WAVELENGTH DEPENDENCE OF THE INTERPLANETARY SCINTILLATION INDEX

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ABSTRACT Published observations of the interplanetary scintillation index m_z are shown to vary with wavelength in a manner consistent with a smooth, power law spectrum of plasma fluctuations. This is in contrast to recent work arguing that the data require a spectrum with two separate regimes. It is concluded that published observations of m_z are consistent with either type of density spectrum.

A problem of considerable interest in the physics of the solar wind is the relation between interplanetary scintillation of radio sources and the structure of the solar wind turbulence. In particular, it is hoped that interplanetary scintillations can be used to help determine the structure of the solar wind in regions not accessible to direct measurement.

A reasonable first approximation to the scintillation problem is given by the "thin-screen" model in which the fluctuating solar wind plasma is replaced by a thin, phase-changing screen perpendicular to the direction of propagation of the wave. The intensity fluctuations are then built up by interference as the wave propagates to the observer. The observer is situated at a distance z from the plane of the screen. In the solar wind, the equivalent screen is assumed to be placed at the point of nearest approach of the ray path to the sun, since this is where the effect of the solar wind is greatest, and then z is of the order of 1 AU. As the fluctuations are carried out from the sun at the solar wind velocity V_W, the intensity fluctuations in the plane of the observer are also convected at the wind velocity. Hence, the spatial variations in intensity with wavelength ℓ are seen as temporal fluctuations with time $\sim \ell/V_W$.

Of interest, then, is the wave number spectrum of intensity fluctuations in the plane of the observer, at a distance z from the phase-changing screen, and the relation of this spectrum to the power spectrum of *density* fluctuations in the solar wind. A thorough discussion of this problem is given by *Salpeter* [1967]

Let $\delta\rho(\mathbf{r})$ be the fluctuation in solar wind plasma density about its mean. Then the power spectrum of density fluctuations is defined as

$$P_\rho(\mathbf{q}) = \int d^3\boldsymbol{\zeta}\ \langle \delta\rho(\mathbf{r})\,\delta\rho\ (\mathbf{r}+\boldsymbol{\zeta})\rangle e^{i\mathbf{q}\cdot\boldsymbol{\zeta}} \qquad (1)$$

Similarly, if $\delta I(\mathbf{r})/\langle I\rangle$ is the relative fluctuation in radio intensity in the plane of the observer, as discussed above, we may define the power spectrum of *intensity* fluctuations as

$$m_z^{\,2}(\mathbf{q}) = \frac{1}{\langle I\rangle^2}\int d^2\boldsymbol{\zeta}\,\langle \delta I(\mathbf{r})\ \delta I\ (\mathbf{r}+\boldsymbol{\zeta})\rangle e^{i\mathbf{q}\cdot\boldsymbol{\zeta}} \qquad (2)$$

where the integration is carried out over the entire plane. It may be shown [*Jokipii*, 1970] that if the scintillation index $m = (\langle\delta I^2\rangle)^{1/2}/\langle I\rangle \ll 1$, the two spectra are related by

$$m_z^{\,2}(q_x,q_y) = 4\sin^2\left[\frac{(q_x^{\,2}+q_y^{\,2})}{2k}\,z\right]\frac{C}{k^2}P_\rho \qquad (q_x,q_y,q_z = 0) \qquad (3)$$

where k is the wave number of the electromagnetic wave, z is the distance from the screen to the observer,

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and C is a constant.

Note that the dependence of the $\sin^2$ factor on $(q_x^2 + q_y^2)$ states quite generally that plasma fluctuations much larger than the Fresnel scale $\sim\sqrt{z/k}$ are not effective in causing scintillations. Using typical values for the parameters, $z \simeq 1$ AU and k corresponding to 100-MHz radio waves, one finds that the Fresnel scale is of the order of 200 km. Using this fact, *Jokipii and Hollweg* [1970] pointed out that the observed scintillation scales of a few hundred km or so are quite consistent with the observed dominant solar wind scales of the order of 10^6 km [*Intriligator and Wolfe,* 1970].

Here we briefly consider the wavelength dependence of the scintillation index m to see whether or not observations of m can be used to rule out certain forms of the spectrum $P_\rho(\mathbf{q})$. *Hewish* [1971] has argued that the data rule out a smooth variation of $P_\rho(\mathbf{q})$ from the small values of q corresponding to the dominant density scales to the higher values relevant to scintillation. We shall argue that the available data do not force such a conclusion. From equation (2), one easily derives

$$m^2 = \frac{\langle \delta I^2 \rangle}{\langle I \rangle^2} = \frac{1}{4\pi^2} \iint_{-\infty}^{\infty} dq_x \, dq_y \, m_z^2(q_x, q_y) \quad (4)$$

