

Magnetosphere: From Plasma Observations to Reconnection Theory

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Abstract I outline the development of my scientific work from graduate school to about the mid-1980s (plus some mention of related later work), in particular the transition from instrument testing to data analysis to abstruse theory, and the origin of some of my key papers.

1. Introduction

This is a partial story of my scientific career, with emphasis on how I arrived at the main ideas I contributed to the physics of magnetospheres. It is neither a review paper nor an autobiography: only as much physics is mentioned as needed to understand what the topic is (for details, the reader may consult the references), and only those events of my life bearing directly on my scientific activity are recounted.

I first heard of the magnetosphere in the fall of 1962 when, after graduating from Harvard College, I entered graduate school at Massachusetts Institute of Technology, Department of Physics. Financially supported by a research assistantship in the Laboratory for Nuclear Science, I was given the opportunity to talk with several research groups (as I remember, 5 or 6, most doing laboratory experiments in nuclear or elementary-particle physics) and to express my preference before being assigned to a particular one. I was immediately attracted by the cosmic ray group, which differed from the others in being concerned more with natural rather than with laboratory phenomena; this was in tune with my own interests since college days (e.g., in my sophomore year at Harvard I took the full-year Astronomy 1 course, although from the very beginning I had chosen physics as my major).

2. Faraday Cup Problems and Magnetospheric Electrons

The cosmic ray group at M.I.T., headed by Bruno Rossi (one of the founding fathers of the whole field of observational cosmic ray research), was at that post-Sputnik time in the process of expanding into space-based research in two directions: measurements of plasmas in space and X-ray and gamma ray astronomy (see Rossi, 1990, for an account of this transformation as well as of his entire fascinating career). When I arrived, it was a year and a half since the Explorer 10 satellite, carrying the first plasma detector built by the group, had been launched and half a year before the final paper on results from the brief (∼50 hr) mission (Bonetti et al., 1963) was submitted for publication. The detector was the (then newly developed) modulated Faraday cup, designed to observe low-energy (<few keV) ions, not only measuring their intensity but also, by virtue of the modulation feature, determining their energy spectrum (which the earlier pioneering space plasma probes of the Soviet group headed by Konstantin Gringauz could not do). The basic functioning of the modulated Faraday cup was well understood, but there were unresolved subtle issues about instrument response and possible sensitivity to unwanted particles. My first assignment as graduate research assistant, under the supervision of Alberto Bonetti, was to investigate the spare unit of the flight instrument, doing laboratory tests, modifications, and additions when needed. Among the results of my work was that the instrument in the electron mode responded to very intense limitation to energy resolution.

The modulated Faraday cup became the trademark experiment of the MIT space plasma group, flown after Explorer 10 on numerous other missions (IMP, OGO, Mariner 10, Voyager 1 and 2). My initial involvement was entirely on the experimental side: running laboratory tests, soldering circuits, occasionally even...
machining on the lathe; only later, beginning with my thesis work, did I gradually switch to data processing and analysis. Nowadays I may be looked on as a five-thumbs theorist (“Do you know which end of the soldering iron is hot?” I was once asked), but that early experience with space instruments has given me an appreciation of their complexity and uncertainty that emboldens me even now to ask penetrating questions on talks by experimenters.

All this was more or less a side occupation. The main task of a graduate student in the physics department during the first 2 years was to take courses, both in general physics (electricity and magnetism, quantum mechanics, nuclear and particle physics, etc.) and in one's intended specialty. There followed the PhD qualifying exam (written and oral, on all fields of physics), plus a test (written and oral) of ability to read scientific literature in two foreign languages, chosen by the candidate out of three—French, German, Russian (I chose the first two, the only ones I had any knowledge of); only after these exams were passed (usually in the third year) was one permitted to begin thesis work.

We were six graduate students in a small crowded office (two windows, six desks, and a blackboard), including Alex Klimas (like me, first year in 1962) and George Siscoe (third year) who later became well-known in magnetospheric physics. An exciting and instructive event for us was a series of four lectures in the spring of 1963 (I think) in Boston College, by the luminaries of the field: Sydney Chapman, Eugene Parker, Ian Axford, and Alex Dessler.

