EVIDENCE FOR AN ANGULAR MOMENTUM FLUX IN THE SOLAR WIND

OPENING REMARKS

R. P. Kraft As an observational astronomer who watches the stars I feel somewhat remote from most of the proceedings that have gone on so far in the conference. Most of us in astronomy usually think that first there is the solar wind, then the physics of the solar wind, and next speculation, then wild speculation, and finally there is astronomy. But after hearing the activities of the last couple of sessions I am not so sure that that is the correct order any more.

This afternoon we will divide the discussion into three parts and begin with a discussion of *evidence for an angular momentum flux in the solar wind.* There will be a summary introductory report by A. J. Hundhausen and, additional contributions, and open discussion, including the floor and members of the round table. I would like to conclude this part after about an hour. It is entirely possible that we will reach the conclusion that there is no angular momentum flux in the solar wind and consequently we can all go out and have a beer because the rest of the afternoon does not matter. However, I rather anticipate that we will not end on that note.

We will pass then onto the second part of the discussion, which concerns *evidence for changes in the angular velocity of the surface regions of the sun and stars,* and its relation to stellar efflux. This will start with a summary introductory talk by me, and some subsequent remarks by others. Then we will have further discussion, round table and floor. The ultimate session this afternoon will consist of *evidence for the distribution of angular velocity inside the sun and stars;* there we will have an introductory talk by Leon Mestel, followed by remarks and open discussion as before.

INTRODUCTION

A. J. Hundhausen The rationale for this strange union of solar wind specialists and more traditional astronomers stems from the fortuitous location of the sun. Not only can the solar rotation rate be measured with considerable precision, but observations of solar wind particles and magnetic fields can be used to infer the loss rate of angular momentum from the sun. This loss, resulting in a small nonradial velocity component of the solar wind plasma, implies a torque that tends to slow the rotation of the sun.

The observations of the angular momentum carried by the solar wind began, as did many other facets of solar wind research, with the study of comet tails. Under the assumption that an ionic comet tail is alined with the solar wind velocity (in the rest frame of the comet) comet tail direction observations can be used to infer the direction of flow of the solar wind. This method has been used in deducing the mean nonradial velocity component of the solar wind by J. C. Brandt and his colleagues [*Brandt,* 1970]. This deduction is based on the difference in orientation of the tails of two different classes of comets (direct and retrograde) and thus does not depend on any "absolute calibration" of the radial direction. The resulting mean nonradial velocity component at 1 AU falls in the range from 6.6 to 8.8 km/sec (depending on how comet observations at heliocentric distances other than 1 AU are transformed to this position) in the direction of corotation with the sun. As the mean radial velocity of the solar wind is ~400 km/sec, this implies that the solar wind flow deviates from the radial by 1° or 2°. The difficulties in measuring this nonradial velocity component, using either comet tail observations or the direct, *in situ*, plasma observations stem from the small size of this deviation from radial flow.

The direct determination of the nonradial component of the solar wind velocity requires measurement of the solar wind flux as a function of angle. The first such observations used to infer a mean nonradial velocity component were made on the twin Vela 2 spacecraft (launched in July 1964) and reported in papers published by the Vela group in 1967.

Figure 1 (from *Strong et al.* [1967]) is a histogram of the measured flow directions (projected into the ecliptic plane) determined from Vela 2 data acquired

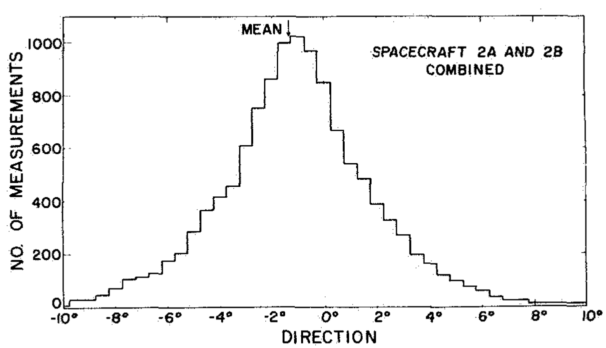

Figure 1 *A histogram of the solar wind flow directions observed by the twin Vela 2 spacecraft between July 1964 and July 1965 [Strong et al., 1967]. Negative angles correspond to flow from the east of the sun, positive angles to flow from the west of the sun.*

between July 1964 and July 1965. The sign convention used in these data is that negative angles are flow *from east* of the sun (i.e., rotation in the same sense as that of the photosphere), positive angles are flow *from west* of the sun (i.e., rotation in the opposite sense as that of the photosphere). The mean direction of flow of the solar wind implied by these observations is from 1.35° *east* of the sun. This is equivalent to an azimuthal velocity component of about 8 km/sec in the same sense as the solar rotation.

The Vela 2 observations are thus in basic agreement with Brandt's conclusions and, as we shall see a bit later, imply a considerable angular momentum loss from the sun. The problem in interpreting these observations is the accuracy of the 0° orientation; that is, do we really know when the Vela detector system is pointing at the sun? This is the "absolute calibration" problem referred to earlier (see Wolfe, p. 184). An analysis of the possible systematic errors in the Vela detector system led to the conclusion that the solar direction was probably accurate to ~0.7°, equivalent to a probable systematic error of about 4 km/sec in the mean nonradial velocity component. With a probable error of this size, one would have to concede that an error as large as the 1.5° mean deviation from radial flow itself could not be ruled out.

