IONIZATION EQUILIBRIUM IN MODELS OF PLANETARY NEBULAE

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ABSTRACT : A critical picture of the results obtained by means of photoionization models during the 70's is presented. Some reasons for the relative failure of these modellings are advanced and a new perspective is proposed for future investigations.

1. INTRODUCTION

My talk was supposed to deal with the effects of the newly introduced collisional processes involved in the ionization equilibrium of planetary nebulae. However P. Storey and R. Mc Carroll provided us with a comprehensive account of all the recombination and charge exchange processes which are currently believed to control ionization. Not to mention the discussion of UV spectra by M. Seaton who stressed the importance of low-temperature dielectronic recombinations in producing specific lines of diagnostic value.

Ionization depends primarily on photoionization rates, that is (1) on the frequency distribution of the primary radiation, derived from the theory of stellar atmospheres and may be, in the future, from direct satellite EUV observation, (2) on the photoabsorption cross-sections discussed by C. Mendoza and (3) on the radiation transfer some aspects of which being considered by D. Hummer.

On the other hand, J.P. Harrington will certainly present an unified view of the chain of physical processes leading from the extreme ultraviolet radiation of the star to the radiation finally detected at Earth and explain how the machinery of the self-consistent photoionization models can be used to derive the physical conditions and the information of astrophysical interest.

In view of such an exhaustive and authorized coverage of my subject, I feel free to comment on closely related but more general questions :
- Why do we make models ?
- What have we learnt through models so far ?
- On which conditions can we expect to learn something today or tomorrow

D. R. Flower (ed.), Planetary Nebulae, 173–185.

from detailed modelling of nebulae ?

2. EMPIRICAL AND THEORETICAL APPROACHES

The concept of a model is widespread in physics and needs to be clarified in a concrete situation. Two extreme approaches to planetary nebulae can be distinguished :

(1) Plasma diagnostics leading to empirical models of real nebulae : this approach is useful to derive information of practical interest for astrophysics (elemental abundances, see Aller in this volume, shape of nebulae, etc...). It is empirical in that there is a one to one correspondance between the observed data and the derived informations without meaningful check on consistency. In the important case of elemental abundance determination, the standard corrections for unobserved ions and "temperature fluctuations" can be classified as "semi-empirical" in that they are based to some extent on theoretical arguments.

(2) Ab initio self-consistent calculations leading to theoretical models without reference to particular objects : this approach is useful to depict what type of "complexity" should be added to get a correspondance with real nebulae. In practice there are two levels of self-consistency :

(2a) The fully (magneto-) hydrodynamic time-dependent approach in which the gas density distribution appears as resulting from an evolution under specified conditions (Lazareff, 1981).This includes more specialized studies on the instabilities that may develop in the flow (Capriotti, 1973).

(2b) The static (not hydrostatic) approach in which the gas density distribution is given so that the ionization and thermal aspects only are treated self-consistently. Although there is evidently no static nebula, this approach is not rudimentary because in most cases the energetics is largely dominated by radiation and the conversion of mechanical energy into radiation is not important. Thus there is no simple hierarchy between (2a) and (2b) but rather complementarity because (2b) is virtually decoupled from (2a) and a large number of initial conditions and time-dependent parameters should be specified in (2a).

Several variants of (2b) exist :

(2b1) Purely static and stationnary models : thermal and ionization balance applies everywhere and the only input of energy is through photoionization (conceivably photoexcitation) of the gas by radiation from a source independent of time (e.g., Harrington, 1968 ; Flower, 1968).

(2b2) Stationnary models but with a hydrodynamical and out-of-equilibrium treatment of ionization fronts of specified type and velocity : the study of the fronts can then usually be decoupled from that of the main body of the nebula. After some ups and downs, these models were useful in confirming that departures from the static case assuming constant thermal pressure are unimportant as far as the emission of stationnary fronts is concerned (Harrington 1977).