1964 was for me an eventful year. In the spring semester I passed my qualifying exam, and the next step was to select a topic for the PhD thesis. An M.I.T. Faraday cup experiment was scheduled to be launched on the OGO 1 (Orbiting Geophysical Observatory) satellite in September, and it was decided that for my thesis I would analyze data from that experiment, under the supervision of Frank Scherb. In the summer Frank Scherb sent me (just for general experience!) to attend the cosmic ray conference in Denver, and late in August I traveled with Herbert Bridge (principal investigator of the experiment) to Cape Canaveral for the OGO 1 launch (the only spacecraft launch I have witnessed in person); these were my first scientific trips. That year I also joined the AGU as student member.

For the rest of the year and well into 1965, I worked mainly on data processing. The OGO 1 launch itself had been successful, but the planned three-axis stabilization had failed and the satellite was left spinning, with the consequence that the positive ion data were practically unusable (because direction could not be reliably determined), and the analysis would have to be restricted to electron data—far less data than originally anticipated but still a heavy load for the primitive (by today's standards) computers of the day. A further perturbation for me occurred in the summer of 1965, when William Kraushaar, one of the senior members of the X-ray and gamma ray branch of Rossi's group, left M.I.T. for University of Wisconsin in Madison, and Frank Scherb (who as graduate student had worked with Kraushaar) went with him. Stanislaw (Stan) Olbert, the theorist of the space plasma group, took over as my thesis supervisor.

Properties of electrons in the energy range of the instrument (125 eV to ~2 keV) was then a poorly explored topic of magnetospheric physics: almost the only available observations were from the early Soviet probes by the Gringauz group (total intensities but no energy spectra) and from the Vela satellites by the Los Alamos group (circular orbit at radial distance ~17 R_E, results not published until early 1966). The eccentric orbit of OGO 1 (apogee 24 R_E) and availability of energy information from the electron experiment thus meant that unknown terrain was being explored; once data processing and display had been made to run smoothly, just about anything found by the analysis would be new and likely significant. And indeed by early 1966 many of the results were clear: spatial distribution of low energy electrons in radial distance and local time, the inner edge of the plasma sheet and the rapid decrease of electron temperature across the inner edge inwards, earthward displacement of the inner edge during what were then called negative magnetic bays (now called magnetospheric substorms), and more.

At the AGU spring meeting in April 1966 I gave a talk on preliminary results—my first AGU meeting and first conference presentation. My thesis was completed in the summer and thesis defense was on 20 August 1966. Alex Klimas defended his thesis 2 days later, on 22 August, and George Siscoe had defended his thesis on 24 August 1964; both were also supervised by Stan Olbert but their theses were on theoretical topics.

A long paper (at that time the second longest single-author paper in JGR), which reported all the principal results of the thesis (plus some later results from a similar experiment on OGO 3, launched in July 1966),
was published almost 2 years later (Vasyliūnas, 1968)—being slow to publish has always been my besetting sin. When I was writing it, I took it for granted that the paper would be by three authors: myself, Stan Olbert as thesis supervisor, and Herb Bridge as principal investigator of the experiment. To my surprise, they both said: “It’s your work, you publish it,” and Stan added, “even though I don’t publish much, I’m sufficiently well known that I don’t need another paper.” (Can one imagine something like this happening today?)

After receiving the PhD, I stayed with the M.I.T. space plasma group (by then affiliated with the newly founded M.I.T. Center for Space Research), first as staff member of the Division of Sponsored Research, then, beginning with fall semester 1967, as assistant professor and from 1970 associate professor of physics.

