PART II

CLASSIFICATION OF OBJECTIVE-PRISM SPECTRA

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INTRODUCTORY TALK FOR SESSION ON OBJECTIVE PRISM SPECTRAL CLASSIFICATION

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Traditionally, introductory talks usually have either the function of justifying the meeting at which they are given, or of attempting to present some sort of overview of the whole subject, or sometimes both or neither. A meeting such as this one is generally in order when the field in question has for any reason been very active since the last meeting about it; and by that standard I should say that this symposium is indeed appropriate inasmuch as the field of spectral classification has surely been very active during the seven years since the last symposium on spectral classification and multicolor photometry. In the area of objective prism spectral classification, the last several years have not seen very much really new in the way of observing techniques or methods of analysis that I know of, at least in comparison with some other fields; but they have certainly seen a great deal of astronomy done.

The vast majority of objective prism spectral classification has been and will continue to be done by visual inspection. This is mainly because the most common object of objective prism spectral classification is the formation of finding lists of special classes of stars. For this purpose the human eye, with its ability to examine large numbers of spectra as it were simultaneously, in order to select without detailed individual analysis those meriting further individual attention, is very well suited. This is doubly true because relatively few classes of stars have differences of major importance between them without also showing fairly strong spectral differences, though the exceptions are large in absolute number and their definition as important is a matter of taste to some extent. On the other hand, if the object is the positive detection of a weak, narrow line – which will, of course, require individual and careful inspection – then the human eye is actually superior to, for example, a tracing (or set of numbers) derived from a slit scanner, because the latter loses the information inherent in the two-dimensionality of the photographed spectrum; the tracing cannot distinguish between a pinhole and a weak and narrow line. For weak features that are broad (the wings of a strong line, for example) the situation is entirely otherwise; but these are seldom the object of an objective-prism search. As an aside, these remarks evidently bear also on the fairly frequent proposal that visual examination of widened spectra be replaced by plate-transmission measurements of unwidened ones.

Considering the span of time during which procedures for performing objective prism surveys has remained but little altered, it might seem remarkable that so much work should be in progress, or remain to be done, or to have been but recently completed. Recently completed projects of appreciable size that I am aware of and that might be mentioned here are Westerlund's infrared survey of the southern Milky Way for M, S, and C stars (cf. Westerlund, 1971); the south galactic pole survey by Slettebak

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and Brundage (1971); and the survey of the southern Milky Way for luminous stars of early spectral type by Sanduleak and myself. Three major surveys now in progress, the first two far advanced and the third just begun, are Stock's blue-region survey of the sky between the Sanduleak-Stephenson and Slettebak-Brundage surveys just mentioned, the search for southern peculiar stars by Bidelman and his colleagues on ~ 110 Å mm^{-1} plates taken with the Curtis Schmidt telescope and its 10° objective prism, and the reclassification of the stars of the HD catalogue on the MK system by Miss Houk using the same plates as Bidelman's group. This last project, if carried to completion, will falsify at least one prophesy that I made some years ago. Writing in *Vistas in Astronomy* in 1964 about objective prism astronomy, I said: "Higher dispersion work will probably be done by objective prisms in the years ahead, but sheer considerations of manpower will probably dictate for many years to come that most of this work will be done in regions of the sky of special interest...". I trust that Miss Houk will have the persistence to make me wrong. I omit a number of other important recent objective prism surveys only because I am not trying here to give an exhaustive summary.

All of the projects that I have just mentioned were started since our last symposium on spectral classification and multicolor photometry, and the main reason that so many things remained then to do is that essentially every new project that I have mentioned pertains to the skies of the far south; even Miss Houk's plate collection is at the moment more or less complete only for the southern hemisphere. Nevertheless the state of work in the southern hemisphere is becoming, surprisingly, more complete than that in the north; complete surveys in the north have yet to be carried out that would correspond to the ones just mentioned by Westerlund, Bidelman, Stock and, to some extent, by Houk.

