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# A stellar view of the Sun

**C.J. (Karel) Schrijver**••

**Abstract** This invited memoir looks back on my scientific career that straddles the solar and stellar branches of astrophysics, with sprinklings of historical context and personal opinion. Except for a description of my life up to my Ph.D. phase, the structure is thematic rather than purely chronological, focusing on those topics that I worked on throughout substantial parts of my life: stars like the Sun and the Sun-as-a-star, surface field evolution, coronal structure and dynamics, heliophysics education, and space weather. Luck and a broadly inquisitive frame of mind shaped a fortunate life on two continents, taking me from one amazing mentor, colleague, and friend to another, working in stimulating settings to interpret data from state-of-the-art space observatories.

## 1. Introduction

When Ed Cliver emailed me to talk "re the memoirs that Solar Physics publishes on a yearly basis," I expected that he wanted to discuss a slate of suitable new candidates, which I could have readily provided. But when, on behalf of the nominating committee, he invited me to write the next memoir, I could not believe my ears, even less so after he reminded me of the exceptional colleagues who came before me, all of whom had worked in the field longer than I have. And was I really "in the field" throughout my career? After all, good parts of it were dedicated to the distant stars, the heliosphere, and the impacts of solar activity on society. Then again, all those themes are connected to the Sun — past, present, and future. So, left humbled by the invitation, I started to reconstruct my life.

The stars grabbed my interest and imagination from a very early age, the Sun among their multitudes as the paragon of cool stars. It is not surprising that almost all of my research into them has used observations made by satellites: born in the year that NASA was founded, I grew up in the space age, was just old enough to be transfixed by the Apollo Moon landings, was in high school as Skylab's Apollo Telescope Mount was taking in the Sun and as the first X-ray signals from cool stars were detected (starting with Capella; Catura, Acton, and

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C.J. Schrijver
karelschrijver@gmail.com

Johnson (1975) and Mewe et al. (1975), who worked at the two institutes that would bracket my career) took on astrophysics as my major when the Einstein HEAO-2 observatory was launched, and started my Ph.D. research as EXOSAT and IUE were being readied for launch to observe, among other things, cool stars like the Sun. What *is* surprising is that I have been improbably fortunate to be able to work my dream job throughout my career, surrounded and supported by wonderful colleagues, with – as you will see in this memoir – unbelievably fortuitous timing relative to the launch of a series of space-based observatories.

Not only that, I have also been exceptionally fortunate in that my research interests and projects were never limited by where I worked. Of course, new projects would start in new settings, but I always had the opportunity to continue, or at least gradually wrap up, projects as I found my footing in a new job. Consequently, it proved easier to write this memoir guided by research themes rather than by chronology, although it is not too far off the latter. A summary of when I worked where is nonetheless helpful as a scaffold for my story, so you will find that in Sect. 4, right after a description of my life up to my Ph.D. phase in Sects. 2 and 3. Then follow the themes: stars like the Sun and the Sun-as-a-star in Sect. 5, the Sun's surface magnetic field in Sect. 6, coronal structure and dynamics in Sect. 7, heliophysics education in Sect. 8, and space weather and its impacts in Sect. 9. Brief concluding Sects. 10 and 11 wrap up this memoir.

I think that we are currently in a golden age of our research, with an abundance of advanced observing platforms, powerful numerical laboratories, and a diversity of technological tools that support us in our quest for understanding. But I also think that we are not optimally utilizing these riches because the infrastructure that manages our research presents several significant impediments to progress in our field. In what follows, I give a number of examples regarding over-compartmentalization in research and its funding, the relative weakness of the academic foundation of solar physics, the piecemeal funding of researchers, and the high cost of space missions. Because these comments and their examples are spread throughout the text and sometimes left implicit there, I summarize these points in the final Sect. 11. There is also some advice to the next generation sprinkled throughout the text and summarized in the final section.

An apology in advance: with each theme, I include a few references for context to highlight earlier developments and also some of the more recent work relevant to my activities, but please realize that this memoir is not meant to serve as a review of any of its topics.

## 2. Early years

My parents were born into families of shopkeepers: sewing machines on my father's side and, directly across the narrow village street of Baarn, the Netherlands, a grocery shop on my mother's side. With only two years difference between them, they told us how they played with each other and with the other children in the quiet street, managed to sneak into the attic of the Flora cinema right next door (where I, too, saw my first movies: subtitled French-language comedies), and shared broken cookies from the grocery that were unsuitable for sale (sometimes advancing that supply by surreptitiously shaking the tins when my mother was left unsupervised in the store). That untroubled village life ended in 1940, early in their elementary school years, when the Second World War came to the Netherlands. My parents' education continued only intermittently for the duration of the war and they lost the opportunity to pursue higher education, which was the main reason why they stimulated my younger sister and me to go for it.

After the war, my parents lost sight of each other for some time as my father went to work with a cousin in Sweden and was then drafted into the army and stationed in New Guinea for almost two years. But they found each other again, married (without the blessing of their parents), and rented a small apartment in a large villa where I was born two years later. Among my earliest memories are those of the large estate where my grandmother then worked: a home for elderly Indonesian expats, filled with mysterious objects on the walls, a cage with tiny monkeys and another with colorful birds, and above all the delicious smells perpetually emanating from the large communal kitchen. The love of Indonesian food, already kindled by my father's cooking of the dishes that he had encountered in New Guinea, has always stayed with me.

When I was almost five years old, we moved into an apartment attached to the factory where my father worked as an on-call repair technician. It was located at the very edge of the small city of Soest: from the balcony of our second-floor home we looked out over cow-filled pastures with the seemingly endless forests of the Royal Domains in the distance, with dark night skies above them providing views of the stars (in later years, the flat roof provided a great location to mount my telescope). There were only a few other homes nearby and hardly any children to play with, with even those disappearing as their homes had to make way for a continuation of the road in front of our apartment. As a result, I grew up largely entertaining myself, something that I was never bothered by but that, I think, made me into a rather shy, introverted child and young adult. Growing out of that took decades.

If I had to characterize my main character trait in one word, it would be curiosity. I was building radios by age 11 (although the equations for circuit design were well beyond me at that time), learned to install the electrical circuits underneath the landscape for my Märklin model trains, had a chemistry set to experiment with early in my teens, and spent hours and hours reading all sorts of science books from the library. And then there was the Apollo project: I followed flight after flight, often watching it live on television deep in the night (as many of the main activities were scheduled for daytime viewing in the USA).

Strangely, my wide-ranging curiosity nearly steered me away from my eventual profession: despite good grades and an excellent score on the national test in the final year, the head of my elementary school figured that I would not succeed at higher education because, he argued, I had too many interests. My parents accepted his counsel and, consequently, placed me in a relatively low level high school (MAVO). But I was lucky: just two years before that, a major reorganization in the Dutch education system had been implemented. It allowed easy transition from one level of high school to another. A year later I had migrated two steps up to the highest level, the atheneum. Instead of a 10-minute bicycle

**Figure 1.** Throughout my high-school years, my summer vacations were filled with camping trips with my parents and sister, several weeks of factory work, and a week or two of sailing through the northern province of Friesland where small lakes (by American standards) are connected by a multitude of waterways that bring one through the centers of picturesque towns.

ride to school in Soest, I was now fighting the all-too-frequent rain and wind on a 40-minute ride to the nearest city, Amersfoort. I loved physics and mathematics, thanks to very inspiring teachers. And, in retrospect, I should also have been most appreciative of my Dutch teacher: much to the puzzlement and amusement of many of my classmates, he would bring in art books from his vast private collection, hand around pictures of architecture, and play (mostly classical) music on a rickety gramophone, but the result was that I still see literature, art, music, and architecture in each other's context within an ever-changing culture.

Throughout all my school years, summers were times of camping, traveling, and sailing (Fig. 1). At first, in my elementary school years, we would camp on the coast of North Holland, where we could easily walk into the dunes and onto the seemingly endless (and then still quiet) beaches. There, my father would take the time to bring me up to speed where I had lagged in my learning; we spent hours and hours working on multiplication tables, often with ice cream as a promised reward. In later years, once my parents had purchased a car, our vacations took us beyond the Dutch borders: I remember campgrounds in the Ardennes in Belgium, on an island in the Seine in western Paris, at Loch Ness in Scotland, in a pasture outside London, at the base of a glacier in Switzerland . . . I documented many of these trips in super-8 home movies or, later, in slide shows, learning how to accompany these with soundtracks using the earliest electronic tools available to the general public.

Throughout these years, my father taught me to use my hands and not to be afraid of mechanical or electrical jobs. During my Ph.D. phase, for example, we once took apart the engine block of my 1100cc motorcycle to find the cause of an odd sound that it had developed; it took us an entire weekend before we had it back together but it ran smoothly ever after. And he managed to convey some of his skills in 3D thinking: he eventually became an instrument maker with an uncanny ability to reproduce or design objects by using the variety of machines that surrounded his work area. I still am grateful to him for his teachings whenever some job needs doing around the house.

## 3. Undergraduate years

Once I started attending the atheneum, the path to a university education opened up. My father worked rotating shifts, sometimes even taking on double duties, and often on Saturdays, to give my sister and me a chance for a better education, but apart from my high school teachers, I knew no one with a university degree at that time: no one in our extended family had ever gone there. Fortunately, there was a system of financial support and long-term, interest-free loans in place for students so I could pursue my new dream. I could not afford student housing, though, so I commuted the 25 kilometers to Utrecht's Uithof campus and back every day, at first on a moped, later on a 400cc motorcycle. I enjoyed the ride over forest-covered roads, although in the winters, when the Sun rose after classes started and set before they ended, it was less enjoyable.

Physics and mathematics (my initial majors) were challenging but fun: classes in the mornings and labs in the afternoons. I particularly enjoyed some of the unusual labs such as learning to develop film and to print photos in evening astronomy labs (how quaint that seems nowadays), and simple glass blowing. And I learned to play bridge in the lunch breaks with my fellow students.

After the first two years, I flipped majors and minors, choosing to focus more on astrophysics than on physics and mathematics. The way physics was taught gave me the impression that most things were known with little left to discover. In astronomy classes, I experienced exactly the opposite. I never regretted that decision, although I have to admit to an unusual degree of naivety back then: I gave hardly any thought to a career after university. Things have fallen into place wonderfully, though, and I was right at least about astronomy: there was, indeed, so much to discover!

In my third year, I elected to compute stellar structure models in an elective lab together with another enthusiastic student. To this day, I don't think we deserved the grade that we were given; neither we nor the teaching assistant who graded us understood the importance of the outer boundary condition and the complexities involved in that. We approached advanced graduate students (Paul Kuin and Piet Martens) for that challenge and they graciously helped us toward the end goal. It taught me a lot about differential equations and numerical methods, and a bit about computers, which, at that time would require entire buildings for something less powerful than what we now have in a laptop. We developed fragments of the code on teletypes: simple keyboards connected to a mainframe, with instructions and responses typed out on rolls of paper. Eventually, the full program was encoded on a stack of punchcards which would be hand-carried to the building housing the large IBM to be read in, and then hours later a printout would appear in one of the many pigeonholes, arranged alphabetically by job name. Still, the big physics computer was quite a bit more powerful than the Commodore 64 computer (with 64 kbytes of memory) that I bought and played with four years later.

On the physics side, I emphasized plasma physics. That turned out to be a wise choice given what the future had in store for me. The oral exam (quite rare in Utrecht, and my first), in the vast office of Prof. Braams, the director of the plasma physics institute in Nieuwegein, was quite the ordeal. In mathematics, I favored the more applied side, in particular numerical methods. I took all the required classes in the astronomy curriculum, including stellar evolution, degenerate matter, gravitation and cosmology, astrophysical magnetohydrodynamics, *etc.*, and, unavoidably, radiative transfer which was taught by my future thesis adviser, Prof. Cornelis (Kees) Zwaan. I still remember Kees' surprise in later years that his course was the only one for which I did not take the exam, simply because it was unnecessary. Coming to the very end of my studies, I had acquired more than enough points to qualify for graduation, so why would I jeopardize my grade point average on such a complex topic?

For what nowadays might be called my master's project, I chose to work on the development of an X-ray position-sensitive proportional counter at the Laboratory for Space Research (known by its Dutch abbreviation LRO) at the other end of the city of Utrecht. The project developed into what I have always liked in the years following – combining observations with modeling. In this case, it also involved learning that it is really hard to straighten metal wires for a detection chamber from a roll and that vibrations of vacuum pumps travel all over the place and can set up resonances in tightly strung metal wires. It was fun, though, to see the green or blue sparks (depending on whether methane or carbon dioxide was added to the argon, xenon, or nitrogen gas) wherever the wires of the upper and lower planes of the chamber came too close to each other. The results were analyzed with LRO's PDP-11 computer which read out the measurements from a paper tape, a very fragile carrier for punch holes that easily tore and then would have to be repaired by adhesive tape, requiring a good memory for encoding the eight bits of a character into zeros and ones, and a punch to create the corresponding holes in the repaired tape. How the world has changed in just four decades!

Discussions between the staff members brought me to the attention of Rolf Mewe at LRO who, together with Kees Zwaan of the Astronomical Institute, was looking for a student to work on X-ray spectroscopy of cool stars. I don't think my experience with gas proportional counters had much to do with their choice, although I did start that work on observations made by the imaging proportional counter onboard the Einstein HEAO-2 spacecraft. It was not quite the direction that I was most interested in at the time – stellar evolution theory. But the work was to involve stars in different evolutionary stages, so I accepted their offer for a three-month project.

Before formally graduating in March of 1981, I took a six-month course to obtain the license qualifying me as a physics teacher at any of our levels of high school — one never knows, after all. It was very interesting to learn about learning, then to actually teach three different age classes in a Montessori high school in Zeist for a month, and in doing so gain confidence in speaking before an (often restless) audience.

In the meantime, Kees Zwaan and Rolf Mewe had reached a decision: they offered me a Ph.D. position, to be shared between the Space Research Organization and the Astronomical Institute.

## 4. Moving about

**1981 — 1986:** *Astronomical Institute Utrecht and Laboratory of Space Research (LRO).* In those days in the Netherlands, the Ph.D. phase consisted exclusively of research. There were no classes to take or give, and no teaching assistantships to take on. I moved back and forth between two offices: one at the modern office building of LRO in the outskirts of Utrecht and one in the Sonnenborgh Observatory[1] in the old city center. At LRO, there were a dozen Ph.D. students, all in about the same phase, forming a group of friends who to this day still occasionally get in touch and who, for years, undertook group outings somewhere around the country with partners and, later, often their children. At the Observatory, in line with Dutch tradition, there were the daily coffee breaks in mid-morning and tea breaks mid-afternoon where staff and students gathered in the small picturesque library for scientific discussions and small talk.

Toward the end of my Ph.D. appointment (Fig. 2), I applied for, and was offered, a postdoctoral position with Carole Jordan, a solar and cool-star spectroscopist in Oxford. Had I gone that way, my career would doubtlessly have taken a turn into spectroscopy, but it was not to be. Until that year, compulsory military draft would be postponed if one had a job abroad, but then the rules changed to require a job outside of Europe. I was not at all motivated to join the army (considering armies tools of suppression), even less so because those born a year earlier or a year later than I were excused as the army was transitioning into an all-professional service. Fortunately, an alternative materialized when Jeff Linsky (an early leader in cool-star physics and in the study of asterospheres and the interstellar medium at the Joint Institute for Laboratory Astrophysics in Boulder, Colorado) happened to visit Utrecht: my thesis advisor, Kees Zwaan, introduced us, and I was interviewed on the spot, cornered on a sofa by Jeff and Kees. Three months later, I joined Jeff's cool stars group in Boulder.