(2b3) Models with non-stationnary primary radiation source with out-of-equilibrium treatment of ionization and thermal balance in the whole nebula but with no reference to a global self-consistent hydrodynamic evolution of the gas (Harrington and Marionni, 1976 ; Tylenda, 1979). This sophistication is not needed in most cases because the local time-scale to reach equilibrium is normally much shorter than the evolution time-scale and much longer than the pulsation time-scale of the central star. However the study of the consequences of, e.g., a burst (see Tylenda, this volume) or a rather sudden shrinking of the source is of theoretical interest at least to learn the limit of validity of the more common stationary approach (2b1) to which we now restrict our attention.

3. FITTING OF THEORETICAL MODELS TO REAL NEBULAE

Purely theoretical approaches by means of, e.g., grids of ab initio models are of limited value in the case of planetary nebulae because :

- the number of input parameters is large,
- the objects accurately observed are not very numerous,
- each nebula is a unique well-characterized object demanding a specific study.

Very schematically, two distinct attitudes can be adopted when performing the fitting of self-consistent models to a given planetary nebula .

(2c) The notion of the model strictly includes atomic physics data currently available and it is implicitly assumed that these data are adequate to describe accurately and exhaustively the situation. The fitting to observed line intensities is then restricted to astrophysical parameters and a satisfactory fit is taken as convincing evidence that reliable information (stellar flux, abundances, gas density distribution, ...) has been obtained.

(2d) The model is viewed as a place of encounter of several types of data, all of them entailing uncertainties and constraints : atomic data are not a priori and systematically assumed to be perfectly adequate (which is evident) so that a certain degree of symmetry is restored between atomic physics and other fields of physics, as they indirectly express themselves through "likelihood" arguments about astrophysical parameters. Stated otherwise the model provides a framework where conceptual experiments in physics (mainly atomic physics) can on occasion be performed with the help of good observations.

No doubt that position (2c) was and still is the most indicated for a first stage and this may explain in part why it seems to be considered up to now as the only conceivable one by many investigators. But it should be recognized that position (2d) may awaken other reservations such as :

- experiments with models are simply meaningless,
- planetary nebulae are too anecdotal or capricious in their structure to constitute reliable laboratories,
- the best we can do with nebulae is to modestly try to wrest snatches of approximate informations for immediate use in Astrophysics without questioning too much about their meaning,

- astronomers would be presomptuous in trespassing on atomic physics' preserves with their simplistic models and skinny observations,
- this new point of view seems to weaken the overwhelming importance of atomic physics,
- astrophysical results would become conditional, which is neither gratifying nor convenient (and may even increase the length of the papers!),
- the additional freedom would introduce an arbitrariness that does not seem to exist in the more traditional approach (2c),
- a problem with models is that they have so many free parameters that they can fit almost everything and you would introduce as many new ones as you want !

Instead of discussing these critics, an appraisal of the last 15 years modelling is presented followed by some general comments.

4. THE MODELS AS A TEST OF IONIZATION EQUILIBRIUM

A. 1968 : The golden age.

In retrospect the event of the first self-consistent photoionization models of planetary nebulae during the late 60's coincides with their golden age : using a priori the simplest assumptions concerning the astrophysical input parameters and the set of freshly obtained atomic data, the models computed simultaneously by several authors were at once able to explain the most important features of the line spectra of two representative planetaries, namely NGC 7662, a high-excitation matter-bounded nebula, and IC 418, a low-excitation ionization-bounded nebula. Because of the apparent coarseness of the assumptions, the weakness of the low-excitation lines, such as [OII]3727 in NGC 7662 or [OI] 6300 in IC 418, as compared to observation, was not yet considered as a serious problem and, in the euphory of this success was born a credo, whose most achieved expression is probably due to Flower (1968) : "It is believed that the atomic data used are reliable and that the computational methods employed by the computer programmes are sufficiently accurate so that discrepancies between theory and observation are essentially a consequence of invalid physical assumptions". In the context, "physical" excludes atomic physics and refers to any mechanism or feature of "astrophysical" significance not considered in calculations. Stated otherwise, the differences between models and observations are supposed to provide information about the structure of the object under study.