Now assume $P_\rho(\mathbf{q})$ is isotropic, so that $m_z^2(q_x, q_y) = m_z^2(q)$, with $q = \sqrt{q_x^2 + q_y^2}$ Then equation (4) becomes

$$m^2 = \frac{2}{\pi} \frac{C}{k^2} \int_0^\infty q \sin^2 \left(\frac{q^2 z}{2k}\right) P_\rho(q) dq \quad (5)$$

We consider a simple power law for $P_\rho(q)$. Let

$$P_\rho(q) = A q^{-\alpha_3} \quad (6)$$

where the subscript 3 is used to emphasize that this is a three-dimensional spectrum. It is possible that a better representation of the actual situation in the solar wind would be given by a power law spectrum with a cutoff at some wave number q_0. This interesting case is not considered further here. For a given value of α_3, the temporal spectrum observed on a spacecraft [*Intriligator and Wolfe,* 1970] would be

$$P_\rho(f) = B f^{-(\alpha_3 - 2)} \quad (7)$$

[*Hollweg,* 1970], where f is frequency.

If $P_\rho(q)$ has the form given in equation (6), it is then a simple matter to substitute this into equation (5) to obtain, for $2 < \alpha_3 < 6$,

$$m^2 = -\frac{A}{\pi} \frac{C}{k^2} (2)^{\left(\frac{\alpha_3}{2} - 2\right)} \left(\frac{z}{2k}\right)^{-\left(1 - \frac{\alpha_3}{2}\right)} \Gamma \times \left(1 - \frac{\alpha_3}{2}\right) \cos\left[\frac{\pi}{2}\left(1 - \frac{\alpha_3}{2}\right)\right] \quad (8)$$

The wavelength dependence of m_z follows immediately as

$$m \propto k^{-(1/2 + \alpha_3/4)} \propto \lambda^{(1/2 + \alpha_3/4)} \quad (9)$$

Thus with $\alpha_3 \sim 3$, as observed [*Intriligator and Wolfe,* 1970], we expect $m \propto \lambda^{1.25}$. This is in contrast to a gaussian density fluctuation spectrum, which leads to $m \propto \lambda$.

Hewish [1971] has argued forcibly that the available published data required $m \propto \lambda^{1 \pm .005}$. He uses this to infer that there are two regimes in the density power spectrum, a long-wavelength regime that contains most of the power and a separate short wavelength regime that causes the observed scintillations. Between these regimes he postulates little or no spectral power. This view, of course, is consistent with the arguments of *Jokipii and Hollweg* [1970] concerning the dominant scale of the density fluctuations.

Such a spectrum, if true, would be of considerable interest physically. Hence we decided to check whether, indeed, the data used by Hewish actually rule out a dependence such as that given in equation (9) with $\alpha_3 = 3$. We find that they do not. Following the procedure outlined by *Hewish* [1971], we utilized the data reported by *Bourgois* [1969], *Harris and Hardebeck* [1969], and *Hewish and Symonds* [1969].

The idea is that if $m \propto \lambda^a$, then $m\nu^a$ (where $\nu = c/\lambda$) should be independent of frequency. Unfortunately, reliable simultaneous measurements of m at different frequencies do not exist. We must instead compare measurements obtained at different times and at different elongations. Since the characteristics of the solar wind vary from day to day, there is considerable spread in the data. Nevertheless, if $m\nu^a$ is plotted as a function of source elongation, data obtained for different observing frequencies should fall, within the aforementioned spread in the data, on a smooth curve. Figure 1(b) shows a plot of $m\nu$ versus elongation. The data fall on a smooth curve, and one might be tempted to conclude that, in fact, $m \propto \lambda^{1.0}$ [*Hewish,* 1971]. But figure 1(a) shows the *same data,* with $m\nu^{1.25}$ plotted against elongation. The data points again lie on a smooth curve. We conclude that within the uncertainties of the data, $m \propto \lambda^{1.25}$ is as good as $m \propto \lambda$. To put this more quantitatively, the mean square deviations of the points