**1986 — 1988:** *JILA, University of Colorado, and visiting scientist at Lockheed Palo Alto Research Laboratory (LPARL) and the National Solar Observatory's (NSO's) Sacramento Peak Observatory.* Boulder was an incredibly stimulating environment, primarily because Jeff Linsky's group ("the Cool Star Mafia") – at the time including Tom Ayres, Tim Bastian, Jay Bookbinder, Alex Brown, Bob Dempsey, Jim Neff, Steve Saar, Fred Walter, and Brian Wood – provided a very welcoming and broadly knowledgeable work environment. Boulder is also blessed with an amazing natural setting that I often explored with long mountain hikes and drives.

Jeff gave me a remarkable amount of freedom in my research into solar and stellar magnetic activity, and also allowed me to visit other research groups. After the first year in Boulder, I packed everything I owned into my car (fortunately, I had been wise enough to invest in a hatchback that even accommodated my bicycle) and drove Route 50 (much of which is "the loneliest road in America") through mountains and deserts to the San Francisco Bay Area, which welcomed me with fog rolling in spectacularly over the Bay and with jammed traffic.

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[1] https://www.sonnenborgh.nl/bezoekersinformatie

**Figure 2.** Kees Zwaan handing me the diploma for my doctorate in the venerable Senaatszaal of the Academiegebouw (University Hall) of the University of Utrecht on 1986/09/22. The title of my thesis, "Stellar Magnetic Activity: Complementing Conclusions based on Solar and Stellar Observations," formed the theme of much of my career. Behind me are my paranymphs (ceremonial assistants): (left) René Rutten with whom I worked closely throughout our Ph.D. phases and (right) Pieter Mulder with whom I go back to freshman physics. Astronomical research in Utrecht ended in 2012 when the Astronomical Institute was closed, 370 years after Utrecht became the second city in the country (after Leiden) with an observatory.

There, I spent three months with Alan Title's group in Palo Alto, working on supergranular diffusion and the decay of active regions. As much as my move to Boulder, this was another pivotal moment in my life. Not only did it set the stage for my later return to Lockheed, I also fell in love with Iris during this visit. She had arranged to go on a tour of the American Southwest with my sister, but as Iris had a longer summer holiday, I invited her to come over early, giving me a traveling companion to explore northern California. Two years later, we were married and she's been my life companion ever since.

After that, I spent a month at Sac Peak at the invitation of Rich Radick, working on relationships between radiative losses from the outer atmospheres of cool stars. I loved it there, both because of the people and the setting; given an apartment in what once were the Visiting Officers Quarters right at the edge of the escarpment, I had a spectacular view of the wide valley holding the White Sands Missile Range, the White Sands National Park and the San Andres Mountains, with summer lightning storms flashing through the distant sky almost every night I was there. From there, I moved back to Boulder to continue in Jeff's cool stars group.

**1989 — 1991:** *European Space Agency (ESA), ESTEC, Noordwijk.* With Iris embarking on her medical training, I looked for a job in the Netherlands. I nearly ended up installing flow-monitoring systems along rivers in China when a postdoc appointment at ESA's European Space Research and Technology Centre (ESTEC) in Noordwijk came through. It was an opportunity to learn about

**Figure 3.** Iris and I both love traveling. Here we are in the Sahara, deep inside Morocco, somewhere near Mhamid close to the border with Algeria toward the end of a long tour around the northern edge of the Mediterranean. We cannot decide on which sights impacted us most in all our travels, but we did notice a shift over the years in our interest from historical and cultural sites to the wildest, most remote places on Earth.

helioseismology. I was tasked to make something of months-long records of solar oscillations observed in the irradiance signal onboard the Russian Mars-bound Phobos mission. It carried a small instrument that had a poorly baffled tube looking back at the Sun and therefore recorded strong internal reflections as the spacecraft's pointing meandered about the Sun. Working in Noordwijk made me appreciate the beauty of the fields of flowers along which I commuted. While at ESTEC, military service called once more, but as the army would have to provide me with a salary matching my tax-free income at ESA because spouses were not supposed to suffer hardship, they gave up on me, this time for good.

**1991 — 1994:** *Fellow of the Royal Academy of Arts and Sciences (KNAW) at the Utrecht Astronomical Institute.* As my appointment at ESTEC ran out, I applied for a KNAW fellowship, which gave me a free choice in research topics and where I would choose to work. It wasn't a hard decision: I went back to my Alma Mater, to Utrecht's Astronomical Institute, to work with Kees Zwaan, Rolf Mewe, Bert van den Oord, and others while able to commute to and from work with Iris. Before Iris started her internships and I my KNAW fellowship, we took a four-month "sabbatical" to literally travel around the world, taking in some of the beauty that our planet has to offer. Our love of traveling has never left us (Fig. 3).

**1995 — 2016:** *Lockheed Martin Advanced Technology Center.* In the meantime, a third major pivotal event in my career was unfolding. I had applied for the directorship of the Kiepenheuer Institute in Freiburg, Germany. Told it would be a good experience to go through such an exercise, I was rather surprised when I was offered the job, particularly given the status and experience of the other applicants. But it was not to be. Without disclosing why, someone in the government of the state ("Land") of Baden-Württemberg decided to modify the job offer to one that I could not accept (I would have understood reservations about my lack of management experience but it appears that not being German

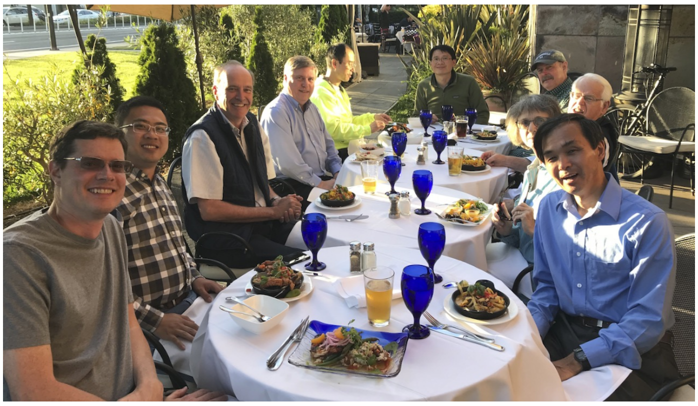

**Figure 4.** Friends at Lockheed in 2017. Clockwise around the tables: Marc DeRosa, Meng Jin, me, Neal Hurlburt, Wei Liu, Mark Cheung, Ted Tarbell, Alan Title, Ruth Peterson, and Nariaki Nitta.

played a major role). Although disappointed at the time, I now think it was the right outcome: instead of heading to Freiburg, I accepted the standing invitation to join Alan Title's group in California, a decision I have never regretted.

There was, however, a challenge to deal with in moving to the USA. Because Iris had just finished her medical training, I was essentially asking her to redo all her qualifying exams and start over in a different language, culture, and healthcare system. We made a deal: if she would not find suitable employment within a year or two, we would move back to the Netherlands. That agreement was never called upon. Fortunately, she found a job at Stanford where she ultimately became a full professor in molecular genetics and clinical pathology. We still look back in amazement at the odds of being able to combine our two careers while working at most a few miles apart.

I joined Lockheed's Solar and Astrophysics Laboratory (LMSAL) in 1995 as the *Transition Region and Coronal Explorer* (TRACE) was entering its construction phase and when the science planning was ramping up. It was a steep learning curve for me, transitioning from a user of spacecraft observations to being involved in planning and – after the 1998/04/01 launch – executing observations. In that period, I became the science lead. In 2005, when Alan Title started as Principal Investigator of the Atmospheric Imaging Assembly (AIA) onboard the Solar Dynamics Observatory (SDO), I took over his role as PI of TRACE. I learned a lot from inspecting TRACE's crisp coronal and transition-region imagery almost every workday for twelve years. And I learned a lot about working with NASA and the international scientific community.

More of those lessons followed when LMSAL (Fig. 4) was awarded the contract to build SDO's AIA. SDO was launched successfully on 2010/02/11. Once its torrent of telemetry came down, it gave us an essentially continuous, high-cadence, multi-temperature view of the entire Earth-facing part of the solar outer atmosphere, revealing its dynamics in unprecedented detail. We retired TRACE soon after AIA was demonstrated to be fully functional. Just short of SDO completing its second year on orbit, I transitioned from AIA's science

**Figure 5.** Iris and I visited Vandenberg Air Force Base (now called a Space Force Base) on 2013/06/22, a week before the launch of the Interface Region Imaging Spectrograph (IRIS). Behind us is the L-1011 TriStar aircraft with IRIS already attached to the Pegasus rocket mounted underneath the plane. Whereas I was very involved in the development and proposal phases of IRIS, I, unfortunately, lacked the time to engage in its research.

lead to PI as Alan started work on the Interface Region Imaging Spectrograph (IRIS; Fig. 5), adding another six years of near-daily inspection of the solar corona before I retired in 2016.

At this point, I will end the chronological summary of my life, instead following the development of selected scientific themes.

## 5. Stars like the Sun and the Sun-as-a-star

A substantial part of my career focused on what is generally referred to as "the solar-stellar connection." A full-text search with the NASA Astrophysics Data System (ADS) (what a wonderful tool to have!) suggests first use of that term by Andrea Dupree (a stellar astrophysicist with focus on cool-star science) in the preface to the proceedings of the very first workshop (held in Cambridge, Mass.; Dupree, 1980) in an ongoing series entitled "Cool Stars, Stellar Systems, and the Sun." That first meeting stimulated the research in the field, further pushed along by a meeting on the other side of the Atlantic the following year, a NATO Advanced Study Institute organized by Roger Bonnet (who would later become ESA's director of science and of ISSI, among other things) and Andrea Dupree (Bonnet and Dupree, 1981) that is widely referred to as "the Bonas meeting." In the introduction to its proceedings, the editors wrote "In the past, stellar and solar astrophysics have more or less followed their own independent tracks. However, with the rapid development of modern techniques, in particular artificial satellites like the International Ultraviolet Explorer [IUE] and the Einstein Observatory, which provide a new wealth of data, it appears that chromospheres, coronae, magnetic fields, mass loss and stellar winds, etc. . . . , are found not only in the Sun but occur also in other stars. Frequently these other stars represent quite different conditions of gravity, luminosity, and other parameters from those occurring in the Sun. The Sun is no longer an isolated astrophysical object but serves the role of representing the basic element of comparison to a large class of objects."

I did not get to attend either of these two meetings but their proceedings formed most of my Ph.D.'s startup material. The above statement by Bonnet and Dupree could have served as the rationale for my thesis work, which started for me with a 3-month pilot project (or Ph.D. probationary period, depending on perspective) in 1981 that launched me on a decades-long study of the relationships between radiative losses from different thermal regions in the atmospheres of Sun-like stars.

### 5.1. Power laws and basal fluxes

Thus, even before formally graduating, I embarked on my first trip to the USA for an extended visit to the Center for Astrophysics (CfA, a collaboration between the Smithsonian Astrophysical Observatory and the Harvard College Observatory, with boundaries running through the building complex that never became clear to me) in Cambridge, Mass. Rolf Mewe accompanied me for that first visit for a week to introduce me to people, places, data, and codes. Kees Zwaan and Rolf Mewe already had a working relationship in place with the Einstein project (Vaiana et al., 1981) and thus gave me a running start.

Rolf had found a boarding house, run by a former secretary of the CfA. The house was almost a real Victorian: all-wood construction (a contrast to the predominantly brick or concrete buildings in the Netherlands), with creaking floorboards, insufficient ventilation on humid summer days and – during later winter visits – steam running through the small radiators in an attempt to fight the loss of heat through the single-pane windows. We would go out for breakfast (another surprise for me: you couldn't go out for breakfast in the Netherlands at the time, except perhaps in a hotel) at a classic American diner and then we would walk up to the CfA, perched on its hill.

We would sit down at the monitors, and fit spectra from the spectral code that Rolf had been developing (Mewe, 1972) to Einstein's observations of select cool stars. There was little information to extract from the signals from the imaging proportional counter: generally a single temperature for the stellar corona combined with a column density of interstellar absorbing hydrogen yielded a perfectly acceptable fit, although it was tempting to try two-temperature approximations. The main findings were the marked distinction in dominant coronal temperatures between main-sequence stars (around 3 MK) and evolved stars (in the 10–20 MK range), and the initial establishment of the relationship between chromospheric Ca II H+K signals in the visual as recorded at Mt. Wilson and the coronal soft X-ray brightness measured by Einstein (Mewe, Schrijver, and Zwaan, 1981), published shortly after Ayres, Marstad, and Linsky (1981) had explored such relationships for UV and coronal diagnostics.

As my Ph.D. phase began, I joined Kees Zwaan's research group, then with Frans Middelkoop (who had made several observing runs at Mt. Wilson, working with Olin Wilson on his project to measure Ca II H and K line strengths in cool stars that had its roots in Wilson and Vainu Bappu, 1957) and Barto Oranje (who focused on IUE spectra of cool stars and on Sun-as-a-star observations with the Utrecht solar spectrograph), but soon after also with René Rutten with whom I worked most closely. Initially, I spent my time mostly at LRO across town, apart from weekly meetings in Kees Zwaan's spacious office at the Observatory. Over time, though, I worked increasingly in the office over the entrance to the Observatory that I shared with Barto, Frans, and René. It offered wonderful views of the majestic old trees in the adjacent park where I often spent lunchtime breaks walking along the Singel – a wide defensive moat running at the base of the Observatory – unless I opted for the old center of Utrecht.

Those were electrifying days: the IUE and Einstein/HEAO-2 spacecraft had been launched in 1978 to observe, among other things, cool stars like the Sun, followed in 1980 by the Solar Maximum Mission (SMM) on the solar side. Discoveries of coronal mass ejections (CMEs, first observed with OSO-7 and then in more detail from the Skylab space station; Tousey, 1973; MacQueen et al., 1974), of the relationship between chromospheric Ca II emission, stellar age, and rotation (Skumanich, 1972, see Lorenzo-Oliveira et al. (2018) for a recent confirmation of his results for a sample of solar twins), and the principles of magnetic braking of stellar rotation (Mestel, 1968) were all hardly a decade old and the research into these phenomena was in full swing. It did not take me long to realize that I lived in an exciting time and was embedded in a wonderfully pivotal research group, being guided by the experienced observational solar physicist Kees Zwaan and the plasma spectroscopist Rolf Mewe, working alongside Ph.D. students using ground- and space-based observatories in a rapidly growing research field.

Establishing and understanding relationships between radiative losses from different physical domains in the outer atmospheres of cool stars was my initial focus area (working in parallel to, primarily, Jeff Linsky's group in Boulder – e.g., Ayres, Marstad, and Linsky (1981) – whose IUE observations were essential to our early research, complemented by our own data). As René Rutten and I were assembling data from Mt. Wilson, IUE, and Einstein, I noticed something in the diagrams, namely a dependence on the stellar spectral color similar to what we had seen when relating coronal soft X-rays and chromospheric Ca II H+K. That color dependence disappeared when I subtracted what I named the "basal flux," an emission level running right below the lowest detections of the stellar signals in flux-color diagrams.

