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On the Relevance of the MK System and Process to the Theory of Stellar Atmospheres*

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Table of Contents

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When Bob Garrison called me and asked me if I could come to this conference, I first treaded water a minute because I had several other things I had to do. But I quickly realized that I had to jump at the opportunity, because it is truly a privilege to participate in a symposium that honors the creative activity of two outstanding, distinguished astronomers. Following on what John just said, I'd like to give my unique qualification for talking this morning - namely, that I have never (in print) classified a stellar spectrum! On the other hand, I did once, with Dr. Morgan's permission, sit in on his course on spectral classification, so I did get a chance to learn how one classifies stellar spectra. I'm not sure, but I think it's the last time he gave that course, and I'll leave it to you to decide after what I say this morning whether or not there was some causal relation.

I feel that I have three tasks this morning. First, I'd like to try to assess the *state* of stellar atmospheres theory. Next I'd like to make some kind of *prognosis* for its future. And third, I'd like to try to address the issue of the relevance of the *MK System* and the *MK Process* to the development of the theory of stellar atmospheres. At the very beginning I would like to express my gratitude to Professor Morgan for introducing at this meeting, for the first time, the concept of what he has called the *MK Process*. That particular idea greatly facilitates my discussion, and in fact I will focus more on the process than on the system per se.

Now I've already admitted my basic incompetence to discuss the nuts and bolts of spectral classfication. Instead, my talk will be an attempt to discuss some issues of what I would call scientific epistemology, as they bear on our particular specialty. I must warn you that this talk is not a coherent whole. I have not been able to do that. There are only certain facets of certain problems that I can hope to examine, and I'm going to have to make some digressions. All I can do is beg your patience, and promise you that I will, in fact, ultimately return to the stated themes. These are issues that I have been struggling with off and on for many years. I must admit that I am still confused by many of them and I would like to share my confusion with you this morning, but I hope to avoid producing new confusion. On the other hand you may discover that this talk is a classic example of the celebrated ``Peter Principle." Again I leave that to you.

I actually have addressed some of these issues once before, in my Warner Lecture in 1974 (Mihalas 1974), and in a paragraph that I was allowed to insert in the article by Morgan and Keenan in the 1973 *Annual Reviews* (Morgan and Keenan 1973). Unfortunately, the (if I can use the word) epistemological content of my Warner Lecture never got printed. It was deleted by what other people have since told me was an act of editorial censorship (a concept I wasn't aware of at the time). I would like to expand upon some of the themes in that lecture this morning. Inevitably I am going to make, I think, some unorthodox remarks. I offer no offense to anyone. If my remarks do not make sense to you, it is probably my fault and I would welcome instruction from you on these points at any time.

The stimulus I had for taking this particular tack this morning was the realization that I finished the computations for my thesis exactly twenty years ago, and I've been active in the field of the theory of stellar atmospheres ever since. It seemed to me that it might be a good time to ask myself the question, ``What have I learned about stellar atmospheres in twenty years?" - a nice round number. Twenty years ago I had a very clear and extremely optimistic picture of what I *knew* about stellar atmospheres. Today I'm not sure what I really know about them. That may sound like a negative statement; but in my opinion it is not. Rather, it reflects the fact that my initial conceptions were hopelessly over-simplified and uninformed, and have now been replaced by a rather broader view of what stellar atmospheres are. That picture has been greatly enriched not so much by theoretical work, but by the *fabulous* observational discoveries that have been made in the past two decades. It further reflects the fact that my initial conceptions of what scientific methods are was much too narrow.

I would like at this point to express my deepest gratitude to W.W. Morgan for immensely enlarging my horizons of what science is all about, even at the price of a feeling of increased uncertainty about what I do and do not know. Parenthetically, I'd like to remark that I can remember many times at Yerkes working on something, and feeling a sense of satisfaction at having just published a nice little bit of work about it, and then having to expect that a week or two later Morgan would walk into my office with some ghastly, horrible spectrogram and hand it to me and say, ``What do you have to say about *that*?" All I could ever do was admit my bafflement and, suitably humbled, go back to work. It was a marvelous growth experience!