Figure 2 (from *Hundhausen et al.* [1970]) shows histograms of the flow directions observed on the next set of Vela spacecraft, launched in July 1965. The plasma detectors on these Vela 3 spacecraft are similar to those on Vela 2. If all of the Vela 3 data (from the two-year period July 1965 to July 1967) are averaged together, the mean flow is from 1.5° east of the sun, in excellent agreement with the Vela 2 result. However, figure 2 has unfortunately been drawn to display the observations from each Vela 3 spacecraft separately, and we discover that these two supposedly identical detector systems lead to mean directions that differ by 1.6°. Thus not only did our Vela 3 observations confirm the mean flow

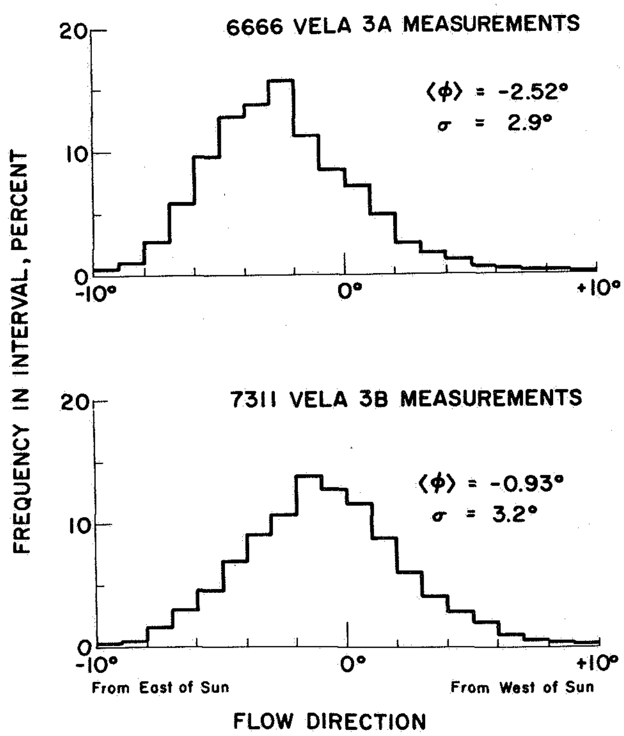

Figure 2 *Histograms of the solar wind flow directions observed by each Vela 3 spacecraft between July 1965 and July 1967 [Hundhausen et al., 1970].*

direction observed by Vela 2, but also gave direct support to our concession that an error as large as the result was possible.

The latter conclusion was also supported by the publication of flow direction observations made on the IMP-1 spacecraft in 1963 [*Egidi et al.,* 1969]. The mean flow deduced from these data was also 1.5° from the radial, but from *west* of the sun. This would imply a solar rotation in the opposite sense from the solar photosphere.

All observations are displayed in a "stationary" frame of reference, with the orbital motion of the spacecraft (and earth) about the sun subtracted from the observed nonradial velocities. The results described above constitute all the published observations on solar wind flow direction as determined by direct spacecraft measurements. J. H. Wolfe has presented (p. 184) previously unpublished results from Pioneers 6 and 7 data acquired over several solar rotations. These observations led to mean flow directions appearing to come from 0.3° *west* of the sun.

A. J. Lazarus will shortly discuss a set of flow direction observations made on Mariner 5 (p. 265). I don't want to steal his thunder, but I must admit I know his observations agree well with the Vela results and with Brandt's comet observations. Thus, we have a set of independent observations that are in agreement in finding a solar wind flow direction at ~1.5° from east of the sun, or an azimuthal flow component of ~8 km/sec. I might be tempted to claim a triumph for the democratic process, but I think that any of this group of observers would concede that the uncertainties in our results could be as large as the final mean values. Nonetheless, let us explore the implication of a mean nonradial velocity component of ~8 km/sec at 1 AU. The angular momentum flux density carried by the solar wind plasma at 1 AU is then 6.3 dyne cm/cm^2. Computation of the torque on the sun requires some assumption regarding the variation of the angular momentum flux density with solar latitude, as all of the observations are made near the ecliptic plane, within ±7° of the solar equator. Assuming either a uniform flux over ±30° of solar latitude or a flux that varies as the cosine of the heliographic latitude leads to a torque of ~10^{31} dyne cm. For anyone unfamiliar with torques of this magnitude, some physical feeling comes from computing the braking time for the sun under such a torque. If the sun were rotating as a solid body this torque would result in a braking time of about 3×10^9 years. That, of course, is approximately equal to the generally accepted lifetime of the sun and means that such a torque could have a significant braking effect in a solar lifetime.

Let us next briefly discuss the theoretical models of the transfer of solar angular momentum to the solar wind, as I think that some reluctance to accept the observations of an 8 km/sec nonradial velocity component near 1 AU stems from an apparent discrepancy between the theories and observations. E. N. Parker pointed out some time ago that a rough estimate of the angular momentum of the solar wind could be obtained by assuming that the solar magnetic field drags the coronal plasma in rigid corotation until the outward flow speed is greater than the Alfvén speed and then assuming conservation of angular momentum at larger heliocentric distances. Application of this idea leads to an azimuthal flow speed component of about 1 km/sec at 1 AU, a result an order of magnitude less than the observations described above. The magnetic force was incorporated into a quantitative model by *Weber and Davis* [1967], and figure 3 shows the azimuthal velocity component v_ϕ predicted by their model as a function of heliocentric distance.

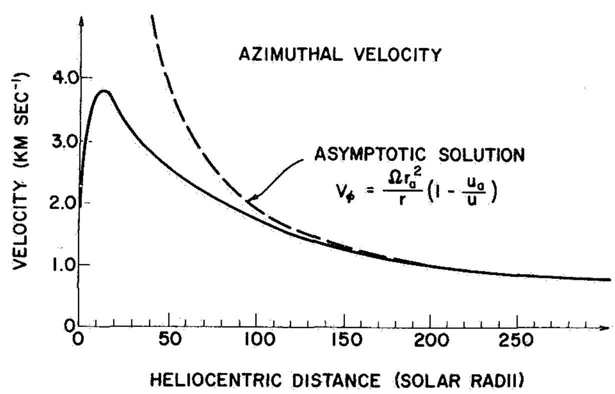

Figure 3 *The nonradial component of solar wind velocity as a function of heliocentric distance predicted by the model of Weber and Davis [1967].*

Rigid corotation of the plasma ($v_\phi \propto r$) is maintained by the magnetic field for only a very short distance from the sun. However, the magnetic field does continue to transfer angular momentum to the plasma and at large heliocentric distances, the Weber and Davis solution approaches that predicted by Parker (as described above) and shown by the dashed line of the figure. The nonradial velocity component predicted at 1 AU (215 $R_\odot$) was again near 1 km/sec.