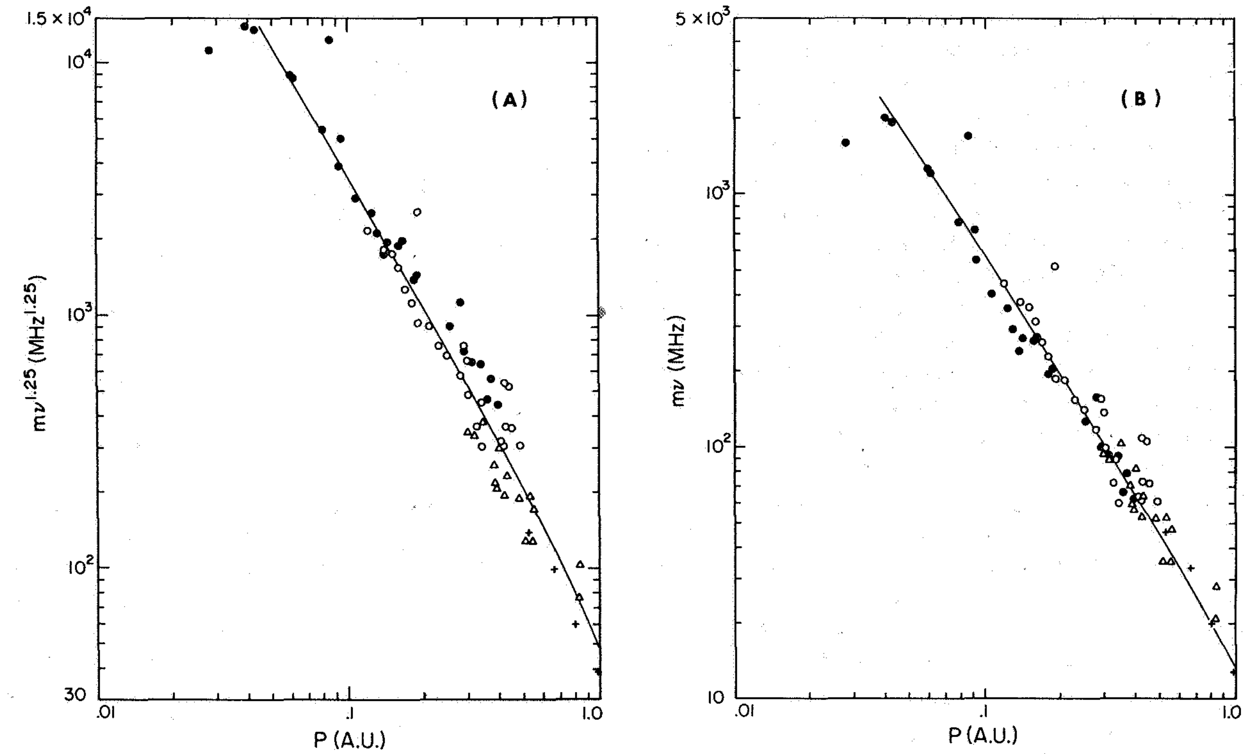

Figure 1. *Scintillation index m multipled by $\nu^{5/4}$ (curve a) and by ν (curve b) versus source elongation. P is the closest distance of the ray path to the sun. Solid circles are 3C279 at 2695 MHz [Bourgois, 1969]; open circles are CTA 21 at 611 MHz [Harris and Hardebeck, 1969]; triangles are 3C138 at 178 MHz; and crosses are 3C287 at 81.5 MHz [Hewish and Symonds, 1969].*

from the smooth curves for $m\nu$ and $m\nu^{1.25}$ versus elongation, are 20 and 23, respectively, in arbitrary units.

We therefore conclude that the published scintillation indices at various frequencies do *not* force the conclusion that m is proportional to λ. Hence smooth, power law, density spectra are consistent with the published scintillation index measurements, in contrast to the conclusions published by *Hewish* [1971]. Hopefully, improved measurements will make it possible to resolve this question in the near future.

ACKNOWLEDGMENTS

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A. Hewish I just have one comment I should like to make before handing over. I knew we were going to have some discussion on this point and my only comment here is that I don't think you can stretch the data to find $\lambda^{1.25}$. Now, this at the moment we can regard as a matter of opinion and leave it there. But you did, I think, dismiss somewhat quickly the evidence I brought forward on the power spectrum. There is a great deal of evidence on the wavelength dependence of the power spectrum, and I find no evidence that the scale size as we measured it is a function of the observing wavelength. This would certainly be true in the case of a spectrum such as you suggest.

J. R. Jokipii I will say that I was not aware of all the evidence that was available. The main point that had been made prior to this meeting and in the literature was that one could use these two types of curves to make the decision. And I was just trying to point out that I at least did not want to make the decision.

B. Rickett Dr. Jokipii, on the curve you showed was that a straight line you draw with λ to the 1.25 or was it a smooth curve?

R. A. Jokipii A smooth curve. There is no particular reason to expect it to be any particular shape. If I could have gotten a figure 8 through the points I would have regarded that as just as good.

