Session VII

CONCLUDING DISCUSSION

PHYSICAL PROCESSES

D. E. OSTERBROCK
(Washburn Observatory, University of Wisconsin, U.S.A.)

When Dr. Seaton asked me to chair the epilogue session of our conference I could not help but wonder if it were to be considered an example of type-casting. He told you that when an Englishman is asked to give a prologue, he immediately begins thinking of quotations from Shakespeare, so I must tell you that when an American is asked to deliver an epilogue, his thoughts turn naturally to Ernest Hemingway. I therefore suggest to you as the theme for our symposium the closing words of the main character of *To Have and Have Not*, which may for publication here be bowdlerized to read "*One* man *alone* ain't no —, — good at all." Over and over again in the last week we have seen that discussions between astronomers have provided new insights, have opened up new fields for investigation, have revealed unsuspected connections between apparently unrelated problems. I believe this has been a very fruitful conference because there have been so many fruitful discussions among the participants. I shall therefore try to summarize briefly my impressions of the things we have learned and the things we may reasonably hope to learn in the next few years about physical processes in planetary nebulae, and then five of our colleagues will summarize the other sessions of our conference. We invite discussion from all of you after each of these summaries, and also during the general discussion at the close.

First of all I think I can say that we have the theoretical tools we need to investigate planetary nebulae. The collision strengths as described by Seaton and Czyzak, the transition probabilities as summarized by Garstang, and the recombination coefficients and photo-ionization cross-sections as summarized by Flower, all seem to be reasonably accurate. Using them we can calculate models of spherically symmetric nebulae. This first approximation agrees reasonably well with the observed data, and we can make some statements about the properties of the central stars. Some particularly quite specific processes are important in determining the structure of the nebulae, e.g., as Hummer has told us, the He II Ly-α photons are important in the ionization of H, particularly because they 'create' high energy electrons in the nebula. A good deal of work has been done on radiative-transfer methods for treating the ultraviolet ionizing radiation. We have heard of the importance of diffuse radiation field, including specific resonance lines such as those I described just a moment ago. The interaction of the diffuse radiation field of one ion (such as the He II Balmer continuum) with another ion (the ionization of H in the present case) is often important. Looking into the future it seems to me that with large computers we will be able to make giant

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strides on this problem. Probably Monte Carlo calculations will become more and more important; also, however, numerical solutions of the relevant transfer equations will become less time-consuming.

We have discussed the importance of fluorescence, not only due to nebular emission lines (as in the Bowen mechanism), but also due to radiation in the stellar continuum (which might be described as case C), as Seaton has pointed out. A good deal of work will undoubtedly be done in the near future to follow up this idea of Seaton and to compare its predictions in a detailed way with observational data. A very hurried and superficial comparison I have been able to make is that in agreement with the predictions of this idea, permitted lines of the same multiplicity as the ground level (such as O I 3^3S–3^3P $\lambda 8446$) tend to be strong in planetary nebulae as compared with lines of different multiplicity than the ground level (such as O I 3^5S–3^5P $\lambda 7774$).

We still do not understand completely the H-line spectrum. Here is one problem which should be calculable to high accuracy, and the results should not depend critically on the physical conditions. The observations seem (by comparison of results of different observers taken with different instruments using different procedures) to be accurate to within 10%. Nevertheless, in some nebulae they disagree with the theoretical ratios by amounts larger than this probable error, and we do not understand why. Possibly the answer may be found in fluorescence in the higher Lyman lines. I think we must be sure we understand the H problem before we can say with certainty that some physical process has not been overlooked.

Once we subject almost any kind of observations of planetary nebulae to detailed scrutiny, we find that they do not agree in a detailed numerical way with calculations based on theoretical models. For instance, the values of T_e, N_e, and T_* calculated from observations of lines of different ions vary widely within a single nebula. The reason must be that different parts of the nebula have different temperatures. Some regions in planetaries cannot be observationally analyzed at all; for instance, at the present time we cannot measure spectroscopically the electron temperature in the most highly ionized regions of planetary nebulae where there is no [O III] or [N II]. This points out the need for ultraviolet observations, e.g. in this example, observations of [Ne III] and [Ne V] lines which can only be made from above the earth's atmosphere.

The problem of the interpretation of the helium emission-line spectrum still exists. The question is, What physical process destroys metastable helium (He I 2^3S) so effectively in planetary nebulae? The $\lambda 10830$ line is much weaker than we would predict for objects at the known electron densities of observed planetary nebulae, which must mean that in some way or other He I atoms are removed from the 2^3S level. Interpretations based on a high density of H I Ly-α radiation are faced with the difficulty that other expected consequences of this radiation are not observed.

