Reflections on a 50-year career as a theoretician in heliospheric and solar physics

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Abstract. This paper is the story of what has motivated my approach to research throughout my career, and a statement of my belief that in order to maintain our scientific discipline as vibrant, dynamic and worth supporting, it is essential that we always seek and welcome new concepts and paradigm shifts.

1. Introduction
This paper is based on the talk I gave at the session of the 17th Annual International Astrophysics Conference that acknowledged my 75 years on this Earth, and my 50-year career as a theoretician in heliospheric and solar physics. The paper is not intended to be a review of the science that has been conducted during the 50 years of my career, by me or by others, but rather is simply the story of what has motivated my approach to research throughout my career, and a statement of my belief that in order to maintain our scientific discipline as vibrant, dynamic and worth supporting, it is essential that we always seek and welcome new concepts and paradigm shifts.

I entered the field of heliospheric research in 1968, when I published my first paper in the Journal of Geophysical Research [1]. My Ph.D. degree is from the University of California, San Diego, in 1969, and was followed by a postdoctoral appointment at the NASA Goddard Space Flight Center, and a civil service position in 1971, working for Frank McDonald, who will appear several times in this story.

The community of space researchers at that time was small, particularly the theoreticians. You could count on two hands your competitors or colleagues. Everything was still new. Much was unknown. We were data starved. New ideas were welcomed and encouraged. There was no conventional wisdom yet.

Theoreticians in some cases had yet to prove their worth. Many of the important scientists were of the opinion that a good scientist can build the instrument, analyze the data, and interpret the results. What value added is there for a theoretician?

Ambitious theoreticians usually took a two-prong approach. You needed some problems to work on and publish that people were interested in. In my case it was cosmic ray modulation. However, you were also always on the lookout for a new observation that demanded a new explanation, for which you could become famous.

We all knew the so-called Parker moment. When Gene Parker developed his theory for the acceleration of the supersonic solar wind in 1958 [2], he was roundly criticized as wrong, only to be vindicated when Mariner 2 in route to Venus in 1962 observed the supersonic wind. This was a goal for us. Have a theory that is controversial and challenged, which you defend against criticism, and then are ultimately proven correct. If you have a few Parker moments in your career, your success is assured.
2. My first Parker moment – Anomalous Cosmic Rays

My first opportunity for a Parker moment came in 1973 with the discovery of what became known as anomalous cosmic rays, or ACRs. As shown in Figure 1, Hovestadt et al. in 1973 [3] and McDonald et al. in 1974 [4] discovered a new component of cosmic rays at energies of a few MeV per nucleon, which had a very unusual composition. In was greatly enhanced in oxygen, but not in carbon, and also had some extra helium [5]. I, Reuven Ramaty and Ben-Zion Kozlovsky looked at this discovery and came up with what ultimately was established as the origin of the ACRs [6]. The composition is the same as interstellar neutral gas, which flows into the heliosphere. It is then ionized by charge exchange and photoionization, picked up by the solar wind, and convected into the outer heliosphere. All you had to do was imagine that there is an acceleration process somewhere in the outer heliosphere that raises the particle energies from 1 keV per nucleon, the energy at which they are first picked up, to 10 MeV per nucleon, the energy at which they are observed. Moreover, the ACR particles would be singly charged, and therefore, particularly for oxygen, of high rigidity, which would allow them to propagate back into the inner heliosphere, maintaining the steep spectrum seen in figure 1.

![Figure 1. Differential intensity spectra of carbon (blue symbols) and oxygen (red symbols) measured by Hovestadt et al. [3] (filled circles) and McDonald et al. [4] (filled triangles). The oxygen spectrum has an extra component labeled “Anomalous Cosmic Rays” in the energy interval from about 1 to 30 MeV/nuc not seen in the carbon spectrum where only upper limits are shown.](image)

This theory had all the ingredients of being my Parker moment. Frank McDonald, who I worked for, hated this theory. He thought he had discovered a new component of galactic cosmic rays, from some exotic object. In fact, some major astrophysicists, e.g. Hoyle and Clayton [7], published a theory arguing that the ACRs could be from white dwarfs in binary star systems. And here was this young scientist, in Frank McDonald’s employ, depriving him of this major discovery.
Frank tried to suppress the publication and redirect my research into other more acceptable areas. However, this was my Parker moment, and I left Goddard in 1977 for the faculty of the University of New Hampshire (UNH).

