# SYNTHESIS OF LINE PROFILES FROM MODELS OF STRUCTURED WINDS

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**Abstract.** On the basis of a a careful analysis of resonance line formation (both for singlets and doublets) in structured winds, present *time dependent* models of the line driven winds of hot stars (Owocki et al., this volume; Feldmeier, this volume) are shown to be able to explain a number of observational features with respect to variability and structure: they are (in principle) able to reproduce the *black* and *broad* troughs (without any artificial "turbulence velocity") and the "blue edge variability" observed in saturated resonance lines; they might explain the "long lived narrow absorption components" often observed in unsaturated lines at high velocities; they predict a relation between the "edge velocity" of UV-lines and the radiation temperature of the observed X-ray emission.

As a first example of the extent to which theoretical models can be constrained by comparisons between observations and profiles calculated by spectrum synthesis from structured winds, we show here that models with deep-seated onset of structure formation ($\gtrsim 1.1R_*$) produce resonance lines which agree *qualitatively* with observational findings; in contrast, the here presented models with structure formation only well out in the wind ($\gtrsim 1.6R_*$) fail in this respect.

**Key words:** Line: formation – Stars: atmospheres – Stars: early type – Stars: mass-loss

## 1. Introduction: Stationary vs. Time-Dependent Wind Models

Over the last two decades, it has generally been established that the driving mechanism for OB star winds is the line scattering of the stellar UV radiation field by heavy ions in the expanding atmosphere. Building upon the pioneering work by Lucy & Solomon (1970), Castor, Abbott & Klein (1975; hereafter CAK) and Abbott (1980, 1982), current models now solve self-consistently the *time-independent* hydrodynamics (Pauldrach, Puls & Kudritzki 1986; see also Friend & Abbott 1986) together with the NLTE equations of state for *all contributing* ions (Pauldrach 1987) and the radiative transfer allowing for multiple scattering (Puls 1987). The present state of the art is described in a recent paper by Pauldrach et al. (1993). Such models are able to reproduce quantitatively numerous observed stationary features for a variety of hot stars of *different metallicity* and *evolutionary status* (cf. Kudritzki et al. 1991 and Puls et al. 1993). Among these are

the product of terminal velocity and time-averaged mass loss rate ($\dot{M} v_\infty$)

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for O-star winds in *different* galaxies (the Galaxy, SMC, LMC, M31) (cf. Kudritzki et al. 1994);

$\dot{M}$ *and* $v_\infty$ when detailed calculations are performed, e.g. for $\zeta$ Pup (O4I(f)) and the LMC star Mk42 (O3If*/WN6-A) (cf. Pauldrach et al. 1993);

the "SiIV luminosity effect" (cf. Walborn & Panek 1984; Drew 1989; Pauldrach et al. 1990);

the optical and IR H/He lines and the observed IR and *thermal* radio excess (cf. Gabler et al. 1989, Sellmaier et al. 1993);

the complete far UV spectrum including the observed "super-ionization", i.e. the presence of high ionization stages (CIV, NV, OVI) in parallel with low ionization stages (CIII, NIII) in a *cool* wind model ($T_e \lesssim T_{eff}$), as well as the large number of "photospheric" (but wind-contaminated!) iron-group lines present in the observations (Pauldrach et al. 1993).

However, as follows directly from the assumption of stationarity, these models are inherently incapable of describing a number of additional observational features, which immediately show that the nonstationary aspects of the wind must be significant. As the majority of these features is discussed in some detail in the present proceedings, they will be listed here only briefly:

the soft **X-ray emission** present in all OB stars (firstly detected by the *EINSTEIN* observatory, cf. Harnden et al. 1979; Seward et al. 1979), where it is now generally assumed that this emission must originate from well above the wind base, most probably from *shocks embedded in the wind* (for a first analysis of the *ROSAT* observations of $\zeta$ Pup, cf. Hillier et al. 1993). The natural extension of this "hot" (some million degrees K) radiation field (small filling factor!) to EUV wavelengths is the supposed source of the observed "superionization" via *direct* ionization (Pauldrach et al. 1993);

the **discrete absorption components** (DACs) often observed in the absorption parts of *unsaturated* lines (Lamers et al., 1982; Prinja & Howarth, 1986; Henrichs, 1988);