The Weber and Davis model assumed a polytropic law relating temperature to density, as described by Parker (p. 162). *Brandt et al.* [1969] have incorporated the same magnetic force into a model in which the energy equation is actually integrated. This model gives a different variation in the radial expansion speed with heliocentric distance, and thus obtains a different nonradial velocity, 2.5 km/sec at 1 AU. The difference between these two results indicates that the models are quite sensitive to the detailed manner in which the solar wind is heated (thus determining the detailed radial velocity variation). You may recall that some sign of a lack of universal agreement has emerged as to how the solar wind is heated. It should be clear at this point that such uncertainties feed back into the angular momentum problem and lead to uncertainties in the theoretical value of the angular momentum and azimuthal flow speed at 1 AU.

The problem can become much more complicated than in the simple models thus far considered. A logical next step is to include viscosity in the computations. A model including viscosity was derived by *Weber and Davis* [1970]. In fact, this particular model also included an anisotropy in the pressure tensor, as anisotropic thermal motions do imply a flux of angular momentum. This effect was put in the model in a rather crude manner by simply assuming a variation of the anisotropy magnitude with heliocentric distance. I should point out also that the *total* pressure tensor, as M. D. Montgomery has shown (p. 208), is not very anisotropic when observed near 1 AU, because the electrons are hotter than the protons and nearly isotropic. Now, this particular model predicts an azimuthal flow speed component at 1 AU of 6 km/sec. This larger transfer of angular momentum to the solar wind is largely due to the viscous force at some tens of solar radii. Six kilometers per second is in fair agreement with the lowest value given by Brandt's comet study, and one might consider the theory and observations to be approaching a common ground.

A viscous model has also been developed by *Wolff et al.* [1971]. The Weber and Davis' model, including viscosity, again used a polytropic index to avoid having to integrate the energy equation. *Wolff et al.* [1971] integrated the energy equation with a thermal conductivity modified by the presence of the magnetic field. The latter predicts a nonradial velocity component at 1 AU of only 1.5 to 2.0 km/sec. The basic reason for this difference is the strong dependence of viscosity on the proton temperature and the by now familiar uncertainties as to how the proton temperature is actually determined in the solar wind.

There are many other ways of complicating the models. For example, there are known to be fluctuations or waves in the interplanetary medium. *Schubert and Coleman* [1968] have pointed out that these waves also carry angular momentum from the sun and that interaction with the particles can ultimately transfer that angular momentum to the plasma, producing a larger nonradial velocity. There has been some debate at this conference about the role of nonsteady phenomena and nonspherical flows in the solar wind. If the expansion from the sun is not spherically symmetric, there are new possibilities for increasing the corotation of the coronal and having a large angular momentum in the solar wind. A rough model including such effects has recently been published by *Sakurai* [1971].

In summary, solar wind observations do give some evidence for an important loss of solar angular momentum in the expansion of the solar wind. These observations are difficult, not always in agreement, and should still be approached with a healthy skepticism. The theoretical models connecting solar rotation and the nonradial flow of the solar wind are oversimplified, sensitive to the unknown mechanisms of heating the plasma, and deserve a similar skepticism.

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COMMENTS

A. J. Lazarus I would like to comment on the points that Hundhausen has brought out. First of all, on IMP-1, where the average flow direction was reported to be from the west, there is a possibility of bias, as he said, because the IMP-1 orbit was unfortuitously commensurate with the passage of the interaction regions between high and low speed streams. It turned out that we missed some of the low velocity plasma nearly every time. That plasma occurred just before the higher speed plasma and (as we now know) it probably would have appeared to be coming from east of the sun. Therefore, the data could very well be biased so that the average flow was from the west. But it is very difficult to prove this without having some comparison spacecraft.

My second comment involves our average direction measurement from Pioneer 6. A photoelectric effect existed when measuring electrons and looking at the sun. On the other hand, it does enable us to check on the sun direction. We used that information to analyze the Pioneer 6 data and find that our experiment responded as if the pulse from the spacecraft sun sensor was offset by approximately 2.5°. Taking that offset into account, the average flow direction we get is very close to radial.

Table 1 shows 1-hr averages of data from the Mariner 5 spacecraft that went in toward Venus. These values in turn are averaged over the six solar rotations for which we have

Table 1. *Solar wind properties observed on Mariner 5 (averaged over solar rotations)*

Solar rotation	$\overline{R}$ (AU)	$\overline{V_r}$ (km/sec)	$\overline{V_n}$	$\overline{V_t}$	Angular momentum flux density: $Nm_pV_rV_tR$ (dyne-cm/cm²)	$B_rB_tR/4\pi$	Flux/$d\Omega$= $\overline{\text{Sum}} \times \overline{R}^2$ (dyne-cm)	Proton flux/$d\Omega$
1832	1.0	392	-1.8	0.84	3.0×10^3	0.9×10^3	8.7×10^{29}	6.7×10^{34}
1833	.95	373	-1.3	5.2	5.3	.8	12	5.8
1834	.87	438	-1.3	8.5	6.1	1.4	13	5.1
1835*	.80	441	-4.1	7.7	9.3	1.6	16	5.0
1836	.73	417	-3.1	11.2	7.7	2.5	12	4.5
1837*	.68	415	-5.3	11.0	8.4	2.2	11	4.5

**Data missing from a portion of the solar rotation.*

data. The first columns show the radial distance from the sun, and the averages over the solar rotations of the radial, normal, and tangential components of the velocities. The components are taken relative to the solar equatorial plane. Positive values indicate corotating flow – from east of the sun. For each rotation we've averaged the two contributions to the angular momentum flux density. For the contribution from the particles we have taken the average of the product of the number density N, the proton mass m_p, the radial velocity V_r, the tangential velocity V_t, and the distance from the sun R. The next column shows the contribution of the anisotropic pressure produced by the magnetic field. These are combined to obtain the flux per solid angle by multiplying the sum by R^2 for the various rotations. The values range from 9 to 16×10^{29} dyne cm. Two of the solar rotations are incomplete. It is very important to obtain complete solar rotations because when the high-low speed stream combination moves over the spacecraft, the tangential component of velocity first arrives from the east and then from the west, and this must be properly averaged out by examining the behavior of all the contributions to the angular momentum flux. For comparison the table also includes the proton flux per solid angle, which is seen to vary with decreasing solar distance.