3. **My second Parker moment – He-rich solar flares**

Almost immediately upon arriving at UNH, I had another opportunity for a Parker moment: He-rich solar flares. The story was the same. There was a new unanticipated observation. In the mid 1970s, observations by Caltech, Goddard and the University of Chicago [8,9,10] found that there are small impulsive solar flares where He is enhanced by up to a factor of 10,000 over cosmic abundances. To many astrophysicists, when you say He they think of nuclear processes, and major astrophysicists like Colgate, Audouze, and Fowler [11] proposed theories that involved extreme conditions of temperatures and densities in solar flares. I went another direction [12] and noted that He is a unique isotope, in that it is the only commonly available isotope whose cyclotron frequency lies between He+2 and H+. Thus, if you can excite waves with frequencies in between the cyclotron frequencies of He+2 and H+, you can preferentially heat the He+ and preferentially inject it into the flare acceleration process. I used electrostatic ion cyclotron waves, driven by current instabilities.

This theory was not really a Parker moment, because there was no real resistance to it. Other researchers developed more complete models, used other instabilities, but the general principle of using the unique cyclotron frequency of He was common to subsequent models. Thus, I considered this a victory.

4. **Other significant events in the 1970s**

In the 1970s there were also several events and activities that have been very important for my career. Perhaps of most importance was the beginning of what has been a career-long collaboration with George Gloeckler. George was at the University of Maryland and recognized that there was a region of the energetic particle spectrum, lying between solar wind energies and a few MeV per nucleon, that was basically unexplored. And therefore, it was virgin territory for a new experimental group that did not compete with the powerhouse cosmic ray groups at Goddard and at the University of Chicago. I was at Goddard, anxious to be involved, and largely being ignored in this ambition. So, George and I formed a collaboration that has been essential to my career. Through his projects he has supported my research. We have coauthored more than 100 papers together, and are still to this day enjoying doing research together.

In the 1970s I also decided that I would like to be influential and help initiate and sell a new major space mission: the joint NASA-ESA dual spacecraft International Solar Polar mission. With a gravitational assist from Jupiter, the NASA spacecraft was to orbit in one direction over the poles of the Sun. The ESA spacecraft was to orbit in the opposite direction over the solar poles. Great mission. I put a lot of effort into selling this mission in the U.S., and we received a new start in 1978.

However, in 1980 Ronald Reagan was elected President of the United States, and his Director of the Office of Management and Budget, David Stockman, slashed the NASA budget and cancelled the U.S. spacecraft. We fought hard to keep the U.S. spacecraft, but to no avail. The European spacecraft, of course, flew and became the Ulysses mission, which had a number of U.S. instruments aboard, including George Gloeckler’s SWICS instrument. Ulysses was an excellent, revolutionary mission, but not as good as it could have been had we had the two spacecraft.

5. **The administration years**

The end of the dual spacecraft Solar Polar mission was discouraging. So, I looked around for something else to do, and decided to try my hand at administration, which to my great surprise I turned out to be good at. In 1983 I became the Director of Research at UNH, a dean’s level position. By 1985 I was the Vice President for Research and Financial Affairs.

Then in 1986 the Challenger accident happened and NASA cleaned out its entire upper management. So, NASA, in fact Frank McDonald, then NASA’s chief scientist, came looking for an Associate
Administrator for Space Science and Applications who was a scientist recognized by the space science community, with senior administration experience. That combination was hard to find, and I was offered the position and moved my family to Washington, DC, in April of 1987.