the **temporal variability of the wind lines**, where the blue edges vary most significantly, while the red emission part remains relatively constant (e.g. Henrichs, 1991 and Prinja, 1992);

the **electron-scattering wings** of recombination lines in Wolf-Rayet stars, which are weaker than models predict, suggesting the possibility of a clumped structure (Hillier, 1991);

the **black troughs** in saturated P-Cygni profiles (Lucy, 1982, 1983), which in the present stationary description are simulated by a velocity-dependent turbulence (cf. Hamann 1980, 1981; Puls 1987; Groenewegen et al. 1989; Groenewegen & Lamers 1989);

the **variability of optical lines** such as $H_\alpha$ (Ebbets, 1982), HeII 4686 (Grady et al. 1983; Henrichs 1991) and HeI 5876 (Fullerton et al. 1992);

the **nonthermal radio emission** observed from roughly 30% of massive stars (cf. Bieging et al. 1989).

Altogether these observational properties suggest that these winds have a significant degree of spatial structure and temporal variability. Theoretical efforts to understand the nature and origin of this structure have generally focused on the line-driving mechanism itself, which linear stability analyses have shown to be highly unstable (MacGregor et al. 1979; Carlberg 1980; Abbott 1980; Lucy 1984; Owocki & Rybicki 1984, 1985). Subsequent work has concentrated on dynamical modelling of the time-dependent wind structure from direct, numerical simulation of the nonlinear evolution of the line-driven flow instability (for references and recent reviews see Owocki 1992 and this volume), where present effort concentrates on the correct consideration of the *energy balance* (Feldmeier, 1993 and this volume; Cooper, this volume) and the calculation of the emitted X-ray spectra (Cooper & Owocki 1992 and this volume).

Although the resulting *spatial* variation of the velocity and density seems to be in stark contrast to the stationary picture (cf. Fig. 1), the *mass* distribution of these quantities are not so very different (Owocki, Castor & Rybicki 1988; Owocki 1990, 1992). Given the intrinsic mass-weighting of spectral formation, and the extensive temporal and spatial averaging involved, it thus seems quite possible that, in some average sense, the observational signatures of such structured models may actually be quite similar to what is derived from the stationary approach.

However, with respect to the present assumptions and approximations (influence and strength of stabilization due to the well-known "line drag" effect [Lucy 1984]; triggering mechanism at the wind base), a *variety* of different structures still seems to be possible (location of onset of structure formation, shock strengths, separation of shells, density contrast etc., cf. Fig. 1). Hence, in order to prove how far the observational signatures of structured models *actually* compare to observations and to provide constraints from the observations for the models, a major effort is needed in the spectral synthesis from these structured models. The following sections discuss the formation of UV resonance lines with special emphasis on the aspects of black (and broad) trough creation and variability, where we will briefly summarize the results obtained by Puls, Owocki & Fullerton (1993, hereafter POF) and present some recent work and implications (§ 3).

## 2. Resonance Line Formation in Structured Wind Models

In order to clarify the above problem of spectral formation in structured wind models, we have at first to question how much the transfer in stationary and time-dependent models actually differ. In the case of resonance line transfer (i.e., the opacity scales roughly linearly with density), one has to consider (cf. Fig. 1):

– Transfer effects are dominating only in *distinct* regions (→ shells/clumps), as the intershell medium is extremely rarefied.

– In those regions, the density is larger than in the corresponding stationary models, and the velocity gradient can be *negative.*

– The velocity field is *non-monotonic*, hence we have to account for the *radiative coupling* of different locations with a zero difference of projected velocity.

It is this last item which is the supposed origin of the observed black troughs. Without going into any detail (for which we refer the reader to the analysis by Lucy (1982, 1983) and by POF), we want to give a brief motivation for this astonishing statement. If we assume here that the number N of shocks per $\Delta r$ is large, so that the number of radiatively coupled resonance zones is high, it is relatively easy to show that for an optically thick scattering line the first (starwards) resonance zone reaches a mean line intensity $\bar{J}$ which is twice as large as in a purely local approach (i.e., neglecting any non-local coupling), whereas $\bar{J}$ drops to zero towards the end of this so-called scattering complex. Hence, such a complex (for large N) can be treated as one *back-scattering* zone.