A conclusion to be drawn from this is that there does appear to be a net angular momentum flux from the sun. It is consistent with Brandt's values and gives a slowing torque on the sun. If you calculate the net slowing torque by assuming that most of the contributions would come from solar wind flow between solar latitudes of ±30°, the net result is a slowing torque of 7×10^{30} dyne cm.

We believe the errors are roughly ±4 km/sec (±0.5°) for an individual measurement of tangential velocity and approximately ±2 km/sec for the average systematic error. As you get closer to the sun, the tangential velocity increases, reducing the effect of systematic error. On the other hand, the only check that we have is during the first solar rotation. The spacecraft was rolled about the sun-spacecraft line and during that time we could see the apparent azimuthal flow direction relative to the sun-spacecraft line change as the spacecraft rotated. This enabled us to find an effective alinement error of 0.5° of our instrument relative to the sun-spacecraft line. But there is no way of checking this later in the flight. So it is conceivable that there could have been some drift, but the numbers do seem to be consistent. (These comments appeared in extended form in a paper by *Goldstein and Lazarus* in *Astrophys. J.*, Vol. 168, 1971, p. 571.)

DISCUSSION

J. H. Wolfe I did want to add one point to Lazarus' comment regarding the Pioneer 6 sun pulse offset of 2.5°. If true, then our overall average of 3° means we are still 0.5° west. Could you indicate the sun pulse offset on Pioneer 7?

A. J. Lazarus I think about 5.5°.

J. H. Wolfe Well, if it's 5.5° on Pioneer 7 and our average is 0.3° . . .

A. J. Lazarus Okay, this is a very complicated business. Pioneer 7 spent some time within the magnetosheath; it then went outside the shock, came back through the tail of the earth, and then out into solar wind. Now, what you need to do is be sure that you have data from a complete solar rotation taken out in the solar wind and then we can compare values. We must compare by solar rotation. If we examine the bias due to looking at the sun inadvertently through the photoelectric effect and correct our data for that, it is consistent with the plasma coming radially. But uncertainties in Pioneer data are greater than uncertainties in Mariner 5 data from which I quoted. So we could certainly have been off by ±0.5°, perhaps even twice that, on Pioneers 6 or 7.

J. H. Wolfe What I still don't understand is that since we did not correct our Pioneer 7 data in terms of the azimuthal histogram for any supposed error in the sun pole, the curve I showed this morning should have been shifted over about 5°.

A. J. Lazarus You've got to look at exactly what solar rotation that was.

J. H. Wolfe Do you agree with that statement?

A. J. Lazarus If you took the data from the time when you were out in the solar wind and if the 5.5° bias, which we know is in our data, was also in your data, then it should be shifted over 5.5°, as you stated. It's a very subtle difference; you're trying to measure small angles, you have to be sure that you get a completely unbiased sample. It's very difficult.

J. H. Wolfe I agree. The point I would like to make is that there are systematic errors that tend to creep into a spacecraft measurement in terms of instrumental alinement, etc. If you have an internal photo effect, how well do you know it? Can you calibrate it in the laboratory convincingly?

A. J. Lazarus There are two points I want to be clear on; what I should really make claims for are our data and not yours. Our data are corrected by what we know is the alinement from the pure geometry of the instrument; we know what our alinement is relative to the sun by looking at the photo effect. We are quite sure of that. It's very difficult to be off – to be off by 0.5° is absolutely as far as I can say we would be off. Now, when our data are corrected by that, we get flow consistent with v_r within the accuracy of our measurements, which is not as great as Mariner 5. I can't say whether or not you had the same bias to your instrument. What we would have to do is compare data for the same solar rotation at the same time and see what happens.

J. H. Wolfe I would just like to reiterate the three points Hundhausen made. I think that the data taken in space today with regard to the average flow direction should be looked upon with skepticism. I say it is in error from one measurement to the next. We see it in our own.

I think the idea of when you sample the data is very important. Probably measurements taken over at least a year are required to get an understanding of this. And, finally, sampling problems with regard to tracking are exceedingly important in getting a good average.

A. J. Hundhausen Well, I was hesitant to put that 1.5° up there, but now I feel pretty good about it. I think that what this points out is that there is a real problem because of the need for an absolute measurement. The roll maneuver of Mariner 5 was very important in this consideration. Now, it's easy to tell people how to design spacecraft, but if this is a real important physical problem, rolling, and the ability to make this kind of calibration in space, is something that we should consider in the future.

J. H. Wolfe Was the experiment on when you went through the roll maneuver?

A. J. Lazarus Yes.

J. H. Wolfe In the scientific format?

A. J. Lazarus Yes. We didn't know this kind of measurement could be made when we planned the experiment. Looking at the data, during the roll maneuver by chance we could make the determination.

R. H. Dicke All remarks so far have been connected with the particle flux of angular momentum. Let's not forget that the twisted negative field also contributes to the transport of momentum. The magnetic stress must be added to the velocity stress.