To preface all of this I'd like to make a general remark about astronomy. It's something which all of you know, but which I think all of us tend to forget. In physical science there are basically three things we can do. First of all we can make observations. By that I mean we can *watch* the system, just sit there and watch it. The second thing we can do is perform an experiment. By that I mean we can go and *tweak* the system. The third thing we can do is construct theories. By that I mean we can *talk about* the system. In astronomy, let me remind you, we are *irrevocably denied* the opportunity to make experiments, or even, except in very rare cases, to make *in situ* measurements.

Have you ever asked yourself: What would *really* happen if we took two stars and just smashed them together? What would *really* happen if we dropped, say, Toronto into a black hole? What *is* the central temperature of the Sun? We are never going to know the answer to any of those questions - *never* - simply because the time, space, and energy scales involved are much too large. Or because the conditions we'd like to know something about are far too harsh for any physical laboratory to survive. So we'll never, ever, ever be able to do those experiments.

The consequence of this limitation is that in astrophysics we must therefore rely on (usually very seriously incomplete) observations of the *simultaneous* interactions of a *very large number* of often highly *nonlinear* phenomena. Then, given that, we have to try to infer what's going on by means of some kind of more or less elaborate theoretical interpretation. These facts have an important implication. They say that astrophysics has, compared to the other physical sciences, an unusually strong *dependence* on theory; hence it is unusually *vulnerable* to any limitations of theory.

I want to stress the vulnerability of astrophysics to any weakness in the theory, and I feel it behooves us to inquire carefully into the *goals* and *activities* of theory. I think it is pretty easy to identify a couple of goals and a couple of activities and I'm going to list a few; I want to emphasize that my list is not exhaustive, just illustrative. Under the general heading of goals, I'd say our number-one goal is something we call ``understanding." What we seek to do is to be able to say *what* is happening, *how* it happens, and *why* it is happening that way. The second goal we can associate with theory is something I'll call ``prediction." What we'd like to do is be able to say what will happen *next*, or what *would* happen if we changed the conditions in such and such a way.

Now how does one attain these goals? The answer is one ``does" theory. What does *that* mean? It means one participates in some kind of theoretical activity. And what are those? Well, a first example would be something we could call *deduction*. Here what one does is start from stated principles and use a ``logic machine" (of your choice) to make some kind of predictive statement. Let me recall to you the allegory that Eddington once wrote, about the infinitely intelligent astronomer who lived on an utterly cloud-bound planet, who (in Eddington's opinion) could ultimately, deductively state what the structure of the Universe would be. Now that's a logical extreme. I'm going to come back to it later (and ridicule it).

The second kind of activity is what I would call *inductive*. Here the idea is to start from the raw data, to try to find (infer) some kind of underlying order, and then to try to find (infer) the principles from which that order follows. In my opinion, this process is the ultimate source of theory, the process by which we literally *make* theory. Let me again remark parenthetically: theory is a totally *man-made product* (man in the generic sense, excuse me: *person-made product*).

Okay: third point, third activity. Another kind of thing you can do is called *interpreting*. Here what one tries to do is arrange some kind of a *confrontation* between, on the one hand, a predictive theory and, on the other hand, observations. In this activity there are a couple of subgoals. One of them (with which most of us who engage in this activity occupy most of our time) is what I would call *calibration* of the data. That is, the goal is to assign a numerical value to some physical parameter - let's say, for example, temperature - needed to produce some kind of observed parameter - let's say, the equivalent width of a silicon line (or something else; you name it). The second subgoal, (and this is something that very few of us engage in) is to *critique* the theory. The basic idea here is to find out where it ``works" and where it doesn't.

In my opinion, the second subgoal I just mentioned is certainly the more important, and I think it bears a little bit of elaboration here. I will return to this particular point again and again in various guises throughout this talk. In principle what we are trying to do is to make a set of predictions from a theory with whatever degree of refinement is allowed by our physical insight and computational skills, and then confront the *facts*. And the moment we start this activity we encounter some problems. I'd like now to read a paragraph from the unpublished part of my Warner Lecture because I can't say any better today what I said ten years ago:

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First, a ``simple'' set of problems arises because we may lack
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physical information of the required precision, or because we must make mathematical approximations in the solution of our equations. These problems can, in principle, be disposed of by further application of effort, and do not present *fundamental* difficulties. Second, and more serious, are questions of what physical effects need to be considered, and in what detail. Sometimes it is possible to sort things into ``first-order'' and ``second-order'' effects, but often the line between these groups is blurred, and we must be on guard constantly to see that we have included everything that is essential.