The years at NASA in the late 1980s and early 1990s were good years. The NASA budget doubled, as did the budget for space science. We had major new starts, increases in the Explorer program and in Research & Analysis funding, etc. However, around 1992 things went sour. The growth in the NASA budget stopped. Dan Goldin became the NASA administrator and drove out the management talent at NASA Headquarters. Within about three years, only two of the senior leaders that were there when Dan arrived were left, and I was among the casualties.

I thought about what to do next. I decided that all I really wanted to administrate was NASA, and with that option closed, I decided to return to my research roots. The University of Michigan gave me a nice offer. In July of 1993 I became a Professor of Space Science at Michigan and started my brain again for research.

6. The community of researchers I returned to
I noted some changes in our research community that had occurred in the approximately 10 years while I was otherwise engaged. The community of heliospheric and solar researchers was very much larger. They had been diligently working over the ten years, and considered many issues settled, particularly in the heliosphere. There were details to work out but the big picture was known.

This was also the beginning of the development of major numerical modeling groups, well-funded with professional programmers. I had neither the interest nor the capability to start a numerical modeling group, so I chose to pursue my research as I had done before I became an administrator: Look for a new observation, which has the potential for resulting in a paradigm shift in our understanding. Run with it. Look for another Parker moment.

7. My third opportunity for a Parker moment
The opportunity for another Parker moment came in 1995. Ulysses, over the south pole of the Sun, observed 26-day increases in the intensity of a few hundred keV electrons and 0.5 MeV protons, which gave every evidence of having originated from corotating interaction regions within 30 degrees of the solar equatorial plane [13, 14]. How can this be? Low-energy electrons in particular follow field lines, and the Parker spiral heliospheric magnetic field, lying on cones of constant latitude, provides no means for electrons to be transported from low to high heliographic latitudes. Time for a new thought.

That new thought is illustrated in figure 2 [15]. The year 1995 was solar minimum, and the heliospheric magnetic field throughout much of the heliosphere originates from the polar coronal hole, which is offset from the solar rotation axis. Coronal holes are observed to rigidly rotate at the equatorial rotation rate of the Sun. The Sun differentially rotates, slower at the poles than at the equator. There is a nonradial expansion of the magnetic field from the polar coronal hole. When you put all this together [15], you find that the location from where the heliospheric magnetic field is convected radially outward with the solar wind moves dramatically in latitude and the field in the heliosphere that results provides a direct magnetic connection from low to high heliographic latitudes. This introduction of a polar component to the heliospheric magnetic field was reasonably well accepted in the heliospheric community. However, the real consequences of this theory were for solar physics. Ulysses also established that there is a single current sheet separating two hemispheres of opposite magnetic polarity in the heliosphere, and at solar minimum it lies near the equatorial plane [16]. The open magnetic flux of the Sun, the component of the solar magnetic field that opens into the heliosphere, is difficult to eliminate. You have to reconnect two oppositely directed open magnetic field lines at one location, at the current sheet, inside the Alfvén point. Since there is very little evidence that such reconnection occurs, as you transport all this open flux down to the current sheet (see figure 2), you need to transport
the open magnetic field around the Sun. As is discussed in Fisk and Schwadron [17] and Fisk [18], this transport can be accomplished by diffusion due to reconnection with coronal loops. Then, when you consider that there is global transport of open magnetic flux on the Sun, you can develop a comprehensive theory for how the solar magnetic field behaves and how the solar wind is accelerated.

**Figure 2.** A schematic of the motions (in red) of open magnetic field lines (in green) from the polar coronal hole resulting from a combination of: (1) the offset of the polar coronal hole, which is observed to rigidly rotate; (2) differential rotation of the Sun; and (3) the nonradial expansion of the magnet field from the polar coronal hole [15]. The insert illustrates the diffusive motions of open field lines that can result from reconnection with coronal loops.

The schematic flow chart in figure 3 is meant to illustrate the various interlocking components of a comprehensive theory, which was developed together with Nathan Schwadron and Thomas Zurbuchen, for how the solar magnetic field behaves and the solar wind is acceleration. The processes associated with each component are discussed in one or more of the many papers that were written about this model [e.g., 17-25]. The model is based on three basic concepts. There is emerging magnetic flux on the Sun.
in the form of loops. The Sun differentially rotates and the open magnetic flux is organized into two regions separated by a single current sheet.