3. Drifting Into Theory

My research was to be a continuation of work with Faraday cups being flown on or proposed for several other missions: data analysis and interpretation, planning of new experiments (but no more hands-on hardware work or assembler-language programing). I was even given the task of writing the chapter for Methods of Experimental Physics which the plasma group had been asked to contribute (Vasyliūnas, 1971). There had always been, however, within the group a strong undercurrent of theoretical discussions, due largely to the influence of Stan Olbert (whose grasp of basic principles was exceptionally broad and sound—see, for example, the textbook Rossi & Olbert, 1970) and also fostered by a vigorous seminar program jointly with the astrophysics group. I remember in particular many discussions on transport of charged particles (the thesis topic both of George Siscoe and of Alex Klimas) with George, Alex, Stan, and later with Miriam Forman with whom I shared an office for a while. Curiously, to my recollection there was not much interaction with scientists in the laboratory plasma group at M.I.T. except with Bruno Coppi.

These were for me discussions “on the side.” What turned me toward explicit theoretical work were two specific events. One was a conversation with Arrigo Finzi, a distinguished astrophysicist who was visiting Bruno Rossi’s group (sometime in 1967, I think). I do not remember the exact words, neither his nor mine, but the following is the substance of what was said. Finzi listened attentively as I described the principal results of my thesis and then asked: “How do you interpret all this? What is the physical explanation?” I said something like my main task was to obtain a coherent and systematic account of the observed phenomena, to which he replied: “That is not sufficient. You must try to understand the physics of what is observed.”

The other event had occurred some years earlier. Among the courses I took in graduate school was one on nuclear physics, taught by Lee Grodzins. One of the assignments was to write a term paper on a specified topic. Grodzins laid down some rules, in particular for use of the phrases “it is obvious” and “it can be shown.” Something that “is obvious” may require thought—may require a lot of thought—but no calculations; for something that “can be shown” one must give a reference, and the reference must be to a place where it is shown, not one where it just says “it can be shown.”

The topic of my term paper was the liquid drop model of the nucleus. Reading the literature, I quickly discovered that an essential link in describing the model was a mathematical relation (on transforming fixed and rotating coordinates) about which everybody said “it can be shown” and referenced a paper by Bohr and Mottelson (1953) in a well-known Danish journal. I went to the library (this was many decades before online availability) and discovered that the journal issue in question had been signed out. I finally read the paper, I found it said the relation “can be shown,” with no derivation and no reference. What to do? By now I was getting mad and said: I am going to derive this relation myself! Starting with Appendix A-2 (“Spherical tensors”) of Physics of the Nucleus by M.A. Preston (the textbook of the course), I worked for several days through a morass of formulas (at one point, I had the blackboard in our little office filled with mathematical symbols, and a fellow graduate student looked at it and said—I do remember the exact words—“You know, if you ask me, it’s sh...”) until I finally arrived at the relation. This episode convinced me that complicated theoretical derivations can be done, given sufficient care and patience, and should not be feared nor held in awe.
4. Calculating Magnetospheric Convection

With recently received impulse and previously gained confidence, I began to think seriously about theoretical explanations, in particular for the observed inner edge of the plasma sheet, where the electron mean energy drops precipitously with decreasing radial distance while the number density changes little if at all. Pitch angle scattering by wave–particle interactions was much discussed at the time, following the paper by Kennel and Petschek (1966). In the strong diffusion limit (scattering rate sufficiently large to keep the loss cone almost full), energetic electrons are lost from the plasma sheet by precipitation, at a rate proportional to the size of the loss cone, but must be replaced by low-energy electrons from the ionosphere if charge quasi-neutrality is to be maintained; this process can reduce the electron mean energy without changing the density (Kennel, 1969). I combined this concept with particle transport equations I had adapted from the theses of Siscoe and Klimas to calculate a simple model of predicted electron temperature as function of equatorial radial distance and local time. The resulting figure reproduced roughly the qualitative features of the observed inner edge, and Kennel (1969) included it as Figure 7 in his paper, referencing my paper “to be published.”

