THE NEXT DECADE IN STELLAR ATMOSPHERES THEORY

P. DELACHE
Observatoire de Nice, Nice, France

It is a pity for all of us that Dimitri Mihalas was unable to come to Sydney, and I wish, in the first place, to thank him for suggesting this discussion, devoted to a prospective study of our research field. There falls to me the task of trying to sketch what might be the axis of progress in stellar atmosphere theory. This subject is pre-eminently one which belongs to D. Mihalas; I am afraid that we are going to miss him now more than ever. Furthermore, I feel myself in a bad position for talking on such a large subject, in front of specialists, while I know so little on it. However, I have accepted the task, at the request of the President of Commission 36, with the goal of giving some physical considerations that may initiate reflections and discussions.

I am not going, in principle, to give a set of topics whose study should be desirable, or possible, arranged according to the internal logic of a specialist of stellar atmospheres, neither shall I look for bibliographical signs allowing the prediction of active zones or flares in our next cycle of activity!

My physical reflections will be based first on a small historical background. This will lead me to consider in the beginning the classical aspect, somewhat restricted, of the theory of stellar atmospheres. We shall find there some well known needs for classical, 'first order', extensions of our competences. In a second part, I shall try to encircle what could be the further extensions of our field, through the two questions:

(i) If it is possible to produce a broad definition of the objects and phenomena that we are to study, what are the competences that we should have, or develop, in order to succeed?

(ii) If we define ourselves by our knowledge, in a wide sense, what are the new problems that we expect to be faced with?

1. Restricted Stellar Atmosphere Theory

The first steps in the understanding of radiation coming from the stars have been possible only after the appearance of radiation physics, that is to say, atomic spectroscopy for the atomic point of view, and thermodynamics of the radiation field and statistical physics for the description of interaction. The synthesis of all that for astrophysical purposes is classically known as radiative transfer theory. It is *uncoupled* from any fluid dynamics, but sometimes it is used in conjunction with the hydrostatic law, and also with some kinematical parameters (the so-called 'velocity field') in order to produce model atmospheres.

Let me make some remarks on radiative transfer theory and on its prospects. I consider that radiative transfer has had bad luck in large portions of its history, and that this fate is still hanging on it. After the two fundamental concepts of source

Contopoulos (ed.), Highlights of Astronomy, 109–120. *All Rights Reserved*

function and optical depth had been worked out, it appeared immediately a very interesting, one-dimensional mathematical problem. First drama, since the physics disappears, swamped by formal flows for years. The LTE vs non-LTE problems could have been posed a long time ago, if the mathematician who is slumbering in every theoretician did not wake up so often! Second drama: the advent of departures from LTE comes just before the advent of computers. The fact that there is no new physics, but simply the recognition that collisions may not dominate the radiative processes and have to be calculated in a self-consistent way, this fact is screened by the quasi-hermetic presentation of the literature. It happened then that, in order to be convinced, most people needed to see good, fully treated examples. But this means going through heavy numerical calculations with a high degree of sophistication. We observe then a tendency to trust FORTRAN more than 'Physics of the Solar Chromosphere', with the result that departure from LTE will be synonymous with machinery open to a few specialists, and too complicated to be operated by oneself. If this difficulty is to remain in the near future, we must be careful to avoid the three following traps:

(i) continue to improve the quality of observations and data reductions, and give them to modelists who do not ask for them.

(ii) take a set of ready-made models and try to fit pieces of data without questioning the legitimacy of the procedure.

(iii) in some cases be satisfied with the internal consistency of LTE calculations without performing the actual non-LTE analysis. Please excuse me if I find it necessary to develop this point which is a question of methodology that arises in other frames, especially in the case of velocity fields in the outer regions of an atmosphere.

You start with a physical description of your medium which includes all the basic phenomena that you want to take into account. Let me take an illustrative simple example in which you assume that in a plane parallel, or spherical geometry the following relations hold:

- statistical equilibrium,
- transfer equation – energy equation,
- momentum equation,
- mass conservation equation.

As you find the problem to be too hard, you make some simplifying hypothesis, e.g. collisions are always dominant (LTE), or velocity $=0$ (static atmosphere). *But those hypotheses cannot be considered as approximations,* since they suppress at the same time one unknown and one equation (this is readily seen on the static hypothesis example in which the mass conservation equation reduces to $0=0$). Then one arrives at a well posed problem, as far as the initial one was well posed. It is then a cheap satisfaction to verify the internal consistency of one's results. Let me be even more specific on the expansion example: the fact that you are not free to choose $v=0$ is illustrated by the well known result in stellar wind theories that as soon as the static constraint is relaxed, you find the value of v through the so-called criticality condition *which is not an extra parameter with which you may play.* Some interesting problems may arise if we cease to think in terms of non-expanding atmospheres. Let me propose one by the way at the end of this digression.