B. 1972 : A confusing situation.

Then what were the findings ? In the framework of static models the only way to increase the concentration of low-charge ions at a given distance to the source is to increase the gas density or consider a zone in which the radiation field is attenuated by shielding. This is the origin of the well-know concepts of "clumping", "filling factor", optically thick "globules" and "shadows".

In its crudest version the concept of clumping means that, on a small scale (that is sufficiently small to be undetectable), the *ionized* gas fills only a *tiny* fraction of the whole volume. This concept would deserve a serious theoretical elaboration to be acceptable on hydrodynamical grounds since the overall expansion velocity of planetary nebulae is usually not so much greater than the thermal velocity. The observational evidences for the existence of inhomogeneities are scarce and usually qualitative and refer to "intermediate-scale" (that is an appreciable fraction of the whole nebula) rather than small-scale structures. These structures being only detectable in the light of low-excitation ions, this was taken erroneously as confirming the inference of clumping from models. On the contrary, this only indicates that *the ionized gas, as depicted by the best tracer, namely Hβ , is quite evenly distributed.*

An outstanding example of small-scale structure is the thin radial filaments best observed at the [NII] wavelength in NGC 7293 (Vorontsov-Vel'yaminov, 1968). This was enough to support one of the most fashionable (and perhaps still alive) way to amplify the low-excitation lines : the head of a filament would be the ionization front of an optically thick globule whose shadow would form the filamentary tail containing low-charged ions (Van Blerkom and Arny, 1972 ; Kirkpatrick, 1972). Although it looks quite attractive, this proposal is not substantiated on closer examination (Hummer and Seaton, 1973 ; Mathis, 1976) because the energy supply of these shadows by the diffuse radiation from the unshielded zone is not adequate in quantity (a small amount of mass can be kept ionized) and quality (the radiation is so soft that the temperature remains low and the emissivity per atom of, e.g., [OII]3727 is divided by 25 from T = 12 000 K to T = 6 000 K). Other critics to this interpretation can be made after detailed inspection of the photographs (Mathews, 1978). In the radiation dominated situation prevailing in this nebula, other energy supplies, such as conduction or turbulence dissipation seem even less likely and one could hardly imagine how shock-wave propagation might be sustained and efficiently heat the ionized gas of the filaments. Thus these filaments, which were considered for a long time as the best observational support to the explanation of the low-excitation line intensities by means of high density condensations, may well constitute the best observational evidence that many other suggestions, made for other nebulae, do not work ; in particular these filaments are not shadows, but they are evidently not ionization fronts either. Now, taking as granted that the gas of the filaments is normally ionized and heated by the star radiation, the enhancement of the low-excitation lines implies that the filaments are denser than their surroundings and the initial assumption of (more or less) optically *thin* condensations must again be considered. However the hydrodynamical objections too remain and the only way round this apparent contradiction is to assume that the filaments constitute trailing flows with *small* density enhancements, which, according to the models constructed (for other nebulae) in the early 70's, were unable to reconcile the low-excitation lines with observation.

This sketchy discussion (not performed at that time) already tends to suggest that the problem -the so-called "[OII] problem" of the late

70's - may not be solved by any satisfactory choice of astrophysical parameters. The models exhibited in the early 70's were apparently as much successes in explaining the low-excitation lines of NGC 7662 or similar objects but they neglected the physical content of the astrophysical parameters as well as the guidance of some fragil but significant morphological data (Aller, 1956 ; Vorontsov-Vel'yaminov, 1968), too hastily classified as corroborating the theory.

In their review of 1973, Hummer and Seaton already emphasized the acuteness of the difficulty but the situation was very confused and they concluded that "the question of the temperature of the transition region is one of the major unsolved problems".