The resulting P-Cygni profile has then the following structure: The blue side is almost completely black, as the observer sees here the *backside* of the scattering complexes. On the other hand, on the red side we see the receding part of the wind and hence the front side of the complexes, so that the profile exhibits roughly double the continuum level.

From Fig. 1, it is obvious that our hydrodynamical simulations show a number of differences to the above extremely simplified model, which influences the quality and strength of the non-local coupling:

The number of radiatively coupled points is *smaller*, as the number of shells covering the same range in velocity is low (N small!). Especially, in the lower wind region the velocities are unique, so that here no coupling occurs. As transfer effects play a rôle primarily inside the shells, which cover only a small spatial extent of the wind, the effective scattering *area* is also small. Finally, due to sphericity and the larger radial separation of the shells, the coupling is present just for a small portion of the complete solid angle, leading again to a diminished effect on the radiation field.

In summary, from our present models we expect a significant *forward* scattering in the P-Cygni absorption part, if compared to the above simplification.

The actual procedure and detailed physics for calculating P-Cygni profiles in structured wind models is extensively discussed by POF. The major challenge hereby is to determine the *consistent source-function* accounting for all radiative couplings, as the shape of the profile at least for saturated lines is determined exclusively by the re-emission process. For reasons of simplicity and in order to be able to check and understand the results carefully, POF considered only the formation of a resonance *singlet* (but cf. § 3) and assumed the line to be pure scattering.

For the "smooth source function" (SSF, Owocki 1991) model of a typical O-type Supergiant discussed by Owocki (1992) – which is very similar to our "Model 1" presented below (Fig. 1) except from a different period of 10,000 s for the triggering sound waves – POF calculated the profiles for both a saturated and a strong, but unsaturated line. For reasons of conciseness and since their results do not differ drastically from the cases discussed below, we will only summarize their results here.

**Saturated Resonance Lines.** The emergent profile of the considered saturated line is given in (POF, Fig. 16), but compares (up to the degree of blackness reached, see below) well to the corresponding profile of "Model 1" (Fig. 3, lowest panel). In this case, a *broad* trough is created without any artificial turbulence! The bluest side of this trough is due to high velocity components in the flow, and its low frequency part stems both from the reduced reemission because of multi-scattering in coupled resonance zones and from a reduction of the effective scattering area (see above). This reduced emission now is in the right direction for reproducing observed profile shapes, which often show just this "extended region of absorption" at low velocities (cf. Groenewegen and Lamers 1989, Fig. 2) and cannot be explained by stationary SEI-fits without assuming unreasonably/unphysically *high "turbulence" velocities near the photosphere.* (For a detailed discussion of this context, see POF).

In contrast to the very sharp edge of the stationary case, the blue edge in the structured model also tends to be quite gradual, implying that there is a range of frequencies over which the wind is only marginally optically thick and originates from the high velocity intershell matter, not necessarily connected with the wind's terminal velocity (cf. Fig. 1).

As a matter of fact, however, in this first simulation no *black* trough could be produced (minimum flux of the order of 20 % of continuum). It is important to emphasize that the particular dynamical parameters chosen by POF were not aimed specifically at producing a high degree of blackness, which was anticipated to be possible for a model with either a larger number of shells (i.e. a higer degree of effective backscattering) and/or a larger outer radial boundary.

By following the temporal evolution of the strong line, both the emission (in agreement to observations!, cf. § 1) and the inner absorption part were found to show only weak evidence of structure and variability (integral effect). On the other hand, significant variations at the *blue edge* were created, which often extended substantially beyond the edge for the stationary model (actually too much in comparison to observations).

**Strong, but Unsaturated Resonance Lines.** By inspection of the time series of a strong, but unsaturated line (POF, Fig. 17), it turned out that the absorption part was extremely variable, whereas the emission part again was almost stationary. Although the propagation of single shells could be followed through the P-Cygni absorption trough, their identification with DACs was speculative because the acceleration of the shells was too fast compared with observational evidence (cf. Prinja et al., 1992; but cf. also Owocki et al., this volume, for the possibility to generate slowly moving shells.)