Suppose that we know that for some reason there is a temperature rise in the outer part of an atmosphere, as is the case, for example, in the Sun. Suppose also that the variation of the temperature is such that it sweeps the region of thermal instability of the hydrogen plasma. In a static regime, one finds immediately the necessary existence of a plateau, followed by a steep rise, as was suggested a long time ago in the solar case. The temperature gradient will then be limited by such things as thermal conduction, and you arrive then at a model of your transition region. But sometimes you go further, and try to study the stability of structures (essentially horizontal), with the hope of finding the source of chromospheric inhomogeneities. My point is the following: if there are instabilities which can develop horizontally and/or if there exists a tenuous region, controlled by conduction, but still thermally unstable, is it not a sign that the local conditions impose a motion, and should we not, in the first place, study the equilibrium situation? We have the greatest chances to find a steady expanding state, in which the instability develops itself smoothly along the outward motion. Only then should we study the behaviour of horizontal perturbations. Put in another way: the existence of an outward temperature rise gives us the necessary ingredients for a thermodynamic machine without violating the second principle. There is no proof that the machine will start off by itself: there is no proof either that it will produce the simplest type of motion, namely steady radial expansion; but maybe we should start sophisticated theories on better grounds, which do not assert a static atmosphere from the beginning, even if it looks satisfactory, and selfconsistent.

Going back to my topic on radiative transfer, let me finish the diagnostic by mentioning the two major extra ingredients that are not at all satisfactory in my opinion: what is called turbulence (either micro-, or macro-), and inhomogeneities. I call them extra ingredients, because they find their physical origin outside the scope of classical stellar atmosphere theory; their description is purely phenomenological, and their role is in most cases reduced to a convenient adjustment of 'free' parameters, with which you can play, sometimes as a virtuoso. Let me be clear: I do not object at all to the use of an extra parameter in order to represent observations, that cannot be interpreted otherwise; I do not object either to the building of model atmosphere that includes formally a source of line broadening of unknown origin. What does not satisfy me, is the fact that our physical understanding of these extra pieces seems to end where it should begin: when they are given a christian name!

Replacing the microturbulence velocity by another local parameter such as the velocity gradient is not better, and in a sense even worse, since microturbulence can be easily incorporated in the procedures of reduction of the spectra, which is not the case for a velocity gradient. Furthermore, the question of the number of free parameters is somewhat misleading: it is true that the physical constraints such as mass conservation prevent the use of arbitrary variations in velocity gradients; but it is only due to our lack of understanding the physics that we think that we can play at leisure with variations of the microturbulence.

One sees in those examples that what I have called classical stellar atmosphere theory must be extended, and should include some hydrodynamics, which is already done by some people, but there are very few studies in which hydrodynamics and radiative transfer are really *coupled.* This subject will be considered in the second part of my talk. For the present time I shall try to bring up some partial conclusions on what can make progress in this restricted field.

(i) The classical calculation of one-dimensional non-LTE models that need big computers and good analysts has still many things to tell us, particularly on the abundances, but not only there. Let me quote a correspondence by D. Mihalas:
"I think that (this kind of calculations) should be pursued vigorously for a wide variety of atoms and ions in as wide a range of stellar temperatures and gravities as possible. ... it will pay enormous rewards both in re-evaluation of the whole 'abundance' question and in delimiting the regions where future work is urgently needed. I am certain that several platoons of graduate students would find this area to be a fruitful source of thesis problems. ... Milkey and Johnson have done some interesting work on the O I lines in 6500° giants and supergiants. They find that much (though apparently not all) of the luminosity effect arises from departures from LTE. ... the LTE abundance required to fit the NLTE computed equivalent width was up by a factor of 1000, or ... the equivalent effect would require 4 km s^{-1} microturbulence (all spurious) to simulate."