Second, I take great comfort from the fact that computations using the measured stress tensor provide results that agree within a factor of 2 with that obtained from the Alfvénic radius, though they are quite different. The Alfvénic radius is perhaps 20 $R_{\odot}$ and the other measurement is taken at ~100 AU. That these two ways of looking at the problem show agreement within a factor of 2 I think is something to take comfort from and not be worried about.

L. Mestel I want to take up that point of Dicke's. If I understand the situation correctly, the difficulty in detailed comparison of the theory with observation, in particular the 1 km/sec as compared with 10 km/sec, is not that the theory predicts too little angular momentum loss. As Dicke points out, there is angular momentum loss transport by the twist in the field, an old idea going back to 1955 or 1956. The difficulty is that one has gotten used to thinking that the Alfvénic surface, the Alfvénic point on the particular stream line, is a water shed and that inside of it the transport is essentially by magnetic stresses and not too far beyond the transport is by plasma and the magnetic stresses can be ignored. At great distance the latter is true, but it is because the increase of velocity in the Parker model is so slow, going perhaps as the square root of log distance; although one is well beyond the Alfvénic point still the simple models we use do predict that the magnetic stresses, the twist in the field, transport most of the angular momentum.

R. H. Dicke It's only about 20 or 30 percent.

L. Mestel Well, depending on which model you're using. I think that's the difference.

A. J. Lazarus I'm sorry, I didn't point out that my figure has both the magnetic field contribution and the particle contribution, and the magnetic field contribution is about one-fourth of the total.

J. C. Brandt There is one other object in the solar system amenable to wind sock type analysis, and that is the earth's magnetic tail. Such analyses have been carried out and to explain the aberration angle requires azimuthal velocity of about 7.5 km/sec. This work has been carried out by Behannon. So another independent piece of information exists for velocities of the kind we are talking about.

COMMENTS

E. J. Weber The torque exerted on the sun by the expanding solar wind is of considerable importance to astrophysics. Yet, while general features of the azimuthal motion of the steady-state solar wind and the resulting torque on the sun are quite well understood theoretically [*Weber and Davis*, 1967], angular velocities at 1 AU predicted by this and similar other models differ significantly from those inferred from the observed deflections of comet tails [*Brandt and Heise*, 1970] and from certain plasma velocity measurements on satellites [*Hundhausen*, 1970]. The observationally determined values of the azimuthal velocity are subject to large uncertainties, especially those obtained from plasma measurements on spacecraft. Since the solar wind flows nearly radially at 1 AU, one attempts to determine deviations from this direction of the order of 1° to 2°. The degree of uncertainty is best illustrated by the average values of the angle between the radial velocity u and the velocity vector in the direction $(u\mathbf{e}_r + v_\phi \mathbf{e}_\phi)/(u^2 + u_\phi{}^2)^{1/2}$ as determined from plasma measurements on different spacecrafts. These values are indicated in table 1. Very recently, *Lazarus and Goldstein* [1971] have presented results for the plasma angular momentum, the torque due to the magnetic field as well as other pertinent data obtained on Mariner 5. Their results are given in terms of averages over solar rotations, in particular for rotations 1832 to 1837.

Table 1: *Average values of angle between the radial velocity and the projection of the total velocity vector into the r-ϕ plane [data are from Wolfe; see p. 183]*

Spacecraft	Average angle, deg
IMP 1	-1.5
Vela 2	+1.4
Vela 3A	+2.5
Vela 3B	+0.9
Pioneer 6	-3.0
Pioneer 7	-0.3

We can define the following constants of the motion, for the mass flux, the "total angular momentum" flux and the magnetic flux per steradian:

$$c = \rho u r^2 \tag{1}$$

$$l = \Omega r_A{}^2 c \tag{2}$$

$$b = r^2 B_r \tag{3}$$

where the symbols used are the same as those in *Weber and Davis* [1967]. Furthermore, there are two contributions to the total angular momentum flux

$$l = \Omega r_A^2 c = r[v_\phi - (B_r/4\pi\rho u)B_\phi]c \tag{4}$$

where the first term represents the contribution due to the angular momentum of the plasma, and the second term represents the torque due to the presence of the magnetic field. In the region between the earth and Venus where the Mariner 5 measurements were obtained, the radial velocity of the solar wind has very nearly its asymptotic value u_∞. *Weber and Davis* [1967] have shown that for this case, the two components of l are given by

$$rv_\phi = \frac{l}{c}\left(1 - \frac{u_A}{u_\infty}\right) = \Omega r_A^2\left(1 - \frac{u_A}{u_\infty}\right) \tag{5}$$

and

$$-\frac{B_\phi B_r}{4\pi\rho u} r = \frac{l}{c}\left(\frac{u_A}{u_\infty}\right) = \Omega r_A^2\left(\frac{u_A}{u_\infty}\right) \tag{6}$$

respectively, Furthermore, we know that at the radial Alfvénic critical point $M_A^2 = 1$, which implies that

$$u_A = \frac{b^2}{4\pi c r_A^2} \tag{7}$$

We have used the above relations to determine r_A, u_A, b, and B_r from the values given by *Lazarus and Goldstein* [1971]. The results are shown in table 2. Note that the position of the radial Alfvénic critical point falls into the region predicted by *Weber and Davis* [1967], but they also determined the radial velocity u_A to be 332 km sec^{-1} with u_∞=425 km sec^{-1}. Thus with the specific boundary values used, the model predicted a total angular momentum flux that was largely due to the torque associated with the

Table 2. *Theoretical solar wind properties determined from Mariner solar wind data*