## 3. Line Synthesis in Models with Different Stuctures

As was pointed out in § 1, our present assumptions and approximations lead to a variety of (theoretical) hydro-structures, so that observational constraints are extremely desirable. Most important here is the question concerning the onset of structure formation, which will have significant influence both on the spectral appearance (e.g width of absorption troughs) and (more indirectly) on the superionized ions. As an example, from the analysis of the OVI-line of $\zeta$ Pup by Pauldrach et al. (1993) it was found that the onset of X-ray emission should occur only from a minimum radius of $\approx 1.5...2R_*$, which is consistent both with the ROSAT data (§ 1) and the analysis by MacFarlane (this volume).

From the theoretical standpoint, this question cannot be decided at present due to the uncertainties concerning the stabilization of the lower wind part both by the line drag (optically thin vs. optically thick source function, see Owocki, this volume) and the thermalization of the driving resonance lines in this region (Feldmeier and Owocki, in preparation), which actually leads to an effective damping of photospheric disturbances. On the other hand, present simulations by Owocki also point to the possibility of a self-excited structure, in which case the onset is well out in the wind.

In order to obtain significant constraints from the observations we started to investigate the spectral signatures of different models in a systematic way. We will present here some first results with respect to resonance lines. For this purpose, one of us (A.F., 1993) calculated a grid of different structures for a model of $\zeta$ Pup (for parameters see Feldmeier, this volume), where the onset of structure formation $R_{min}$ was triggered by the choice of the source function in the SSF-approximation and the amplitude of the photospheric sound waves $\delta\rho/\rho_0$. Fig. 1 shows three different models, where the parameters are (period of sound waves always 5,000 s):

| | | | |
|---|---|---|---|
| Model 1: | optically *thin* SSF, | $\delta\rho/\rho_0 = 0.1$, | $R_{min} \approx 1.1R_*$ |
| Model 2: | optically *thick* SSF, | $\delta\rho/\rho_0 = 0.01$, | $R_{min} \approx 2.4R_*$ |
| Model 3: | optically *thick* SSF, | $\delta\rho/\rho_0 = 0.1$, | $R_{min} \approx 1.6R_*$ |

**Singlet Formation.** Fig. 2 displays the corresponding profiles of a saturated line. Evidently, all models yield an almost black trough (residual