From the modelist's point of view, there is no basic conceptual difficulty in introducing the following improvements of the physics: transfer in the presence of a magnetic field and polarisation of the transfered radiation, departure from LTE in molecules, partial coherency of the diffusion. It is however difficult to progress in these directions for various reasons: the handling of the whole set of Stokes parameters is very heavy, and will be reserved to a very few people. Still, the polarization observations are improving, together with the recognition of the importance of magnetic stars, and of large scale magnetic structures on rotating stars. Concerning partial coherency, and molecular structure, we must turn to our usual companions working on atomic physics. It should not be difficult to convince them of the interest of those topics. If the chromosphere problem is really a basic one – as most of us are convinced – we must recognize that we do not understand in enough detail what we see in the H and K lines. Among other open questions, is the one of the scattering process. The discovery of the importance of molecules, essentially CN and CO, in the solar atmosphere as probes of the minimum temperature region and the advent of stellar ultra violet and infrared observations with good spectral resolution are signs of the predictable importance of molecular lines in the future. We are going to need detailed configurations and good radiative and collisional parameters. So we see that the future of classical radiative transfer is wide, worth working on, but *becoming more and more difficult.*

(ii) Some words can be said on the phenomenological approach of the velocity fields problems in connection with radiative transfer. First, if we are dealing with fields that are described in a purely kinematical and deterministic way, there are no conceptual difficulties, since we simply deal with a given variation of the absorption coefficient. However, original methods can be worked out for special cases, in order for example, to see what is the gross effect of a velocity gradient on the line shapes. The use of computers might not be necessary to give us new physical 'feelings' on this type of situation. On the contrary, if the velocity field is basically stochastic, it is most probable that the microturbulence velocity is not the right parameter. Radiative transfer in stochastic media is basically *a problem of physics of turbulence*, before being a problem of radiative transfer. It is likely that the concept of correlation length is a better tool than microturbulence velocity alone, as it can be a link between stochastic hydrodynamics and transfer. Sets of curves of growth in terms of correlations lengths have already been produced in LTE. As expected, they mimic the microturbulent result in the small correlation length limit, and the macroturbulent result in the large length limit. But there exist an infinity of intermediate states that are physically sound which shows that this approach can be of great interest. Since I have just mentioned the curve of growth, which has proved to be such a high value tool in LTE analysis, let me ask a simple question: does there exist an extension, even approximate, or empirically computed, of curves of growth theories to non-LTE diagnostic? (Athay and Skumanich have treated the case of coherent scattering where the source function can be computed exactly).

(iii) Another extension of radiative transfer theory has started to show up, and should develop rapidly: the treatment of geometries far from the plane parallel case. First in a spherical atmosphere, or shell, and second in wholly inhomogeneous structures. This latter problem is essential in the solar chromosphere, and most probably in stellar chromospheres too. To what extent are we making fundamentally serious errors in the diagnostic procedure using homogeneous models? The answer will be eventually given by a careful comparison of homogeneous models giving some kind of 'mean' information with the ultimate inhomogeneous model of the same object. At the present time, the Sun is the only star in which this study can be done, but obviously this remark shows why it is still important to work also on a 'mean solar chromosphere' even if the solar physicists do not share this opinion! Quoting again D. Mihalas: "In fact we may validate (or invalidate) almost all of stellar spectroscopy by knowing the answer to such questions."

It is in place to mention here the extension of radiative transfer in the time dependent case. It can be shown that a quasi-static description of evolving structures can be erroneous in some cases, for example in chromospheric spicules, or in large prestellar contracting clouds. The available literature on neutron transfer can help to understand this kind of problems.

(iv) Less predictable, but highly desirable, is the advent of new useful physical concepts, which will simplify, if not the exact calculations, at least the understanding and permit approximate approach of non-LTE transfer. I refer to the various probabilistic treatments that appeared recently, and also to such things as R. N. Thomas's 'temperature control bracket'. If we could have some new tools that are *at the same time physically meaningful, and simple to operate*, so that a 'do-it-yourself' kit for non-LTE radiative transfer can be put on the market, much progress could be made in what I call the extended theory of stellar atmosphere, in which radiative transfer is an ingredient among others. Of course, we would be satisfied by a kit which does not give exact results, but only a sufficient degree of approximation.

2. Extended Stellar Atmosphere Theory

In the first part of this talk, I limited myself to the classical, restricted stellar atmosphere theory. We were faced with the necessity of including some velocity field theory (or theories). I do not share completely the opinion of the President of Commision 36 about the fundamental need of mass flows, and I shall come back to this point in a few moments, but I salute his unifying work which has permitted clarification and sorting of an impressive bibliography along directions that are very strongly defended. His Report might be arguable, but its content is of a very convenient use, and I am sure all of us are grateful to him for this piece of work.