Finally, and most serious, is the problem that we may implicitly have made some assumptions which *appear* to be plausible and self-consistent, and yet are *systematically wrong*. It is worthwhile to quote Poincaré (1913) on this point. ``The firm determination to submit to experiment is not enough. There are still dangerous hypotheses: first, and above all, those which are tacit and unconscious. Since we make them without knowing it, we are powerless to abandon them.'' In this circumstance we may find it possible to ``explain'' the data by certain ranges of parameters in our ``theory,'' and yet find that these values are wrong when we recognize and remove some underlying inadequacy of the theory.

Before I go any further, I'd like to point out that I just pulled a fast one on you. That is, I mentioned that there were problems with the theory, but I didn't say a word about the other member of the team: the *facts*. And it's actually an interesting question to ask, ``What *is* a fact?" Have you ever asked someone, what is a fact? When you do this, the typical response you get is a long-winded lecture about about something we call ``reality," which we are going to, in some way or other, ``determine by observation and experiment." The discussion of this ideal ``fact" can go on at painful length.

I would like to make a rash assertion: such a discussion is mostly meaningless blather, because in reality what we typically call ``facts" are actually statements. It's an important point. They are statements *in words* about something; the full significance of that remark will emerge later. Furthermore, they actually follow by a process of *inference*. There is no *pristine* fact. The instant I say, ``I know the following is a fact," think about what I've just done. I've introduced at least two *complex systems* into the argument. The first one is a thing called ``me," whatever *that* is (i.e., whatever my ``consciousness" is). The second is this process of *knowing*, of *cognition*, and that is very complicated. So the bottom line is that ``facts" are *not* pristine; they are logical *constructs*, and they may not be basic (though they could be in some cases).

Poincaré - again my friend Poincaré - once asked ``*Which* facts should one observe? There is an infinity of them." That's a very interesting remark. We all know how difficult it is to deal with an infinite process; and there's an *infinity* of facts. He concluded that in fact (pardon me), there is a *hierarchy of facts*. Let me offer some profound quotes about facts before I tell you their source, because that source is unconventional and I don't want to prejudice you. Here are the quotes; I think they are very good.

The more *general* a fact, the more precious it is. Those which serve *many* times are better than those which have little chance of coming up again.... Which facts are likely to reappear? The *simple* facts. How to recognize them? Choose those that *seem simple*.... *Where* is the simple fact? Scientists have been seeking them in two extremes, in the infinitely great and in the infinitely small.... By going very far away in space or very far away in time, we may find our usual rules entirely overturned, and these grand overturnings enable us the better to see the little changes that may happen nearer to us. [Italics added.]

One final quote:

How then to choose the *interesting* fact, the one that begins again and again? *Method* is precisely this choice of facts; *it is needful then to be occupied first with creating a method; and many have been imagined since none imposes itself*. [Italics added.]

Now this is pretty strong stuff! What it's asserting is that ``facts'' (which are *constructs*) require a *method*. Furthermore it asserts that we must *create* the method. In doing so we must necessarily *create* facts. Now let me tell you the source. This is from a very interesting book called *Zen and the Art of Motorcycle Maintenance* by Robert Pirsig (1974), which, if you are at all interested in the meaning of science, I recommend that you read. (It was recommended to me twice, independently, by two well-known astronomers.) It makes very interesting reading indeed.