For the Ca II H+K signal the presence of a minimal level was to be expected because the Mt. Wilson spectrograph also transmitted a contribution from the line wings. But for the UV, with little or no continuum radiation underlying the emission lines for many cool stars, it did come as a surprise.

My thesis supervisor and my fellow Ph.D. students were skeptical at first. They had not seen a need to introduce such a basal level in the IUE UV data themselves, and neither did the Boulder group which was also producing flux-flux diagrams at the time. But as more and more data came in, the evidence convinced us. Once the basal level was removed, flux-flux relationships held up for giants and dwarf stars alike, regardless of surface gravity or temperature (except, as we noticed later on, for the very cool M-type dwarfs which are deficient in chromospheric emission, Rutten et al., 1991). This saw me embark on a 14-year research thread that eventually culminated in a review (Schrijver, 1995) concluding that “There is substantial quantitative observational and theoretical evidence that the basal emission from stellar outer atmospheres is caused by the dissipation of acoustic waves generated by turbulent convection” although “effects of intrinsically weak fields on the basal mechanism cannot be entirely ruled out.” A decade later, noting an error in my calibration of solar to stellar data for the high-chromospheric C II signal, using new observations made with SoHO’s SUMER instrument, and supported by state-of-the-art radiative hydrodynamic modeling, Judge, Carlsson, and Stein (2003) stated that they “believe (but cannot prove) that [their] analysis points to [internetwork] magnetic fields as the dominant origin of the basal component seen in C II lines.” Even as of today, more observations and modeling are required to sort this out: is all of the basal flux related to weak turbulent fields, or is there a shift from acoustic to magnetic going up in temperature and height, or is it all dominated by the dissipation of mechanical energy?

Reaching the conclusion that basal fluxes are a reality (regardless of their physical origin) would take me through several projects as well as to interesting collaborations and locations in my postdoc years. Among these was, for example, a study of the transition-region C IV emission as observed by IUE for cool stars, for the Sun-as-a-star by the Solar Mesosphere Observer (SME), and with spatial resolution by the SMM-UVSP (with help from Dick Shine at Lockheed), in comparison with magnetogram data (recovered from the NSO archive with the help of Stuart Ferguson and Jack Harvey). Then there was a project for which Rich Radick invited me to Kitt Peak to work with him and Andrea Dobson on relating stellar observations of Ca II H+K, Mg II h&k and stellar rotation rates. A few years later, Rich invited me once more for a study, again with Andrea, on establishing flux-flux relationships between stellar Ca II H+K, Mg II h&k, and soft X-ray fluxes using observations with the smallest available time differences between these various observations, which, unfortunately, still ranged from a day or two to in excess of a month even for the best cases – clearly not ideal.

Allow me to make a critical comment here: it has always baffled me, and it continues to baffle me, that the coordination between various space- and ground-based observatories is so exceedingly difficult to achieve. Doing so between different countries is particularly challenging, but within a country with the resources managed by only a few organizations – such as within the US primarily by NASA and NSF – it should be readily feasible. I mean, we have computers to sort out schedules, and if airlines can do so most of the time for most of its passengers, surely astronomers should be able to pull it off for a handful of observatories. But as long as funding streams keep requiring multiple proposals for multiple observatories and time-allocation committees are not coordinated, we shall continue to waste promising opportunities and a lot of researchers’ time.

In this context, there is, unfortunately, also the dominant role of personal interests to contend with: I just could not understand why, for example, even the instruments on a single spacecraft such as SoHO often did not focus on the same region on the Sun. When I was planner for TRACE, or in my role as its science lead and later PI, I would often opt to follow the lead of one of SoHO’s

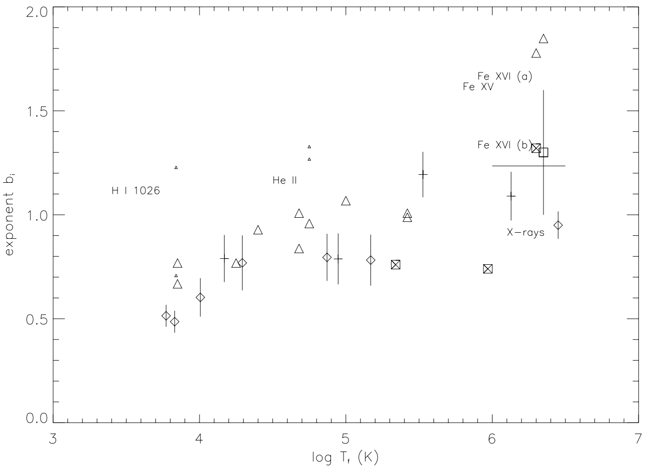

**Figure 6.** Radiative losses $\Delta F_{\rm i}$ in excess of any basal emissions from the outer atmospheres of cool stars are related to each other and to the underlying magnetic flux densities $\langle|\phi|\rangle$ through power laws independent of stellar color or luminosity: $\Delta F_{\rm i} \propto \langle|\phi|\rangle^{b_{\rm i}}$. This diagram is a compilation of power-law indices $b_{\rm i}$ for radiative diagnostics with formation temperatures that range from chromosphere to corona, revealing the tendency towards increasing steepness with temperature. (From Schrijver and Siscoe, 2010) This diagram is similar to Fig. 7 in Toriumi et al. (2022), although the slopes for the chromospheric diagnostics in that study are higher, which may have to do with their definition of the "basal" level to be subtracted.[2]

observers, and then frequently stick with a target for days on end. I think that paid off as witnessed by the many multi-instrument papers written involving TRACE observations. Moreover, with this strategy, we also managed to capture the majority of the most energetic events on the Sun.

I can only hope things will improve because there is such a promise of discovery in joint observations. I think it was Jeff Linsky who came up with the word "panchromatic" as he pointed out the value of such observations decades ago. And I appreciated it first hand when observers combined data from six international spacecraft, covering the wavelength range from the optical into gamma rays, to study the initiation and evolution of the powerful X17 flare SOL2003-10-28T09:51 (Schrijver et al., 2006).

### 5.2. Sun-as-a-star construction kit

Let's go back to my Ph.D. years. After correction for the basal emission, radiative losses from chromospheres through coronae of all sorts of cool stars defined power laws (Fig. 6).[2] Of course, I was intrigued by the non-linearity of these flux-flux

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[2] Differing power-law indices have been reported for the relationships between a given radiative signal and magnetic flux densities for a variety of spectral lines (although authors generally agree that the power-law index tends to increase with formation temperature from the chromosphere upward). Among the causes for these differences are the variety of definitions of what the authors consider the basal level (solar network interior or average network or lowest magnetic signals or minima from stellar samples, with uncertainties in the conversion between stellar

relationships that held over some four orders of magnitude in soft X-ray flux density, regardless of which type of star we looked at within the late-F, G, K, and early-M type dwarfs and giants. The fact that the Sun appeared to move largely along these relationships going from solar minimum to maximum triggered the concept of a digital "construction kit" (Schrijver, 1988). Using the solar cycle as a guide, I placed different numbers of active regions on a simulated solar surface, included mixed-polarity quiet Sun, and thus studied the behavior of the Sun as a whole. That required, of course, knowledge of the constituent parts: the quiet Sun, the variety of active regions, and their radiative losses.

It started easily enough. I looked through the listings of the Solar Geophysical Data to determine the frequency of active regions existing on the solar surface at any instant in time as a function of their Ca II K plage area (following Tang, Howard, and Adkins, 1984, who differentiated cycle minimum and maximum phases using Mt. Wilson magnetograms). I remember Kees Zwaan's surprise at the smoothly decreasing curve with increasing region size: until presented with this data, he had had the impression that there were distinct populations of small and large active regions. Moreover, the frequency distribution changed relatively through the solar cycle other than by a multiplicative factor.

Karen Harvey (also a graduate student of Kees Zwaan, defending her thesis in 1993) would later take a somewhat different approach to come to a comparable, although distinct, conclusion (Harvey and Zwaan, 1993). Instead of determining how many regions of what size existed on the solar surface at a given time, she painstakingly reviewed daily Kitt Peak magnetograms to determine how many newly emerging regions ultimately grew to what size at full development (Fig. 7), thereby removing lifetime effects from my distribution to establish a true source function (I return to that in Sect. 6). Here, too, a fixed (in fact, power-law) shape of the distribution emerged, modulated over time through the solar cycle by a multiplicative factor (later extended by Mandy Hagenaar to the ephemeral-region range, see Fig. 7). An under-appreciated (and remarkably little-known) property of active regions is that about half of them emerge inside existing regions: bipolar regions are strongly nested (originally noted by Cassini (1729), studied in detail by Harvey and Zwaan (1993), and later by, *e.g.*, Pojoga and Cudnik (2002), and inferred for Sun-like cool stars by Işık et al. (2020)). It seems that this is a key fact in how flux escapes from the dynamo source region onto the surface, but it receives little attention. Comparison of observed rotational modulation to simulated signals suggests that active-region nesting may be even more pronounced in more active stars (Nèmec et al., 2023).

The next step toward the "construction kit" was to establish the brightness of active regions in a variety of spectral lines and bands. I returned to the CfA for that, working with Charles Maxson and Bob Noyes. There, I toiled through the records of the S-055 ultraviolet spectrograph and the S-054 imaging X-ray telescope on Skylab's Apollo Telescope Mount. S-054 images were recorded on

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flux densities and the solar intensities) and the sensitivity of the results to bandwidths (with different scalings reported within profiles of optically-thick diagnostics); see, *e.g.*, Barczynski et al. (2018); Toriumi et al. (2022). In my work, the basal emission is that emission level that removes any dependence on fundamental stellar parameters from flux-flux relationships.

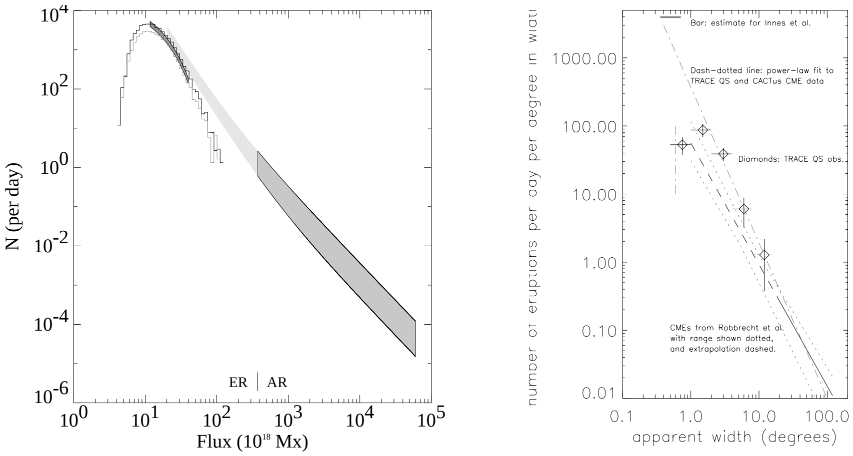

**Figure 7.** The unsigned flux distributions for bipolar regions (*left*) and the angular widths of eruptions (*right*) appear to smoothly connect from the small ephemeral-region scales to active regions and their CMEs. *Left*: Frequency distribution of bipoles emerging on the Sun per day, per flux interval of $10^{18}$ Mx. The number of active regions varies by about a factor of 8 through a typical cycle (indicated by the shaded area on the right) while the variation of ephemeral regions is much smaller (and possibly in antiphase). The turnover below $10^{18}$ Mx likely reflects the detection threshold of SoHO/MDI. The range between ephemeral regions and active regions still awaits observational confirmation. (From Hagenaar, Schrijver, and Title, 2003) *Right*: Frequency of observed eruptions for TRACE, SoHO/LASCO, and STEREO/EUVI versus angular width. (From Schrijver, 2010)

70 mm film that was brought down to Earth by the astronauts, developed, and – at least for the CfA – printed onto transparencies of some 25 cm in size (as far as I recall), stored in drawer after drawer in a small room off CfA's hallways. I plowed through these to identify different-sized regions at different positions on the disk with sufficient separation between them to ensure they were effectively isolated. Then I worked through the inches-thick stacks of computer output that held the S-055 log files to determine which of these regions were scanned in their entirety by that instrument in a set of chromospheric and coronal lines. The final step in obtaining the data involved finding a working 7-track tape reader to extract the data from the many rows of tapes in the archive down the hall.

Collecting that data for two dozen active and quiet regions took two month-long visits to Cambridge, Mass., followed by a two-week visit to NSO in Tucson to recover magnetograms for a subset of these for which such data were available. What a contrast with the modern day in which observing logs and archives, and even event listings, are available online, often with search and analysis software provided, for example through SolarSoft[3] (Freeland and Handy, 1998). And yet, despite that, population studies of active region properties, flares, or coronal mass ejections that combine data from multiple instruments remain rare. That is disappointing given the efforts put into enabling such studies by, among others, the science teams of SoHO and SDO.

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[3] https://www.lmsal.com/solarsoft/

During my stays at CfA, I didn't take nearly enough time off to explore Boston and its surroundings, but I did append a trek across the USA to each of these visits, traveling in a small van with 11 like-minded young people. The first trip took me from New York to Los Angeles, and the second from Seattle to Miami (with a short stop in Boulder, CO, where I had no inkling that I would be working there just a few years later). It was during these trips that I fell in love with the American West. I was thrilled by the wide open spaces, by the colorful geology often laid bare to the eye in desert areas, and by the abundant wildlife (in the small, densely-populated country of the Netherlands, wildlife was largely limited to birds; I think I saw one squirrel in all the years that I lived there, and only a few wild boars and deer in an enclosed park, and that only from afar).

Combination of the collected data into the rather crude first "construction kit" (enabling the last chapter of my Ph.D. thesis) showed how the emission from quiet and active regions combined into Sun-as-a-star signals that fell roughly, but not quite, along the stellar flux-flux relationships. Two other conclusions started to take shape: (1) that the main determinant of the brightness of active regions appeared to be their area, and (2) that in order for the Sun to fully follow the stellar flux-flux relationships, the quiet Sun also needed to change with activity level. The first of these conclusions led to the discovery of the "plage state" and Alan Title's first invitation to Lockheed in the San Francisco Bay area (Sect. 6).

The second of these conclusions led to a month-long visit to the National Solar Observatory in Tucson. Karen Harvey, a marvelously warm-hearted person, helped me work through almost a full solar cycle of synoptic magnetic maps from which we derived distribution functions, $h_t(|\phi|)$, of magnetic flux densities across the solar surface over time. We found that the properties of these time-dependent distribution functions were such that they transformed power-law relationships between local radiative and magnetic flux densities, $f(|\phi|)$, into essentially the same relationships between disk-integrated signals, $F(\langle|\phi|\rangle)_t$, as function of surface-averaged magnetic flux density, $\langle|\phi|\rangle_t$ (here ignoring radiative-transfer effects):

$$F(\langle|\phi|\rangle_t) \approx \int h_t(|\phi|) f(|\phi|) \mathrm{d}|\phi| / S. \tag{1}$$

With that we understood how it could be that the solar spatially-resolved observations and the stellar observations could lead to the same power laws: the origin lay in the continuous, smoothly-changing distribution of the surface magnetic field, from the quietest areas into the population of active regions, through the solar cycle. That, of course, led to the next question: how did that particular type of distribution function come about? This eventually led to the surface flux-transport model that formed the basis for the second-generation construction kit, as described in Sect. 6.