But why should we limit ourselves to the inclusion of hydrodynamics textbooks in our bedside books? I would like to ask a more general question on the personal status of a specialist in Stellar Atmosphere Theory, who is faced at the same time with astronomical observations and facts, and with a whole batch of physical knowledge. Since the radiation that we receive from the stars, and from interstellar matter gives us information not only on the parameters of classical thermodynamics (temperature, density, chemical composition), but also on data whose physical origin lies elsewhere (gravities, velocity fields, magnetic fields), it is impossible not to wish to incorporate as much of this external physics as possible. We arrive then at the two possible approaches that I outlined in my introduction.

(1) If it is possible to produce a wide definition of the objects and phenomena that we are to study, what are the fields of General Physics that we will be using?

It is a very ambitious project to look for a physical unity, for a continuity, among all the objects, all the zones in which our extended ability should apply. This attempt has been recently made by R. N. Thomas, in his IAU Report, and elsewhere.

In brief, let me summarize what are the leading ideas, as I understand them: we must recognize the following facts: we do observe mass motions, mass exchanges, either directly in the outer layers where for example organized velocities can be seen through line shifts, or less directly in inner layers where, as we have seen previously, we are forced to include kinematical descriptions of the velocity field in modeling the atmospheres of stars. Furthermore in many cases, the theories which could explain the observations of mass flows are lacking. So, to put it simply, we are forced to consider the mass motions as well as the photon motions; i.e. as well as radiative transport. Now the question is: is the analogy meaningful, and are those two types of flux of the same basic importance, in other words:

(i) is this unification well founded, or is it not conveying with it some dangerous simplifications?

(ii) is it useful for the progress of our knowledge?

On the second point, my impression is that, yes, such a tentative approach is very useful, since it makes us think in new terms and in a critical way about physical situations we are familiar with. As a result, we may deepen our physical understanding of interrelations between various parts of a stellar atmosphere, and perhaps discover new theoretical features (as the chromosphere-corona transition zone was theoretically discovered, and the solar wind theoretically understood). As we say in French: 'de la discussion jaillit la lumière'. I am convinced that there will be discussions, but I shall not attempt to predict where the flashes will appear!!

On the first question, namely whether the idea that the mass flow concept plays a role analogous to radiative energy flow is well-founded, I have some comments which, hopefully, may help to define our status vs the General Physics.

Let me state first that I do not think that mass flow is of such a fundamental character as radiative energy flow, and try to explain why: we have a star which is a hot body surrounded by a cool vacuum. This body has at least its internal energy content to release, but it also has generally some energy source (nuclear reactions, contraction in a quasi static description, etc., ...). There is no screen between the star and the vacuum that can prevent radiative energy exchanges, so an irreversible flow sets up, increasing the entropy of the whole system. On the contrary, mass is bounded by the gravitational field, and is not flowing simply because there is a decreasing density outwards. One might argue that, even from a solid surface, evaporation takes place. But this is essentially a microscopic effect, due to the fact that in the microscopic velocity distribution function, there is a certain number of particles running faster than the escape velocity. It simply shows that the gravitational screen is not perfect, but it is in no way comparable to the first order effect of radiative energy flow. (I call it first order because it can be handled with the classical thermodynamics tools.)

Now comes a question: is it possible that a mass flow, even if it is not a direct consequence of the simplified boundary conditions that I have described, always sets up? I think that the answer is in general unknown: as I mentioned in the last paragraph, we have, all the way through our irreversible system, the necessary condition for a thermodynamic machine to work, namely contacts (through radiation) between two sources of different temperatures. Depending upon the type of machine, it may work or not. For example, if you think of convection, then you compare the temperature gradient to the so-called adiabatic gradient, and you work out the details. There is no proof that in general we are going to generate mass motions, and furthermore that these mass motions will induce a flow. This can be seen in the solar example where the convection zone generates non thermal energy, which travels through the photosphere and dissipates somewhere in the chromosphere and corona. It is then this hot base of the corona which, connected to the vacuum interstellar spaces, is at the origin of the solar wind. We see that the Sun's mass flow is the result of two thermodynamic machines, working in two different ways. Another type of argument could be put in favor of mass flows: the outgoing radiation transports with itself a certain amount of momentum; most probably, the non-radiative energy generated in convection zones is also accompanied by some momentum. When those flowing energies interact with matter, they transfer part of their momentum. This is called radiation pressure in the case of electromagnetic or acoustic radiation. One should not draw the conclusion that this transfer of momentum will cause a flow of matter. Again, it is counterbalanced by the gravitational attraction; the complete answer comes only from the study of the whole steady state situation.