Some of the astrophysically most interesting work that gets done via objective-prism spectral classifications is not necessarily part of such large surveys as I have been mentioning; or it may be only a very important by-product of those surveys, though perhaps the main source of zest on the part of the people doing the work. For example, in our southern luminous stars survey we found, and published (Hiltner *et al.*, 1968), an HD star with a spectrum very like a fast nova near maximum; a star which optically is very like the X-ray source Sco X-1 (Stephenson *et al.*, 1968; Hiltner and Gordon, 1971); and a previously unrecognised white dwarf of unusually small proper motion apparently at a distance of only 20 pc (Stephenson *et al.*, 1968). All of the examples just mentioned were incidentally confirmed independently by other means.

I should like to comment briefly upon two other accomplishments of recent times that bear on our subject. One is the atlas of objective prism spectra of normal stars that Dr Seitter has produced at Bonn. No doubt everyone here has seen this long since; but if anyone has not, I commend it to your attention. The other work is Wackerling's catalogue of early-type stars that have shown Hα in emission (Wackerling, 1970). In my own work I have found Wackerling's catalogue immensely valuable, and there is no doubt that objective prism workers in particular owe him a great debt of gratitude.

Mentioning Wackerling's catalogue brings me to a point bearing upon the question of how to facilitate our work, and that after all is one of the central questions addressed by the IAU. There is no doubt in my mind that one of the serious needs of observational astronomers today is for a greater number of general catalogues of astronomical data, and this is particularly true of the people who look at objective prism plates. By catalogues, I mean published catalogues lodged in observatory libraries. I am aware that thought is being given to establishing astronomical data centers, where everything but the local telephone directory will be stored upon magnetic tape. I am entirely in favor of such centres, but it is unfortunately true that information that is stored in a distant center is a bit like a dictionary two rooms away – it simply does not get used as often as if it were right at hand, and when it does the information too often does not come forth at the ideal moment at which it would have been most useful. A number of such general catalogues have recently appeared; Wackerling's is one example, the MK classification catalogue of Jaschek *et al.* another; and I hope myself within a year or two to publish a catalogue of all stars that have been published as carbon stars plus some new ones. But we need many more, which brings me to my point. We who do objective prism astronomy are responsible for no small portion of the great flood of astronomical data that is forever pouring into our libraries. Surely we would be wise, as an aid to compilers of general catalogues, always to include in our publications star positions with respect to a single standard equinox and equator (in addition to positions for observers), or at least to include one position referred to an equinox that is one of the two most commonly used for documentation? I am aware that an IAU group is supposed to be preparing a recommendation on this right now; meanwhile I can recall offhand having had to work with papers published in the last 25 yr that used equinoxes of 1855, 1875, 1900, 1945, 1950, 1955, and several others; and now, increasingly, we see 1975 used as the sole equinox for a given paper. This sort of thing is guaranteed to discourage compilers of general catalogues! Personally, for catalogues of spectral types I think the only logical standard equinox is 1900; it is the equinox of the HD, of nearly all the Warner and Swasey surveys except for the northern Luminous Stars, and incidentally our southern Luminous Stars catalogue uses 1900; it is the equinox of Bidelman's late-type emission-line catalogue, of his unpublished spectral bibliography, of the GCVS, of the Jaschek MK-type catalogue, the Wackerling Hα-emission catalogue, of my unpublished carbon star catalogue, and so on. Surely, however, if we don't use 1900, then the only other equinox to refer positions to for compilers is 1950.