That paper was never written. The transport/precipitation model depended on several unsupported assumptions in which I had lost confidence. Most questionable was the assumed plasma flow in the magnetosphere, obtained by the (then standard) method of superposing uniform sunward flow plus corotation. Rather than developing the model any further, I began to think how to calculate the plasma flow, taking the basic equations as the starting point. Among the important inputs were the plasma moment equations and their relation to single particle drifts, which I had learned already as graduate student from the lectures by Rossi and by Olbert, as well as the papers Axford and Hines (1961) (the basic qualitative description that I was trying to make quantitative), Swift (1967, 1968) (partial ring current models), Fejer (1964) (role of ionospheric currents), and others. From all this I synthesized a general scheme for calculating magnetosphere-ionosphere coupling and presented it, plus some simple models obtained with it, at the Summer Advanced Study Institute (in the well-known series organized by Billy McCormac) in Santa Barbara, California, in August 1969, publishing it in the proceedings (Vasyliunas, 1970) as a relatively short paper; the calculational scheme is reproduced here (Figure 1). (My intention was to submit a more extensive account to a journal and I included a reference “to be published,” but again that paper was never written.) Wolf (1970) independently derived a completely equivalent scheme and applied it to develop the well-known Rice Convection Model.

The scheme had in fact been applied (implicitly) already by Fejer (1964) but only to obtain a specific model of geomagnetic disturbances, not as a general method to calculate magnetospheric convection.

Some aspects of my paper were later summarized by J. C. Armstrong (1974): “... Vasyliunas (1970) appears to have first interpreted the magnetotail measurements of Behannon (1969) in terms of field aligned currents on the outer boundary of the plasma sheet, determined that the currents flow toward the Earth on the dawn side of the tail and away from the Earth on the dusk side, and showed that this current configuration was consistent with his mathematical model of magnetospheric convection (in three sentences).” Two points should be noted. The closing remark “in three sentences” highlights the fact that the paper is quite condensed, almost an overgrown extended abstract with equations and illustrations—largely the result of constraints imposed by the proceedings format; publishing the results in a full journal paper remained an unrealized intention because of reduced pressure once the results were out in some form, as well as of my being a slow writer. The same thing happened with the later paper which extended the scheme to include ring current shielding effects (Vasyliunas, 1972), also published only in the proceedings of a Billy McCormac conference. The last time I met Neil Brice in 1973, only a few months before his tragic death, he had just become aware of these two papers and really gave me hell for not publishing them in journals.

The other point highlighted by Armstrong’s summary is how little notice I took of electric currents, which play a central role in the calculational scheme I described. In the 1970 paper I had in effect identified what was later called the region 1 current system but did not draw attention to it. In the 1972 paper the calculations contained in addition what was later called region 2 current system, but I did not bother mentioning currents at all. The terms “region 1” and “region 2” were introduced by Iijima and Potemra in 1976, and their famous figure showing the spatial distribution of region 1 and 2 currents was published in 1978 (Iijima & Potemra, 1976, 1978).

More recently, although I still admit the scheme of Figure 1 and its extensions as the basic mathematical formulation of how to calculate magnetospheric convection, I now view its physical meaning somewhat
differently. This development of my thinking began only in the mid-1990s and (strictly speaking) is outside the nominal time frame of this paper, but for completeness I summarize it in section 7.

5. Can Magnetic Field Lines Be Cut?

The year I began my thesis work was also the year the epoch-making paper by Harry Petschek on magnetic field reconnection (or annihilation, as he called it) was published (Petschek, 1964). Magnetic field line reconnection was a familiar topic of discussion within the group around Stan Olbert and his students and coworkers. It was an interesting subject of plasma physics, and the fact that Petschek worked at Avco-Everett Research Laboratory in nearby Everett facilitated personal contact; I remember Petschek’s seminar to our group on his model. At the time, the primary application was to energy release in solar flares, but there was also a direct impact on magnetospheric physics, where the outstanding controversy then was whether the magnetosphere was magnetically open or closed, or equivalently whether (and to what extent) reconnection between geomagnetic and interplanetary magnetic fields occurred.

A further puzzle arose when Yeh and Axford (1970) and Sonnerup (1970) published their “similarity” models of reconnection, describing ostensibly the same configuration as that treated by Petschek but obtaining very different results. Which model—if any—was correct? The flat assertion by Yeh and Axford that “Petschek does not have a valid solution of the equations of magnetohydrodynamics” was not convincing, given that all the models involved major approximations and these were very different from model to model. Still another model, “vacuum merging,” had been published by Dessler (1968); it took a completely different approach, related to some ideas of Alfvén (1968a), although Alfvén himself always regarded anything that mentioned “reconnection” or “magnetic merging” as nonsense.