To summarize, let me sketch again how we differ in understanding how the stellar machinery is working:

First, we start with an adiabatic star in LTE, in which the temperature and density drop to zero at the surface. The LTE assumption works as a screen for escaping radiation, and the gravity is the screen against outflow. Apparently at the surface our screens are not perfect exactly in the same manner: within a mean free path, for the photons as well as for the atoms. But the radiative screen is wide open, at the surface (half of the photons escape) whereas the gravitational leak is small.

So our star radiates, and slightly evaporates. Of course there will be a point, far away, where most of the particles will be evaporating, as in a planetary atmosphere. Then, things can stay like that, or the modifications induced by the radiation field in the temperature structure may create motions with some more or less high value of internal kinetic energy storage in hydrodynamics modes. As a consequence, the amount of energy eventually transported and dissipated in the atmosphere may lead to important expansion bringing the point of sensible mass motions towards the photospheric layers; or the basic hydrodynamic modes may affect directly the photosphere.

One sees that I have tried to distinguish effects from causes. If it happens that an effect can react upon the cause, then indeed we shall need a self-consistent treatment, and we may not have the right to disentangle what creates what. We arrive then at a picture of a Thomas-type. I simply do not think that it is the rule; and I would have prefered the use of the words ‘hydrodynamic forms of energy-storage and flow’ to the somewhat misleading term ‘mass flux’ or ‘mass flow’.

I apologize for having spent some time on this picture; but as I have said before, this is a kind of reflection which help to understand better the underlying ideas that are concealed in one’s mind.

Several conclusions can now be drawn:

- Mass flow, as a general expansion of the atmosphere, or mass motions, that is matter moving along some kind of pattern, may exist as consequences of the irreversible radiative energy flow. That they always exist remains to be proved (or observed).
- The existence of mass exchange between two different places in the atmosphere is not a simple question, and its solution may necessitate the solution of the steady state of the whole atmosphere.
- It remains that we should be careful to consider, especially in the outer parts of an atmosphere, the possibility of ‘material links’, that can convey with them kinematical energy, enthalpy flux, abundances peculiarities, patterns, etc., ...

It will be a good thing, if we cease to think in terms of 'zones' more or less arbitrarily defined, connected through 'interfaces' that only the radiation and some acoustical or MHD waves can cross. Moreover, not only can the interfaces be crossed by matter, but they are not necessarily defining boundary conditions, as if one was going always from the causes to the effects in the way out from the center of the star.

I have not given a general answer to my initial question, but I arrive at the conclusion that by trying a unifying description of Stellar Atmosphere Theory, we recognize the necessity of coupling the restricted theory with hydrodynamics, MHD, etc.,..., we recognize the interest of looking at problems with new eyes, we recognize the importance of thermodynamical arguments, and that thermodynamics of irreversible processes should be included in our tools.

But, at the same time, I have personally the impression that I risk becoming dogmatic. If every problem that we are going to encounter in the future were to fall in a large frame or in another that we define today, I am pessimistic; because it nearly means that we will become technicians operating a large factory, with various skills enabling one or the other to treat such and such a topic, leading to a construction, complicated, but with no failure in it, with a final point in perspective.

The second question, even if it is less ambitious at first sight, is fortunately more optimistic. May I add also that I have found it to be more convenient to classify some points of future work?

(2) Let us try to define our physical competences, and imagine what will be the progresses that we are going to make in using them.

One can say that we are specialized in physics of low density matter: our gases are perfect, first order development of statistical physics is satisfactory in most cases, our fields (gravitational as well as electromagnetic) are mild, and we have nearly always direct observations, in one wavelength or in another, of the object that we study. We apply radiative transfer techniques, classical hydrodynamics, and MHD, and we are prepared to incorporate turbulence, and non-equilibrium thermodynamics.

We can first look into progresses expected from extrapolation of our present work. We already met numerous topics, but I think that we have missed some of them, and first let me come back to the abundance problem, disconnected from the modeling process. We do not want good values of abundances per se, or as a product that we deliver to our colleagues working on evolution or nucleosynthesis. We may find interesting problems inside our atmospheres:

- I do not think that the iron solar abundance is a closed question; different values from the photosphere up to the corona are still inside the errors bars, with still a tendancy towards higher abundance in the corona.
- Variation of abundances inside a single star either real, or due to large scale inhomogeneities, seems to be observed.
- The possibility of sorting the elements inside an atmosphere as studied by G. Michaud is very exciting.