### 5.3. Linking radiative losses to the magnetic field

The question of how the atmospheric radiative losses connected to the magnetic field played throughout this research into the nature of flux-flux relationships. An early answer had been provided by Skumanich, Smythe, and Frazier (1975) who established a linear relationship between the Ca II K line brightness and the underlying magnetic flux density for a quiet-Sun area. But that was inconsistent with what Steve Saar and I found based on stellar data (Saar and Schrijver, 1987). Steve, under Jeff Linsky's mentorship, had developed a method to estimate surface-averaged magnetic flux densities on cool stars. When we compared these to coronal soft X-ray data, including solar measurements, an essentially linear relationship emerged. Through the relationship between coronal and chromospheric losses, this implied an approximately square-root relationship between magnetic and chromospheric flux densities (Fig. 6), which should hold both for stars as a whole and for the spatially-resolved Sun. To resolve this, I submitted a proposal to NSO to extend the relationship established by Skumanich, Smythe, and Frazier (1975) to enhanced network and active regions.

I was awarded two week-long observing runs on the McMath solar telescope on Kitt Peak. When I arrived in Tucson for the first observing run, I was given an NSO car and told to drive into the desert on my way to the telescope along a deserted road lined with saguaros, ocotillos, and sagebrush, encountering the occasional tumbleweed. I had never driven an automatic car, in fact had never driven a car in the USA, and had only just arrived from the Netherlands with no opportunity to adjust to the time difference, so it was quite the adventure. That continued once I reached the mountain and was shown how to use the enormous telescope (guided through the setup by the very patient and helpful Bill Livingston, Jack Harvey, and Buzz Graves). During the observing run before mine, something in the telescope building had tripped the circuit breakers repeatedly, and the observer had simply reset the breakers each time. This caused the power cable going into the building to melt. Repair would take time, so NSO ordered a large mobile generator to power the building. Unfortunately, its running speed varied irregularly, sometimes causing the telescope's electronics to complain, if not the circuit breakers to trip. That, the clouds, the rains of the monsoon season, and the refusal of the Sun to provide an active region when I could have observed it, made for a failed observing run.

The second run, half a year later, did succeed, with beautiful weather, a cooperating Sun, and a fully functioning telescope. The data were exactly what Jacqueline Coté, Kees Zwaan, Steve Saar, and I needed to extend the relationship by Skumanich, Smythe, and Frazier (1975). As we had anticipated, their limited range in flux densities had led them to make a linear fit whereas a power-law fit with an index of $\approx 0.6$ lined up the quietest regions to the active-region core.

### 5.4. The magnetic carpet

The analysis of how flux elements are dispersed across the solar surface led to excursions into percolation theory and tessellation patterns and from there to a zero-dimensional model of the distribution of fluxes in concentrations across the quiet-Sun network (Schrijver et al., 1997, for a global 2D model see Meyer and Mackay (2016)). There, the emergence of ephemeral regions, advection into the supergranular boundaries, merging, fragmentations, and cancellations maintain a wide distribution of fluxes, rapidly decreasing in number with increasing flux content. I called the process magneto-chemistry and Alan Title named the

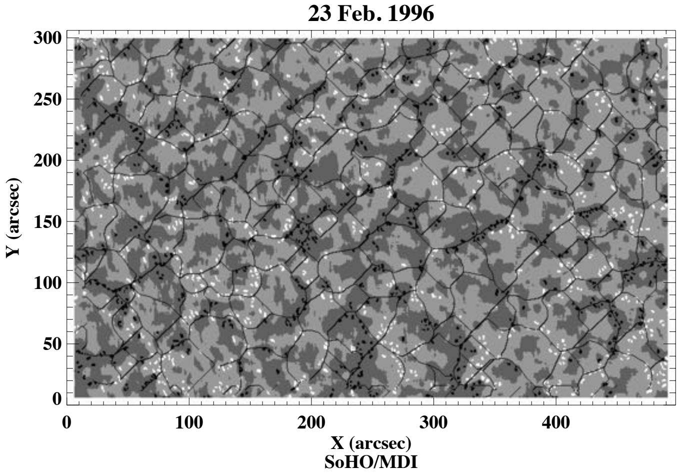

**Figure 8.** The magnetic carpet, *i.e.*, the mixed-polarity quiet Sun, results from the continual injection of ephemeral regions, their dispersal in the supergranular network, and the cancellation of flux in opposite-polarity encounters. The black and white patches show the flux concentrations for the two polarities in a SoHO/MDI magnetogram, while the light and dark gray areas show a map of the two polarities after a 6-arcsec smoothing was applied. The black lines show the cellular network formed by converging flows, determined from local correlation tracking; this supergranular network corresponds closely to the chromospheric and magnetic networks. (From Schrijver et al., 1997)

resulting pattern the magnetic carpet (Fig. 8). Simon, Title, and Weiss (2001) followed up on this with "cork movies" in a two-dimensional flow pattern mimicking supergranulation to show how the ephemeral-region population is crucial in maintaining the mixed-polarity network.

I should emphasize that the ephemeral-region population has a much weaker dependence on cycle phase than the active-region population. In fact, it appears that there is a background population in the least active areas of the quiet Sun that is essentially independent of the cycle, as born out by the dedicated work of Livingston et al. (2007). They observed a set of Fraunhofer lines over 33 years, including the Ca II K line. When compiling observations of the quietest disk-center measurements (in a 2' circular patch) over each of the many successive solar rotations, they found a cycle-independent level: "Our center disk results show that both Ca II K and C I 5380 Å intensities are constant, indicating that the basal quiet atmosphere is unaffected by cycle magnetism within our observational error." Such remarkable dedication to measurements (by the same people with the same instrumentation) can reveal important clues, in this case of impact even for, *e.g.*, studies of solar irradiance on terrestrial climate (Yeo et al., 2020). Note, by the way, that the use of the term "basal" by Livingston et al. (2007) – a minimum average level of magnetism of the quiet-Sun network – is distinct in its definition from what I discussed above; see footnote 2.

In addition to an extended temporal base, we can also benefit from an extended spatial range. As an example in the context of the magnetic carpet, I mention Thornton and Parnell (2011). They identify clumps of photospheric flux

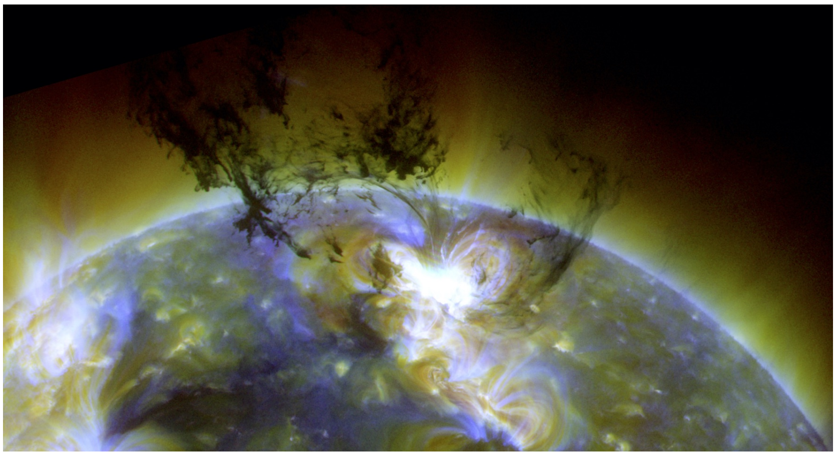

**Figure 9.** The Sun has provided us with prototypes of many of the phenomena that we consider responsible for stellar magnetic variability but Fabio Reale and colleagues took it one step further when they saw this: SOL2011-06-07T06:16 (here seen in a 171-193-211 Å SDO/AIA composite rotated by +90°) remains, as far as I know, the most impressive eruption of relatively cool matter from the Sun observed by SDO. The return of much of that matter, in what struck me as a gigantic bombing raid on the chromosphere, looked to Fabio's group as a plausible solar counterpart to what happens in stellar accretion (Reale et al., 2013; Orlando et al., 2019).

from $10^{16}$ Mx up to $10^{23}$ Mx, from internetwork scales observed with Hinode's Solar Optical Telescope up to active-region scales observed by SDO's HMI. They find a smooth number distribution over 18 orders of magnitude in frequency (see a study by Sakurai and Toriumi, 2023, on the shape of the distribution). Note, however, that there is a pronounced cycle modulation at the large scales in contrast to what happens at small scales, as is the case for the emergence frequency of bipolar regions (Fig. 7 *left*). Another example of a lesson from an extended spatial range is discussed in Sect. 7.1 (and shown in Fig. 7 *right*).

### 5.5. Stellar magnetic activity

In this summary, I am skipping multiple topics that I worked on in the course of these years: cool-star coronal spectroscopy with EXOSAT, EUVE, ASCA, and (preparatory work for) AXAF; the deviation from flux-flux relationships of mid to late M-type dwarf stars; the suppressed activity of F-type main-sequence stars and the enhanced activity of tidally interacting binaries given their rotation period; the exploration of stellar coronal loop geometries and the possibility of non-negligible optical depth in the strongest coronal lines; rotation-activity relationships; … I do not want these stellar topics to make this Sun-centered memoir unduly long; they could fill a book. In fact, they do. In the early 1990s, I had been pushing Kees Zwaan hoping to convince him to write a book about solar and stellar magnetic activity, a field in which he was one of the pioneers. But when he finally agreed, it was on the condition that I would be his co-author (Schrijver and Zwaan, 2000). This sparked a period of some three years in which we would both write on our respective sides of the world, occasionally coming together for a week of intense discussions in the beautiful, quiet forest-setting of

Kees' home in Driebergen in the center of the Netherlands or, when Kees and his wife Prisca were visiting Iris and me, in our home in San Jose, CA. Writing a book, I quickly realized, is hard but rewarding work. I learned so much from processing and integrating the literature and from discussing things with Kees.

Leaving so much stellar work out of this memoir goes against my conviction that we can only learn how the Sun's magnetism has affected, affects, and will affect Earth by using the population of cool stars: looking at a mix of young and old stars, stars with different fundamental properties, with different abundances, and with or without tidally interacting neighbors is essential to advancing solar physics, as this essentially provides us with equivalents of both a time machine and an astrophysical laboratory (*cf.*, Fig. 9). Of course, there are interfaces at meetings where solar and stellar astrophysicists come together, and a few of us actually work in these two seemingly distinct areas, but funding agencies are remarkably efficient at keeping these closely-related and mutually-dependent specialties apart. Moreover, much to my (naive?) surprise, these two communities often see each other as competitors for resources. I remember a discussion at an AGU meeting where I suggested that a better integration of cool-star science with the solar-heliospheric community would benefit all, only to be rebuffed brusquely by the statement that "we're not giving *them* any money." It seems the tide is turning, though: with the discovery of exoplanets and the growth of the field of planetary habitability, the "solar-stellar connection" is evolving into an all-encompassing stellar-astrospheric-planetary ecosystem science (*e.g.*, Garcia-Sage et al., 2023), *i.e.*, into a field that could be referred to as comparative heliophysics – a point to which I return in Sect. 8.

## 6. Emergence, dispersal, and cancellation of magnetic field

### 6.1. The plage state and flux dispersal

The development of the second-generation construction kit to provide deeper insight into the connection between local and global flux-flux relationships through the distribution $h_t(|\phi|)$ of flux on the solar/stellar surface required several enabling steps. First among these was an analysis of how active regions decay into the surrounding network. The prevailing idea behind this was, and remains, that it is a consequence of the random walk of flux concentrations caught in the evolving surface convection, dominated by supergranulation (initially proposed by Leighton, 1964, although he missed the role of meridional advection on the largest scales of the process which was not picked up on until the Ph.D. thesis by Mosher (1977), a text that greatly helped shape my understanding of flux transport). But such a decay should lead to a gradual transition from high flux density interior regions into low flux density network, with a profile gradually spreading out while reducing its amplitude until the active region has disappeared into the network. That concept clashed with my finding that active regions have a common mean flux density of some $100\,\mathrm{Mx\,cm^{-2}}$ within a fairly well-defined periphery (Schrijver, 1987). When Alan Title learned about this so-called plage state via Kees Zwaan, he invited me for an extended visit to Palo Alto, temporarily taking me away from my postdoc position in Boulder.

Alan Title, Dick Shine, George Simon, and a team of colleagues had been analyzing observations of the solar surface taken during a space shuttle flight (with the Solar Optical Universal Polarimeter (SOUP) instrument on Spacelab 2 that recorded images from the 30-cm telescope onto 35 mm film that was later digitized; Title et al., 1986; Simon et al., 1988). They had developed a flow-mapping algorithm ("local correlation tracking") to map the divergence of the flows, and then trace the motions of simulated test particles in that flow (Simon et al., 1988). They called the resulting image sequences "cork movies." Alan showed me these and suggested they might hold a clue to the plage state.

I was given access to the original data and the derived products, stored on what was then state of the art: so-called optical disks which enabled quick data access and efficient viewing. I spent many hours staring at these movies. I cannot reconstruct whether it was Alan Title or Kees Zwaan who made the suggestion, but as I was visiting Palo Alto, I connected with Sara Martin at BBSO/CalTech to see if they had suitable observations of active regions. After reviewing many hours of films in the Pasadena archive – trying to find image sequences with the least interruptions by weather and distortion by atmospheric turbulence – we did identify the kind of observations we were looking for, and tracked the movement of flux concentrations over a five day period.

The combination of these space- and ground-based data led me to the conclusion that perhaps a reduction of supergranular scale size (possibly through a suppression of the largest scales) combined with flux movements primarily along the boundaries between convective cells could account for the plage state and for a contrast between effective flux diffusion coefficients of more than a factor of two between inside and outside active regions. Some time later, I was privileged as a largely-unknown postdoc to sit down with a very kind Gene Parker over a lunch during a science meeting in Freiburg, Germany, to discuss possible reasons for this, but even using back-of-the-envelope (well, actually front-of-a-paper-napkin) sketches, we came no closer to a solution (not surprisingly: we still do not fully understand supergranulation, see *e.g.*, Rincon and Rieutord, 2018).

Later work with then-Ph.D. student Mandy Hagenaar on SoHO/MDI magnetogram sequences suggested that a flux dispersal coefficient $D$ useful for the modeling that I was thinking of would depend on the flux $|\Phi|$ of magnetic concentrations. In quiet, mixed polarity regions, flux concentrations would typically be small, being fragmented in the flows and weakened by cancellation against opposite polarity, and these small flux concentrations would be quite mobile corresponding to a relatively high diffusion coefficient. In the predominantly unipolar active-region plage, frequent mergers and the absence of cancellation collisions results in larger concentrations with lower mobility and thus reduced effective diffusion. That non-linearity in the dispersal of flux through a flux-dependent diffusion coefficient proved an important ingredient in the surface flux dispersal modeling that formed the foundation of the second-generation construction kit. Note that Worden and Harvey (2000) independently confirmed the need to lower the flux dispersal within magnetic plages based on full-sphere flux transport modeling (implemented in different ways in the LMSAL assimilation model[4] and in the ADAPT model described by Hickmann et al., 2015).