C. 1973 : A message.

The introduction by Williams (1973) of the resonant charge exchange reaction

$$O^+ + H^o \longleftrightarrow O^o + H^+ \qquad (1)$$

in model nebulae was a very significant stage because it suddenly allowed to explain the [OI] line intensity in, e.g., IC 418 and therefore to demonstrate that a new atomic physics process could prosaically but efficiently resolve an "astrophysical" problem[2]. This (provisionally ?) ends interesting speculations about the existence of shockwaves preceeding the ionization front in compact planetaries.

The significance of this "message" was not fully appreciated and the pursuit of astrophysical facts hidden behind discrepancies continued.

D. 1976 : The stagnation.

The success of Williams concealed negative repercussions in that the gain in O° concentration within the ionized gas was obtained at the expense of O^+ so that the [OII] line intensity became more evidently and systematically unaccountable, particulary in high-excitation radiation-bounded planetaries such as NGC 7027. Moreover the early guess that a larger temperature of the transition region could solve the problem lost its last trace of credibility since, in such a case, [OI] would be amplified in a larger proportion than [OII] in view of the spatial distribution of the ions.

Taken together, the success of §C and the rationalizations of §§B and D irresistibly suggest that the question was one of ionization equilibrium of the oxygen ions. The successive attempts with either the gas density distribution or the central star spectrum were not specially convincing but a latent period seemed necessary before leaving the impasse. Some of the causes and consequences of this situation may be as follows.

Excessive attention was focused on a few objects such as NGC 7662, which was then attributed two contradictory roles. On one hand the case is taken as representative and it is sufficient to explain its spectrum to conclude that the modelling (not only the model) is satisfactory. On the other hand the case is particular and it is evidently allowed to take into account the specificity of the object : this specificity is indeed what we are supposed to look for and it results from an analysis which amounts to eliminate the discrepancies with observation. However there is a priori no clear limit to the "plasticity" of the free parameters and, with the presupposition of correct atomic physics (and correct observations), the largest difficulties to fit the data lead invariably to the most fascinating results on the condition that a model can finally be exhibited. If the most extreme caution is not exercised, an unconscious shift towards the most "gratifying" cases, namely those which put up a credible resistance and finally give in, is to be feared. It is very unfortunate that NGC 7662, in all likelihood, belonged to this category at least during the 70's and was also taken as an archetype.

The fact that the models were considered as satisfactory in explaining "typical" nebulae had at least three nefast consequences. Firstly the specialists in atomic physics were not on the look-out for discovering new processes since it is never gratifying to quibble the decimals of a closed matter. Secondly the status of the planetaries which resisted modelling was dramatically changed and it is by no means exaggerated to state that they were most often classified as monsters deprived of real interest (at least for modelling) : in such cases the discrepancies were not eliminated but their existence was either "justified" by a ritual (and platonic) recourse to an inextricably complex geometry or "promizingly explained" by an imaginative (and no less platonic) recourse to exotic mechanisms which had a marked tendency to mimic those proposed to solve the very similar problems faced by very similar models in very different contexts. And actually the third consequence was to give support to the conclusions drawn from the successes and reverses of the photoionized models when applied to active nuclei of galaxies and supernova remnants : no doubt that some of the exotic aspects of these objects during the 70's were due to overinterpretation of the weakness of the models.

At least one positive consequence of considering the subject of static models as a closed matter was to direct attention to other studies (see Section 2 ; Harrington, 1978 ; Harrington, this volume) whose methodology will certainly prove useful in the future.

E. 1977 : The charge exchange reactions.

The previous comments justify a change of perspective. The archetype among monsters was NGC 7027. This high-excitation planetary nebula has every reasons to be radiation bounded. Since the density distribution was considered as the stumbling block, two remarks seem in order : (1) the nebula is obscured by dust but it appears fairly symmetrical at radio wavelengths; (2) a full development of hypothetical condensations may not be expected in this moderately expanded nebula and, at any rate, optically thick condensations would just produce fluctuations in the position of the ionization front.