Throughout my career I have maintained close contact with Alex Dessler and his group at Rice University. To date I have given 36 seminars/colloquia at Rice (not counting presentations at conferences held there),
the first a colloquium in October 1967, barely a year after my PhD—my first unsolicited outside invitation. Inevitably Alex and I had many discussions about magnetic reconnection (or merging, as he preferred to call it). Unlike Alfvén, Alex had an open mind on this as on many other questions. The ultimate test of any theory is its ability to predict observational results, and any theory with a reasonably sound physical basis should be considered unless and until it is found to disagree with observations. The multiplicity of theoretical models proposed for magnetic reconnection, absent a compelling observational test, presented a challenge to understand how and why the models differ.

It was some such consideration that led Alex to suggest (I do not remember exactly when, but probably in June 1971, after a seminar I gave at Rice on the topic) that I write a short paper summarizing the various models, their similarities, and differences. At first this was to be a contribution to Comments on Astrophysics and Space Physics, of which I was a correspondent at the time, but I soon found the journal’s limit of roughly half a dozen booklet-size pages too constraining. Alex then proposed that I submit the paper as a topical review to Reviews of Geophysics and Space Physics, of which at the time he was editor for space physics. (Originally Reviews of Geophysics, the journal had been renamed starting with Volume 8 in 1970, and reverted to its original name starting with Volume 23 in 1985.) Before long, my concept of what the paper was to be had progressed from a mere summary of published work to a systematic critical account, and the format of a topical review had also become too constraining, leaving a full-fledged review paper as the only alternative.

What had become apparent as I delved more deeply into the topic was that simply consulting and citing the original publications could provide at most a superficial view; for a real physical understanding of interrelationships between the various models, it was necessary to work anew through the derivation of each model, with critical examination (and, where necessary, modification) of every assumption and approximation. This approach proved very effective in uncovering hitherto unsuspected aspects and links, as well as providing new insights. There was, however, a side effect: as each new insight was pursued, the scope and length of the paper kept growing, and consequently the anticipated completion date kept receding farther and farther into the future.

On and off, I worked on the paper through 1972, 1973, and into 1974, with no great reduction of the gap between the present and the expected completion date. What finally broke the impasse was the suggestion by Tom Hill that I split the paper into two parts, submit the first and keep working on the second. The obvious split was between hydromagnetic models (on which I had nearly finished much of the work) and single-particle models (on which I had many concrete ideas but still much to do). “Theoretical Models of Magnetic Field Line Merging, I” was submitted in August 1974 and appeared as lead paper in the February 1975 issue of Reviews of Geophysics and Space Physics (Vasyliunas, 1975). Part 2 was never completed, for various reasons; that I left M.I.T at the end of July 1975, to join Ian Axford as scientific member of and somewhat later also as director at the Max Planck Institute for Aeronomy (as it was then called), was not helpful in this regard.

Truncated or not, this review arguably has been my most widely read and influential paper: according to data in the AGU-Wiley publications library, it has been cited a total of 1,104 times up to 26 March 2020 (on the average, 24 citations per year) and is still being cited at an appreciable rate (about 20 citations in 2019).

6. Plasmoids at Earth and Other Planets

While working on a critical review of the magnetic reconnection process itself, I also kept thinking about specific aspects of reconnection in the magnetosphere. There was much discussion in the 1970s of reconnection in the nightside of the magnetosphere, associated with occurrence of magnetospheric substorms, which was widely interpreted as the result of a new magnetic X-line formed relatively near the Earth, distinct from the distant X-line associated with flux return in the open magnetosphere (a subtle distinction, often ignored even today). Tearing-mode instability of some type at the magnetotail current sheet (e.g., Schindler, 1974) was the most commonly invoked mechanism to form the X-line.