![Diagram](image-url)

**Figure 3.** A flow chart of the interlocking processes that control the evolution of the solar magnetic field and the acceleration of the solar wind in the model of Fisk and colleagues [17–25].

The emerging loops are responsible for diffusion of the open flux and, together with differential rotation and the constraint that the open flux is organized, distributes the open flux around the Sun. It accumulates at the solar poles at solar minimum and this yields the polar field that drives the solar dynamo. In fact, you can derive that open flux will accumulate and form coronal holes in regions where the emergence of new magnetic flux is a local minimum, a prediction that was confirmed through solar observations by Abromenko et al. [24]. The diffusion by reconnection of open field lines with loops releases mass and results in a Poynting flux, which can be shown to be consistent with the resulting mass flux and flow energy in the solar wind. And so on.

So, was this a Parker moment? To be a Parker moment, you have to ultimately be proven to be correct. Unfortunately, when this model was first introduced, many solar physicists chose to ignore this work on the grounds that they didn’t need new concepts for the solar magnetic field.

Spiro Antiochos’s group has been working on aspects of how open flux becomes embedded in closed magnetic fields and the consequences for the solar wind, and their approach has been evolving to be similar to mine [e.g., 26, 27]. A very recent paper by Gary Zank both acknowledges my earlier work and develops a comprehensive model for the interaction of small loops and open field lines, and the resulting heating [28]. Thus, perhaps in time the concepts contained in my work, and possibly even the
broader concepts for how the solar magnetic field behaves, will find their way into mainstream solar physics.

8. **My fourth opportunity for a Parker moment – the common spectral shape of accelerated particles**

As my work on the solar magnetic field was coming to an end, there was a new observation and another opportunity for a Parker moment. Voyager 1 finally crossed the termination shock. It did not observe that the ACRs were accelerated at the termination shock, but as it traveled downstream from the termination shock, it observed something remarkable. The spectrum of low-energy particles that were somehow being accelerated locked onto a power law spectrum with a specific spectral index, a differential intensity spectrum in energy, with spectral index of -1.5 [29, 30]. If we express the spectrum as a distribution function in velocity, the spectral index is -5. Remarkably, when Voyager 2 crossed the termination shock four years later, the exact same spectrum was observed. And much later, when Voyager 1, now deep in the heliosheath, observed the full accelerated ACR spectrum, it also had a -5 spectrum when expressed as a distribution function (also referred to as phase space density), as seen in figure 4 [31].

![Figure 4. Phase space density of ACR oxygen as a function of oxygen speed/solar wind in the prime acceleration region at ~120 AU. The solar wind speed is taken to be 100 km/s. The data is from the LECP instrument on Voyager 1. The equation shown in the figure is the formula for the common spectrum and provides an excellent fit to the observations (from [31]).](image-url)

The initial Voyager observations caused George Gloeckler to go back and look at the spectrum of much lower energy suprathermal tails from Ulysses and from ACE, and he found with a careful transformation into the frame of the solar wind that there were -5 spectra everywhere: Downstream from shocks, in regions where there were no shocks around. Everywhere there is a region of acceleration, where particles are being accelerated locally, the spectra of the suprathermal tails had a spectral index of -5, as illustrated in figure 5 and with other observations summarized in [32].
Figure 5. One-hour averaged solar wind frame velocity distribution functions showing the proton bulk solar wind, the halo and the tail segments during hour 11 of 12 August 2001, during which the strong (compression ratio of 3.85 ±0.15) shock passed ACE (left panel) and during the hour of peak tail density that was observed one hour downstream of the shock (right panel) (from [32]).

There was no acceleration theory known at that time that automatically yields a single common spectral shape for the tails on the distribution function, a power law spectrum with spectral index of -5. Diffusive shock acceleration yields power law spectra, but with variable spectral index depending on the compression ratio at the shock. Besides, the -5 spectra can be observed with no shock nearby. Stochastic acceleration is more likely to yield exponential spectra. This is exciting. We need a new acceleration mechanism.