COMMENTS

D. S. Intriligator I have been asked to review the space observation that everybody has been referring to this morning: the power spectra of the number density fluctuations of the protons in the solar wind. First, I would like to briefly discuss the motivation for our doing this. As you have heard this morning, from the interplanetary scintillation data one finds that the scale size for the interplanetary plasma is around 100 to 200 km, which is really quite small. From previous power spectra of the magnetic field done by *Coleman* [1966] and others a scale size was found of approximately 10^6 km. The difference between these magnetic field scale sizes and the ones inferred for the plasma from the interplanetary scintillation data is approximately four orders of magnitude. *Jokipii and Hollweg* [1970] suggested that direct spacecraft observations of the fluctuations of the proton number density of the solar wind plasma might yield scale sizes similar to those that had been found for the magnetic field. Those are the data we have. We have direct observations of the number density fluctuations in the solar wind and we find that we can set a limit—a lower limit—to the scale size of fluctuations for the solar wind plasma and that in fact it is at least 10^6 km. So it is different from the scale size inferred from the interplanetary scintillation data by approximately four orders of magnitude. I don't feel this is inconsistent with the interplanetary scintillation data because we are just measuring two different frequencies of the plasma. At this time our measurements, as you have heard several times already, cannot be directly connected with this interplanetary scintillation scale size. There are several possibilities: the 100 to 200 km could relate to the inner scale of the large-scale turbulence that we observed or it could be related to a different plasma regime. The data at this point do not necessarily distinguish between the two. Next I will review the data that we have. Some have been published [*Intriligator and Wolfe,* 1970].

Figure 1 shows nine power spectra that were obtained from Pioneer 6 solar wind data from the Ames Research Center plasma probe. Each of the spectra represents approximately one-half day's worth of data. We feel that the variation in power levels of these data sets is relevant and that it represents the lower frequency power fluctuations associated with the solar stream structure you heard about earlier during the week. The sectors and the high velocity streams are a few days wide and in general the time between our data sets is a few days. Since these data were taken, we have run many more power spectra, and we find the same conclusion, that the slope for the most part is the same, it approximately goes as $f^{-1.3\pm0.1}$, but that the level of power of the different curves does vary depending on the fluctuations that are going on in the plasma at that time.

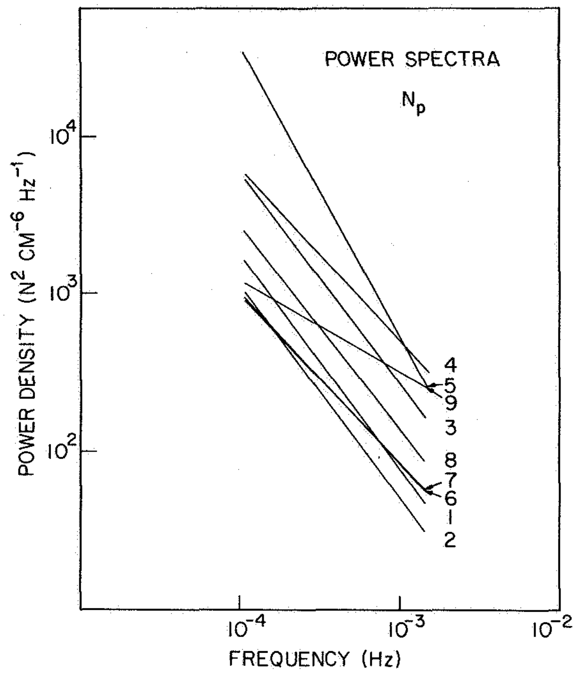

Figure 1. *Power spectra of N_p, the number density of protons in the solar wind, for each of the nine data sets listed in table 1.*

The way we get our limit to the scale size of the turbulence of the solar wind plasma is that the curve in figure 1 is rising at 10^{-4} Hz. A frequency of 10^{-4} Hz corresponds to a scale size of 10^6 km. Since this curve has not turned over yet–in other words, the two-point correlation function has not fallen to zero–there clearly is a lot of power here and that this is associated with the scale size of at least 10^6 km. The interplanetary scintillation data are off the figure to the far right and we are not looking at this regime.

Table 1 gives some of the specifics associated with each of the nine data sets in figure 1. It shows that the data were taken between December 21, 1965, and January 13, 1966. As noted, we have filled in consecutively between all of these; we also have extended the data for essentially 2-1/2 solar rotations from the launch of Pioneer 6 and the 2 months after the launch of Pioneer 7. Unlike the interplanetary scintillation data, our data are not constrained primarily by telephone lines and noise but rather by tracking gaps. The only criteria we used in selecting these data sets was that each set be of equal length (100 possible data points, about half a day) and that the number of data gaps during the interval be 20 or less. We have a data point every 7 min so each data set is $\sim$ 11.6 hr. There are two types of data gaps. The first is the interval when there was no tracking of the spacecraft. The other results from the "aliasing" of the time series of the plasma data. That is, during the time (the 7-min interval) we are trying to measure the exact solar wind parameters (number density, temperature, and velocity) the plasma parameters are changing so fast that meaningful parameters such as number density cannot be obtained. In table 1 the the equivalent degrees of freedom reflects the amount of data gaps in the data and also the distribution of the gaps. This was just taken the standard way using the *Blackman and Tukey* [1959] method. The equivalent degrees of freedom is, of course, smaller than the degrees of freedom one would obtain if there were no data gaps. Column 4 of table 1 lists the slopes of each of the curves in figure 1. Calculating the mean slope from nine individual slopes listed yields a slope of $f^{-1.3\pm0.1}$ for the frequency range