Now, to return to the theme from the digression. If I say facts are not basic, then what is more basic than a fact? My response to that is ``a specimen.'' What I call ``a specimen'' is the *real* (whatever that means) space-time event which we encounter on our world-lines. And it is *specimens*, not facts, that are the ultimate empirical currency that we must use if we wish to purchase a valid theory. I would now like to tell you a funny story about specimens. It comes from an interesting book called *The ABC of Reading* by Ezra Pound (1934). It really gets to the point. Pound said:

No man is equipped for modern thinking until he has understood the anecdote of Agassiz and the fish: A postgraduate student equipped with honors and diplomas went to Agassiz to receive the final and finishing touches. The great man offered him a small fish and told him to describe it.

Postgraduate student: ``That's only a sun fish.''.
Agassiz: ``I know that. Write a description of it.''

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After a few minutes, the student returned with a description of the
Icthis Heliodiplodokus, or whatever term is used to conceal the common
sunfish from vulgar knowledge, the family of Helichtherinkus, etc., as
found in textbooks on the subject.
Agasissiz again told the student to describe the fish.
The student produced a four-page essay. Agassiz then told the student
to look at the fish. At the end of three weeks the fish was in an
advanced state of decomposition, but the student knew something about
it.
By this method modern science has arisen, not on the narrow edge of
medieval logic suspended in a vacuum.
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You know, it's a pity Pound wasn't a scientist. He probably could have done very great things.

So, central issues of my talk concern: (a) method, and (b) specimens. And lest I seem to be rambling totally aimlessly, let us return now to one of the advertised themes of this talk: the theory of stellar atmospheres. Let us view it as a method. Now I could say that in one sense the progress in the theory of stellar atmospheres over the last twenty years has been pretty impressive. It's unquestionably true that we have made major strides in getting a better hold on the underlying physical ideas. It is certainly true that we have had a powerful new tool - namely, the computer. As a result we have been able to attack a lot of more-and-more- complicated problems. For example, in the past twenty years, people have been able to compute fully nongray atmospheres, as well as extended atmospheres; we have done problems of line formation with velocity fields; people have calculated non-LTE atmospheres; and recently people are beginning to study and understand dynamical atmospheres.

You could say that's pretty good progress. On the other hand [might respond, ``Is it indeed?'' because you may remember I said we attacked more *complicated* problems, not more *realistic* problems. In my opinion, the really *big* gains have come, *not* from the sophisticated computer modeling, but in the basic *paradigm shifts forced upon us by observation*. And you know the names of the players as well as I do. They are funny names, like *OAO*, *Copernicus*, *IUE*, *Einstein*, *VLA*, and *IRAS*. And the simple truth is that our pretty little theory has been *blown away* by the observations from these observatories. There have been major discoveries. I'll mention only the ubiquity of stellar winds, the fact that there are coronae everywhere in the HR diagram where the theory says there ought not to be. Let me also mention my friend the Sun. From the detailed solar data that we get from ground-based observation and from more funny names such as *ATM*, *SMM*, and others, we see a whole *zoo* of magnetically dominated phenomena.

Now what emerges from all this is, in my opinion, a brand new picture: that your benign, friendly old stellar atmosphere is actually a highly *structured*, *dynamic*, even *violent*, medium. In my opinion, it's from the observations that we've had what I call the ``Ah Ha!'' experiences. And there are more to come. We have *Space Telescope*; we have *SOT*. The short message from these data is that our present theories are *not* realistic. They are inadequate; indeed they are *puny*. They are feeble, and they are certainly mostly wrong.

So, to answer my earlier question - from working on stellar-atmospheres theory for twenty years, I feel that I have learned a lot of interesting things about physics, about mathematics, and about computers. But I don't think I've learned a whole lot about stellar atmospheres. I feel as if I've been made a member of a secret lodge, and after many years I've learned the secret handshake and have been admitted into the inner sanctum. And when I looked around, it turned out it was empty - just a great big empty room! I could even go so far (but I wouldn't be so rash!) as to say that there's a *crisis* in stellar-atmospheres theory. The problem with doing that is that it's a kind of minicrisis (or, better, microcrisis); most people would say ``huh?'' The point is that the more that we look at the specimens the more *complexity* we see. And as nearly as I can tell, the process seems *divergent*. I don't see any end.