Surface flux dispersal was developed by Ken Schatten et al. (1972), Neil Sheeley, Yi-Ming Wang, and colleagues (see Sheeley, Jr., 2005). Until 2000, this modeling was essentially in the hands of Wang and Sheeley, who, through a series of illustrative and insightful papers, showed the fundamental applicability of the concept that the Sun's magnetic field, once at the surface, behaved essentially as a freely-advecting signed scalar subject to diffusive dispersal while embedded in the large-scale differential rotation and meridional advection. But I wanted to develop a model that could be used as a numerical laboratory, in which each of the components could be varied in order to explore conditions on other real or hypothetical stars as well as for the Sun in time (Schrijver, 2001). Others have done so too (most using a continuum description for the field, in contrast to the gridless particle-tracking approximation that I used, or the grid-warping concept independently developed in parallel to my work by Worden and Harvey, 2000), demonstrating the value of different approaches and assumptions (*e.g.*, Mackay, Priest, and Lockwood, 2002; Upton and Hathaway, 2014; Martin-Belda and Cameron, 2016; Nèmec et al., 2023; Yeates et al., 2023), and with different modes of data assimilation (see a comparative assessment by Barnes et al., 2023). The implication behind that statement deserves an explicit formulation: funding agencies often consider only a single model needed, but I have repeatedly seen the value of having several independently-developed models in homing in on a deepened understanding of a process; other examples can be found in Sect. 6.2 on coronal heating and Sect. 7.2 on coronal field modeling.

In the first application of my flux dispersal model, I demonstrated that the distribution function of flux densities across the solar surface, $h_t(|\phi|)$, arises as a natural consequence of the flux emergence patterns (including the butterfly diagram, nesting properties, ephemeral regions and active regions, and all the other ingredients based on solar observations) through the solar cycle. I also showed that by only changing the total number of emerging bipolar regions, local and disk-integrated flux-flux relationships naturally transformed into each other (through Eq. 1), even when going well beyond the flux-emergence rates exhibited in the course of the modern-day solar cycle, thus showing that a truly Sun-like star with a range of different levels of activity would be consistent with many stellar observations. There is a problem of non-uniqueness here, of course; the above does not mean that the dynamo in young, active stars does not behave differently from that of the Sun, but it implies it need not do so.

### 6.2. Select applications of the second-generation construction kit

A large increase in the rate of flux emergence to mimic the most active stars, led to an unanticipated outcome through the functional dependence of $D(|\Phi|)$: in the most active simulated stars, the mean field strength of the polar caps could match or exceed that found in the penumbrae of sunspots, thus suggesting

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[4] https://www.lmsal.com/forecast/

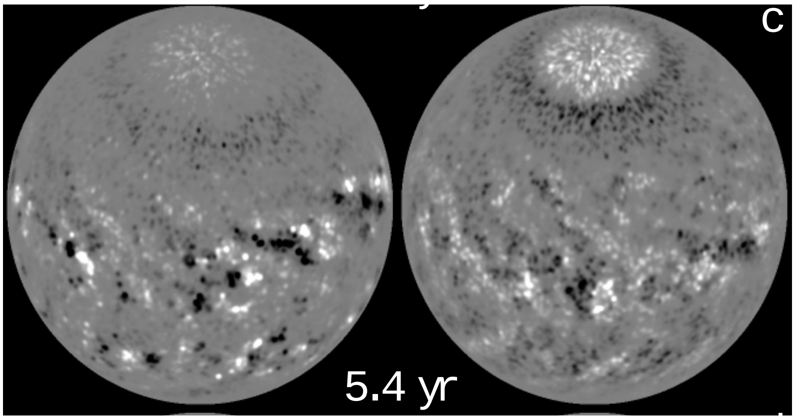

**Figure 10.** Simulated magnetograms for the Sun (*left*) and a Sun-like star (or a young Sun) with a strongly enhanced rate of flux emergence (*right*), visualized from a perspective 30° above the stellar equator about midway through a magnetic cycle. The observed decrease of flux dispersal with increasing size of the concentrations (see Sect. 6.2) suggests that strong polar caps, if not polar starspots, may form on more active stars through increased clustering of flux that is swept poleward in the meridional advection even if the geometry of the butterfly diagram does not change. (From Schrijver and Title, 2001)

one possible formation mechanism of polar starspots (Schrijver and Title, 2001, see Fig. 10) that are inferred to exist on such stars through (Zeeman) Doppler imaging. But of course there are alternative formation mechanisms. In rapidly rotating stars, flux emergence is likely affected by Coriolis forces that would act to deflect rising flux to higher latitudes; the effects of that for flux distribution on the stellar surface and the potential formation of polar spots has been studied by, *e.g.*, Holzwarth, Mackay, and Jardine (2007) and Işık et al. (2018).

Another surprise resulted when Marc DeRosa, Alan Title, and I drove the model with a sunspot record that mimicked that observed for the Sun since the Maunder Minimum. We found that big differences between successive cycles could cause the simulated polar caps to occasionally not reverse polarity, inconsistent with historical records (Schrijver, DeRosa, and Title, 2002). We proposed a flux decay process that was later given a physical foundation by Baumann, Schmitt, and Schüssler (2006). Wang, Sheeley, and Lean (2002) suggested alternatively that modulation of the meridional advection could (also) be involved in avoiding such non-reversals of the polar field polarity, while Cameron et al. (2010) looked into the effects of changing active-region tilt angles. How much any of these processes are involved in the solar dynamo remains a field of study (*e.g.*, Jiang et al., 2014; Yeates et al., 2023).

After these and other experiments, Marc DeRosa and I transformed the code so that it could assimilate magnetograms (first those of SoHO/MDI, later SDO/HMI) and coupled it to a potential-field source-surface (PFSS) model that Marc coded up (and that has found frequent use after Marc made it part of the SolarSoft library). From it we learned (Schrijver and DeRosa, 2003), for example, how open field originates in both quiet Sun and active regions, even reaching upward from sunspots (Fig. 11). The flux-assimilation-and-transport model (see footnote 4), along with its PFSS superstructure, continues to run at LMSAL, at

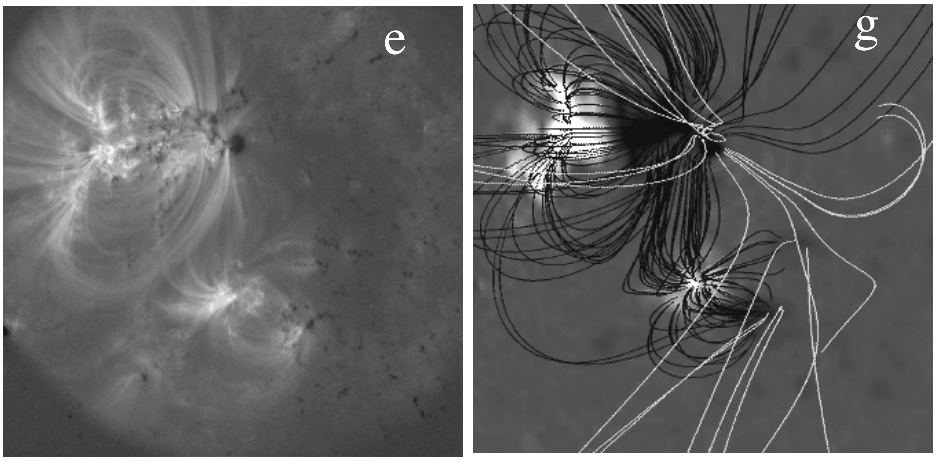

**Figure 11.** The heliospheric field has its roots not only in large-scale coronal holes over quiet Sun, but also in active regions, often involving sunspots, as in this example. *Left*: a blend of TRACE 171 Å and a SoHO/MDI magnetogram shows both coronal loops and sunspots. *Right*: A global PFSS model based on a low-resolution magnetic map (shown in the background) shows connections of closed (black) field lines and open (white) connections to the heliosphere. (Panels from a figure by Schrijver and DeRosa, 2003)

the time of this writing extending over 28 years, having provided guidance and field models to many community studies of solar and heliospheric activity.

With the PFSS package came the possibility of exploring coronal heating, not for individual loops – which are hard to disentangle from others and the background diffuse emission in the transparent corona and hard to trace completely given their multi-thermal nature – but instead for ensembles, *i.e.*, for entire active regions or, as we attempted, for the global corona. At my initiative, Markus Aschwanden developed a set of analytical approximations to quasi-hydrostatic loop atmospheres with different heating scale heights and expansions with height (Aschwanden and Schrijver, 2002) that enabled us to quickly populate multitudes of coronal "loops" that were traces of the field from the PFSS model. A student, Annie Sandman, then bravely took on the challenge of populating a volume with such loop atmospheres for subsequent 2D rendering. When JPL's Paulett Liewer learned of this, she asked for examples rendered from different pairs of viewing angles as they were preparing for the launch of the STEREO spacecraft in October of 2006. When we put these images on our (then brand new) stereoscopic displays, one of the things we learned was that the human brain is best at interpreting these transparent volumes as 3D structures when the separation in viewing angles is of the order of 5–10 degrees; that is not surprising, as that is what we use in daily life, too. But it makes one think of a mission that would provide such image pairs to interpret coronal structures and the initiation of CMEs, which would be of great help since we cannot at present reliably model the Sun's non-potential fields; more on that in Sect. 7.

With this visualization code in hand, we experimented with different parameterizations of the dependence of coronal heating on the base field strength and loop length (Schrijver et al., 2004). Simulated images for the EUV and soft X-ray corona could approximate observations for a heating function that pointed us toward a mechanism dominated by nanoflare reconnection induced by footpoint braiding, at least for loops with lengths up to some 50,000 km. For longer loops (typically the cooler ones), however, more heating was needed than provided by that best-fit scaling of the coronal heating, so it remained an ambiguous outcome and a limited success.

Later, Warren and Winebarger (2007) performed a similar visual rendering, but focused on a single active region. They based their modeling on hydrodynamic simulations of loops in a PFSS model rather than approximated quasi-static ones, finding “that it is possible to reproduce the total observed soft X-ray emission in all of the [Yohkoh] SXT filters with a dynamical heating model, indicating that nanoflare heating is consistent with the observational properties of the high-temperature solar corona.” They, too, however, noted problems in describing the cooler EUV loops.

Lundquist et al. (2008) chose yet another approach: for a set of 10 active regions, they computed a non-potential “quasi force-free” field model and populated loops with quasi-static atmospheres. They, too, homed in on braiding-driven DC heating, but concluded that although “this parameterization best matches the observations, it does not match well enough to make a definitive statement as to the nature of coronal heating. Instead, we conclude that (1) the technique requires better magnetic field measurement and extrapolation techniques than currently available, and (2) forward-modeling methods that incorporate properties of transiently heated loops are necessary to make a more conclusive statement about coronal heating mechanisms.” I could not agree more with that assessment, even now, 15 years later. Moreover, the views that a single heating mechanism dominates throughout the entire corona and that different energy-conversion mechanisms operate independently of each other are increasingly questioned (see for example the reviews by Klimchuk, 2006; De Moortel and Browning, 2015).

I will mention one more application of the surface flux dispersal code, with some introduction. With the increasing number of transiting exoplanets came the need to also understand the contributions to the transit light curves from structures on the surfaces of their parent stars, particularly when transit spectroscopy is used to derive information about the exoplanetary atmosphere (*e.g.*, Rackham, Apai, and Giampapa, 2018, 2019). Conversely, however, the transiting exoplanet can tell us about the structures on stellar surfaces down to resolutions that are far below what can be achieved by rotational-modulation or Doppler-imaging studies (*e.g.*, Silva-Valio and Lanza, 2011). To explore that for myself, I used the construction kit (Schrijver, 2020) to simulate the quiet regions, active plage, and starspots on stars with flux emergence rates up to 30 times the solar-maximum value, and used the results of the MURam code for facular contrast versus limb distance at three wavelengths published by Norris et al. (2017). The conclusions exemplify the value of the solar paradigm to astrophysics just as much as the value of stellar studies to solar physics: “This (1) indicates that the solar paradigm is consistent with transit observations for stars throughout the activity range explored, provided that infrequent large active regions with fluxes up to $\sim 3 \times 10^{23}$ Mx are included in the emergence spectrum,

(2) quantitatively confirms that for such a model, faculae brighten relatively inactive stars while starspots dim more-active stars, and suggests (3) that large starspots inferred from transits of active stars are consistent with clusters of more compact spots seen in the model runs, (4) that wavelength-dependent transit-depth effects caused by stellar magnetic activity for the range of activity and the planetary diameter studied here can introduce apparent changes in the inferred exoplanetary radii across wavelengths from a few hundred to a few thousand kilometers, increasing with activity, and (5) that activity-modulated distortions of broadband stellar radiance across the visible to near-IR spectrum can reach several percent." I note that the apparent need to include some very large active regions in the most active stars may be an alternative formulation to the need for enhanced active-region nesting to explain the observed rotational modulation of stars (Nèmec et al., 2023).

This last project led to an accepted proposal to ISSI, together with Louise Harra, Kevin Heng, Moira Jardine, Adam Kowalski and Ben Rackham, to bring together the solar, cool-star, and exoplanetary communities in a meeting in Bern to learn how to disentangle the various contributions to transit spectra to derive information on stellar magnetism and on exoplanetary atmospheres. Unfortunately, we had to cancel that meeting because of both COVID-19 and personal health reasons. It would have been a truly fun project! In this context, let me point out the excellent book by Jeff Linsky (2019) the title of which states its focus: "Host stars and their effects on exoplanet atmospheres."

## 7. Coronal structure and dynamics

### 7.1. Learning from the corona

By the late 1960s, rocket-borne X-ray telescopes had already revealed that the solar corona is structured in loop-like shapes determined by the Sun's magnetic field. A decade later, Rosner, Tucker, and Vaiana (1978) developed their famous quasi-static loop model linking loop length, density, temperature, and heating rate through scaling laws. When I started working on the interpretation of stellar XUV spectra, it was in the context of this "RTV" loop concept. In the 1980s, many authors assumed that coronal loops were long-lived structures and consequently felt the need to prove that loops would be thermally stable when subjected to variable heating. I even started writing a review of such studies that I thought might be included in my Ph.D. thesis. I never completed that chapter, however, because many of the studies appeared to rely on rather artificial assumptions and approximations. It was not until the late 1990s, however, that the TRACE mission with its arcsecond resolution (slightly better even than that of SDO's AIA) and 30–60 s image cadence demonstrated that coronal loops were not long-lived at all.