I have always been bothered by the two-dimensional nature of nearly all discussions on this topic. (Once I complained about this to Karl Schindler, who replied: “it took one year to understand the tearing mode in one dimension, it took ten years to understand it in two dimensions ....”) The insight how to visualize
what happens in three dimensions came to me, however, while I was thinking about a different problem: not the near-Earth but the distant X-line, at which the open magnetic field lines threading the lobes of the magnetotail reconnect to form closed lines of the plasma sheet. The specific picture I had in mind was figure 1 of Pilipp and Morfill (1976, 1978), who took for granted that the distant X-line was more or less stationary, but to me it seemed that, since the open field lines also thread the plasma mantle and the magnetosheath, plasma flow in those regions might carry the distant X-line antisunward. The magnetotail then increases steadily in length, and the question is: can the magnetotail become indefinitely long, and if not, what sets the limit and how?

The answer became apparent in a flash, after a lot of thought (therefore “it is obvious” in the sense discussed by Grodzins): tearing-model instability sets in when the magnetotail has become stretched too much, but the resulting magnetic X-line configuration must initially be confined to a localized region (because all physical effects propagate at a finite speed) and only gradually spreads into the third dimension. With this concept, I sketched a highly simplified model of the three-dimensional magnetic topology during magnetospheric substorms and presented it at the last of Billy McCormac’s magnetospheric conferences, in Graz, Austria in August 1975, publishing it as part of the usual short paper in the proceedings (Vasyliunas, 1976). The figure reproduced here (Figure 2) illustrates the postulated temporal evolution in a sequence of five panels, each
one showing magnetic field lines in the noon-midnight meridian plane (upper left) and singular (X and O) lines in the equatorial plane (lower left) and projected on the polar ionosphere (right). The pattern in the meridian plane is largely the same as in the two-dimensional tearing mode; only when one also considers the configuration out of the plane is the plasmoid revealed as an intrinsically three-dimensional structure, consisting of magnetic field lines that connect neither to the Earth nor to the interplanetary medium.

From the very beginning, the figure was intended (and described) as only a simplified sketch to illustrate magnetic topology. Ed Hones, who together with coworkers had contributed much of the observational data analysis that had established the existence of the near-Earth X-line (I myself had worked with him during several prolonged visits to Los Alamos), treated it, however, as a serious description of detailed physical phenomena. He redrew the figure, including only the meridian plane projections (thus suppressing completely the three-dimensional aspects which were my reason for the investigation in the first place), expanded the temporal sequence from five to nine panels, and added “realistic” details in each panel to match observations, then drafted a paper and asked me to join him as coauthor. I declined, for two reasons: (1) I did not see sufficient justification for the added details; (2) the paper was to be a contribution to the “fireball” controversy between Hones and Lou Frank and, as one who was working with Lou Frank as well, I had no wish to get involved (particularly after my attempts at “shuttle diplomacy” between Los Alamos and Iowa City had failed to reconcile the two sides). Ultimately the paper was published as a “fireball” comment (Hones, 1977), and versions of the redrawn figure appeared also in numerous other publications by Hones, attracting wide attention as the iconic image of the near-Earth X-line substorm model.

The 1970s were the years when in situ exploration of the magnetosphere of Jupiter began. The two Voyager spacecraft, carrying, among other experiments, M.I.T. Faraday cup plasma detectors (Bridge et al., 1977), were to be launched in summer 1977 and to encounter Jupiter in March and July 1979. As co-investigator with the Voyager PLS team since the proposal stage (1972) and continuing after I left M.I.T., I wanted to understand, as much as possible before the encounters, what to expect at Jupiter. Many of the basic properties were already known from earlier theoretical estimates and from the Pioneer 10 and 11 missions. The dominant effect on plasma flow was from planetary rotation, not from the solar wind, and reconnection with interplanetary magnetic field was likely to be of minor importance. Formation of plasmoids as in Earth’s magnetosphere, as result of stretching the magnetotail, was thus not anticipated—at first sight!