Table 1. *Relevant parameters for each of the nine data sets used in this analysis. Number of the data set in column (1) refers to number of the corresponding curve plotted in figure 1. Column (2) is the date the observations were made by the Ames Research Center solar wind plasma probe on Pioneer 6. The "equivalent" number of degrees of freedom, listed in column (3), reflects the presence of the number of data gaps and their distribution within each data set [Blackman and Tukey, 1959]. Column (4) lists the slope for each of the individual data sets shown in figure 1. Columns (5) and (6) list the average number density of protons in the solar wind and the average bulk velocity, respectively.*

Data set	Observation date	Equivalent number of degrees of freedom	Slope	Average proton number density, protons cm^{-3}	Average streaming velocity, km sec^{-1}
1	Dec. 21, 1965	15.6	-1.4	6.7	340
2	Dec. 22-23, 1965	12.9	-1.3	4.3	421
3	Dec. 24-25, 1965	17.6	-1.3	7.3	435
4	Jan. 3-4, 1966	10.2	-1.1	6.5	378
5	Jan. 6-7, 1966	10.9	-1.8	10.7	337
6	Jan. 8-9, 1966	9.6	-1.1	6.3	456
7	Jan. 10-11, 1966	11.6	-1.1	3.8	409
8	Jan. 12, 1966	11.6	-1.3	7.1	345
9	Jan. 12-13, 1966	10.2	-0.6	10.0	335

shown. As mentioned, more recently we have studied a number of other data sets and found similar results.

Figure 2 is the same spectrum that Jokipii just showed, and it indicates the mean slope of $f^{-1.3\pm0.1}$ obtained from the nine data sets where the points are the averaged power of the individual data points at each of the difference frequencies. Since we consider the changes in the level of the power between the nine spectra to be real, reflecting the lower frequency variations in the solar wind stream structure (the high velocity streams, etc.), it would be wrong to take a slope that would average these out. Therefore, the line calculated in this curve is the mean of the slopes of the individual curves. In other words, this reflects the fact that the slopes for most of the data sets are similar but the levels of power are different at these frequencies. Since the slope is still rising at 10^{-4} Hz this is evidence that the scale size of turbulence for the solar wind protons is at least 10^6 km. This is similar to the scale size obtained from the magnetic field measurements but differs

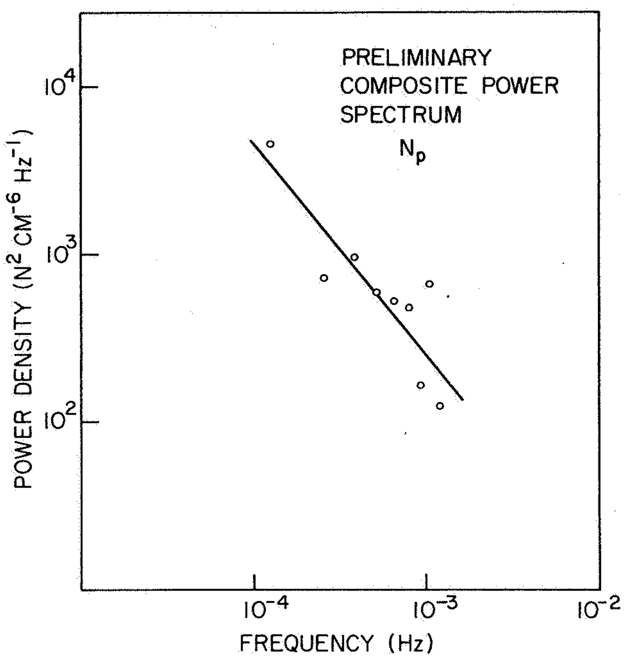

Figure 2. *Preliminary composite power spectrum of N_p for December 1965 and January 1966. The individual data points represent the average value [Blackman and Tukey, 1959; Coleman, 1966] of the number density fluctuations in the solar wind at each of the frequencies shown and are based on the nine data sets in figure 1. The curve was obtained by calculating the mean slope from the slopes of the nine data sets.*

from that previously inferred from the interplanetary scintillation measurements by four orders of magnitude.