In my opinion, some of us are faced with some *very* rotten sunfish indeed. We cannot handle today even the complexities that we know about. I mean, are you ready this morning to do the fully three-dimensional, time-dependent magnetohydrodynamics of a highly turbulent medium in the presence of a strong radiation field? Would you please hold up your hands? No? No one? The question is, can we *ever* do it?

Now I won't say ``never''; never is a long time, but I would say that I certainly do not see the solution in sight in my lifetime. Given the present state of affairs, I think that Eddington's idea of the infinitely clever cloud-bound astronomer seems pathetic, and indeed ludicrous. My guess is that that astronomer's picture of the Universe would *always* be too simple. He might guess that there were stars, but they would be *smooth* stars; they wouldn't be covered with nasty crawling things like plages and spots and magnetic flux tubes and flares and on and on. He surely would never, ever guess the existence of self-organizing, let alone self-replicating, physical systems (for example, DNA) that thrive in the fundamentally nonequilibrium part of the Universe in which we live. Those of you who are interested in such things ought to read the marvelous book by Prigogene (1980) on the subject.

Another way of putting what I'm trying to say is this: the Universe is simply much more complex and surprising than we can begin to grasp, *even when we are faced with the data*, let alone imagine or predict a priori. You get it shoved down your throat, and it's still hard to cope with it.

Now let's suppose that 100 years from now (that's assuming that our planet is still here in the face of our present nuclear insanity), someone writes what I like to call laughingly the HUMUNGOUS CODE: 20 million lines of code, which solves this three-dimensional-you-heard- all-of-the-problems on the CRAY LXVIII, which is a mere 10^{20} times faster than your friendly CRAY I. Could we still learn *everything* that we want to know from theory? I personally suspect that the answer to that question is ``no,'' and my reasons for having that suspicion may seem esoteric, but nevertheless I hold that particular view. It has to do with, if you will pardon the expression, the ``fact'' that theory, and by that I mean *all* theory, is a language activity, and is what I might call a *language game*.

To make this point I want to read again some of the unprinted part of my Warner Lecture. In doing so I think I can make a bridge to one of the other themes of my talk (which I haven't forgotten about, in case you thought I had): namely, the MK Process. So let me begin; it goes something like this.

It is worthwhile to step back a pace and look at theory as an *activity*. Specifically, I would like to stress that while it will be fruitful to press ahead energetically with the projects outlined above [i.e., the ones talked about in that lecture], it is important to realize that we have by no means arrived at the final structure of the theory. There is a tendency, especially when a large expenditure of effort has been made to develop a theoretical framework, to want to stop there, and a temptation to *make* it work, even when it doesn't, by forcing extreme ranges of parameters and by ignoring the danger signals that we are not really fitting the data. Very soon we become enmeshed in a theory that is virtually an orthodoxy, replete with dogma (and saints). We are then victimized by any systematic errors inherent in the structure. [Recall the remark by Poincaré, quoted earlier.] We may always hope for rescue by the iconoclast, but we may have to wait a long time for his (or her) appearance. As the theories with which we deal become more and more complex, we are more and more exposed to this danger.

I would therefore like to draw an analogy which I think bears on this problem. It is interesting to consider theory as a *language activity*, for there are parallels. It was not by accident that the word interpretation appears in the title of this talk [i.e., the Warner Lecture]. I chose it purposely to emphasize the view that all of our equations, computations and so on, are acts of *saying* something about stellar atmospheres and stellar spectra. Our approach is essentially to develop ``word pictures'' of reality by invoking certain physical assumptions and principles, upon which we operate according to prespecified mathematical rules. In doing this we tend, in the words of Korzybski (1933), to move to higher and higher levels of abstraction. Ultimately we attempt to close the circle of abstraction by a predictive reference back to observable facts. But a basic problem with any language is that, again in Korzybski's terms, it is *self-reflexive*. That is, in the end, language is defined in terms of itself. This leads to an interesting conclusion, as stressed by Wittgenstein, that even if we grant that all propositions in the language are analyzable into *elementary propositions* (mind you that's a great big concession) then we find that statements can be constructed that are *without meaning*, that assertions can be made that are formally *undecidable*. The parallel is strong with Gödel's conclusion that, with a given axiomatic structure, there exist mathematical propositions which can neither be proved nor disproved; they are undecideable. (I might now make a parenthetical remark that probably the great thrust in twentieth-century mathematics has been to show the ubiquity of the undecideable propositions that permeate essentially all of mathematics.) It is as if the structure of the language casts a *shadow* into which we cannot see. Of course we may then construct yet another language which allows us to speak about areas hitherto in the shadow. (Example: quantum mechanics allows us to say things about atoms which cannot be said in classical mechanics). But this new language will in turn also cast similar ``shadows,'' though they may lie in other places. Thus the analogy between theoretical interpretation and the language activity of ``saying something about something'' leads us to conclude that in certain situations we cannot actually say *anything* about a phenomenon because this is forbidden by the underlying structure of the theoretical system.