Secular variations of abundances due to selective evaporation, or accretion might be within the scope of future observational evidence.

All those problems will probably be related to transport processes. We have means to attack them, but one element is missing: a comprehension of turbulent transport and mixing. This is a very difficult problem, and most of us are not prepared to consider it. It is likely that meteorologists and oceanographers have the same kind of worries.

Concerning interstellar matter, at least in regions close to stars, the hydrodynamical

[redacted]

essentially by radiation is typical of the desirable coupling between our knowledge in transfer and in hydrodynamics.

However, even without dynamical problems, many questions can be treated in greater detail than is actually done. A circumstellar envelope modifies the diagnostic for example in the single case of shocks, not to speak of other types of waves, including MHD waves! Again, we have much to learn from the Sun: we know roughly the amount of energy which is deposited in the low corona, but it is not yet possible to draw from the observations what is the dissipation law with height. So the question of the nature of the energy, and, *a fortiori*, of the dissipating mechanism is a matter of personal theoretical opinion.

Last, but not least, let me mention again turbulence, or rather its hydrodynamical effects: turbulent structures convey with them some gross effects such as pressure, and energy. If there are motions such as expansion, the decay of eddies will be accompanied by energy dissipation, and pressure effects (remember that the 'microturbulent velocity' can be of the order of the thermal velocity). Let me quote here a new result obtained recently in Nice by U. Frisch and coworkers just to show the complexity in which we are to fall: it can be shown theoretically that the decay of eddies in two dimensions is radically different from the 3-dimensional classical Kolmogorov behaviour. In a stellar atmosphere, the gravitational field can induce structures that rely more on a 2-dimensional analysis than on a 3-dimensional one; who knows? But remember some observational distinctions between 'horizontal microturbulence', and 'vertical microturbulence' in the Sun.

As theoreticians, we should also put efforts on the new methods of handling observational data. Speckle interferometry has recently grown up, and it poses many interesting analysis problems. The deconvolution of the effect of rotation can certainly be approached in modern terms. Some tools such as maximum entropy methods can help us to define what we are looking for in the observations. Let me take again a solar example, namely the photospheric velocity field. I do not think that we really need a wholly detailed description of what happens in a calm region with high temporal and spatial and spectral resolution. What is important for the physics are spatio-temporal correlations, spectral density of energy, in one word, statistical informations. Good methods of reduction, and also ingeneous observational devices can give this kind of information without going through intermediate steps of deterministic description or recordings.

To finish, I shall mention briefly other domains of Physics in which we shall have to look.

- Plasma physics of course, but nearly all what I have said on hydrodynamics encompass plasma physics too. So I let you make the generalization. Still, non-thermal processes, such as in flare-stars, or the emission of neutron stars, are important peculiarities of the Sky that deserve separate studies. But they are in the childhood stage, and a prediction of the kind of work that will appear is outside my possibilities.
- Very low density organic chemistry is a necessity. Hopefully once the reactions constants will be known, the diagnostic of interstellar matter through emission of organic material should resemble strongly our practice of atoms and ions.
- We have a very interesting and hard problem to ask to solid state physicists: interstellar matter contains dust particles. Presumably, those particles contain a number of atoms too small for applying extrapolation of classical knowledge. One knows that

the study of thin films is a very peculiar part of solid state physics. 1000 Å is a common dimension in both cases. We badly need to know much about the behaviour of small solid particles in their interactions with themselves (Van der Waals forces, electrostatic attraction, polarisability), with neutral and charged particles, and with the radiation field. There is also the interesting question of the anomalous behaviour of transport coefficients of dust particles in gases, which depends on statistical physics.

- And finally there is the eternal wish to use the powerful tools of thermodynamics, and especially the new ones of non-equilibrium thermodynamics. All the objects that we study are the seats of basically irreversible processes. Why are we unable to produce non-trivial results while powerful means are at our disposal? Those means are now applied to biology; I am not a specialist in biology, but I have the impression that it is not less complicated than stellar atmospheres!

3. Conclusion

I have tried to separate the various problems that one expects to see being solved in the future. They all rely on today experience – some on yesterday's too. Some are more precise than others, some are more difficult than others. There are certainly many gaps that you will fill in the next hour of discussion. But it is a hard game to tell the future, and the facts come very often to deny your predictions (see L. Kuhi's talk.) I am not an astrologer; and if I had been you surely would not have let me make this talk.

Acknowledgements

I am indebted to F. Praderie, H. Frisch, L. Kuhi and R. N. Thomas for valuable discussions during the preparation of this paper.