Recently we have performed many other power spectral analyses. We have continued to run the number density spectra, and we have also looked at other quantities. For example, we have calculated the power spectra of the different components of the solar wind velocity (V_r, V_θ, and V_ϕ). These are also the first power spectra of this type that have ever been run since previously all of the power spectra have been for the solar wind speed not velocity. That is, they assume that the velocity is completely radial. The Ames Research Center plasma probe can measure the three components of the velocity. We have used this data to obtain power spectra for these different quantities. This has been done for a number of data intervals; figures 3 and 4 show the results for two of the data sets.

In general, the slope of the curves V_r, V_θ, and V_ϕ in figure 3 are quite similar to the slope we found for the power spectrum of the number density. We find that just as the power spectrum of the number density in the frequency range ~10^{-4} Hz to 10^{-3} Hz varies as $f^{-1.3}$ the spectra of V_r, V_θ, V_ϕ vary as $f^{-1.0}$.

Figure 4 is from December 26 and 27, 1965, and it shows the same quantities as figure 3. The slope of the number density and the power of number density versus frequency, as well as for V_r, V_θ, and V_ϕ. As in figure 3, the slopes of the spectrum for the three velocity components are quite similar. This is what I noted yesterday in reference to Burlaga's paper (p. 309); we have looked at 30 half-day intervals for the velocity, and in these data there are no systematic differences between the curves of V_r, V_θ, and V_ϕ: they all generally fall together; sometimes the V_θ curve lies a little above the V_ϕ, as in the previous figure, or it's vice versa. It doesn't seem to matter.

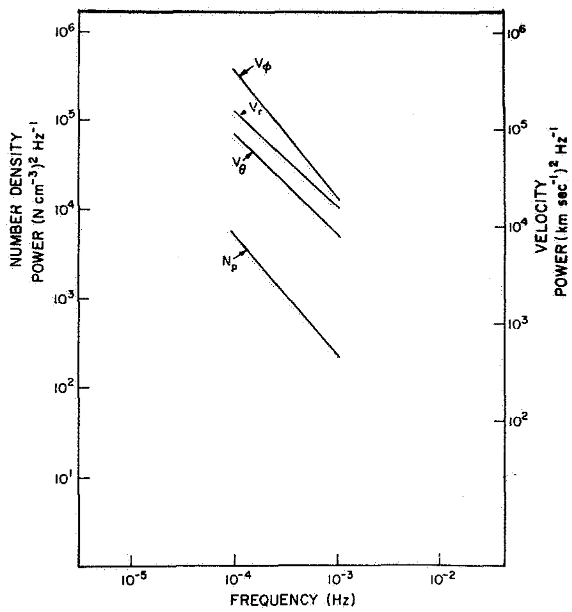

Figure 3. *Power spectra from December 24-25, 1965 (data set 3 in figure 1) for the number density and the three components of solar wind velocity V_r (the radial component), V_θ (perpendicular to the ecliptic plane), and V_ϕ (in the ecliptic plane). The left ordinate refers to the power associated with the number density spectrum. The right ordinate refers to the power spectra of the components of solar wind velocity.*

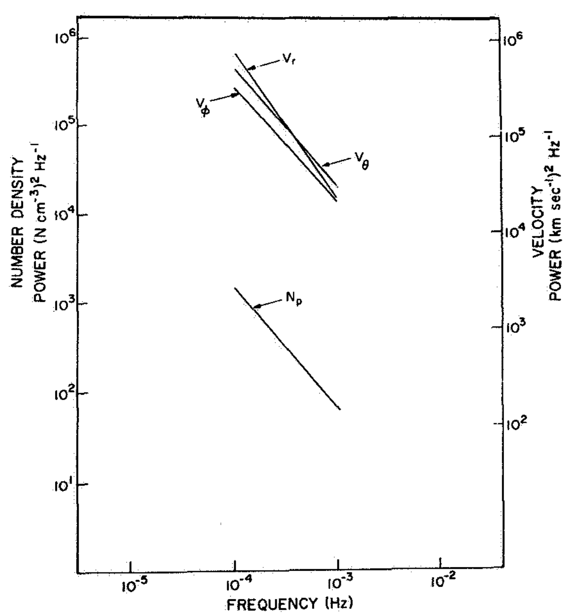

Figure 4. *Power spectra from December 26-27, 1965, for the number density and the three components of solar wind velocity. The ordinates are the same as those in figure 3, and the data are also from an 11.6-hr time interval.*

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DISCUSSION

A. J. Hundhausen I think if one looks at observed plasma properties as a function of time one indeed sees a good bit of wiggling up and down in anything one measures. Some of this may in fact be due to real fluctuations in the solar wind. And I think if you look at much data, you decide some of it might be due to instrumental problems and, in fact, I would state the opinion that when one talks about density one is on rather precarious grounds. I vaguely recall a comment by somebody from Ames in the panel Tuesday afternoon that such problems as how one fits one's data often show up most clearly in the density. Now, when one goes ahead and takes power spectra for the time series of observations I think one should give consideration to this other possible source of fluctuations. I would like to ask if you have given hard analysis to the possibility that all these fluctuations may in fact be due to problems in time, problems in accuracy in recording your spectra, or curve-fitting problems.