What then are we to do? One clear possibility is constant reference back to observables, to specimens. Wittgenstein particularly spent much effort to explain that it is possible *to show* the meaning of things which *cannot be said*; this is indeed a profound and powerful observation.

I tried to pack a lot of ideas into those few sentences, and I must admit that this particular view is not widely held. I could spend hours talking about it, but I can't, alas, do that today. I can only invite you to *read* Poincaré and Korzybski and Wittgenstein, and then go *think* about what they say and draw your own conclusions. The essence is this: if we are to find our way around what I consider to be the present (and perhaps ultimate) impasse of theory, we must engage in what I just called *showing*, not what I called *talking*. We must appeal back to the specimens through *autonomous* empirical systems, and therein lies the central importance of the MK System.

The MK System is an *example* of an autonomous empirical system. Now what are the attributes of the MK System? I'd like to read something that I wrote once for this little textbook on galactic astronomy:

1. [The MK System] is empirical; only directly observable features of the spectrum are used to determine a star's classification.

2. It is based on homogeneous material. It uses well-widened spectra with a dispersion of around 60-130 Å/mm. which gives enough resolution to provide sensitive criteria, and is low enough to allow one to reach stars at large distances in the Galaxy.

3. It is defined by standards. Thus the classification system is autonomous in the sense that it remains unchanged even when the interpretation of spectral classes in terms of physical conditions in a star changes as models of stellar structure are refined.

From the point of view of the theoretician the last attribute that I just mentioned is the most important one, because it implies that the system does not, from the beginning, contain theoretical preconceptions. That's the absolute bane of trying to abstract anything from an empirical system. You build a theoretical preconception into it and all it does is give it back to you! This *must* be avoided from the outset.

Morgan once remarked to me that the essential point of his papers was not the *results* that they contained, but rather that they provided *examples of a procedure, a method* by which such results *can* be obtained. That's a very perceptive remark. The real message of his papers is not the ``answers," but the *approach*. And he has now formally introduced the notion of the *MK Process*. I'm going to have to leave that term undefined. That's Morgan's job; he'll talk about that later this morning. But even with it undefined, the substance is clear: it is the *method* by which results, such as are *exemplified* by the MK System, are obtained. And it's clear that we must now use the MK Process to construct *new* examples of such systems. To understand the phenomenology of the *whole* spectrum we can now see (all the way from the X-ray to the radio), we need to construct a whole new *set* of systems having the same autonomous character as the MK System. This can be done, if you are willing to follow *scrupulously* the precepts of the MK Process.

Just as an example, suppose I were Morgan at age 27, not 77, today, and I wanted to classify satellite UV spectra, say. Would I arrive at the MK System? If I were W.W. Morgan, the answer would be ``surely not." The reason is that it is not at all clear that a typical temperature and density in the atmosphere are the *fundamental* parameters that control the UV spectrum. Even if they were fundamental, they are surely not the *only* fundamental parameters. I can instantly think of a couple more candidates - for example, the mass *flux* from the star. Stellar winds are ubiquitous. The crucial question is, how much mass is there in that wind? It determines how optically thick the lines are and how intense they will be. What is the *velocity gradient* in the wind? A wind with a very shallow gradient looks very different from a wind with a very steep gradient. Okay, so I've got - what? Three parameters? Four parameters? I don't know. A three dimensional system? A four-dimensional system?