Solar rotation	Distance from sun AU	Observed radial velocity $\bar{u}$ 10^7 cm sec^{-1}	Values obtained from model $\bar{r}_A$ $r_\odot$	$\bar{u}_A$ 10^7 cm sec^{-1}	$\bar{b}$ $\gamma \times$ AU^2	$\bar{B}_r$ γ	Observed magnetic field magnitude $\overline{\lvert B \rvert}$ γ	$\bar{B}_r/\overline{\lvert B \rvert}$
1832	1.00	3.94	21.1	0.91	2.6	2.6	5.9	0.43
1833	0.95	3.73	26.7	0.48	2.2	2.4	6.6	0.37
1834	0.87	4.38	30.0	0.83	3.0	4.0	7.6	0.52
1835	0.80	4.41	33.2	0.66	3.0	4.6	9.5	0.49
1836	0.73	4.17	30.3	1.04	3.2	6.1	11.5	0.54
1837	0.68	4.15	29.0	0.79	2.7	5.8	11.3	0.51

magnetic field, as indicated in figure 1. This particular result is partially due to the model itself and partially due to the specific boundary conditions used. The model employs a polytrope relationship instead of a full energy equation, which will result in a radial solution that rises relatively rapidly close to the sun; thus in general we would expect to calculate from the model a u_A that is too high. This could be changed by using a model with a different energy equation [*Brandt et al.*, 1969] for which the radial velocity rises much more slowly. However, even with such a model it is not quite apparent that one can obtain radial velocities as low as required by the data in table 2. These values could only be explained if there would be a very significant energy flux due to waves and other factors, even at 30 $r_\odot$.

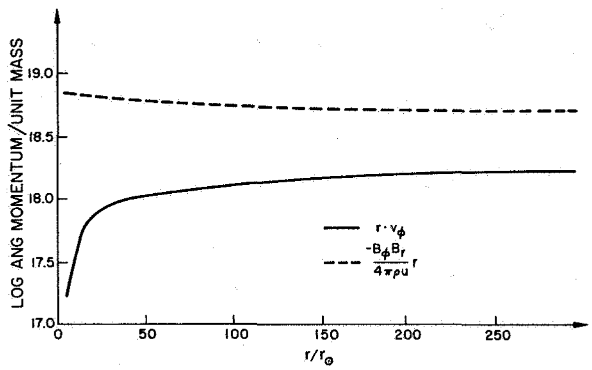

Figure 1. *Comments on recent observations of the angular momentum flux in the solar wind.*

Since the corrections to the actual radial velocity due to the inclusion of the azimuthal velocity and the magnetic field are only second-order effects – very small – we can assume that we know a radial solution of the solar wind and thus the plasma density everywhere. The density at the critical point is given by

$$\rho_A = 4\pi c^2/b^2 \tag{8}$$

and thus increasing the magnetic flux b will decrease ρ_A and move the critical Alfvénic point farther away from the sun. This also implies that (u_A/u_∞) is increasing and thus the fraction of the total angular momentum flux due to the angular momentum associated with the plasma will decrease. The value of 5γ assumed for B_r at 1 AU in the numerical calculations by *Weber and Davis* [1967] as well as by *Brandt et al.* [1969] is somewhat larger than the average value of about 3γ observed near the orbit of earth. Using this low value for B_r, we would obtain a density at the critical Alfvénic point that is larger by approximately a factor of 2. This would imply that the total angular momentum flux removed from the sun would be smaller, but that a larger fraction would be carried away at 1 AU by the plasma itself.

In summary, we wish to point out that while there is some discrepancy between the observed azimuthal plasma velocities at 1 AU and the values predicted by calculations from the models, the differences are not too large and not that significant due to the great uncertainties associated with the observational results. The anisotropy in the pressure tensor due to the nonalinement of the magnetic field vector with the plasma flow direction will also produce an increase in the predicted azimuthal velocity as has been shown by *Weber* [1970], using a rather simple, heuristically derived model for the anisotropic pressure. More refined models for the radial motion of the solar wind which take into account heating due to waves (see Barnes' discussion, p.219) may result in densities that fall off much more slowly and, correspondingly, in radial velocities that increase much less rapidly than predicted by presently used models. We may thus obtain even with a radial magnetic field of only 3γ at 1 AU a torque on the order of 10^{30} dyne cm steradian^{-1} and thus a significant torque on the sun. At the same time one would find that at 1 AU the angular momentum associated with the plasma would account for the major portion of this torque. Finally we can see from equation (8) that if over the solar cycle b varies significantly while c remains relatively constant, the position of the Alfvénic critical radius will shift and the torque on the sun can vary significantly.

An accurate determination of the azimuthal motion of the solar wind at 1 AU is thus of prime importance, since it would not only give us more information on the spindown of the sun, but it would also provide us indirectly, and in conjunction with a theoretical model, information about the properties of the solar wind at the radial Alfvénic critical point.

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DISCUSSION

J. C. Brandt Well, in reply to something you said, Ed, I don't really disagree. I think that the instant we understand how energy is inserted low in the solar atmosphere we'll have to develop other models. Our two-fluid model with viscosity gives a velocity of about 2 km/sec. We could have made it 3 km/sec had we been willing to spend another two weeks on the computer. I personally suspect that if you dump in more energy, you will get a higher azimuthal velocity. And that's the only real sense of disagreement I would have with you.

I think the model we now have is a perfectly good representation of the quiet solar wind and has only one "fudge" factor in it. All we've had to "fudge" was the electron conductivity. The model reproduces the azimuthal velocity, which is low. It reproduces both the proton and electron temperature, the density, and the radial velocity. We're a little high on the magnetic field, but I don't think that would seriously influence our results.

E. J. Weber But if you're slightly high in the magnetic field you just have too much stress in the magnetic field versus the plasma. That is just really what you are fighting. And I think the whole idea is again not a disagreement. The important point is what the radial velocity is at the stage when the Alfvénic critical point is reached. If you have a fast flow, you might push it out – get more angular momentum. But you then also decrease the difference between u_∞ and u_a at the critical point. If that's the case, then, I just say it ought to be inversely correlated to the more quiet zone.