So, you think. You note first that the acceleration is most likely to be strong in subsonic regions, in the heliosheath and downstream from shocks. Subsonic regions contain moderate scale compression regions and so the new acceleration mechanisms should involve compressions and expansions, and it should include an irreversible process. You note second that we have an equation that describes particle acceleration by compressions and expansions, with irreversible spatial diffusion, the Parker transport equation, written in terms of the distribution function \( f \) as a function of particle speed \( v \) [32]:

\[
\frac{\partial f}{\partial t} + \mathbf{\delta u} \cdot \nabla f = \frac{\nabla \cdot \mathbf{\delta u}}{3} v \frac{\partial f}{\partial v} + \nabla \cdot \nabla (\bar{\kappa} \cdot \nabla f),
\]

where \( \mathbf{\delta u} \) is the random convective velocity of the compressions and expansions, and \( \bar{\kappa} \) is the spatial diffusion tensor.

The Parker transport equation describes acceleration and deceleration in individual compression and expansion regions. We need to average this equation to determine the net acceleration due to multiple compressions and expansion regions. However, we need to be careful. Some of usual tricks of solving for the average distribution function of accelerated particles do not work here. We cannot treat this as a Markovian process, in which particles at different energies behave independently of each other. There is a cohesiveness in this problem that we have to capture. All particles in a compression or expansion region are either accelerated or decelerated. All compression regions must be surrounded by expansion regions.
To build in this cohesiveness, as is discussed in detail in [32, 34], we need to solve for the distribution function that defines the mean pressure, $f_o$, and then show that this distribution function can to a very good approximation be compared to observations. This is the equation that results. It describes the time evolution of the spectrum of the accelerated particles.

$$\frac{\partial f_o}{\partial t} = \frac{1}{v^3} \frac{\partial}{\partial v} \left( \frac{\delta u^2}{9 \kappa} v \frac{\partial}{\partial v} \left( v^5 f_o \right) \right),$$

(2)

where $\kappa$ is the cross-field diffusion coefficient.

The new acceleration mechanism is a pump acceleration process. It is a first order acceleration, and thus competitive with diffusive shock acceleration. The solutions to this equation fit the observations. For example, the excellent fit to the full ACR spectrum shown in figure 4 is from a solution to this equation.

So, was this a Parker moment? Well, not yet. There are many in our field who are firmly attached to diffusive shock acceleration as the preferred and dominant acceleration mechanism, and are reluctant to consider this new acceleration process, or, in the case of theorists, to extend and improve it. There are others now who are finding the -5 spectra when they consider acceleration with the same constraints as apply for the pump acceleration. There may be new observations that will be available from the upcoming IMAP mission that can verify the pump acceleration mechanism. And so, we will have to wait to see if this is a Parker moment.

9. **My fifth opportunity for a Parker moment – the interaction of the heliosphere with the local interstellar medium**

As my work on this new acceleration theory was winding down, there was yet another new observation and another opportunity for a Parker moment. As Voyager 1 penetrated further into the heliosheath, amazing new conditions and regions were discovered, which are summarized with references in [35]. First, the dominant pressure in the heliosheath is in mobile ACRs and pickup ions. Second, the solar wind flow, which on Voyager 1 is measured by observing the convective anisotropies of low-energy energetic particles, slows systematically down in the radial direction, and also in the polar and azimuthal directions, and creates what the Voyager investigators refer to as a stagnation region. Third, at about 122 AU, the particles accelerated in the heliosheath abruptly disappear and the galactic cosmic ray intensity increases. Fourth, Gurnett et al. [36] observe plasma waves, which provide a direct measure of the electron density, in the vicinity of ~122 AU and beyond. The density is ~0.1 per cc, much larger than the density that Voyager 2, with its working plasma detector, was observing closer to the termination shock, and comparable to the density expected in the interstellar medium.