D. S. Intriligator The two reasons we have gaps is either from data gaps due to tracking or from data we did feel was aliased. The data that I looked at are detailed least-squares iterations of the fit to the plasma parameters using our calibration function. If there was any doubt as to how good the fit was that point was deleted.

N. F. Ness I have a general comment about the presentation of power spectra results. The point is that in the computations of the results you presented the implication is that it is a continuous function of frequency, barring the fact that your computer processing extends it by an order of magnitude over the real Nyquist frequency for the data. In fact, of course, one is making a spectral estimate over a finite frequency interval, and one should be presenting experimental results in spectral estimates more in the nature of a histogram. And associated with that would be appropriately presented not just degrees of freedom, which is a little bit difficult to convert to the appropriate scaling, but something like the 95 percent confidence limits in the spectral estimates. This would then permit one to at least judge the statistical significance of the data at hand. This is a general comment for you and for other people who present power spectra, but without this concept of what you are really computing. It's a discrete set of numbers; it's not a continuum. And leaving out the error bars, you know, when you present averages as has been done also here, is a little bit misleading as to what the data set really means.

The question I have is motivated by a presentation given yesterday by Chris Russell on the possible effects of aliasing of power spectra depending on the spectral slope. Now, for spectral slopes of about minus one and with folding factors of something much less than I think you have present in the data, your sampling rate gives you a Nyquist frequency of about 0.001 Hz. That is about three orders of magnitude removed from the kind of frequency I think is relevant to the scintillation measurements. So I don't believe the slopes you are deriving are correct, and I suspect the levels you are quoting are not correct either.

D. S. Intriligator Well, I would like to add one more dimension to the dimensions that you put in here about discrete points. That is very true, but as I tried to point out, we feel that you can wash out a very important effect, which is the change as a function of time of the levels of power. If we were to take a histogram of the points, disregarding the time intervals we were taking, it would be like comparing apples and bananas. It's important to keep in mind that the plasma parameters and the magnetic field parameters do reflect lower frequency solar stream structures, and that you have to understand exactly what it is that you're averaging for. Now, it's true, it's clearly true, that we are separated from the interplanetary scintillation data by three orders of magnitude. On the other hand, as Parker and Jokipii and some others have so often told us, one of the interesting problems in astrophysics is the scale size of turbulence, and it does affect many problems, and that is specifically the motivation for this work that we have done. We are looking at the astrophysical implications of space physics data and the scale size of turbulence in the interplanetary medium, and we find that we can set a lower limit to it of at least 10^6 km.

COMMENTS

J. V. Hollweg Dr. Intriligator has just presented evidence that the power spectra for density fluctuations in the solar wind tend to resemble the power spectra obtained for magnetic field fluctuations. Since it is now known that Alfvén waves play a fairly important role in the fluctuations in the solar wind, it is interesting to inquire whether Alfvén waves can have density fluctuations associated with them. This is a point that I think quite a few people have some small misconceptions about. I just want to point out that it is fairly reasonable. The top line in figure 1 is an equation for the component of velocity parallel to the direction of propagation of an Alfvén wave and also parallel to the average magnetic field. The only point is that if you take a linearly polarized Alfvén wave

$$\vec{k} \parallel \vec{B}_o: \quad \rho\left(\frac{\partial v_z}{\partial t} + v_z \frac{\partial v_z}{\partial z}\right) = -\frac{\partial}{\partial z}\left(\delta p + \frac{\delta B_x^2}{8\pi}\right)$$

$$\text{LET } \delta B_x = \delta B_x^{(o)} \cos k(z - v_A t)$$

$$\text{THEN } \delta\rho = \delta\rho^{(o)} \cos 2k(z - v_A t)$$

$$\frac{\delta\rho^{(o)}}{\rho^{(o)}} = \frac{(\delta B_x^{(o)}/2B_o)^2}{1 - v_S^2/v_A^2}$$

$$\text{AT } 1 \text{ a.u.}: \quad (\delta B_x^{(o)}/2B_o)^2 \sim 12.5\%$$

Figure 1. *Alfvén waves and density fluctuations.* v_A *is the Alfvén speed and* v_S *the sound speed.*

with the fluctuating component of magnetic field in one direction, the x direction, say, then there's a fluctuation in the pressure associated with the magnetic field, and this term will tend to drive compressional oscillations. You just put in some other equations, fool around a bit, and it turns out if you put in, say, a cosine wave for the Alfvén wave then you find that the density fluctuations are also a cosine, but at twice the frequency. The amplitude of the density fluctuations normalized with respect to the average density depends on the magnitude of the Alfvén wave squared, and then there is an interesting resonant denominator which indicates that what one is really doing is driving sound waves or ion sound waves and that when the two-phase speeds become equal there is a resonance and really a coupling between the waves. The numerator represents in a sense the average or the magnitude of the density fluctuations. If you look at the space-probe data at 1 AU, and if you take naively one of the components you get about 12.5 percent. Thus one can have, if you have a linearly polarized Alfvén wave, a fairly substantial fluctuation in the density associated with it. The question is, of course, whether the waves are at times linearly polarized or, as perhaps might be more often the case, circularly polarized, or something similar.

