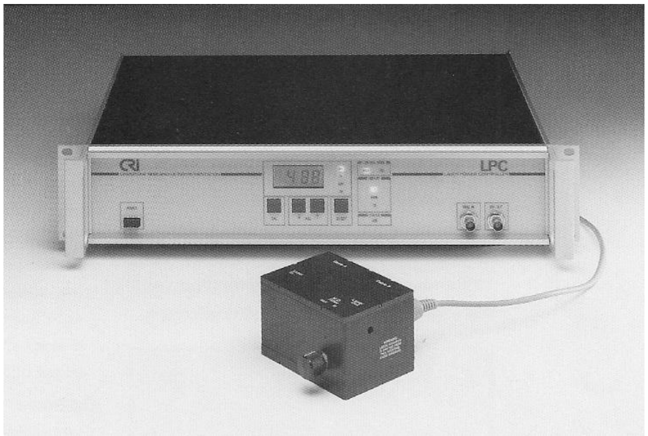
The CRI management team: Elisabeth, Peter Miller, Cliff Hoyt, PVF

I organized a small international workshop of radiometrists to put CRI on the map, so to speak. This meeting gathered, for the first time, individuals from metrology labs like NIST and NPL and users from atmospheric and solar physics as well as those from the defense industry. It was a big success and became a biennial event called *NewRad,* almost forty years later still attracting several hundred participants to venues at national metrology labs around the world.



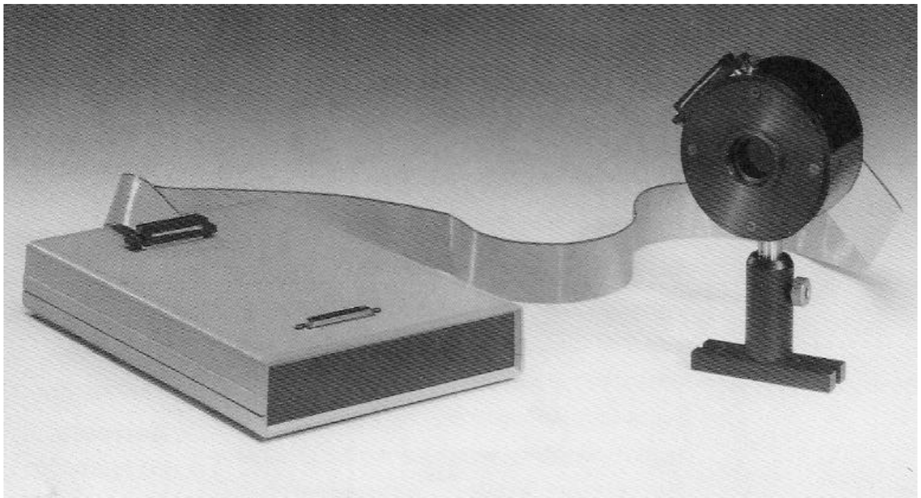
LaseRad built under license to CRI by L-1 Inc., for the University of Colorado.

Thanks to such energetic marketing our LaseRad was purchased by more than half of the major national labs, including the US, Canada, France, Germany and the Scandinavian countries. Our laser servo, also a much-improved version of a NIST design, provided the only sufficiently stable light source for users of the LaseRad and the Oxford competitor, so it sold well.



CRI LPC laser stabilizer

Peter and Cliff later also built on another suggestion from Ed Zalewski at NIST, to explore the uses of liquid crystals as tunable light filters and polarization analyzers. The VariSpec filter and its offshoots opened much larger markets in biomedical applications. We had, since the founding of CRI in 1985, been very successful in attracting funding from both the pure research and Small Business Innovation Research arms of the NSF, NASA and NOAA. Now this was augmented by SBIR funding from the National Institutes of Health (NIH).


The VariSpec filter

In 1999 Elisabeth and I decided to take the advice of some of our Board members to sell them the company and enable them to grow it to the next level. CRI had fulfilled my aim of providing a stable environment for my solar research in a location where Lizzie and I wanted to live. I had little interest in growing it beyond its 15-20 employees, but I realized that it would be unfair to Peter and Cliff to stand in the way of their hopes of realizing a larger commercial potential.

The new owners invested private funds, tripled the head count, and sold a few years later to Caliper Life Sciences, Inc., which was itself soon purchased by the PerkinElmer Corp., the largest US optical manufacturer (and builder of the Hubble Space Telescope). The technologies we had developed since 1985 fulfilled their potential in worldwide applications including metrology, cancer research, and IVF.

