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tionary model of the early universe during a seminar I gave at Drexel University in Philadelphia in November 1981. Some people have interpreted this passage as implying that I am suggesting that Paul Steinhardt and Andreas Albrecht plagiarized Linde's proposal. This is definitely not the case. I have always been quite sure that Steinhardt and Albrecht came to the idea of a slow rolldown completely independently of Linde. And I have now seen a videotape of the seminar that, although not quite complete, does not show me mentioning Linde's idea. I have therefore had that passage removed from the more recent printings of the book. I am quite sure that the work of Steinhardt and Albrecht was independent of Linde's. I am very sorry if some people have gotten the wrong impression from what I wrote.

S. W. HAWKING

University of Cambridge  
Cambridge, England

12/88

## The Different Circles of Solar Cycle Research

I read with interest the section on astrophysics in *Physics News* in 1987, and in particular the article by Herschel B. Snodgrass about the "Evidence for a Solar Cycle" (*PHYSICS TODAY*, January 1988, page S-11). Although I essentially agree with the point of view developed by Snodgrass, I feel annoyed by the references he cited. Not that these references are wrong or inadequate. But some of them are review papers, in particular the paper by Peter R. Wilson and his colleagues (number 1 in Snodgrass's reference list).

The "citation index" is often used in America, but also in France, to promote young scientists and to help their careers. But the people who will benefit from Snodgrass's article are the authors of his nine references, and this may be unfair to a large number of young scientists. Because many more review papers are written in English and published in American journals than are published in Europe by European authors (whether English speaking, French speaking or German speaking), the frequency of citation of articles by scientists outside the circle of the American authors of review papers will, on the whole, tend to be diminished.

In the particular case of the solar cycle, I would like to cite a recent review I have written for the *Irish Astronomical Journal*.<sup>1</sup> This review intends to put in their proper place

many little-cited papers, many of which have priority over papers that are cited much more frequently, but not always with justification. Without mentioning any particular paper now (I refer the reader to my review), I would like to pay tribute to the very important work in this field by some French scientists: Michel Trellis, Jean-Louis Leroy and Jacques-Clair Noëns, concerning the coronal features; Paul Simon and Jean-Pierre Legrand, concerning the consequences for the solar cycle of geomagnetic and other observations; Elizabeth Ribes, André Mangeney and others, concerning the emergence of new active regions, and the evidence from such studies of a long migration period; and also the important recent discovery, by Ziad Mouradian, Irina Soru-Escout and Marie-Josèphe Martres, of the so-called pivot points.

This list is not exhaustive, and American scientists often cite these papers in their original work. But they tend to disappear in review papers, where one is tempted to cite papers that already review most of the publications and that come from, let us say, the neighborhood.

I do not want to say that Snodgrass is unfair to these authors. But the "snowball system" by which review papers cite other reviews necessarily gives much less weight to original papers by scientists outside the American community, especially those who are not asked to give review papers. This, certainly, is unfair.

### Reference

1. J.-C. Pecker, *Irish Astron. J.* 18, 133 (1988).

JEAN-CLAUDE PECKER  
Collège de France  
Paris, France

2/88

The article entitled "Evidence for a Solar Cycle" by Herschel Snodgrass, which was presented as a review of current ideas on the solar cycle, is essentially a summary of American research in this area, ignoring the substantial contributions made by French scientists. Regrettably, this biased approach appears all too often in the scientific literature and in fields of endeavor far removed from solar cycle research. This is a wider issue, however, and at present my concern is with the content of Snodgrass's article, to which I would like to add the main French contributions and place them in the context presented by Snodgrass.

▷ The concept of a 17-year solar cycle having two simultaneous branches of activity, a high-latitude band and a low-latitude band, was first suggested by Michel Trellis (1963) and by Jean-

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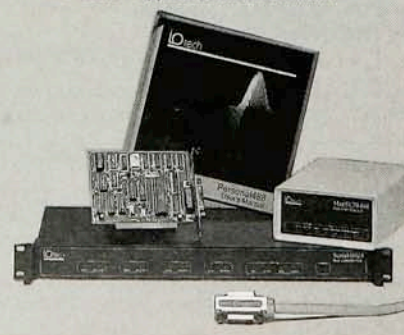
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Louis Leroy and Jacques-Clair Noëns (1983), based upon observations of the green coronal iron line emission at the Pic du Midi Observatory over three 11-year solar cycles. Paul Simon and Jean-Pierre Legrand (1981) independently arrived at the conclusion of an extended 17-year cycle from the analysis of ten 11-year cycles of solar and geomagnetic activity.

▷ In 1979 Karen Harvey and Sara Martin noticed that the high-latitude ephemeral regions (small-scale active regions) displayed on the Kitt Peak magnetograms were of opposite polarity to those of the low-latitude ephemeral regions. This finding was interpreted as indicating the presence of two simultaneous solar cycles, and was later confirmed by Peter Wilson, Richard Altrick, Martin, Harvey and Snodgrass (1988).

▷ In 1980 Robert Howard and Barry LaBonte, using magnetograph observations from the Mount Wilson Observatory, showed that the differential rotation was also varying with time, exhibiting zones rotating slower and faster than average. The travel time of these "torsional waves" from pole to equator was 22 years.

▷ In 1984 Pierre Mein and I presented the first evidence of a complex latitudinal meridional circulation in the convective zone. In 1985, with André Mangeney, we suggested the presence of azimuthal (east-west oriented) rolls. The rolls could explain a number of puzzling observations. In particular, the torsional wave pattern found by Howard and LaBonte was interpreted in terms of azimuthal rolls by me (1985, 1986) and by Snodgrass and Wilson (1987).