Figure 2 illustrates this normalized density fluctuation, and it shows that there is a resonant peak and near the earth you can get a rather large contribution.

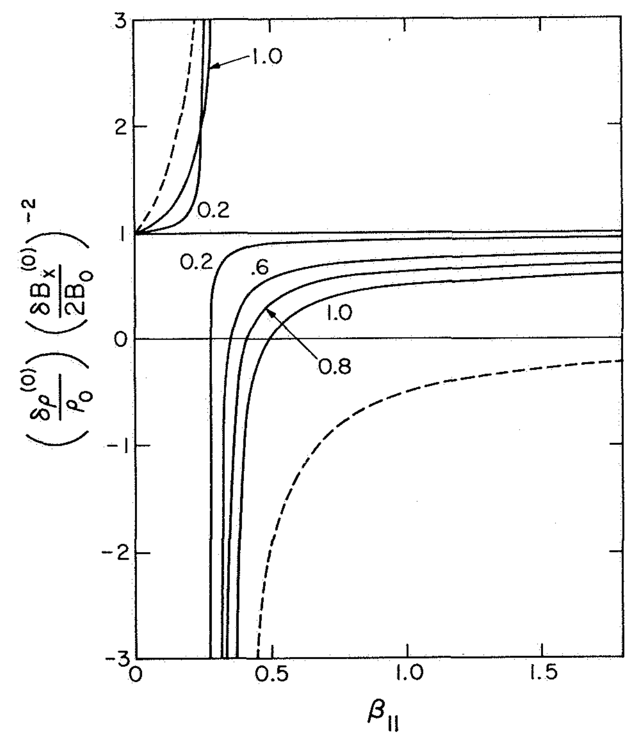

Figure 2. *Normalized density fluctuation versus* $\beta_{\|} = 4\pi n_0 K T_{\|}/B_0^2$. *The dashed lines are for an adiabatic equation of state with* $\gamma = 3$; *solid lines are for the double adiabatic (CGL) equation of state.*

DISCUSSION *B. Rickett* Do the density fluctuations driven from these Alfvén waves depend on the wavelength of the Alfvén waves?

J. V. Hollweg No. As long as you're really talking about Alfvén waves for which the phase speed is not dependent on the wavelength, then no, it doesn't.

A. Barnes As we discussed yesterday, I'm not quite sure whether the effect that you find should be called mode coupling or not, but whether it is or not first let me ask you, as I understand it, this is strictly an MHD theory, is that right?

J. V. Hollweg Right.

A. Barnes Now, when you do these nonlinear calculations, sometimes the effect of Landau damping can significantly modify the results that you get. This is certainly true for mode coupling. Do you have any comments on what the effects might be? Because you're generating acoustic waves which, of course, in general are pretty rapidly Landau damped.

J. V. Hollweg There is a little funny problem of words here. Yesterday I used the words "mode coupling" and that is really not strictly correct. What you wind up with is on the left-hand side of an equation you have the wave equation for sound waves. On the right-hand side you have a driving term that is due to the Alfvén wave. This driving term is going with the Alfvén speed, so what you generate is something that looks like a sound wave but it's going at the Alfvén speed. So one really isn't generating an ion sound wave bacause that would go at the ion sound speed; instead one has a sort of a driven ion sound wave going at the Alfvén speed. There will be Landau damping, but this will be smaller than one would expect for the ion sound wave as long as the Alfvén speed is larger than the ion sound speed. So that the resonance that you get is out on the tail of the distribution function, or farther out on the tail of the distribution function than you would expect for really an ion sound wave. So near the earth this might be important, but farther in the Landau damping will be smaller.

A. Barnes Yes, what you say is true, the damping will probably be smaller than for a real ion sound wave. But if beta is of the order one-half or so it can still be a pretty strong effect, because the Alfvén speed is not all that far out on the tail of the distribution.

J. V. Hollweg I haven't calculated yet how fast the Landau damping would go for these things. If it did get fast near the earth it wouldn't be all that bad. It would sort of heat up plasma a bit, and damp out the Alfvén waves and that wouldn't bother me. I don't think it would particularly affect the density fluctuations because it would still go into the sound wave.