J. C. Brandt The other point I would like to make is that in treating viscosity it is very important to go immediately to the two-fluid model. I think Hundhausen hinted at this. To be more specific, the coefficient of the viscosity is so dependent on temperature that you must correctly use the lower temperature. And then you open up another Pandora's box, which I will open and shut immediately, of the question of what the viscosity in the solar wind really is. I personally have good reasons to believe that at quiet times the normal value that one would get out of Spitzer's book is correct. I also have good reason to suspect that at the hotter times with larger velocities that value will be in error by orders of magnitude. So this is a problem that has to be I think taken into account.

C. P. Sonett When you carry out a plasma experiment you get a three-dimensional distribution function; sometimes it has a high energy tail. In the latter case, fitting procedures are needed. The typical way is to fit a Maxwellian velocity distribution, subtracting off the high-energy tail. A further problem is the He^{++} contribution, and I guess sometimes one can even see a little bit of He^{+}. When done, one comes up with some sort of a mean velocity. It seems to me that in the process of carrying out this rather involved set of trial fits there is room for considerable margin.

E. J. Weber I think if you really want to go into that detail with the helium at the present stage, then any relationship between the models and actual nature is purely coincidental because the models are just too simple to have all these very fine features in there. I don't know of any model that has all the helium He^{+} and He^{++}. If you do that you get into trouble because there isn't just a single critical point but a lot of them – not only that, but more than one branch, as I showed you once before. So the problem I think becomes insoluble.

A. J. Hundhausen I think your question is do we know well what we are doing when dealing with data analysis. I think that's the entire question. However, I would like to point out that in the Vela data I don't think the problems with high-energy tails or helium really interfered with our determination of the proton distribution. Further, the analysis has been done as far as I know in these results by two different methods: bimaxwellian fitting and a more empirical piece-wise gaussian fitting that does show the basic asymmetries in the proton distribution function. When the latter is true, of course, one must use the mean, and that's what has been used. And my guess of the error from the difficulties unfolding in the distribution function is that it's not going to amount to a degree and a half. You can see that by looking at the contour plots that were shown this morning (Hundhausen, fig. 2, p. 262).

J. H. Wolfe I think that typically where the errors seem to show up in the end product is usually density more than anything else. And that the flow directions and velocities and so forth are less susceptible to the error.

C. P. Sonett I hoped when we set the conference up that this particular subject would be explored because it seems critical; the numbers we are dealing with are quite small. In addition, what is the proper definition of bulk velocity? Is it the most probable or the mean?

A. J. Hundhausen The first moment of the distribution function [mean value].

C. P. Sonett What do you do now about things like the heat flux, which throws in a skewness?

A. J. Hundhausen When done right it is taken into account in the calculation; you really reconstruct the distribution function and recompute a mean.

C. P. Sonett What sort of estimate is there that lends confidence to the validity the very small values used in estimates of the angular momentum?

A. J. Hundhausen Well, I've given my answer.

R. P. Kraft Can some person in the audience comment on this?

J. D. Mihalov At Ames Research Center we've studied the errors associated with the reduced Pioneer 6 and 7 Ames plasma probe data. One of the results is that the most probable formal statistical error for the unaberrated azimuthal flow angles normalized to a chi square of one is 0.9° and this seems to be comparable to the size of some of the theoretical flow angles mentioned previously.

I. B. Strong I would like to agree with Hundhausen except for one thing. I think the Vela data have been measured in three different ways. Recall that the Vela 2 data, shown first, were reduced by a simple gaussian fit to the distribution. Since then, this has been done on Vela 3 by bimaxwellian fit and integration over the whole distribution function. Although not published, I've done this for the Vela 2 data. It seems to give the same answer within 10 percent as the very crude methods. So I think the worry about the exact method is valid, but it doesn't seem to make too much difference whether the same data are treated in different ways.

R. Lüst I have a question for the table, namely, what about the various observations of comets and what we have heard here, that there's a tail, a gas magnetic tail; my impression from the solar wind data is that the question is still completely open – I mean, we are uncertain that the theory can explain the observations. Is it not true that one should be very cautious about comet magnetic tail observations since they rely on such few comets? There could also be a strong time effect.

J. C. Brandt The uncertainty in one observation is quite large, of course, and this is because in addition to the radial velocity of 400 km/sec, the comet velocity of about 40 km/sec, and an azimuthal velocity of somewhere between 5 and 10 km/sec, there is superimposed a random isotropic velocity of 30-50 km/sec. This comes out of the data quite well. However, I have a sample distributed over many years beginning in 1889 of 600-800 observations, and it is quite true that a given observation must be taken with great reserve; in fact, you can't derive anything from one observation. But for a large group the radial velocity, the azimuthal velocity, and a peculiar velocity separate from this quite naturally. The peculiar velocity can be checked directly without any assumptions about the comet by looking at the relatively small sample of comets (~100), where we can view the comet tail orientation exactly in the plane of the orbit. To a very good approximation, the radial velocity separates, and the azimuthal velocity has almost no effect at all. You then see the dispersion in velocities perpendicular to the plane of the orbit, and the entire picture falls together, I think, quite well. I might add that the spacecraft dispersions and the comet dispersions agree quite well, and the average radial velocity that comes from the sample of comets is 450 km/sec – again in very good agreement with the spacecraft measurements. So, from the viewpoint of other checks, I see no reason to doubt the comet observations. It doesn't mean there are not difficulties in it, it doesn't mean there might not be an error, but there is no obvious source of error I've been able to turn up in the last 5 years.

E. J. Weber I don't know what the present state of comet theory is but I recall that there was some work done at the Max-Planck Institute by Biermann and others. They say that if there's a mass loading you get a big shock wave developing ahead of the comet. If that's the case, then I think we have to be very careful about whether the comet really represents a weather vane in the wind.

Further there may be a mechanism around the comet forbidding transfer or changing the magnetic stress inherent in the solar wind into actual plasma motion. If you transfer by some mechanism, then Brandt's numbers come out right. In other words, if you assume something like 9×10^{29} dyne-cm, the plasma motion comes out to be about 9 km/sec. So I think it's of the right order in that respect, except I don't know of any mechanism; I just say it's a possibility.