From time to time the subject of automation is introduced in connection with the searching and classifying of objective prism plates. There is, of course, no doubt whatever that the growth of computer technology in recent years has been a great boon to objective prism workers – enabling us, for instance, to make very rapid identifications of stars on a plate from their equatorial coordinates by means of overlays, to cite only one simple example. But the automation I refer to is the virtual elimination of human manipulation of the plate. I have no doubt that this can be done, although I do not think that a successful computer program for classifying a random

spectrum about which nothing is known in advance will be a very simple one, and I would certainly prefer the scanner to do a two-dimensional analysis of the image for the reasons given earlier. However, like the supersonic transport the question is, is it worth it? Having just made a very long air journey myself, perhaps I should have phrased that question as, To whom is it worth it? In an age of dwindling funding, I do not think that question is very trivial. Although experiments along these lines are bound to be interesting, for my part I would prefer to see any massive infusions of automated labor in the immediate future directed to a few alternate tasks. High on the list of priorities I would put: Preparation of charts for the parts of the C.P.D. catalogue that were not done by Ristenpart; a new edition of charts for the astrographic catalogue; preparation of charts for the Hamburg zones of Luminous Stars in the Northern Milky Way; and publication of a collation of C.D. vs C.P.D. numbers. I am aware, and grateful, that the same computer science that would make these things more feasible nowadays also makes their lack often less painful than formerly; but still it would be nice to have them done. Although the items mentioned would in some measure benefit practically all observational astronomers at some time or other, I believe that their lack is more troublesome to objective prism workers than to most people. Another item that we sorely need is the publication of Dr Bidelman's bibliography of spectroscopic data, or something very like it; but I do not see any prospect of automation contributing very much here.

Lest the foregoing remarks seem too disrespectful of the march of technological progress, let me say that I have become sufficiently impressed by the great improvements brought about in recent years at my own institution by our modest acquisitions of new equipment and materials that I have more than once given serious thought to postponing indefinitely some new project until more expeditious means of carrying it out come into being. I can illustrate this with a few examples, all drawn from the OB surveys done at the Warner and Swasey Observatory. Several years ago, when we did the winter Milky Way survey that was published as Luminous Stars VI, we were plagued by a long-standing problem: the Schmidt mirror would be pinched in cold weather, or if relieved would slip around and spoil the collimation during a warming trend; many of the plates that we had to use for that survey were not very good. A few years later we got funded to re-mount the mirror, and now for its radial support it floats in mercury and we can get perfectly good plates all the time in cold weather; but the Luminous Stars VI survey was finished years before that happened. When we took our objective prism plates at Cerro Tololo for the southern luminous stars survey they were having trouble with the spectral widening, with the result that some of the plates have the wrong widening or the wrong exposure times; today I understand that there is a new and very satisfactory system for widening the spectra – but our plates are all taken. When we started using the Smithsonian Astrophysical Observatory Star Catalogue to mark astrometric reference stars on these plates, we were plagued by the fact that the southernmost zones give mainly C.P.D. numbers, and we had no satisfactory C.P.D. charts. Pursuing the matter, we learned of the existence of the Ristenpart charts, and eventually obtained some – after we had finished working

in the regions covered by those charts. Still later, of course, the SAO charts themselves arrived – after we were through marking reference stars in *all* the zones. And so it went – eventually we acquired a plotter for our desk computer, which makes it very easy and convenient to find out what is wrong when the astrometric plate reduction program rejects a plate – but all 450-plus plates for the survey had been reduced by the time we had the plotter. The moral of this could be that we should have greatly postponed starting the southern luminous stars program – in that case we would still be postponing it instead of being already finished, for ideally I would certainly wait until we had a digitised measuring engine, which even now is not in sight. I think the moral in fact ought to be that one should nearly always go ahead, and just call it the way the cookie crumbles when the sorts of things happen that I have been mentioning. The alternative is to risk the dilemma posed by Bondi (1970) in the talk that he gave for the sesquicentennial cermonies of the Royal Astronomical Society. He proposed that there is a serious, perhaps insurmountable psychological barrier to launching interstellar space probes. The barrier works this way: Why should I launch this probe now, seeing it will require 300 yr to return its results to us, when by waiting 5 yr I may be able to launch one that will get the data back in 250 yr? Bondi suggested that by asking such questions continually the result will be that the probe will never be launched. I would add, however, that 300 yr after the first decision not to launch, somebody is going to start being awfully annoyed with the people who gave in to that psychology! And I think that is just the way it is with our science.

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