the ionization equilibrium is governed by the equation :

$$n(O^{++}) \,/\, n(O^{+}) = \zeta / \alpha n_e$$

where ζ is the photoionization rate and αn_e the (electron) recombination rate of O^+ ; (2) from the angular size of the nebula and the (de-reddened) flux of $H\beta$ and He II 4686, a quite accurate - indeed very large - ζ can be computed, which depends neither on the distance of the nebula nor on the effective temperature of the star (taking as granted that this temperature is very high) ; (3) the absorption of radiation by the inner zones of the nebula has a moderate and late effect on ζ because the gas is particularly transparent in the energy range 31.1 ev (potential of O^+) - 54.4 ev (potential of He^+) because of the rapid decline of the cross-section of H° and the virtual absence of He° ; (4) when finally ζ is significantly decreased, the photoionization rate of H° is already depleted and the edge of the Stromgren sphere is virtually reached so that the zone where O^+ can coexist with free electrons is exceedingly small ; (5) the O^+ ions are also absent when hydrogen is mainly neutral because of the reaction between O^+ and H° ; (6) assuming uniform distribution, the mean density of the nebula is much above the critical density for collisional de-excitation of the upper level of [O II] 3727 so that the computed intensity is independent of the assumed density distribution (the relative abundance of O^+ is roughly proportional to n_e while the relative efficiency of the line emission is inversely proportional to n_e) ; (7) in the simple case of a radiation-bounded model with constant density, the intensity of [O II] 3727 is one tenth the observed one when [O III] 5007 is correct ; (8) the difficulty is thus fundamental, especially since it exists also in Seyfert 2 galaxies, and the solution probably deals with atomic physics ; (9) but for oxygen the atomic data involving free electron collisions or photons are among the best known ; (10) thus until a very powerful new mechanism is proposed to selectively amplify [O II] , a complementary mechanism must exist to recombine O^{++} into O^+ : since, in addition, the discrepancy is more serious in high-excitation objects,where O^{++} and H° are known to better coexist, it is tempting to attribute this extra recombination to charge exchanges between O^{++} and H°.

Other less blinding but still evident discrepancies existed. Thus Péquignot, Aldrovandi and Stasinska (1978) adopted the following exploratory point of view : (1) the density distribution is spherically symmetrical and is the simplest one compatible with density indicators, (2) the central star has a standard spectrum, (3) the classical atomic data are accurate, (4) the discrepancies between observation and model calculations that can be resolved by modifying the ionization equilibria are systematically attributed to charge exchange reactions with atomic hydrogen and (5) the empirical rate coefficients derived for each reaction are open to test by performing atomic physics calculations and by modelling other nebulae.

In 1977, the charge exchange rates were, for the most part, known qualitatively by means of the asymptotic Landau-Zener rule (Dalgarno and Butler, 1978). By a strange coincidence this rule erroneously predicted a negligible rate for the kee reaction

$$O^{++} + H° \longrightarrow O^{+} + H^{+}$$

and this may have cast some discredit on the principle of this approach

(approach (2d) of §3). By now most empirical rates (Péquignot, 1980) are in rather good quantitative agreement with the theoretical rates (see the fundamental papers : Butler, Heil and Dalgarno, 1980 ; Butler and Dalgarno, 1980a). This indicates that many other atomic data are probably quite reliable and that, for the time being, a good working hypothesis is that the planetary nebulae are not more complex than what was believed before inventing models.

However the situation is not completely satisfactory for :
1) The reaction Ne^{++} + H° does not exist contrary to the empirical result so that the line [Ne II] 12.8μ is not explained.
2) If [C I] 9849 and [S I] 7725 are collisionally excited, there is not enough C° and S° in the ionized zone (Butler and Dalgarno, 1980b).
3) The empirical rates involving multiply charged ions were obtained assuming that the next ions (then not observed) were free from extremely fast charge exchange reactions with H° : this is apparently not the case for, e.g., the important reaction O^{+3} + H° and the excellent agreement for O^{++} and N^{++} + H° may be partly fortuitous.
4) Several other moderate discrepancies between observation and recent (unpublished) models are apparent.