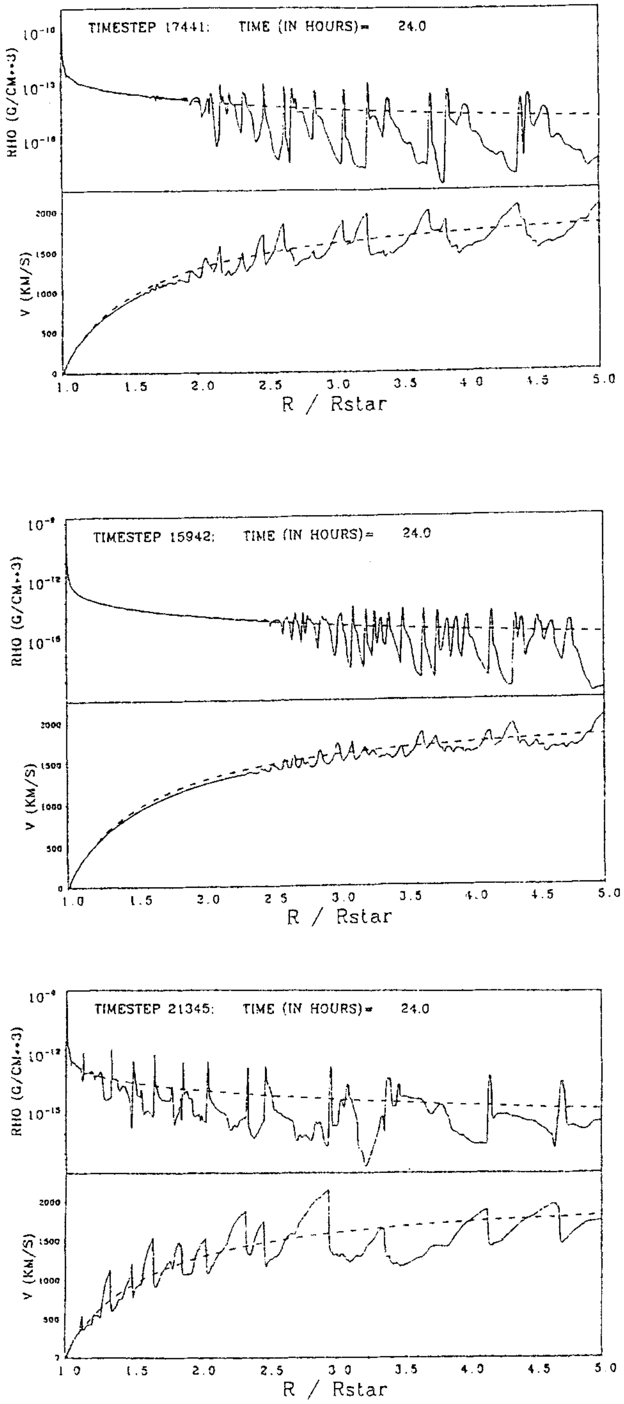

**Fig. 1.** Density- and velocity structure of Model 1 (lower panel), 2 and 3 (upper panel, parameters see text) 24h after model start, compared with the corresponding stationary model (dashed).

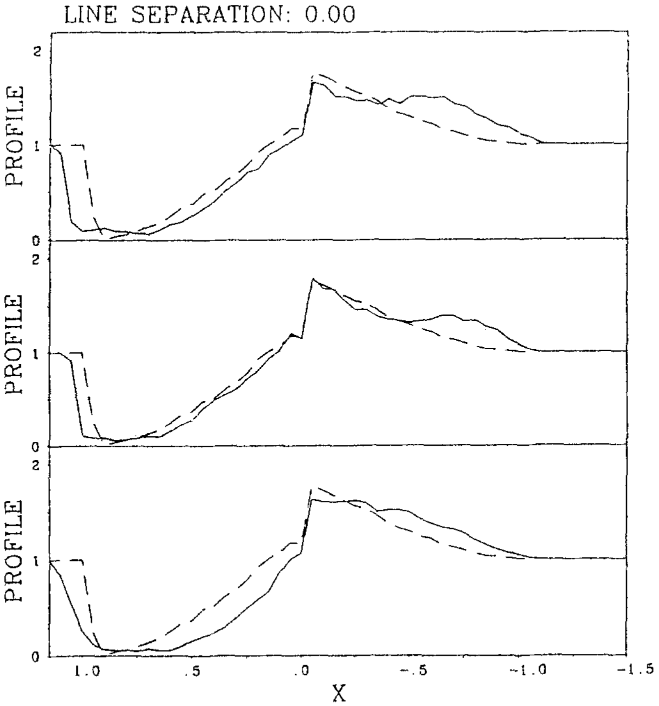

Fig. 2. Profile of a saturated resonance line for Models 1 - 3 (lower to upper panel) in comparison with profile from corresponing stationary model (dashed).

intensity typically $\lesssim 2\%$), where the width of the trough on the blue side is controlled by the amplitude of the high-velocity material, and by the onset of the structure formation on the red side. Inspection of the time-sequences of these profiles indicate that all three models show an almost stationary behaviour, up to the blue edge variability, which is now comparable to observed values ($\approx 10\%$ of $v_\infty$, see Fig. 4). Even for the intermediate line of Model 1, the propagation of any single shell is no longer visible, in contrast to the case considered by POF. (Due to the higher shell frequency, all distinct features are "smeared out".)

The temporal sequence for Model 2 (Fig. 3) is especially interesting. Firstly, the extreme invariability of both the strong and the intermediate line is evident (onset of structure formation at $0.77 v_\infty$ !). Secondly, there is an additional and almost *stationary* absorption feature at $\approx 0.9 v_\infty$ exhibited by the intermediate line, which is created by multiple absorption from a large number of shells in the outer wind part with roughly the same absorbing velocity. This feature is remarkably similar to the "long-lived narrow absorption features" detected frequently in unsaturated lines at high velocities (cf. Prinja & Howarth, 1986, e.g., in the CIV and NV lines of 15