Plasma electric fields revisited

Still at AER I had pursued my interest in solar plasma electric fields by analyzing spectra of the large prominences observed at the solar limb, taken at the Sacramento Peak Observatory (SPO) The spectral lines higher in the Balmer series than the H-alpha I had observed at BBSO exhibited greater Stark effect broadening. But this broadening had previously been ascribed to the small-scale e-fields expected from collision between hydrogen atoms and electrons. I showed that, at least in the brightest prominences, the plasma densities measured by using the intensity ratios of density-sensitive emission lines seemed insufficient to entirely account for the Stark broadening, suggesting a contribution from larger-scale plasma electric fields.

But the broadening and density diagnostics were not accurate enough to prove the point – a more sensitive technique was required. Together with Tom Moran a talented post doc from MIT, we built an electrograph to search for the polarization signature of Stark effect caused by large scale fields. We installed this at SPO and a Czech astronomer whom I had invited lived at SPO for several months to operate it.

The Stark effect increases with wavelength so I also tried polarimetry at much longer infrared wavelengths around 11 microns, using the Fourier Transform spectrometer at the McMath telescope on Kitt Peak. Unfortunately, the Doppler broadening due to motions of the gas atoms also scales as the wavelength, so the results were no better than in the visible and near-IR.

This electrograph was perfected later by Brad Behr, a clever Williams College grad who worked with me for a year before embarking on an astronomy Ph.D.. We published his measurements of a solar surge eruption showing a promising polarization signature, but never followed up after Brad left for CalTech. The search is now being continued by a Japanese group using the giant DKIST telescope on Haleakala.

The Solar Bolometric Imager

While still at CRI, I had worked with Scott Libonate, a skillful post doc, to build a novel telescope that could image the Sun in total, wavelength-integrated light, with constant response for all included wavelengths. Such a telescope could measure the uncertain photometric contrasts of faculae with the same response as the space borne radiometers measuring TSI variations. This would remove the main uncertainty in reconstructions of TSI variation. Wide-band facular contrasts so far had been interpolated from monochromatic measurements at a few wavelengths, or estimated from uncertain models of facular atmospheres.

I had noticed a technology developed for the military for night viewing in the infrared, that might make this novel imager possible. We acquired a few of the thermal arrays and I tried depositing gold black on them using a vacuum chamber at NIST without destroying the tiny array. It was a delicate procedure but amazingly, it worked.

We removed the aluminum coating from the mirrors of a commercial reflecting telescope to enable direct viewing of the Sun without any filters that would introduce a wavelength dependent transmission. The bare glass had relatively constant response over most of the solar spectrum. We used this prototype to obtain the first solar imagery limited only by the Earth's atmospheric transmission. To do better we needed to get above the atmosphere.

A colleague, David Rust, at the Applied Physics Lab of Johns Hopkins University, had just flown a standard solar telescope on a high-altitude balloon and he agreed to make his gondola and facilities available. After several years of effort by Pietro Bernasconi and the APL engineering staff the Solar Bolometric Imager was transported to the NASA Balloon facility at Ft. Sumner, N.M.

The first flight yielded over 100,000 images of the Sun and its faculae and spots, in integrated light from about 320 nm in the ultraviolet through about 3000 nm in the IR. We published the first measurements of these structures' integrated light photometric contrasts, but a longer flight would be required to measure these values over a range of disk positions, using the Sun's rotation.

SBI before launch

This necessitated a flight lasting at least a couple of weeks-achievable only from Antarctica where a balloon can orbit the Earth forever without passing over inhabited territory. Pietro led the team to McMurdo base in 2006 and I joined them for the launch but the tape drive froze at altitude and the expensive mission had to be scrapped. Our proposals to re- fly the SBI on a satellite were unsuccessful because its objective didn't engage a large enough community to justify the expense. So the potential of the SBI went unfulfilled but it had been an interesting experiment.

Heliophysics, Inc: TSI behavior at the extremes of solar activity.

By the time we flew the SBI from McMurdo in 2006 I had re-incorporated as a one-man show named Heliophysics, Inc. I wasn't ready to retire at age fifty-five after we sold CRI in 1999 and incorporation enabled me to still obtain funding for the SBI and other research.

a) Solar dimming at lowest activity levels like the Maunder Minimum

I noticed a finding by a Spanish colleague Ada Ortiz that, surprisingly, the photometric contrast of the bright faculae and network *increased* with decreasing area. This was, again, predicted by Henk's original, time- independent model, but hadn't been observed previously. Consequently, I pointed out, removal of even the smallest photospheric magnetic elements during a long hiatus in solar activity like the Maunder Minimum would decrease TSI more than had been estimated from models that assumed their contrast was independent of size.

This argument was supported by my Swiss colleague Claus Frohlich's finding that the TSI values measured by his space-borne radiometer during the extended 2007-2009 solar activity minimum dipped well below the modeled values. This activity lull lasted only a couple of years, so an even larger TSI decrease during the 70- year Maunder Minimum seemed likely.