Does it mean that approach (2d) already reached its limits and that it is time to go back to the comforting position (2c) ? More accurate observations are becoming available and more systematic modellings are in progress with many improved atomic data so that a new interesting confront may be expected in a near future. We are free to dream that some discrepancies will survive to tell us something about the physical processes at work in planetary nebulae.

F. 1981 : The low-temperature di-electronic recombinations.

From a theoretical standpoint it was not fully realized to which extent the models - during the 70's - were essentially a check of ionization equilibria[3]. Our ignorance about the collisional processes governing ionization was such that we needed ten years to understand that these processes were at the origin of the failure of the models. In the case of charge exchange reactions an attentive inspection of past results in physics might have lead to consider them since the construction of the first models (see Note 2). This is not the case for the "low-temperature" di-electronic recombinations recently introduced by Storey (1981). A complete table of rate coefficients is not yet available (Storey, this volume) but we already know that this is a new crucial ingredient of models since, for selected ions, the classical radiative recombinations can almost be looked on as perturbations !

Up to very recently, this was probably not much a trouble in the comparison of models to observation because this process seems more effective for ions not easily detected with ground base observations. And it may not be pure coincidence if this process was discovered soon after the event of detailed UV observations (Seaton, this volume). Nonetheless the vanity of the abundance corrections based on theoretical models can be appreciated.

CONCLUDING REMARKS

A model in physics is usually a reasoned simplification of reality intended to make easier the description or the understanding of a particular process and it is evaluated by the richness and exactness of its consequences. Now the (astrophysical) outputs of a model - the "results"- can hardly be considered as "predictions" because no experiment can be performed with the true object and, in a sense, the model is the only reality to handle.

The current models certainly appear very coarse when compared to photographs of most planetaries but the aim of these models is not predominantly to give faithful monochromatic images of nebulae with all their anecdotal accidents (even though it may be the case in the future); rather, models intend to account for observed line spectra with exact physics and thus try to represent these "accidents" in a physically realistic but geometrically schematic manner. Thus how and to which extent the structure of planetary nebulae should be considered as complex is a question that typically falls within the scope of modelling and that should not be stated a priori from superficial inspection of beautiful but qualitative and purposedly exposed photographs. In this connexion it should be stressed that the assumption of overall spherical symmetry (used globally or just locally to compute the diffuse radiation field) has often less consequences on the value of the results than one generally believes and, in most cases, attention should rather focus on the postulates concerning the small scale density distribution. Important developments are expected if monochromatic electronographic images of high angular resolution can be obtained (N.K. Reay, this volume).

In perspective (2d), the intention is clearly not to compete with atomic physics but to draw attention to challenging problems in a suggestive and quantitative manner, namely by giving some sort of "equivalence" between a discrepancy with observation and the correction to atomic data that would be needed to resolve it under the assumption that other astrophysical parameters, e.g., the stellar flux distribution or the gas density distribution, are not particularly exotic when there is no good reason to beleive that they should be. Then the model recovers its prerogatives : a view of the object is proposed and is open to the best possible test, namely that of atomic physics. Evidently only those empirical revisions are allowed that seems to "reasonably" fall within current precision of the atomic data in question and that are sufficiently well motivated on astrophysical grounds. If and only if atomic physics and observation maintain their positions are we founded to consider new physical processes or astrophysical particularities.

The pre-eminence of atomic physics is not disputed but strengthened, as it should, in this approach. As we have seen, many of the "astrophysical discoveries" made by means of models in the domains where they are essential were spurious as a consequence of inadequacies of atomic data and it is time to acknowledge this misfortune of astronomers : a planetary nebula is indeed such a good atomic physics laboratory that it is still extremely difficult to do something else with it ! If approach (2c) was a reasonable first guess, approach (2d) is necessary at least as an intermediate step which will eventually make easier a return to (2c) on sounder basis.

The freedom of models is partly a myth when good and numerous observations are available and real tests of consistency are possible in specific cases. The impression that almost everything can be fitted is often due to the arbitrary choice of the astrophysical parameters when they are taken as formal and completely disposable regardless of the physics they are laden with.