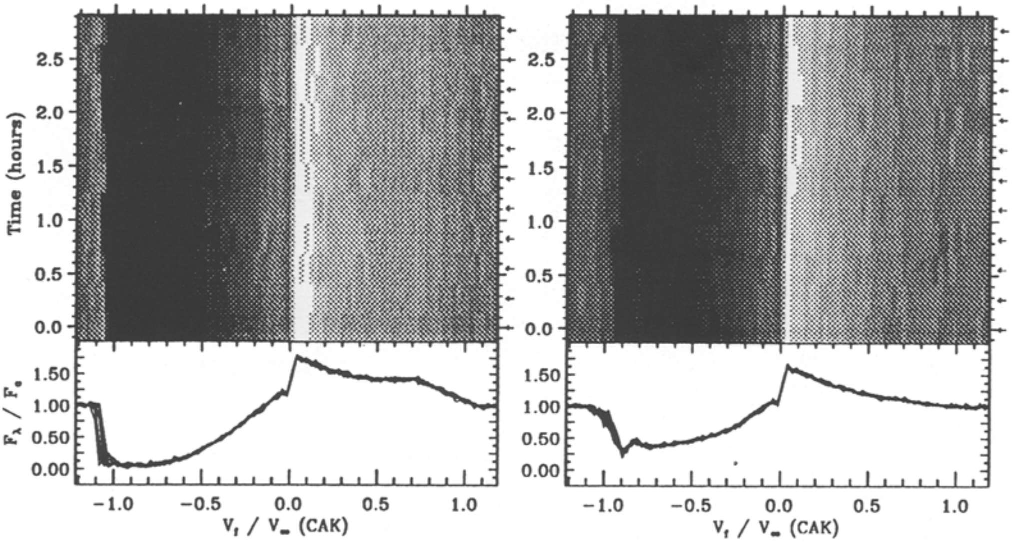

Fig. 3. Temporal sequence of strong (left) and intermediate resonance line for Model 2 (artifical noise with S/N=50 applied).

Mon). We speculate that these features may be explained by such a regular structure in the outer part of the wind.

A direct comparison of our model profiles with observations is unfortunately hampered by the fact that almost all (observable) resonance lines are *doublets*. Nevertheless, at least the SiIV doublet in late O-Supergiants is partly appropriate for this purpose, as both components are separated by 1,930 km/s and saturated at this spectral type. Hence, at least the blue absorption component is not affected by line overlap and can be compared to our computed profiles. This is done in Fig. 4 for the example of $\alpha$ Cam (O9.5Ia), which exhibits an extremely broad trough (in the blue component) of $\approx 550 km/s$. It is obvious, that Model 1 ($R_{min} \approx 1.1 R_*$) fits quite nicely, whereas Model 3 ($R_{min} \approx 2.4 R_*$) exhibits an absorption trough which is much too narrow!

**Implications for X-ray temperatures.** An interesting by-product of our models (independent of singlets/doublets) is the possiblity to derive – *from an analysis of UV-lines* – the radiation temperature of the emitted *X-ray spectrum*, at least for the emission in the intermediate part of the wind and provided that the reverse shock model is correct. In this case, the highest measured absorbing velocity (usually called $v_{edge}$) is just the preshock velocity measured in the stellar rest frame. From Fig. 1 it is then obvious that the difference $v_{edge} - v_\infty$ gives the jump velocity of the reverse shocks in the intermediate wind part. Using the (strong shock) Rankine- Hugoniot relations, we can immediately derive the corresponding shock temperatures.