A remark I'd like to make is that, in my opinion, it may not be particularly useful for the progress of theory to try to correlate UV spectra, UV behavior, only with the *existing* MK System - that is, with spectral type and luminosity class. A far more useful thing to do would be a fundamental bottom-to-top *construction* of a *brand-new* system. Now it would only take you ten or twenty years to do that. It's a big investment of your time. And what about the X-ray spectrum? There, who knows what the fundamental parameters are? Maybe it's rotation. Maybe it's the depth of the convection zone. I'm guessing. We know that rotation has got something to do with it. We wouldn't have known that except that the observations showed us that. I cite these just as examples of situations where the MK *System* per se, as it now exists, may be powerless, but where the MK *Process* may provide a key to the construction of new autonomous systems that can provide the conceptual framework for theoretical discussions of the whole spectrum.

Another point I'd like to make is that in future work we must at least try to be more precise about what *part* of the atmosphere we are talking about. Are we talking about the deep photosphere, the chromosphere, the corona, or the wind? A set of stars that fit into one classification box for one set of phenomena (say, from the photosphere) could be scattered over several different boxes for another set of phenomena (say, coronae). The point is that a similar photospheric structure need not imply similar total-atmospheric structure. That's a point you must always bear in mind.

Yet another point: I think it behooves us to remain sensitive to the question of where what we call the ``atmosphere'' ends, and where what we call the ``environment'' begins? Does the atmosphere really include something we might call a ``circumstellar shell'' or a ``dust shell''? How far out is the wind still part of the atmosphere? Is it just at the point where it becomes supersonic and cannot have any impact on what happens upstream anymore, or is it the whole wind? What about binaries in a common envelope? Where is the atmosphere? How much of that is the atmosphere? These are very fuzzy questions, and I don't think they are amenable to crisp answers at the present time. I think we should be alert, however, to oversimplifying our picture.

Let us return at last to the tasks that I set at the beginning of this talk. (I assure you that I will not stand here and talk aimlessly all morning.) What about the assessment of the status of the theory? I would say it's alive, but not well. It's not on its deathbed, but it's not well. For its prognosis, I'd like to go back to the unpublished part of the Warner Lecture and offer you another little quote:

All theoretical systems are necessarily incomplete descriptions of the phenomena of stellar spectra. Ultimately we must show the strengths and limitations of the theory in the context of, and by reference to, an empirical system of stellar spectroscopy. If this is to be meaningful, then it is *essential* that the empirical system be *autonomous*, i.e., *that it avoid having a structure preimposed upon it by presuppositions of a theoretical nature*. In the end. the approach we must take in such an empirical system is to define it in terms of *real objects* (specimens) *without further comment*. That is, we should choose standards, and *refer all other objects to these standards by a process of differential comparison*. The fundamental example of an autonomous empirical system in stellar spectroscopy today is the classification system of Morgan and Keenan. *If* a system is truly autonomous, then a critical morphological comparison of the totality of the theoretical predictions with the totality of the observed properties of spectra can expose the discrepancies which *genuinely* imply inconsistencies and/or systematic errors of the theory. *At that point the observations can show us how to design a new theory*. I am convinced that we *can* continue to make large strides forward in the interpretation of stellar spectra for an unforeseeably long period of time. But I'm also convinced that this will occur only if the observers keep showing to us those things which our theories say are wrong, and, more important, those things about which our theories can say nothing. By this process of discovery we can rebuild our theories, or invent new ones, in a form more suitable to allow us to say what `there is to be said.`

So finally, I think that the relevance of the MK Process to the theory of stellar atmospheres should be pretty clear. Basically it will provide the *tools* with which we can build a *complete* autonomous empirical system for stellar spectroscopy. It is the *means* by which observers can *learn* - and I stress the word learn - to show us those things our theories can say right, and those they can say wrong, or those about which they can say nothing.

In closing, I offer my apologies for having run on at length in such a rambling fashion. I have to thank my audience for their patience, but most of all I must thank W.W. Morgan for sharing with me his scientific insights and his friendship. It is a great pleasure to say that he's been a constant source of inspiration, both as a scientist and as a man. Thank you.

Next Contents

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