The need for a second sight became apparent after I read the paper by Michel and Sturrock (1974). They pointed out that corotation requires an inward force to maintain the centripetal acceleration, which on the dayside of the magnetosphere can be provided by the confining external (solar-wind) pressure; on the nightside, however, there is no confining external pressure, and the corotating plasma is free to flow out (forming what they called the planetary wind), pulling magnetic field lines into an open field line configuration beyond some distance. Now if one thinks of magnetic field lines on a particular corotating meridian surface, the evolution as this meridian rotates from noon into the nightside corresponds precisely to the time sequence of field line patterns in the noon-midnight meridian plane which I had sketched for plasmoid formation. As counterpart of Figure 2 for Earth, I drew Figure 3 for Jupiter (panels 1 to 4 being the same in the two figures, no panel 5 at Jupiter because the interplanetary magnetic field was neglected) and presented the figure in a talk “Plasma observations within the magnetosphere of Jupiter: prospects for Voyager” at a meeting in Munich in April 1978. I did not publish it then, partly because the prospects of observing the phenomenon did not seem very practical at the time. Some years after the Voyager flybys past Jupiter, Alex Dessler edited a book of review papers (Dessler, 1983) which became the “bible” of Jovian magnetospheric physics. I contributed a chapter on “Plasma distribution and flow” (Vasyliunas, 1983), reviewing the observational and theoretical results available by this time, and included Figure 3 as the 19th (and last) figure of the chapter.

Figure 3 has been widely cited in discussions of magnetospheric dynamics, both at Jupiter and at Saturn. It has been named the “Vasyliunas cycle” and contrasted with the “Dungey cycle”—rotationally driven magnetotail reconnection versus solar-wind-driven magnetopause reconnection. Both figures are highly simplified schematic presentations of possible topological developments, with no pretense to detailed applicability, but they do attempt to visualize at least conceptually the configuration in three dimensions. The question what form a plasmoid, understood as a volume of field lines not connected to anything outside itself, can take in three dimensions (without special symmetry assumptions) has troubled me for a long time (Vasyliunas, 2011), and only in the last few years I have begun to see some possibilities (but this is work in progress, not yet ripe for publication).
7. Understanding Electrodynamics in Magnetospheres

There have always been two ways of describing the interaction of electric and magnetic fields with plasmas in magnetospheres and other outer-space regions: in terms primarily either of electric field and current or of magnetic stress and plasma flow. Already in my first paper on magnetosphere-ionosphere coupling (Vasyliūnas, 1970) I noted the distinction and chose the first way as mathematically more convenient but with the remark “fundamentally, of course, these two modes of treating the problem are equivalent, since in a plasma there is a close connection between the flow and the electric field and between the stress and the electric current.”

The two ways were labeled “J-E” and “B-V” approaches by Parker (1996), in a paper which ignited a full-blown controversy on the merits of the two (Heikkila, 1997; Lui, 2000; Parker, 1996, 2000, 2007). Parker’s essential point was that, although the two approaches can be mathematically equivalent in simple steady states, they differ fundamentally when time variations and questions of physical causality were considered. Needless to say, I was deeply interested in the controversy, which in essence was about the conundrum stated already in the earliest textbooks on the subject (Cowling, 1957; Dungey, 1958): in magnetohydrodynamics, unlike elementary E&M textbook discussions, the electric current is determined by the magnetic field (through the requirement that magnetic stress be balanced by mechanical stress), and the electric field is determined by plasma flow.

In quantitative terms, the controversy may be reduced to questions about two simple equations:

\[ cE = -V \times B \]  
\[ \nabla \times B = (4\pi/c)J \]

Both are approximations (to more complicated equations, containing time-derivative terms), accepted as valid under certain generally well-understood conditions, and the disputed question is: which side of the equation is the cause and which the effect? Does the electric field make plasma flow, or does the flow create the electric field? Does the magnetic field change because the current is changing, or does the current change to match the curl of the changing magnetic field? With an equation of form \( a = b \), it is difficult to make a distinction between \( a \) and \( b \), and it was thus no surprise that many arguments in the literature were
purely verbal (often just philosophical disputes about what one means by “cause”). What was needed, I felt, was some quantitative physical argument based on equations.