On reflexion a self-consistent model of a nebula may not (or should not) be much more than a super plasma-diagnostic, even so have it to be properly calibrated to act this role. The extreme view which rejects any use of models "before the completion of atomic physics" is evidently absurd and position (2d) appears as a possible resort to help in obtaining astrophysical facts rather than atomic physics artefacts.

The massive supply of UV observations and of new or accurate atomic data prompted a renewal of models. Concerning the use of models for practical purpose, there is certainly a shift of interest ; thus the abundance corrections for unseen ions of C, N, O and Ne are now less crucial than an accurate determination of the temperature in the high-excitation zones. However the theoretical situation is not unlike the one of 1968 in several respects. Firstly the atomic data for heavier elements are not yet well circonscribed, not to say that they are often rudimentary in the case of iron in spite of outstanding efforts, and one must be prepared to face, once again, problems of ionization equilibrium with however the help of lighter elements which can calibrate the astrophysical parameters. Secondly the question of the lighter elements themselves cannot be considered as settled if one pretends to use the models to learn something more than abundances about the "astrophysics" of planetary nebulae. Most of the fascinating ideas debated during the 70's have sufficiently good physical basis that some of them will one day revive, probably in the form of "second order perturbations" to the observables. The past 10 years experience with models should be meditated in order not to fall pitifully into the same pitfalls. The risk does exist since, apart from uncompletedness, many coefficients are not guaranteed to better than a factor two by Mc Carroll and Storey and the recent results presented by Mendoza invite to prudence.

NOTES

1. The survival of such filaments almost certainly implies the existence of collapsed neutral "cores", possibly at a temperature not exceeding a few hundred Kelvin, in rough pressure equilibrium with their surroundings. The detection of the H_2 molecule, e.g. in NGC 6720 (Beckwith et al, 1978), provides support to this view and indicates that the case of NGC 7293 is probably not unique. The filaments have often been compared to comets.

This analogy may well be much deeper than would be suggested by some early interpretations : as an example, it may be that the cold neutral core (the "nucleus") intercepts only a very small fraction of the stellar radiation so that the bulk of the emission arises from some type of large matter-bounded "head" surrounding the (presumably evaporating) core and prolonged by a trailing tail. The detailed analysis of these remarkable structures has not yet been undertaken possibly because it was hampered by the pending question of the low-excitation lines.

2. In fact reaction (1) was invoked as early as 1956 by Chamberlain in a slightly different context. After being forgotten for twenty years, we should acknowledge the extraordinary insight of Chamberlain who resolved an astrophysical dilemma, put forward one year before, by means of an atomic physics theory, discovered one year before, and who even speculates about the possible relevance for nebulae of charge exchange reactions between atomic hydrogen and multiply charged ions.

3. In particular the unavoidable freedom on the elemental abundances allowed to account quite easily for the few and relatively inaccurate informations about temperature. In fact a correct account of both the temperature indicators and the strongest line intensity (usually [O III])demonstrated that the main energy source of planetary nebulae was indeed photoionization by the central star radiation and that the relevant collision strengths were reasonably accurate.

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DOPITA: I worry about the $O^{++} + H \rightarrow O^{+} + H^{+}$ charge exchange rate, as published. My models for either "power law" excited regions or H II regions have the [O III] lines much too weak compared with the [O II] lines. How confident are you that the rate coefficient you use is not too large?

PÉQUIGNOT: To date, I did not experience this problem in my own calculations for fairly well-defined nebulae. On the contrary, [O II] tends to be too weak relative to [O III] in the most recent studies. Thus, I have no reason to suspect an overestimate of the rate coefficient in this case (even though the theoretical or empirical estimates may not be more accurate than a factor 2 to 3). I am afraid that dielectronic recombination of O^{++}, discussed by Storey at this meeting, could worsen your problem.

KÖPPEN: My own work and that of Aller on fitting the spectra of a fairly large number of PN show that we now have the problem that the models predict [O II] λ 3727 to be stronger than observed!