G. Siscoe In these discussions about the angular momentum flux, there are really two different physical quantities used. One is the average transverse component of the speed, the other is the average angular momentum of the flux, which is the product of the transverse component of the speed, the radial component of the speed, and the density. So to go from one to the other would you have to make some assumptions about the correlation coefficient between the transverse component and the mass flux? That's one thing, at least, that the solar wind people could answer, what the correlation coefficient is between B_ϕ and ρB_r; if that's a large number you cannot go directly from average B_ϕ to angular momentum flux.

F. C. Michel If I understand correctly, you indicated that you could get more angular momentum deposited in the particles than in the fields. But I didn't understand whether that was the result of an actual calculation or a proposed idea. And I would be a little conservative about that because within the assumptions of the calculation you would have no angular momentum transfer if the field were taken to zero. Consequently, almost all the angular momentum initially resides in the field and to get the bulk of the angular momentum ultimately deposited in the particles at 1 AU means the field must be willing to surrender the bulk of its angular momentum; it may be a little more difficult than proposed to actually find the state that accomplishes that.

E. J. Weber I did not imply that this happens to plasma in general – that is, to the plasma you measure in spacecraft. I just say this might be a possibility for explaining the comet tail data, because at the comet there are shock lines, there is interaction, there is something similar to a magnetosphere, a magnetosheath possibly.

H. Schmidt I think one doesn't have to worry about inferences on the aberration of the comet tails from shocks and plasma dynamics because an average interplanetary magnetic field line is delayed in the visible comet tail by a day or more, and it has a width of, say, 100,000 km at the most. So it is an extremely good weather vane in the solar wind. From that respect, no matter what the complicated dynamics of a bow shock of a comet is, the visible tail is a very good weather vane.

J. C. Brandt If we can show the first slide (fig. 1) I think we will see very convincing evidence of this. This is a plot of angular dispersion made in three different ways. One was stolen from the Vela group, Strong *et al.*'s data. I simply divided their distribution in their viewing plane in half, folded them over, and then made the same calculation from the comet data. The dashed line is the dispersion of angles around the mean in the plane of the comet, and the dotted line is the dispersion viewed perpendicular to the plane. You

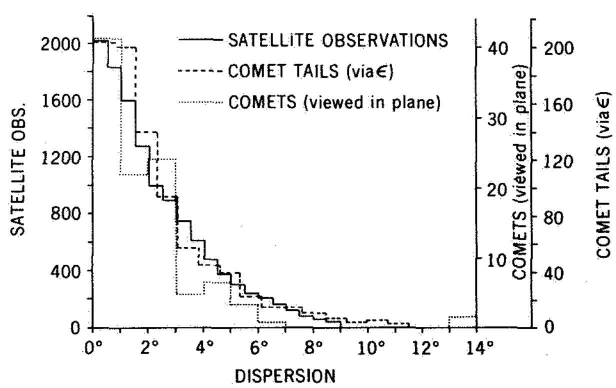

Figure 1 *Contributions of the plasma angular momentum and the torque due to the magnetic field to the total torque on the sun as calculated by Weber and Davis [1967].*

can see that although the curves are ragged they are essentially similar. This means we are observing the same plasma directions, and it encourages you as far as the comet observations are concerned, because it means that the comets do in fact respond rather well to changes in the solar wind plasma direction, so that an individual comet that may have some peculiar structure probably does not bias the distribution of angles.

G. Siscoe I realize what I said wasn't altogether understood. The quantities to be measured are B_ϕ, B_r, and density. But there also must be added a correlation coefficient between B_ϕ and the mass flux.

M. Dryer I want to make a comment with regard to the theoretical interaction of the solar wind with, let us say, a magnetosphere or a comet. Some recent work applicable to the meridian plane containing the solar wind magnetic field and velocity vector includes the magnetic field explicitly, not implicitly, the way we've done it in the past. So if you take the magnetic field and pass it through the bow shock, and it refracts or whatever, the pressure is going to be asymmetrical. This harks back to suggestions made a long time ago by Walters. Now, what effect this asymmetrical pressure is going to have on the overall wind sock angle is still not quite clear. I agree completely with Brandt and Schmidt that the wind sock does respond to the solar wind. I just want to point out there might be a small bias introduced due to the asymmetrical calculation that is limited to the plane including the velocity and the magnetic field vectors.

COMMENTS

L. Davis and I. Strong As we have seen from Brandt's figure, the flow direction of the solar wind as deduced from the space probe and comet tail observations shows substantial dispersion about the mean velocity (extreme deviations of nearly 10° and a half-width at half maximum of about 3° to 5°). Is this variability dominated by the now well-known deflections produced in the interaction regions between fast and slow streams? Alternative possibilities are that it may be due to: (1) effects derived from variations in the radial flow parameters that lead to a variation in the steady-state azimuthal velocity; (2) more or less random fluctuations due to Alfvén waves or other oscillations in the plasma; (3) random uncertainties in the observations.

To the extent that the first possibility accounts for the variability, its theoretical explanation is qualitatively very easily understood. In any case, it poses no problem in any purely theoretical investigation of the angular momentum loss of the sun. It suggests that in determining the observed angular momentum loss one might do better to reject all data that might be contaminated with this deflection produced by stream-stream interaction than to try to uniformly sample the fluctuations.

If alternative possibility (1) is the most likely, theorists should attempt to meet the challenge of providing a quantitative explanation. Any theory that predicts only a small range of azimuthal velocities will be unsatisfactory. What is needed is a theory that quite easily gives a range of velocities as one makes nominal variations in the boundary conditions but perhaps does not allow a unique determination of conditions at the sun from the observarions at 1 AU.

The effects of alternative (2) can presumably be eliminated by choosing a suitable averaging period.