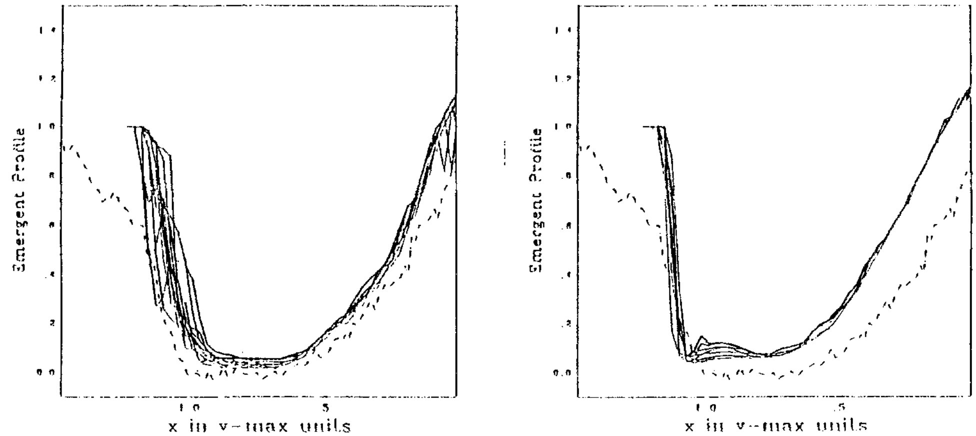

Fig. 4. Comparison of *blue* absorption trough of SiIV ($\alpha$ Cam, dashed-dotted) with temporal sequence of saturated resonance line from Model 1 (left) and Model 3.

As an example, $v_{edge}$ for $\zeta$ Pup of CIV and NV is $\approx 2,700 km/s$, and with $v_\infty \approx 2,200 km/s$ (e.g. Pauldrach et al. 1993) we derive a shock temperature of $3 \cdot 10^6 K$, a value which is exactly the same as found from a *one-component fit* to the *ROSAT* spectrum by Hillier et al. (1993). By a careful analysis of the accumulated *ROSAT* data for a variety of OB-stars, this relation should be further investigated in order to check the reliability of our hydrodynamical scenario.

**Doublet Formation.** In order to be able to compare our results directly to observations, we must consider principally the formation of resonance *doublets* (see above). This is, however, by no means just the problem of "putting together" two singlets, since the presence of a second component introduces a *new quality* to the process. For the basic effects we refer the reader to the analysis by Lucy (1983), who considered the situation in the limit of completely back-scattering complexes (§ 2). The basic effect is the following: Due to the presence of a 2nd component, the photons scattered back by the first (blue) line can be back-scattered again in the 2nd (red) one, so that (at least for not too widely separated lines) more effectively *forward* scattered photons are present compared to the singlet case. Hence, a certain refilling of the absorption part of the P-Cygni profile is to be expected. In order to obtain quantitative results, two of us (U.S. and J.P.) analyzed this process by means of a Monte Carlo simulation. Detailed results will be published in a forthcoming paper; however, the general trend is that just in the interesting domain of line separations $\delta = \Delta\lambda/\lambda\, c/v_\infty \lesssim 0.5$ ($\delta$(CIV) $\approx 0.2$, $\delta$(NV) $\approx 0.4$ for $v_\infty \approx 2{,}000$ km/s) the *refilling is significant.* Fig. 5a displays the corresponding profiles for Model 1 - 3 for $\delta = 0.4$ (cf.