In a paper in 2011 with Ortiz and Scherrer we argued that the 17th century TSI might have dipped to a value twice as far below the normal quiet Sun value as estimated from standard models. The dip was probably still only by about 0.15%, but perhaps sufficient to provide detectable driving of climate models.

b) Even greater solar dimming at the *highest* solar activity levels?

In 1991 I had published a paper titled "The Curious Case of the Greenwich Faculae" drawing attention to the unexpected *decrease* in facular area at high levels of spot area, seen in the 102-year record of solar activity compiled by the Royal Greenwich Observatory (RGO) between 1874 and 1976. This had been pointed out earlier in a little-noticed 1980 paper by the Welshmen G. Brown and D. Evans.

I confirmed that their result had not been caused by observational effects and pointed out in a short 1994 *Science* paper how it might explain why the Sun's brightness was anomalously constant compared to other similar stars. The reason seemed to be that, over most of its activity range, the area of dark spots and bright faculae varied in unison, roughly cancelling their opposite effects on brightness. It was only at the highest activity levels, such as those seen on younger late type stars, that the spot dimming dominated.

Building on this earlier work, I reconstructed TSI variation using the RGO spot and facular areas. This showed that in the 1940's and 50's, during the highest activity levels ever recorded, spot dimming seems to have dominated facular brightening sufficiently to decrease TSI to levels equal to, or lower than, estimated for the Maunder Minimum.

This surprising result had been obscured in TSI reconstructions (including those by Judith Lean and myself) since the 1980's because they had used the better-known time series of Ca K plage areas, and even the sunspot number, as a proxy of the facular contribution to TSI. These time series failed to capture the negative correlation of faculae versus spots at high activity because they included essentially all magnetic structures that were bright in Ca K, including the large number of small spots.

This finding, published in the *Astrophysical Journal* in 2015, indicates that the Sun is unusual in being poised on the transition between older solar-type stars that brighten with increasing activity and younger ones that darken. It also has implications for predictions of future behavior of TSI on climate. TSI decrease during a future solar cycle as large or larger than cycle 19 in 1957 could help mitigate global warming. Interestingly, it has been cited only eight times in nine years-perhaps because of concern that publicizing this result might encourage skeptics of global warming.

Solar cycle prediction

Prediction of the amplitude and timing of the maxima of the quasi-11 yr sunspot cycle is one of the most important aims of solar terrestrial research and many different schemes have been proposed. Yet, prediction accuracy remains low. So I was surprised that a better technique proposed by Brown and Evans in their statistical study of faculae had not attracted more attention.

They pointed out in 1980 that the ratio of spot to facular area measured in the first two years of a sunspot cycle was well correlated with that cycle's amplitude. The relation held over the *nine cycles* covered by the RGO data. A prediction based on this technique could only be produced about 3-4 years before a cycle maximum, but would still be very helpful.

Unfortunately, the discontinuation of the RGO program in 1976 precluded extension of this technique because other solar patrol programs focused on flares and mass ejections failed to provide the daily broad-band imaging of sufficient quality to measure facular areas.

I realized that the MDI and HMI NASA missions had begun obtaining daily broad band-like images of high quality around 2000. I used them to measure the areas of spots and faculae at the outset of cycle 24 and of the present cycle 25. Using the facula/spot ratio technique I found that the low cycle 24 sunspot number amplitude of 124 was exactly reproduced and the prediction of 13-month smoothed amplitude for cycle 25 was 185.

My paper on this technique and its prediction for this present cycle was turned down by journals two years ago because of the referees' concern that I had failed to use their work on the uncertain inter-calibration of the RGO, MDI and HMI imaging. With the smoothed cycle 25 sunspot number slowly rising towards 185, time will soon tell whether this interesting technique deserves to be better known.

Looking back

When I was packing up my office to leave Harvard in 1978, a more senior colleague, George Withbroe, dropped in to bid me farewell. George and I had differed on some issues over the years and his comment that “You may not be the smartest person at the CFA, but you sure see things differently” seemed like a back-handed compliment. But looking back over my career, it rings true; hence the title of this memoir.

From my McGill undergraduate days I have been aware that some of my fellow students and later, colleagues, possessed skills in computer modelling, mathematical techniques, or instrument technology that I could not match. But I also realized that they often lacked a good idea on where to apply those skills. My career success, such as it may be, was based mainly on some fruitful ideas that we pursued in collaboration to produce meaningful advances. Scientists differ as widely in originality as in other talents, but as George realized, the presence or absence of this attribute can only be judged in retrospect.