At this point I remembered something Stan Olbert had told me years before. All the fundamental equations of classical physics, except three, can be expressed in the form: time derivative of any quantity is given by a function of space and (present) time. The three exceptions are the divergence equations for electric, magnetic, and gravitational field, respectively; “classical” in this context means excluding quantum mechanics and related theories. (An obvious truth, once it has been pointed out, but I do not recall it ever being explicitly said in any of the physics courses I took at Harvard and M.I.T.) An immediate consequence, also pointed out by Stan, is that you can assume any initial conditions at one given time $t = 0$, subject only to the constraint that they satisfy the three divergence equations, and the equations will then tell you what happens at all other times.

Here was the physical way to answer the questions posed above: assume as initial condition at $t = 0$ arbitrary different values for the quantities on the two sides of Equation 1 or Equation 2 and trace their subsequent temporal variation by solving the corresponding exact equations. I carried out this procedure first for Equation 1 (Vasyliūnas, 2001): relation between electric field and plasma flow (“E-V theorem”)—relatively simple because Equation 1 contains no spatial derivatives. Next I addressed Equation 2 indirectly (Vasyliūnas, 2005a), examining equations for time derivatives of current and magnetic field, respectively. And finally I applied the initial-condition procedure to Equation 2 directly (Vasyliūnas, 2005b): relation between current and magnetic field—complicated mathematically, because of curl $B$ in Equation 2, and requiring some wizardry with Laplace transforms, but simple physically.

The results unambiguously support the B-V approach: (1) plasma flow creates the electric field (not vice versa), provided the Alfvén speed is much smaller than the speed of light (i.e., the mass density of the plasma is much larger than the relativistic energy-equivalent mass density of the magnetic field), a condition that holds in most regions of interest for space plasma physics; (2) electric current changes to equal the curl of the magnetic field (not vice versa), provided the spatial scales are much larger and the time scales much longer than those of electron plasma oscillations—spatial scale $>>$ electron inertial length (collisionless...
skin depth) and time scale >> reciprocal of plasma frequency; again, conditions that hold for most phenomenata of space plasma physics, with the exception of high-frequency waves and local discontinuities.

In the fall of 2007 I found out that, on the E-V theorem, I had been anticipated by Oscar Buneman (of Farley-Buneman instability fame). Having reached retirement age, I had to clear out my office and (in a big pile of preprints and reprints, set aside over the years for later perusal) discovered a reprint “Internal dynamics of a plasma propelled across a magnetic field” (Buneman, 1992), in which precisely the same calculations were carried out and the same results obtained as in my 2001 paper. The difference was just that Buneman dealt only with laboratory plasma experiments of a specific type and did not present his conclusion as a generally applicable principle; this, and publication in IEEE Transactions on Plasma Science, may be among the reasons why his paper has been totally overlooked by the space physics community.

I sometimes call the three papers (Vasyliunas, 2001, 2005a, 2005b) my “three-volume exegesis of the gospel according to Parker” (some colleagues, though, particularly younger ones, miss the joke—they have never heard the word “exegesis” before and have no idea what it means). I have also referred to the conceptual framework developed in these three papers as “The third approach to cosmic electrodynamics,” in contrast to the “second approach” of Alfvén (1968b, 1981) whose ideas provided much inspiration to the J-E side of the controversy.

More recently, in an extensive review paper on “Physics of magnetospheric variability” (Vasyliunas, 2011)—complimented (?) by a speaker at an international conference in 2013 as “surprisingly readable”—I presented, among other topics, a systematic summary of this approach to electrodynamics (section 3) and its consequences, including (subsection 3.5) a reformulation of magnetosphere-ionosphere coupling theory, this time “with emphasis on physical understanding instead of mathematical convenience”; the calculational scheme of Figure 1 (Vasyliunas, 1970) is now replaced by Figure 4. I discussed ionospheric aspects of the theory in a poster presented at the AGU 2010 Fall Meeting under the title “The Ptolemaic approach to ionospheric electrodynamics,” later expanded into a full-scale journal publication (Vasyliunas, 2012); I wanted to use the poster title for the paper as well, but the referees and the editor objected, and the historical analogy is only mentioned in the introduction.

Data Availability Statement

No data were used in the preparation of this manuscript.

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