▷ At a scientific meeting in Pasadena in January 1987, as well as in later articles in *New Scientist*, *Sky and Telescope*, *Nature* and other journals, it was stated that Snodgrass discovered the existence of azimuthal rolls, inferring them from the torsional oscillation pattern. In some of those articles, the detection of the azimuthal roll pattern by the French team in 1984 and 1985 was not even mentioned or was briefly reported as an independent finding.

▷ In the early 1980s, some interesting work by Francis Laclare at the Centre d'Etudes et de Recherches Géodynamiques et Astronomiques observatory in Nice showed that the apparent solar diameter was oscillating with a number of periods (1000 days, 320 days, 150 days, 75 to 50 days). These periodicities are present in a number of solar, geomagnetic and atmospheric indices. The contribution of other European colleagues in this field has also been substantial.

▷ Laclare and I (1988) explained why the equatorward migration of the solar activity is not in conflict with the poleward-moving azimuthal rolls. We also suggested that the azimuthal rolls might be the long-sought giant convection, and thus responsible for the diameter modulation.

▷ As early as 1970, John Wilcox, Kenneth Schatten, Arthur Tanenbaum and Howard had questioned the concept of a mean differential rotation of the solar surface. Irina Soru-Escut, Zaid Mouradian and Marie-Joséphine Martres subsequently showed, in work beginning in 1984, the presence of a number of points on the surface of the Sun that exhibit a rigid rotation. These "pivot points" are the places where new active regions form. The rigid rotation of the Sun's active regions challenges current ideas on the dynamo mechanism and its location.

From these examples, it is clear that the progress in our understanding of the complex solar machinery has not been the result of efforts at a single laboratory or a single nation. It is also evident that the French contribution to the understanding of the solar cycle is extremely significant. Advanced US technology and the dynamism of American research are well appreciated, not only in France but throughout Europe. Many US colleagues are very cooperative and most generous with their data. A full list of names is too long to be given here. The fact remains, however, that French and perhaps other European research often does not receive sufficient recognition in American scientific writing.

ELIZABETH RIBES  
Observatoire de Paris  
Meudon, France

10/88

SNODGRASS REPLIES: When I was asked to write a news article describing recent work on the "extended" solar cycle (it was unfortunately mis-titled in publication—the title submitted was "Evidence for an Extended Solar Cycle"), I was told to limit my citations to the few that were immediately relevant. This is standard for the *Physics News* supplement. In my reference 2, I cited the French scientist Michel Trellis, who is acknowledged as being the first to hypothesize an extended (> 11 year) migratory pattern in the solar activity cycle. A list of subsequent work that I am aware of on this and related ideas includes and goes beyond the papers mentioned in the letters by Jean-Claude Pecker and Elizabeth Ribes. I would have liked to cite all of it since it is an interesting history, but



I was explicitly asked not to do so by the editor of *Physics News* in 1987. I and my colleagues have referred to these papers, particularly those of the French, repeatedly in our publications (see, for example, the citations in references 1, 5 and 8 of my *Physics News* article). As both Pecker and Ribes are aware of these citations, I take their letters to be aimed not so much at my presentation as at pointing out to Americans the pioneering work of French scientists in this and related areas. I agree with them that such work is often passed over by American researchers, but I feel it is simply a matter of moving in different circles.

Although I also agree with Pecker's point about citing original sources rather than review articles, I disagree with his characterization of the *Nature* letter by Peter R. Wilson and his colleagues as a review article. The paper includes a review of previous work with a long list of references, but then presents new work that has not been published elsewhere. The point is that all the evidence, when correlated together, makes a strong case for the still controversial hypothesis that the solar cycle is caused by, or at least associated with, the pole-to-equator migration of the azimuthal zone. I also note that Wilson, the leader of the present research effort, is Australian, not American. Hence my *Physics News* article is not just a summary, as Ribes contends, of American research.

In our first papers, Wilson and I made considerable reference to the work of Ribes and her collaborators, although most of her work had not appeared in refereed journals. At the press conference during the January 1987 Pasadena meeting of the American Astronomical Society, to which we were invited and to which she refers in her letter, I began my presentation with a reference to her ideas. This was picked up by the press, and she was mentioned prominently in several American magazine and newspaper articles. I therefore find her complaint disturbing. She is not cited in either the Wilson article or in my *Physics News* article because we do not find that her work connects with the "extended" cycle hypothesis.

Ribes indeed suggested the existence of azimuthal convective rolls on the Sun before we did, inferring them from her analysis of the observations of the meridional motions of young sunspots. But others, particularly Robert Howard and Peter Gilman, who also analyze the motions of sunspots, dispute her findings. Furthermore she is not the first person to

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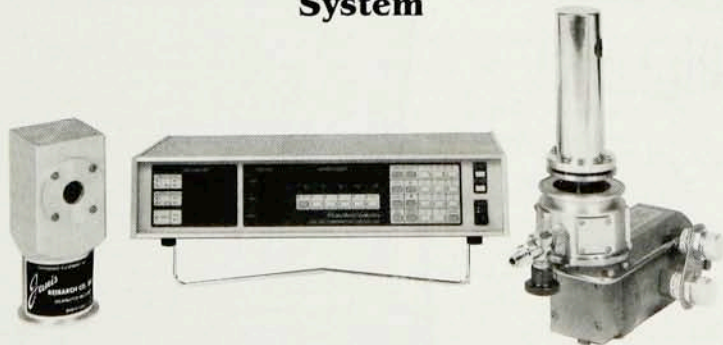
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