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DISCUSSION FOLLOWING THE INTRODUCTORY TALK BY JORDAN

Skumanich — I would like to ask a question about the zeroth order atmosphere for which you are doing the calculation of this heating. Do you start with the models that we radiative transfer types give you?

Jordan — Yes. The calculations in my talk were done for a number of models including a current version of the Harvard-Smithsonian Reference

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Atmosphere, which is, I think, the best current model. In general, one sees that the results for the heating are almost model independent, as the crucial parameter, the scale height, is not strongly model dependent.

Skumanich — Keeping in mind that these are average models, where do we look to better understand heating in the light of these theories, in the network or in the cells?

Jordan — We look in the cells. What is really interesting, however, is the fact that when we calculate mechanical dissipation rates with the weak shock theory in the low chromosphere, using these average models, and compare the results with computed values for the net radiative loss due to H", or even make some rough approximation to the blanketing by using the Athay-Skumanich blanketing functions, we get surprisingly good agreement. I think that this is good evidence that the weak-shock theory is a good first approximation theory for the heating in the low chromosphere.

Beckers — In connection with the observation, several years ago, I took observations in the K and H lines on the disk with a time resolution of 5 sec and a spatial resolution of one or two arc sec. I never saw any periodic phenomena—varying with periods less than 100 seconds.

Ulrich — In your relation between pressure and velocity, did you include the effect of radiativs dissipation?

Jordan — No.

Ulrich — As I shall discuss later, this could be important.

Stein — Would you really expect to see waves of such high frequency, since the spectral lines you used to study the oscillations are formed over a certain atmospheric depth? The velocity profile goes from maximum to minimum over a period in a nearly linear way. or the variation is slow, whereas the pressure goes from maximum to minimum rather steeply. The question is, then: Is the perturbed atmospheric region small compared to the region over which the line is formed? Can the oscillation even be detected?

Athay — I've computed the width of the contribution function in the chromosphere for the K line for a region making about equal contribution to the intensity. It comes out to 300400 km. This is the same order as the wave length you are talking about.

Skumanich — But the velocity field is going to be weighted most heavily by the emission at the head of the wave, so it's not a simple question.

Jordan — And you must keep in mind that high time resolution is necessary if there is to be any hope at all, as the time of a single

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observation must be short compared to a wave period or we won't see any periodic variations. Both high time resolution and a careful analysis will be necessary to settle this question.

Sheely - I'd just like to point out that we do have data to answer some of these questions. Time resolutions of 5, 10, 15 sec and high spatial resolution in a great number of lines: Ha, the K line, etc.

Thomas — I'd like you to clarify once again what region of the atmosphere most of your remarks pertain to?

Jordan - The cell. The non-magnetic chromosphere above the supergranulation element. Not the sunspot. Not the plage. Not the spicule.

Thomas — Why do you restrict yourself to this region?

Jordan — Because there is where we think the bulk of the chromospheric gas is located. The good correspondence between calculated dissipation and net radiative losses as a function of height throughout this low chromospheric region suggests that these simple, one-dimensional models which ignore the magnetic fields may not be too bad. Thus, though we admit, or at least I do, that we can't do the heating calculation in the presence of magnetic fields yet, this may not be too serious for the solar chromosphere.

Skumanich — In doing this you're avoiding the coronal heating problem.

Jordan — More. You're avoiding the role of the transition region, which could produce a large conductive flux down. This could be serious.

Skumanich — I'm worried about the fact that, in the results you showed, the shock strength parameter becomes uncomfortably close to unity. I recall the value 1/3.

Jordan — But 1/3 is not uncomfortably close to one in this theory. First, the coefficients of the higher order terms are very small. More reassuring, laboratory experiments show that the theory is very accurate in this Mach number range.

Frisch — Why is the knowledge of the temperature structure not sufficient to determine the conductive flux?

Jordan — If we write down the usual expression used to compute conductive flux, where this flux varies as the 5/2 power of the temperature, you might think that all we had to do was to differentiate this expression to get the heating; but that's not necessarily true. We don't know the value of the coefficient, which depends upon, among other things, the magnitude and direction of the magnetic fields.

Skumanich — But we know about these fields in the network.

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Jordan - But that's not the region we're talking about. What about strong, as yet unobserved, horizontal magnetic fields over the cells, above where the weak shock heating occurs, yet in the transition region of strong conductive flux? This is a real possibility.

Jefferies — You went over coronal heating rather swiftly. What seems to be the essence of the problem?

Jordan — Among other things, I don't think we really know what wave modes exist in the corona. There may be some who would take issue with that statement, but if you accept it, then you can see that it would be rather meaningless to estimate the heating theoretically. Estimates based on observations have been offered, of course, equating necessary heating to net radiative losses, conductive losses down, and convective losses out.

Stein — I think the problem goes deeper than that. I believe that in the near future we'll be able to say what wave modes exist in the corona. But there is the further problem that the total amount of energy needed to heat the corona is small compared to the total energy in the waves when they are generated lower down. When you consider the errors inherent in estimating the energy generated, the dissipation lower down, and the energy in waves produced by wave-wave interactions, you find that these errors are of the same order as the amount you need to heat the corona.

Thomas — Are you saying that most of the energy of these waves is lost before they reach the corona? I'm not sure of the picture.

Stein — All I'm saying is that estimates of the amount of energy needed to balance coronal radiative losses, conductive flux, and the solar wind are small compared to the amount originally generated. We can estimate the amount of energy in the 300 sec oscillations, for example, and then when we consider the errors in this estimate, they might be of the same order as the amount of heat needed in the corona.

Skumanich — **I** think you fluid mechanics people are avoiding the question of reproducing the dissipation that can be inferred from the temperatures which we spectroscopists derive for the corona and the transition region. The real problem is that you are unable, with your theories, to predict the observed flux divergences high enough in the atmosphere that are inferred from spectroscopically determined temperature distributions.

Schwartz — But you're talking about the difference between two very large numbers, and this difference can be very small.

Thomas — If I understand the picture correctly, we have not one, but two competing mechanisms operating here in the low corona just above

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the transition region. In addition to the conductive flux down, we have also the convection outward, both in that region where mechanical heating due to some mechanism is taking place. This is a more complex picture than the one you're talking about Andy.

Skumanich — That's right.

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