Yohkoh's X-ray telescope and SoHO's Extreme-ultraviolet Imaging Telescope (EIT) had been observing the Sun's corona for some seven and three years, respectively, by the time TRACE was launched. Their combined full-Sun, multi-thermal coverage provided many new insights into the large-scale physics of

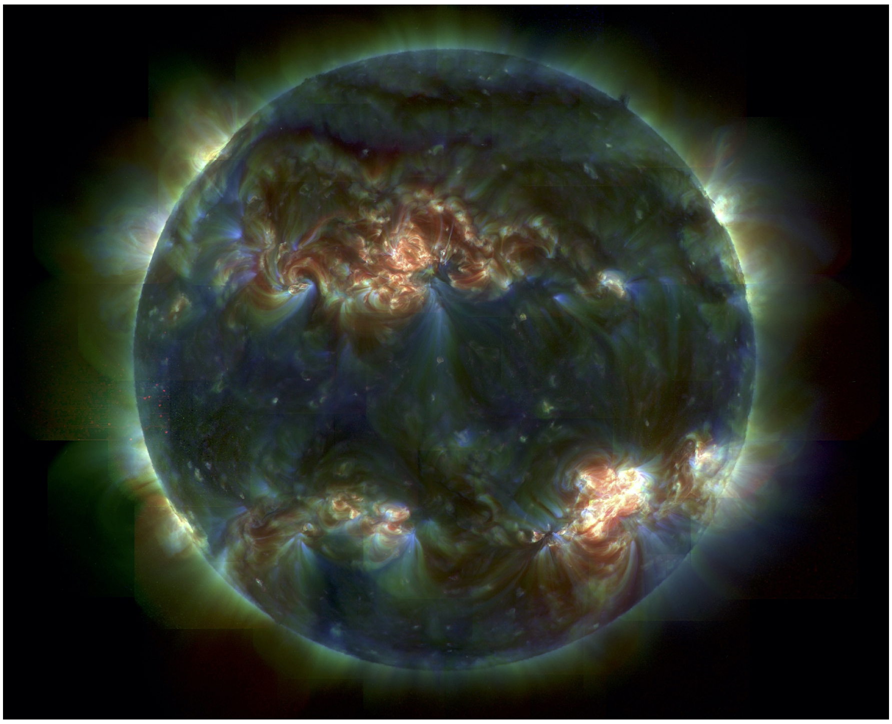

**Figure 12.** A three-wavelength full-disk mosaic of the EUV corona observed by TRACE on 1999/08/02. These images (put together with software developed by Ed DeLuca at CfA, Sam Freeland at LMSAL, and others) took well over a full TRACE orbit of 96 minutes, during which Earth eclipses and high-latitude radiation zones had to be avoided. SDO/AIA made a gigantic leap forward by providing eight-wavelength full-Sun images on a 12-second cadence with very few interruptions throughout its now 13 years of service.

the corona (*e.g.*, Parnell, 1998). When TRACE came along in 1998 (Fig. 12), it opened a new window on the smaller length and time scales. Its initial discoveries were formulated in a paper that appeared only a year after its science observing began (Schrijver et al., 1999).

The first year of TRACE was a very busy time for the science team spread out over LMSAL, CfA, and Montana State University (MSU). The planners rotated on a weekly schedule, flying out to Goddard Space Flight Center (GSFC) in Greenbelt, MD, once every four to six weeks, to spend much of a week holed up in the basement of the flight operations building, one floor below the much more spacious SoHO counterpart. We coordinated our planning with SoHO's instruments, thus increasing the coverage in spectral, temporal, and spatial dimensions (although, as I mentioned above, SoHO's instruments were often off looking at different solar targets forcing us to choose who to coordinate with). The mornings were taken up by formulating an observing plan, selecting a target (although after the first months of exploratory observing we generally stuck with a given target for multiple days), and communicating with the SoHO team and members of the scientific community with specific observing requests, often in coordination with ground-based observatories around the globe.

Afternoons and evenings were dedicated to reviewing the observations of the preceding day(s) (using a movie-generation toolset developed by Dick Shine) and distilling out of these what we could learn about the solar corona. Among other things, we noted the shifting around of sites of strong coronal heating, frequent coronal rain in apparently quiescent settings, pulsations traveling into loops upward from the solar surface, linkages between distant regions with near-simultaneous emission features at times of flares and eruptions, and the intricate dynamics of the transition region seen in TRACE imagery as "moss." Many of these early discoveries spurred series of subsequent studies. For example, the transverse oscillations that excited Dick Shine, Charles Kankelborg and me as we first saw them upon reviewing a preceding day's TRACE sequence, led to hundreds of studies on transverse coronal loop oscillations: their initial study reported by Aschwanden et al. (1999) has more than 750 citations in ADS.

For twelve years, I collected images and movies by TRACE from our daily inspections by the planners and sometimes from the community to put online as "picture of the day" (POD). After a while, they were published weekly, a practice kept up throughout TRACE's 12-year operational period. Alan Title (the PI for most of the mission's duration) and I (science lead and then PI for the last four years) shared the same philosophy: there is so much to discover in what has been publicly funded, that we wanted to present events of interest to the worldwide community, hoping they would pick up on their analysis and interpretation. In many cases, it worked; although we never did a quantitative assessment, we have the impression that many of our PODs were the seeds for refereed publications.

This brings me to a note about the "open data policy" that is now mandatory within NASA's Science Mission Directorate. Although Earth science data at NASA were openly accessible since 1994, there was no such policy in the other science divisions. Alan Title pushed hard for this in the Sun-Earth Connections Division on the TRACE mission. Let me quote him directly on that (from an AIP Oral History interview): "When we flew TRACE, I decided that all the data that came out of TRACE would be available to everybody at the same time as we got it. There was a lot of internal discussion and a lot more discussion with our co-investigators. All kinds of concerns, for example, what would happen if after you've worked on a project eight or nine years and because the observations are public a person who was not on the team makes an interesting discovery and publishes? Why can't we have, as had been done in the past, the right to review any paper that comes out from the data? Or, why can't we have this data stay in a locked box in our building for a year before anybody else gets to look at it or forever before anybody gets it? [...] there's a moral issue too. The government spends a 100 million dollars or more of the nation's science budget on your instrument, and it costs another large amount of money to launch the mission. It is my opinion that you really have an obligation to society that this knowledge is used as broadly and as a well as possible. The other thing is, that no matter how clever you are, there's somebody else who will have new insights. [...] The TRACE policy worked really well. We didn't have any of the problems that were so worrisome initially. It got NASA to agree that this was the way to go. [...] The open data policy was the hardest battle that we fought."

When preparing for SDO, we settled on a different model to bring solar events to the community's attention. Phil Scherrer, the SDO/HMI PI, had formulated the idea that with a full-Sun observatory with continuous observing, the community would be operating in a mode in which they would, in effect, be observing the archive. In parallel, Neal Hurlburt, our data system lead, came up with the idea of an event database: instead of having to download and search through terabytes of HMI and AIA data to find events, Neal's programmers developed an archive of events to be populated by team members who would identify events in observations on a daily basis, complemented by software that would run on summary data in house or elsewhere in the world. That led to the heliophysics events knowledgebase (HEK, Hurlburt et al., 2012)[5] and a search interface for users (iSolSearch)[6] which now has a web browser interface and also IDL- and Sunpy-based AIPs. This, too, has guided the solar-physics and heliospheric communities to events of interest that led to research publications, although – as I noted above – it still too infrequently leads to population studies.

The TRACE team executed its own research, too, of course. I focused preferentially on things where being right on top of the data archive of TRACE and near to that of SoHO's MDI was beneficial to reviewing large volumes of data. These included the study of transverse loop oscillations compiled from years of TRACE observations, an assessment of the conditions under which the corona over active regions showed clear signs of non-potentiality (using the flux assimilation and PFSS modeling described above), and – in 2010, the final year of TRACE operations – a study that showed that the distribution of the number versus the size of eruptions from ephemeral regions smoothly extended such a distribution for coronal mass ejections (see Fig. 7).

### 7.2. Non-potential fields and its instabilities

For the first two decades of my scientific career, I steered clear of solar and stellar flares and coronal mass ejections, mostly because I saw them as infrequent interruptions of the (relatively) quiescent background state, and also because it seemed that observational data rarely sufficed for a comprehensive picture. I avoided flare studies despite working at the space research laboratory that built the Hard X-ray Imaging Spectrometer (HXIS) for the Solar Maximum Mission that was launched in 1980, just prior to my Ph.D. phase. But the frequently beautiful TRACE observations of eruptive and explosive processes made a huge difference in my view of flares. Then NASA's Sun-Earth Connections (SEC) Division launched its "Living With a Star" (LWS) program (a term that, I think, originated from Bill Wagner at NASA/HQ) and I found myself invited to its founding LWS Science Architecture Team; more on that in Sect. 8. When we were awarded the SDO/AIA contract in 2003 as part of the first LWS mission, I could no longer walk around solar energetic events or its causes at and below the solar surface. And when the SEC Division became the Heliophysics Division in 2007

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[5] https://www.lmsal.com/hek/

[6] https://www.lmsal.com/isolsearch

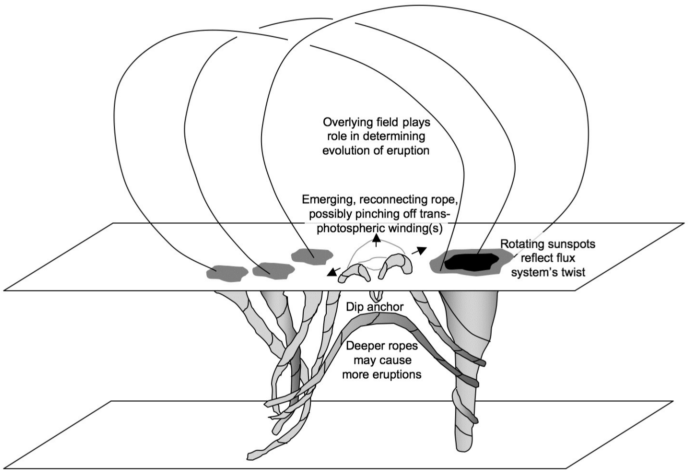

**Figure 13.** Schematic representation for an emerging field configuration involved in flaring or flux-rope eruption. (From Schrijver, 2009)

(a name adopted by Division director Dick Fisher when the NASA administrator challenged him more or less on the spot to come up with a one-word name for the division "that had better end in –physics"), it was clear that flares and eruptions needed to be an integral part of the SDO/AIA science program, which I helped shape as the team's science lead. What I mostly focused on, though, was not so much the flare or eruption itself as the conditions under which non-potential magnetic energy came to exist in the corona and the circumstances that might trigger the conversion of that free energy into other forms of energy.

Marc DeRosa, Alan Title, and Tom Metcalf (who died far too young in a skiing accident in the Rocky Mountains) joined me on the path to magnetic instabilities by reviewing the configurations and dynamics of the photospheric field that were associated with substantial non-potentiality of the coronal field of active regions as evidenced by mismatches between observed loop patterns and modeled potential fields. Then there was an opportunity that came out of a workshop to analyze observations of a large X17 flare observed from gamma rays to optical with US and Russian spacecraft. Another step to studying eruptions came when TRACE observed the accelerating rise of a limb filament (analyzed by Chris Elmore, a summer student, and interpreted with modeling by Bernhard Kliem and Tibor Török). And my initial learning curve culminated in a review presentation at the COSPAR general assembly in Montréal, Canada, in 2008 (*cf.*, Fig. 13). I learn far better by doing than by reading, so getting hands-on experience with such studies and writing a review proved most helpful.

In preparing for the flight phase of SDO it seemed a good idea to assemble an international team of experts to learn to use the future SDO/HMI vector magnetograms to create non-potential models of active-region coronae. So, in 2004, Marc DeRosa, Tom Metcalf, and I invited experts in non-potential force-free field (NLFFF) modeling to come to LMSAL in Palo Alto to prepare for SDO: we were joined by Yang Liu, Jim McTiernan, Stéphane Régnier, Gherardo Valori, Mike Wheatland, and Thomas Wiegelmann. In my unfamiliarity with the problem, I had imagined that the key problems would be validation of the vector field map (with its intrinsic ambiguity in the field direction transverse to the line of sight) and acquiring adequate resources to efficiently compute the field. How wrong I was! That initial meeting caused the team to walk back from that ambitious modeling goal and to start with what seemed the simplest first step: reproduce an analytically known NLFFF model based on the lower boundary vector field. This led to a series of meetings that we held both in the USA and in Europe, resulting in five in-depth team studies and several papers by various sub-groupings of the evolving team. And the result: it may be possible to successfully model the non-potential field of solar active regions based on photospheric vector field measurements, but one should never trust the result of any of the methods unless there is a validation of that model by a match to the observed solar corona (DeRosa et al., 2009). How to quantify the quality of such a model still remains to be seen. I continue to be amazed that NLFFF models are produced (and let pass by referees) without at least a visual comparison with coronal images!

In parallel to the NLFFF project, I started looking at the evolving configurations of active-region magnetic fields involved in major flaring using the multitude of SoHO/MDI magnetograms. After reviewing 2,500 magnetograms and almost 300 M and X-class flares, there was no pattern that I could recognize that was uniquely associated with such large flares. That was hardly surprising because many colleagues had spent decades looking for such a signature. But I did notice "(1) that large flares, without exception, are associated with pronounced high-gradient polarity-separation lines [(certainly not a new finding, see the review by Toriumi and Wang, 2019)], while (2) the free energy that emerges with these fibrils is converted into flare energy in a broad spectrum of flare magnitudes [(also noted before, as well as after, *e.g.*, Wheatland, 2001; Cicogna et al., 2021)] that may well be selected at random from a power-law distribution up to a maximum value," and noted that such features tend to form in association with flux emergence (confirmed by Welsch and Li, 2008). Based upon this, I proposed a metric, which I named "$R$", as a measure for the flux encompassed in the area near high-gradient polarity-separation lines which sets an upper limit to the flare intensity that could occur (Schrijver, 2007). That metric has been used in many subsequent studies as a forecasting metric for flares or the absence thereof, with Barnes et al. (2016) concluding that in a comparison of metrics "no participating method [proved] substantially better than climatological forecasts." But it seems to me that the final part of the second conclusion above is important because it contains the possibility that deterministic forecasting of flare magnitudes is, as a matter of principle, unachievable. Then again, expecting a deterministic forecast based on a single parameter for a complex, evolving magnetic field would be naive.

When Anny Malanushenko joined our group after completing her Ph.D. with Dana Longcope at MSU, I asked her to develop a method of NLFFF modeling that could be guided by observed loops as a form of scaffolding to which an algorithm would nudge a model field. She courageously accepted that challenge and succeeded (Fig.14; Malanushenko et al., 2012, 2014)! I hope that her algorithm and others like it help us move forward with NLFFF modeling.

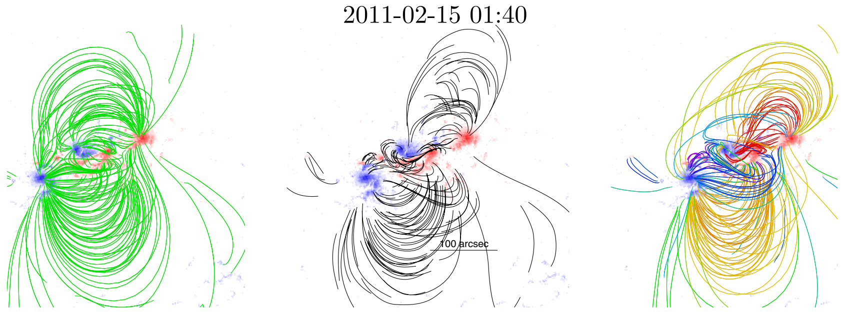

**Figure 14.** Coronal loops can in principle be used to guide nonlinear force-free field models over active regions. This image composite shows coronal loops seen over AR 11158, traced from coronal images (*center*), best-fitting field lines of a potential field (*left*), and a Grad-Rubin solution (*right*) over the magnetograms (the background red/blue images). The colors of the field lines in the nonlinear force-free field model in the *right* panel encode the value of $\alpha$ (defined by $\nabla\times\mathbf{B}\equiv\alpha\mathbf{B}$) ranging from strongly positive (red) to strongly negative (blue) through $\alpha\approx 0$ (green). (From Malanushenko et al., 2014)

### 7.3. The long reach of magnetism

The high-cadence full-Sun view of SDO's AIA revealed connections between activity in distant regions: filament eruptions or flux emergence in one location can deform the magnetic configuration sometimes more than a solar radius away. Combined with the full-Sun field models and STEREO EUVI observations of parts of the Sun not visible from Earth, we could, in one well-studied case, connect several flares and filament eruptions to the locations where the field topology was being modified by the emergence of an active region well behind the solar limb (note that the region's emergence was observed with EUVI, *i.e.*, in coronal emission only, because the STEREO spacecraft carried no magnetographs; Schrijver and Title, 2011). A few years later, we looked into another set of "sympathetic events" (Jin et al., 2016) using an MHD model to explore mechanisms of couplings; this became possible after Meng Jin joined us at LMSAL (first as a Jack Eddy Fellow and eventually taking on the role of PI of SDO/AIA that I had handed over to Mark Cheung when I retired), bringing with him his expertise with the AWSoM MHD model developed at the University of Michigan.