As the Voyager observations became public, George Gloeckler and I undertook to develop a model that could explain the observations. Our first model involved the solar wind flowing in supersonic jets out the sides of the heliosheath [31]. However, as this paper was being completed, the density observations of Gurnett et al. became available and were inconsistent with our first model. Hence, we developed an entirely new model, which is illustrated in figure 6 [35]. The model is intended to be consistent with every Voyager observation known at that time.

The streamlines of the solar wind cross the termination shock, and then turn initially azimuthal, so the solar wind can flow out of the nose region of the heliosheath. The azimuthal flow creates a centerline region, which causes the flow speed to decrease. The azimuthal speed goes to zero first, and then the streamlines flow radially outward at a very low speed, across 122 AU, which we refer to as the heliocliff, out to the actual heliopause, which lies out at 140 AU or more. The solar wind flows across the heliopause, which is a rotational discontinuity.
Figure 6. A simplified schematic of the basic features of the Fisk and Gloeckler model for the nose region of the heliosheath [35].

The new model is consistent with all Voyager observations. It naturally yields the stagnation region, where the solar wind flow becomes small in all directions. The solar wind is compressed and heated in this region, consistent with IBEX ENA observations [37]. Beyond 122 AU, where the solar wind and energetic particles can escape along the azimuthal magnetic field across the actual heliopause, the solar wind is cold and dense.

As we were completing this model, the Voyager investigators decided that Gurnett’s density measurements [36], densities comparable to what is expected in the interstellar medium, is in fact conclusive evidence that the heliocliff is the heliopause, and Voyager 1 had crossed into the interstellar medium.

But wait. The Voyager investigators are the ones that said there is a stagnation region just inside the heliocliff where the solar wind flow velocity becomes small in all directions. And the mass conservation equation clearly states if a flow is compressed in all directions the density increases. In fact, subsequent IBEX observations of the ENA spectrum along the line of sight of the Voyager 1 trajectory can be understood only if the solar wind inside the heliocliff is compressed and heated [37].

And if you take away the argument that the only explanation for the Gurnett et al. density observations is that they are observing interstellar gas, the argument that Voyager 1 has passed into the interstellar medium dissolves. However, the press conference was held, and the public was thrilled to learn that Voyager 1 is now in the interstellar medium.
So, will this turn out to be a Parker moment, when I am ultimately proven to be correct? There may be some measurements possible from Voyager 2 that will support my position, but I expect that I will have to wait until an interstellar probe flies through the heliosheath and shows that indeed the heliopause is not at 122 AU, and there is a fascinating new region of compressed solar wind lying between 122 AU and the actual heliopause. I hope someone lets my great-grandchildren know.

10. Concluding remarks
Working on these last three theories has brought me enjoyment and satisfaction. It has been stimulating and challenging to seek and to recognize new observations that are significant, in that they cannot be understood with existing theories. And then to develop a new theory that can account for the observations. And then to pursue the broader ramifications for our understanding of the Sun and the heliosphere that result from this new theory. There has been a certain thrill in risking one’s reputation by defending the new theory against other theoreticians and experimentalists. There has been satisfaction in recognizing that it is essential for the future of our field that we continuously develop and consider new concepts, and paradigm shifts.

Indeed, we cannot allow our field to evolve to where new concepts, where paradigm shifts, are neither looked for nor welcome. Where we have become complacent that everything we observe now or in the future can be understood with concepts and physical processes that were developed years ago. Should this be the fate of our field, we will cease to be a scientific discipline worth supporting.

As scientists, we should always be in search of the real truth. Not simply the validation of our preconceptions. As theorists we should always recognize that our theories will be judged as to whether they can account for observations. And all of us should embrace change as essential to a dynamic scientific discipline. Cherished theories, models and explanations that are found to be wanting should be quickly improved or replaced.

And we should recognize that change should come from the young. It has been disappointing to me that there were very few if any young people who have worked on explaining the observations on which these last three theories were based. I am in my mid 70s. Much of this work was done when I was in my 60s or late 50s. Where were the young people who viewed these observations as their opportunity for a Parker moment?

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