Many of the papers at this conference have emphasized the importance of fine-scale structure in planetary nebulae. There are differences from point to point in a nebula of N_e, T_e, ionization, etc. Discrepancies in the 'observed' T_e, as measured e.g. by the

[O III] method and by the radio-frequency method, undoubtedly are due to these temperature fluctuations. Comparison of different kinds of observations (as in the example I have just cited) will give information on the scale and amplitude of these fluctuations. I think we are coming to realize more and more the importance of this small-scale structure. Our observational measurements must be aimed, as much as possible, at analyzing individually the small condensations within nebulae. I think we will see more concentration (with specialized instruments that combine large scale and high luminosity) on individual features within a relatively few planetary nebulae. We will have to be selective, and try to understand well a few typical examples, from which we may then hope to generalize to the class.

Actually of course, we cannot discuss the physical processes in a nebula independently of its dynamics; stationary, equilibrium planetaries do not exist. The nebula is expanding, the central star is evolving, and these non-stationary physical effects have to be included in our attempt at understanding planetary nebulae.

Several theoretical papers have described the possible importance of non-thermal particles in planetary nebulae – e.g., a stellar wind was mentioned by several speakers. We should make observational searches for these non-thermal particles, that is, we should see if some features of the planetary-nebulae spectra point to the presence (or absence) of fast protons or of fast electrons in nebulae.

The abundance determinations show that there are no large composition differences between nebulae and common stars, nor between one nebula and another. However, all the abundance determinations have a relatively large margin of error at the present time, because of our uncertainty about specific numerical values of T_e, N_e, etc. With better knowledge of these quantities, and with observations of all stages of ionization (made in the satellite ultraviolet and infrared regions, as well as in the photographic and visible regions accessible from the ground) we can possibly begin to look for subtle abundance changes (smaller than factors of 3) and try to understand the nuclear evolution of planetary nebulae and their progenitors.

DISCUSSION

Gurzadian: Can you explain the observations of Aller, Minkowski and Kaler, that the high Balmer lines appear to be stronger than calculated?

Seaton: I do not have an explanation, but I would like to make a suggestion for future observational work. The intensities of the high Balmer lines should be measured relative to the intensity of the Balmer continuum. This would enable us to deduce b_n factors from observation. If we assume that the recombination theory is correct for the low Balmer lines, the intensities of the high lines lead to b_n factors greater than unity. It would be very valuable to check this by measuring the high lines relative to the continuum.

Aller: It is indeed possible to measure the intensities of the high Balmer lines with respect to the continuum, but there is trouble with the Hg (mercury) lines near 3650. Kaler noted that the deviations occurred not only for H, but also for He I and He II, the deviation depending on the number of j-states involved!

Capriotti: The classical Case C of Aller, Menzel and Baker assumes that there is no absorption of the Lyman-line radiation that is produced in the nebula and that radiation from the central star in the spectral region right below the Lyman limit is absorbed in 1s–*n*p transitions. The predicted relative Balmer-line intensities from Case C deviate from the observed intensities when *n* is large as do the predicted intensities from Cases A and B of Menzel and Baker. If one includes the effect of the absorption radiation that is produced in the nebula in a modified Case C, one can produce the observed Balmer-line intensities with the proper combination of optical thickness at the Lyman limit in the continuum, stellar black-body temperature and fraction of stellar Lyman continuum radiation that is converted to Balmer-line radiation by the nebula.

Seaton: I have also considered this possibility, but find that it is capable of explaining the observations only if one assumes a large excess of the stellar radiation in the region of the high Lyman lines.

Aller: Kaler tried to correlate abnormal H-line intensities with density, electron temperature, properties of central star, and filamentary structure. He found no clear-cut correlation, save some suggestion that the more inhomogeneous the nebula the greater the effect.

Capriotti: How did Kaler consider the correlation between the central star characteristics and the deviations of the observed relative Balmer-line intensities with respect to the predicted intensities?

Aller: Kaler tried to see if the Zanstra-Menzel temperature of the central star – or the brightness of the central star – influenced the intensities of the high Balmer lines. He found no clear-cut correlation.

Van Horn: In the study by Kaler, was the other obvious correlation tried of the strength of this effect relative to position along the Harman-Seaton sequence?

Aller: Kaler did not have enough observational data to allow him to correlate effects with position of the central star on the Harman-Seaton sequence. You need high dispersion spectra which restricts consideration to bright nebulae.