Paul Higgins and I performed a statistical study of the timings of events observed by AIA to find that large flares in one region not infrequently were at least temporally associated with activity in other, distant regions within a matter of hours, possibly enabling the release of non-potential energy there through gradual field deformations or wave-like perturbations, so that after that release activity in distant regions is temporarily somewhat less likely. But those effects are relatively weak, not quite at the $2\sigma$ level even for the large sample of events that we studied. Harvey (2020) notes, in his Sect. 5, that hints of such couplings were already seen in H$\alpha$ flare patrol movies of 1961 that eventually revealed Moreton-Ramsey waves.

The sensitivity of AIA and its ability to trace the evolution of the corona through a range of temperatures (*e.g.*, Cheung et al., 2015) revealed that whereas

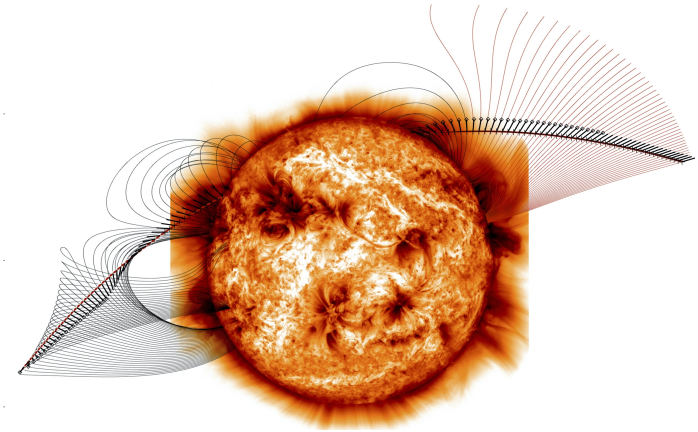

**Figure 15.** A Sun-grazing comet provided a (thus far) unique means to test MHD models of the connection between the high corona and the heliosphere. This image shows the (red) orbit of (Kreuz-group) comet C/2011 W3 (Lovejoy) superposed on a negative of an EUV image of the solar corona. The tangents (arrows) to the coronal magnetic field projected against the sky were compared to the trajectories of ionized material escaping from the comet and locking onto the magnetic field as seen in SDO/AIA EUV off-point images (not shown) as explored by Downs et al. (2013). (Courtesy Marc DeRosa)

such direct causal links may be relatively infrequent, the assumption that the coronal field surrounding destabilizing regions is relatively static is wrong: "the post-eruption reconfiguration timescale of the large-scale corona, estimated from the extreme-ultraviolet afterglow, is on average longer than the mean time between coronal mass ejections (CMEs), so that many CMEs originate from a corona that is still adjusting from a previous event" (Schrijver et al., 2013, and references therein).

### 7.4. With the help of solar meteors

Sometimes, opportunities to test our ability to model the Sun's magnetic field arise from entirely unexpected directions. In the days leading up to 2011/07/06, SoHO's LASCO coronagraphs observed one of the thousands of Sun-grazing comets that now populate its archive[7]. This one, C/2011 N3 (SoHO), was unusually bright in LASCO and could be traced in the images to the inner edge of the instrument's field of view. As I was inspecting the AIA data for 2011/07/06, I thought it would be fun to see if there was any signature of that comet, although I did not expect anything of that exercise. But much to my surprise, it did show up: for some 20 minutes, the comet was observed in up to five of AIA's EUV channels, first off-limb and later in the corona seen against the disk. STEREO-B's EUVI also recorded the comet, seeing it off the limb on closest approach from its near-quadrature position at the time. Orbital data showed that the comet (likely disintegrating as it sublimated in the sunlight) penetrated to about 100,000 km

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[7] https://sungrazer.nrl.navy.mil

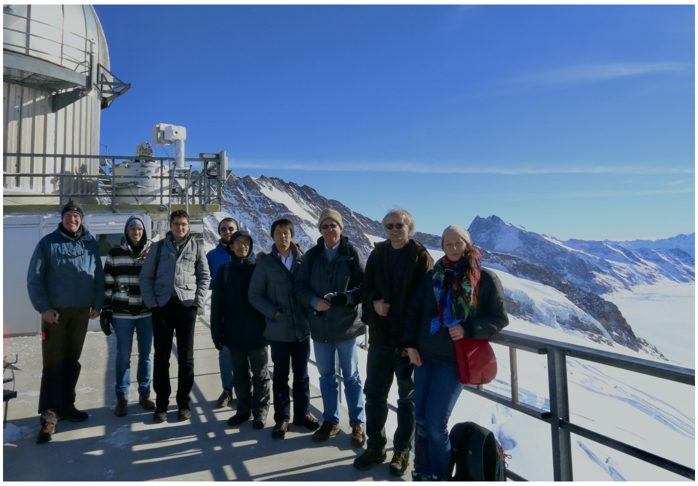

**Figure 16.** I have always found the International Space Science Institute's (ISSI's) welcoming environment and the many opportunities for relaxed, informal discussions (where it is okay to show one's unfamiliarity with the specialties of others) an efficient catalyst for discovery and learning. This photo shows members of the ISSI team on "Energy transformation in solar and stellar flares" (proposed by Louise Harra and Manuel Güdel) during their excursion to the Jungfraujoch Observatory. From left to right: Karel Schrijver, Adam Kowalski, our observatory host, Magnus Woods, Hirohisa Hara, Shin Toriumi, Hugh Hudson, Manuel Güdel, and Theresa Lüftinger (missing are Louise Harra, Miho Janvier, Kanya Kusano, Sarah Matthews, and Rachel Osten).

from the surface before its demise. AIA data clearly showed the deceleration of the ionized material in the comet's tail, enabling us to use the anticipated Lorentz force to estimate the comet's mass loss rate. A team of co-authors rapidly assembled around this first-ever detection of a coronal meteor, which brought me in touch with a very enthusiastic cometary community that I never anticipated crossing paths with.

It made all the teams involved more aware of the potential of detecting more such Sun-grazing comets, and for some time busy email traffic ensued each time LASCO detected a promising candidate. Amazingly, we had to wait for only a few months: on 2011/12/15–16 comet C/2011 W3 (Lovejoy) obliged. We observed it as it approached the Sun over the eastern limb, disappeared behind the Sun, and reappeared on the other side where it survived only briefly before it vanished. We were entirely ready for it this time, even performing a very rare set of off-point maneuvers with SDO. I realized that this time, the observations provided a unique view of the solar corona between the EUV-bright regions and the inner edge to where coronagraphs operated: seeing the ionized material trace trajectories that outlined the Sun's magnetic field provided a rare (and so far unique) opportunity to validate global MHD models in 3D (Fig. 15). It took a while to convince Jon Linker at Predictive Science Inc. of the promise of this project, and of the likelihood of interesting the journal Science in its publication, but a subcontract was set up and the results can be found in the study by Downs et al. (2013) published a year and a half later.

## 7.5. The largest flares

Another unanticipated opportunity to learn about solar activity, albeit on the topic of statistics, came from the Kepler spacecraft. Its results triggered Jürg Beer (who uses radionuclides to extract information from ice cores, tree rings, and rocks) and me to propose an ISSI team project: Kazunari Shibata had assembled a group of talented students to search the Kepler data for signatures of flaring on cool stars and the first results suggested a power-law frequency distribution extending that established for flares on the Sun to considerably more energetic events. The International Space Science Institute is a marvelous environment for what Jürg and I had in mind, namely to bring together experts on solar energetic events and stellar flaring, along with experts in cosmogenic radionuclides and their atmospheric transport and biospheric/geologic storage systems to gather information on flare-energy distributions to assess the probability of extreme flares on the Sun. Our team spent many hours assembled in Bern discussing how to compare the disparate passbands in which flares were observed, what kinds of signatures the associated energetic particles might leave in terrestrial tree-ring and glacier records and in lunar rocks, and how to bring all that information onto a common scale. One of the results of the activities of that team was that a proposed connection between large solar energetic particle events and enhanced nitrate concentrations in polar ice cores (McCracken et al., 2001) was shown to be untenable. At our first team meeting, Eric Wolff (a glaciologist and climate scientist) walked over to the University of Bern to retrieve chemical analyses of ice cores that showed that nitrate spikes were generally associated with biomass-burning plumes, thus removing nitrate as a plausible tracer of extreme flaring (Wolff et al., 2012). The ISSI team concluded that the Sun likely could produce flares and particle storms of strengths not observed within the half-century of flare monitoring. Follow-up analyses of the Kepler stars by Shibata and students left us with a less clearcut conclusion: many of the superflaring stars turned out to be binary stars that muddied the waters, leaving us uncertain about the most extreme solar events (Cliver et al., 2022).

A subsequent ISSI team project led by Louise Harra (Fig. 16) developed the rationale to look for the stellar equivalent of CMEs in coronal dimmings (Harra et al., 2016), since pursued in several theoretical and observational studies (*e.g.*, Jin et al., 2020; Veronig et al., 2021).

## 7.6. SDO beyond our peers

It proved to be great fun to show the capabilities of AIA to solar physicists and to our stellar colleagues (whose first reaction, upon seeing the early imagery at a Cool Stars meeting, included mostly fascination but also a discouraged “I'll just have to give up and retire after seeing this”) but also, of course, the general public (Fig. 17). Within a few years, AIA images made the covers of over 50 magazines ranging from Nature to Sky and Telescope. We showed the imagery at public talks in museums (including a near-live exhibit at the National Air and Space Museum developed by our Harvard team members), in planetaria, and at amateur astronomy clubs, domestic and abroad. I even found myself invited to

**Figure 17.** The LMSAL video wall was used daily for the annotation of events seen in the SDO/AIA data as input for the Heliophysics Events Knowledgebase (HEK) and to show the solar corona to visiting scientists and VIPs. Here, I explain the purpose of our studies to Aníbal Cavaco Silva, then president of Portugal, while Ralph Seguin (seated), who developed the display system, drives the imagery.

the White House for an "Astronomy Night" on its lawn (Fig. 18). And to this day, an automated Google search of news media sends me an email every day or two on SDO science or images being included somewhere in the world.

## 8. Heliophysics education

Next to doing research, a good part of my time was devoted to committee work as a representative of our community and to the education of the next generation of researchers. Little did I realize that in doing so, I would embark on over two decades of educating myself. It started one morning when George Withbroe, then Director of NASA's Sun-Earth Connection (SEC) Division called me with the invitation to join the 2000 SEC Roadmap team charged with developing a strategic plan for the next two decades. When a few months later I found myself at NASA/HQ in DC surrounded by two dozen of my peers, I could not believe that I was there. I think that the formal term is "imposter syndrome." It is something that I experienced on each of the twenty-some committees I have served on (including 25 years on the editorial board of Solar Physics) amidst scientifically knowledgeable and politically experienced peers (although the sense that they were by far my seniors has faded over the years).

That Roadmap exercise made me realize that I had much to learn about not only the neighboring sciences of heliospheric, magnetospheric, and Earth-atmospheric physics, but also about solar physics itself. That Roadmap was followed by an invitation to join the inaugural advisory committee for the Living With a Star (LWS) program the year after. Of course, I was there to represent the interests of solar physics, but I was also committed to advancing the intimately connected sister sciences. That attitude at times raised eyebrows. I

**Figure 18.** The second “White House Astronomy Night” hosted by President Obama on 2015/10/19. Alison Nordt (bottom right, who later became LMSAL’s manager) and I brought a large 4K screen onto the White House lawn to show SDO images along with a model of the IRIS spacecraft. Here we are on either side of the Mythbusters (Adam Savage and Jamie Hyneman) and staff of the Office of Management and Budget (OMB) explaining why solar physics is important to society.

remember, for example, a committee meeting in which I asked for a clarification into arguments used in a discussion between ionospheric physicists, upon which one of them asked me “What’s it to you? You’re a solar physicist!” That was a learning moment for me in an entirely different dimension. Although the LWS program has enabled many more interfaces between its SEC (now Heliophysics) subdisciplines, I fear that we have a long way to go before heliophysics is viewed as a coherent science rather than as a collection of its subdisciplines.

I also had much to learn to recognize and navigate the politics that hides below the surface in committees, including appreciating the importance of choosing words with care. Using (what appears to be) the wrong word can get you into trouble. Dick Fisher, the NASA Division director under whom SEC became Heliophysics, experienced this when he proposed the new name for the Division: there was a lot of criticism because it sounded too much like solar physics to the majority of the scientists working in the Division’s fields. I experienced his predecessor’s, George Withbroe’s, unexpectedly angry retort when I suggested in a Roadmap meeting at NASA/HQ that a mission to monitor the entire Sun in a variety of wavelengths would be of great value to SEC: “We do science at NASA! We do not ’monitor’!” Being a relatively junior member of the committee, I did not have the courage to argue with him at that time that maybe I should have used the word observe, but such a mission, SDO, did, in time, emerge and its ’monitoring’ led to many discoveries.

After learning a lot more during one more Roadmap and a 3-year membership of the Space Studies Board of the National Academies of Sciences, NASA/HQ’s Madhulika Guhathakurta (LWS program scientist at the time) and I sat down for lunch in a break at a meeting on “Earth-Sun System Exploration” in Hawaii

**Figure 19.** The first cohort of the Heliophysics Summer School in front of the HAO building in Boulder, CO (*left*), and the leadership of the first decade of the school (*right*): Nick Gross, Jan Sojka, George Siscoe, Karel Schrijver, Fran Bagenal, Dana Longcope, Lika Guhathakurta, Amitava Bhattacharjee, Meg Austen, and Dick Fisher (director of NASA's Sun-Earth Connections Division at the time of the School's inception).

to discuss a summer school in heliophysics. She brought me in touch with the incredibly sage and patient George Siscoe (a professor of space physics) with whom I submitted a proposal to NASA under the umbrella of UCAR's Cooperative Programs for the Advancement of Earth System Science (CPAESS) where the summer school has been ever since.[8]

George's advocacy for "universal processes" and my interest in the sister sciences of Heliophysics shaped the first Summer Schools held 2007–2009 in Boulder, CO (Fig. 19). I learned so much from the talented teachers that we had the privilege of working with, and they graciously accepted the detailed editing of the written versions of their lectures. But, boy, did I underestimate how much work editing three 400+-page volumes of the Heliophysics series would be! Although George and I were hoping for online books, NASA wanted printed volumes; we found Cambridge University Press to be an efficient partner in this process (and benefitted greatly from the CPEASS staff who helped us in obtaining permissions to use the many figures).

The largest challenge – which persisted throughout the now 17-year history of the School – was to get teachers to reach beyond their own discipline. The challenge there is not only the uncomfortable unfamiliarity with neighboring disciplines (demonstrated by the fact that George and I very rarely found a given reference to appear in more than a single topical chapter), but also the different uses of terms and even (apparently) fundamentally different frames of thinking about things. Take, for example, the use of the word dynamo, which can mean "work applied against the magnetic field" or "the process that maintains a magnetic field against decay," or the word convection, which can mean "advection" or "the hydrodynamic energy transport by overturning motions." Another example is that ionospheric physicists think in terms of electric field, $\mathbf{E}$, and current, $\mathbf{j}$, while their solar-heliospheric colleagues use magnetic field, $\mathbf{B}$, and velocity, $\mathbf{v}$. Gene Parker expressed his unambiguous preference for $\mathbf{B}$ and $\mathbf{v}$ to avoid "intractable global integro-differential equations"; he notes that "It is forgotten that the equations of Newton, Maxwell, and Lorentz are self

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[8] https://heliophysics.ucar.edu/summer-school

consistent. A complete solution in terms of **B** and **v** automatically implies the proper treatment of **E** and **j**" (Parker, 2014).

That challenge of teaching beyond the stovepipe separation of subdisciplines remained my main motivator for continuing to support the school. We were not only educating the next generation of researchers (with some 600 students participating thus far) but also giving our established peers views of the sciences that they might otherwise not have time for. In the end, George and I co-wrote some of the chapters and provided endless (and likely annoying) streams of hints to the lecturers/authors to reach across discipline boundaries.

After a three-year break, I engaged with the School for another cycle, resulting in a fourth book, a volume that emphasized comparative heliophysics of planets and stars surrounded by their astrospheres. That was far from the end of this project, though. After I retired and had more time to pursue things outside my direct professional commitments, I was awarded the 2018 Johannes Geiss Fellowship of ISSI. Spread out over two years, I spent several months in ISSI's wonderful setting in Bern (and added some nice long hiking vacations through the high Alps) creating the book that I – and I think also George Siscoe – had hoped for when we set out on the Summer School project but that I lacked the knowledge to create at that time: with a focus on "universal processes," the resulting book, "Principles of Heliophysics," presents a general introduction to heliophysics.

I also added 200 "activities" (homework exercises, discussion topics, background reading, etc.), a feature we overlooked in the original books. Only in 2021-2022 did I add "solutions" to the more challenging of these exercises at the urging of Nick Gross who continues to be an indefatigably upbeat partner in the School and without whom the practical "lab" side of the School could never have come about. This final product is now available at no cost on ar$\chi$iv and at printing cost on Amazon (Schrijver et al., 2022).

One envisioned volume was never completed. George and I hoped that a volume dedicated to the mechanisms and impacts of space weather would be added to the series. It was something that we hoped would grow with two or three lectures/chapters each year. With changing leaderships and evolving focus of the School, a completed, printed version never came to be, although a beginning is available online at UCAR's CPEASS[9] (with apologies to its authors who were told a printed book would someday appear).

Although this summer school provides an exploratory introduction to the field of heliophysics, it is obviously impossible to transfer sufficient knowledge in a 10-day experience to create anything approaching heliophysicists. And yet, there is no place where heliophysics is taught as a discipline. Heliophysics seems to be where the medical field was centuries ago, namely in a master-apprentice setting rather than in its modern-day university system of generalist-to-specialist training programs. Do we really expect that broadly knowledgeable heliophysicists somehow materialize through on-the-job training? With the rapidly growing field of comparative heliophysics, *i.e.*, the study of star-planet interactions in the

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[9] https://heliophysics.ucar.edu/sites/default/files/heliophysics/documents/HSS5.pdf

broadest sense, is it not high time to significantly invest in the education of the next generations of researchers?

The broad perspective provided by heliophysics sparked an entirely different project. As my wife and I discussed our professional activities, we realized that although she came at life from a molecular geneticist's perspective and I from the astrophysical side, there was an overlap when looking at the transient nature of our existence: the body of every living thing on Earth is in constant flux, with cells dying and being replaced, all with materials made sometime in the Universe's history. We decided to write a popular science book about all that entitled "Living with the Stars" (Oxford University Press). Iris and I enjoyed giving talks about this in planetaria, in science museums, and to college students, sometimes even together, emphasizing not only our transient nature but also that the idea that we exist separate from nature is completely flawed: we are part of the network of life and messing with our environment (from biodiversity to climate) jeopardizes our very existence.

## 9. Space weather and its societal impacts

Space weather (SWx), by several other names, has a long history, advancing from aurorae, geomagnetic variability, radio interference, and radiation hazards for astronauts, to societal impacts through our growing technological infrastructures on the ground and in space. The science of space weather is recognized as a core focus of heliophysics, and the activities of the solar physics community aimed at understanding solar variability on time scales from minutes to millennia are part of that. But as I sat in committee meeting after committee meeting, it began to bother me that investing in the forecasting of space weather was given a high priority even though SWx impacts seemed to be either minor for known events or speculative for hypothetical extreme events. Studies were being published warning the world of major, long-lasting power outages, of significant loss of satellite communications and navigation systems, of particle radiation causing upsets in aircraft avionics systems … but apparently without an adequate quantitative foundation of the likelihood or frequency of even the most impactful events. I add the qualifier "apparently" here because some (in fact likely much) of that quantitative foundation is shielded from the view of academic researchers, being considered competition sensitive or a matter of national security. Nonetheless, not seeing that foundation prompted me to attempt some assessments of impacts based on relatively common space weather.

This became a possibility when Sarah Mitchell (my SDO/AIA program manager at the time) came across a website of the North American Electric Reliability Corporation (NERC) where power-grid "system disturbances" were compiled. Comparing the timing of these events with geomagnetic records from around the USA, we could establish that during times of enhanced geomagnetic variability, the frequency of these system disturbances increased by a small (4%) but significant ($> 3\sigma$) fraction. The publication of that result put me on the invitation list for a space weather meeting in the catacombs of the US Capitol Building, where I was approached by Buddy Dobbins from Zurich Risk Engineering. We worked out a way that information on insurance claims to Zurich in the USA relating to electrical systems could be used in a comparison with geomagnetic activity without running into issues with client confidentiality. We concluded that regular space weather has impacts through the electrical power grids in Europe and the USA of the order of multiple billions of dollars each year, warranting the research investment even without considering potentially devastating extreme events (Schrijver et al., 2014).

Hoping to better quantify impacts, I hired an economics postdoc, supported by NASA/Ames, to try to come up with financial equivalents of impacts. It proved too hard, so we regrouped and decided to pursue a survey of the customer interests and needs of the forecasts issued by NOAA's Space Weather Prediction Center (SWPC). The SWPC leadership helped us by pointing its user base to our online survey, resulting in almost 3,000 responses. We learned that SWx "information is primarily used for situational awareness, as aid to understand anomalies, to avoid impacts on current and near-future operations by implementing mitigating strategies, and to prepare for potential near-future impacts that might occur in conjunction with contingencies that include electric power outages or GPS perturbations"(Schrijver and Rabanal, 2013). The users of these forecasts could not help us quantify the financial risks of space weather.

Despite the improvement of my understanding of space weather through these studies, the heliophysics summer school, and committee work, I was still frustrated at what I saw in the political arena. Space weather agencies, both in the EU and USA, would formulate instrument requirements based largely on desires for increased coverage by known types of instruments, albeit perhaps from new vantage points, such as in an orbital position trailing Earth to provide a side view of CMEs. This might work if the aim were, for example, to improve the forecasts of the arrival time at Earth of CMEs but it would not be of much help to the primary users of such information, namely the power-grid and power-plant operators. This was made explicit in one of many SWx meetings when, after the director of the SWPC claimed that the uncertainty of the arrival window of CMEs could be substantially reduced, I asked the NERC representative what he would do with such information. He stated that he would not recommend their "members" in the power sector take preventive action simply because the magnitude of the impact on the power grid would still be unknown. After all, apart from arrival time, we need to provide the magnetic field orientation of the CME complex in order to work through a model of the response of the Earth's magnetic field (which itself is an enormous challenge). I've likened it in discussions to telling people who are preparing for an outdoor wedding that the weather might change in the course of the event without information about what would change and by how much.

Of course, I was certainly not the only one troubled by the allocation of significant resources with insufficient scientific knowledge. In a discussion with Roger Bonnet and Madhulika Guhathakurta at the COSPAR meeting in Mysore, India, in 2012, we came up with the idea of a COSPAR-sponsored strategic roadmap on the science needs to improve SWx forecasts. As the saying goes, be careful what you wish for: soon after, I was invited to chair that roadmap exercise together with Kirsti Kauristie, an ionospheric-magnetospheric scientist at the

**Figure 20.** The total solar eclipse of 2017/08/21 was the first such event that I witnessed in person (*left*), along with some 40,000 others who flooded the small town of Madras, Oregon, and (*right*) the barn in which my NASA colleagues set up shop. It is the only venue that I have ever been to where there was more interest in the presentations by the scientists than in the live music being performed on the stage in the far reaches of the county fairgrounds.

Finnish Meteorological Institute. COSPAR provided us with the team and we set about with meetings, in person and by phone (Zoom and Skype weren't accepted as tools by all the parties involved, most annoyingly by my own organization), to shape a roadmap for the science of space weather (Schrijver et al., 2015). It was a wonderful team: in a truly collaborative spirit, we developed a consensus plan, starting with user requirements, via science needs, to potential instrumentation. The roadmap was presented at the 2014 COSPAR General Assembly in Moscow, Russia. Subsequently, Kirsti and I presented it to various stakeholder agencies. We hope that the case we made was heard: there is much to learn about the physics of space weather before forecasts become reliably actionable.

## 10. Retired?

Life is short and uncertain, so my wife and I decided to step away from employment fairly early. But – yes, the principal of my grade school was right – I have many interests, and science stayed with me. I already mentioned the book "Principles of Heliophysics," the Heliophysics Summer School, and work on exoplanetary transits. But how could I not be intrigued by the discoveries about exoplanets, their stars, their planetary systems, the issue of habitability, . . . ? Learning best by hands-on activities, I wrote a book about it all entitled "One of ten billion Earths: How we Learn about our Planet's Past and Future from Distant Exoplanets" (Schrijver, 2018). Oh, and I finally gave in to seeing a total solar eclipse with the naked eye. How could I not since, having moved to Oregon, it came practically through our backyard (Fig. 20)?

## 11. Good fortune and a flexible frame of mind

I have been incredibly lucky in my career, with remarkably fortuitous timing relative to fascinating space missions, the good fortune to be employed where the action was (it helped that I was very flexible in what I thought of as "the action"), and with amazing mentors, most notably my father with his technical skills, Kees Zwaan with his big-picture views, Rolf Mewe who reminded me to keep track of the details, Jeff Linsky who introduced me to the world of proposals and grants and who brought me to the USA, and Alan Title who introduced me to the politics of science and with whom, for over two decades, I sat down most mornings before the duties of the day for an often hour-long discussion of solar movies that he had just made, of slide sets for presentations, of papers, of proposals, of instrument designs, or of life in general. I am grateful to them and to all others who helped shape my path through science. I am also grateful for the many friendships that developed over the years.

In the Introduction, I promised a summing-up of critical observations about the research enterprise of solar physics:

- *Over-compartmentalization.* The solar and stellar branches of the study of cool-star magnetic activity are pursued by essentially disjoint communities and supported by segregated funding streams, as are the subdisciplines of heliophysics. In the USA, ground- and space-based solar and stellar research are supported by different agencies that focus more on integrating hardware than knowledge. Agency-level international coordination and collaboration is weak. Research funding is generally tied to specific observational platforms which makes it a high-risk – and thus generally avoided – endeavor to try to obtain coordinated observations (such as across the electromagnetic spectrum) through separate observing proposals for existing observatories, and even more so for proposed future ones. All this divides what could be a coherent community, making colleagues into competitors.
- *A weak academic foundation.* Solar physics is not taught as a science field in its own right, let alone the science of heliophysics (which to me encompasses cool-star physics as well as the study of (exo-)planetary systems). A broad education prepares one to spot opportunities, readies one to move with the trends of the day, and fosters appreciation of the work of others in neighboring disciplines. In order to appeal to modern-day students, we need to present them with an exciting, vibrant field full of opportunities.
- *Piecemeal funding.* Too many of our colleagues – certainly in the USA – make do with small grants and contracts that support them for at best months at a time. I do not understand why this is: a desire to micromanage, fear of the failure of larger projects, or community pressure to fund many proposals however inadequately on the side of funding agencies, or a community that finds fewer but larger grants too much of a continuity risk in a highly competitive environment? Whatever the reason(s), one should not expect frequent major breakthroughs from a few months of work. We are partly at fault ourselves: we take it that teaming arrangements increase chances of success for those few larger opportunities that exist (if they are

not required by the funders), but in doing so we effectively divide such grants into small, less impactful parcels.
- *High cost of space missions.* The high cost of building and launching space missions translates into infrequent mission opportunities, which indirectly leads to peculiar outcomes from this scientist's point of view: a STEREO mission without magnetographs; vector-magnetographic instruments, including that on SDO, without a demonstrated ability to successfully utilize the data; a Solar Orbiter with high-resolution instruments without the needed telemetry and an orbit that neither co-rotates with the Sun nor reaches the orbital inclination initially listed as scientific requirements; a Living With a Star program and a Space Weather Enterprise in which the component missions meant to study the system as a whole do not adequately cover it, sometimes not even overlapping in time … To improve on this state, (1) there need to be more opportunities, that are less costly to build and operate (and, in the case of spacecraft, cheaper to launch), and (2) there needs to be increased international cooperation led by the science community.

And when I review my life lessons as a researcher, these come out at the top:

- Never jeopardize your integrity.
- Treat everyone with respect, and learn from their viewpoints.
- Respect the time your audience gives you when you give a presentation: treat it like a performance, well-practiced and -timed, just as actors, musicians, or other stage performers would do.
- No matter how "difficult" a referee or reviewer may seem, carefully consider the advice: these colleagues generally mean to help you by pointing out that something can be improved about your methods, analysis, or presentation.
- Be willing to move into new research (and funding) areas, even though you will feel "out of your depth" as you dive into them: discoveries often lie at the interface of disciplines where "fresh eyes" may spot them.
- Be guided by intuition but work your logic hard.
- Do not move away from a situation that makes you unhappy unless you have worked out where you would like to find yourself instead.
- If your desired next job slips by you, consider that, in the end, you may find that new paths present themselves that are as rewarding.
- Share your knowledge and your data: there is plenty to discover.
- Collaborate with colleagues and support the community, both nationally and internationally: working in teams, panels, and committees broadens your expertise, gives you visibility, and offers new perspectives.
- A career in science is challenging, employment never certain, and willingness to move around required, but it is wonderfully rewarding to discover things, spot connections, and deepen your understanding of the Universe!

**Acknowledgments** Many of my scientific colleagues who have helped me in my development as heliophysicist are mentioned above but there are many more who remained unnamed to avoid this manuscript from becoming unreasonably long. There are many others still who helped somewhere along the way without whom things simply would not function or even exist: the hardware and IT engineers; those who make conferences, workshops, and summer schools actually happen; secretaries; administrators; the building support staff; and more. I thank you all. I thank Ed Cliver, Alan Title, Bert van den Oord, and the referee for comments on this manuscript. In writing this manuscript (and many others over the past thirty-odd years) I have made extensive use of the research-sharing platform ar$\chi$iv and of NASA's Astrophysics Data System Bibliographic Services.

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