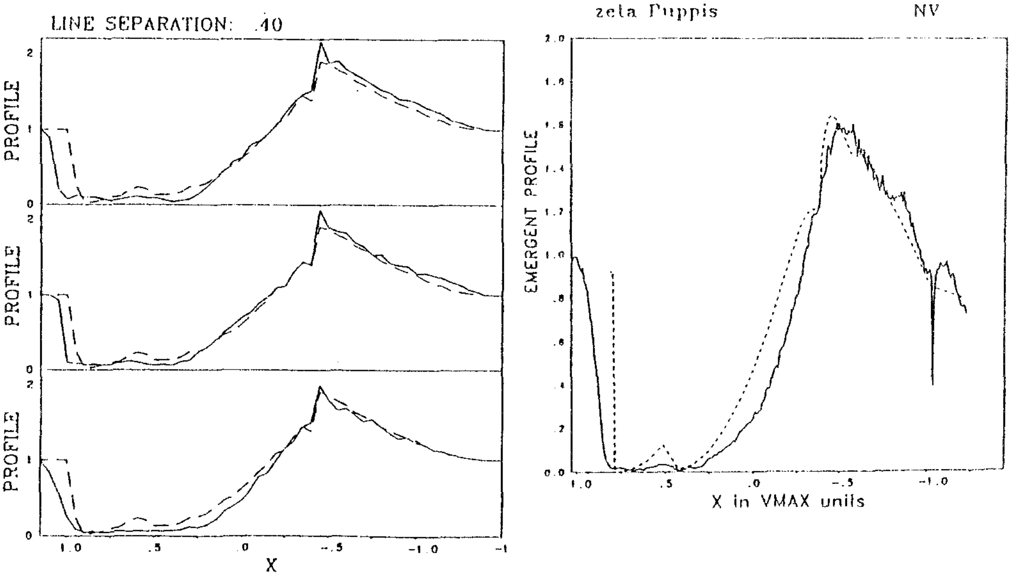

Fig. 5. a) Profile of a saturated resonance doublet ($\delta = 0.4$) for Models 1 - 3 (lower to upper panel) in comparison with profile from corresponing stationary model (dashed).
b) observed NV profile of $\zeta$ Pup and corresponding stationary profile with $v_{turb}= 0$, dashed.(From S.M. Haser, p.c.)

the analogue singlets, Fig. 2). Most important here is the fact that for both models with a late onset of structure formation (Model 2, 3) the trough on the *red* side is now no longer broadened in comparison to the stationary $v_{turb}= 0$ case, which is in no way consistent with the observational findings. This is shown in Fig. 5b for the NV doublet of $\zeta$ Pup ($\delta = 0.43$), which clearly demonstrates that the red side of the trough lies well below the $v_{turb}= 0$ case. In contrast, the model with deep-seated structure (Model 1) is also here able to reproduce the observations. We emphasize that in order to explain the observations consistently, it is necessary not "only" to create a *black* trough, but also a *broad* one, both on the blue *and* red side.

**Some Open Questions.** The above arguments imply that models with significant structure only well out in the wind may be excluded because of their inability to create a *broad* trough and instead favour models with a deep-seated onset of structure formation. However, for a final conclusion an extensive inspection of different models and processes has to be awaited. On the one hand, models with *self-excited* structure formation (then well above the wind base: Owocki, this volume) show a much higher degree of (also higher frequency) structure than models with a *triggered* formation process, which were exclusively considered in the present report. Hence, it may be possible that this higher frequency structure (→ N large, cf. § 2) at least partly compensates for the late onset of formation. Additionally, the sensitivity of $\rho$-squared processes such as $H_\alpha$, IR-lines ($\rightarrow$ *ISO!*) and the IR/Radio continuum to the density contrast of shell and intershell matter ("clumping") will provide a rather tight constraint on any "allowed" structure, unless one is willing to lower all mass loss rates considerably (for some first test calculations, cf. Puls et al. 1993).

Although there are numerous obvious questions related to the above discussed problems, e.g.: – the origin of the migrating DACs and their presumed connection with stellar rotation; the triggering mechanism and its dependence on spectral type; triggered vs. intrinsic instability; shell vs. (at least 2-D) blob structure –, we want to mention one final point which has to be clarified as soon as possible. The current generation of time-dependent models with their typical *reverse shock structure* relies almost completely on the *anti-correlation* of velocity and density phase, which is intrinsic to the present SSF formulation of the line force. However, as shown by Owocki and Rybicki (1985), a consistent treatment of the diffuse radiation field (i.e., which also considers the reaction of this quantity to perturbations) might lead to certain modifications. In this case, the phase relation at least for moderate and small perturbation wavelengths changes and velocity and density will become *correlated*, hence also favouring the formation of *forward* shocks in the non-linear regime. The extent to which this process might change the overall structure of present time-dependent simulations has to be carefully investigated by means of applied *radiative transfer in structured winds.*

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# DISCUSSION

**Howarth**: The black profiles looked as if they had flat bottoms, but they weren't completely black.

**Puls**: Really, that's well observed. The difference between blackness and these, our present models, is, I would say, of the order of three percent. So, if you would have a bit more structure, you would get complete blackness. But you need the structure mainly outside, because it's observed at high velocities and the conclusion is simply that if you have the small [structure] going to ten stellar radii or so, I bet you would get the profiles black. If you have sort of other models, as Stan is presently calculating, with a lot of small scale structure also inside, you get them also black. So the actual question is: It's very good news that you get the profiles black, but I think it's much more interesting that you get them *broad.*

**Howarth**: Also, at zero velocity, it looks as if still quite often you have this Sobolev "jump" between the absorption and the emission part.

**Puls**: Yes.

**Howarth**: Is that a problem or not?

**Puls**: I don't know, [but] I don't think this is a problem. We have to discuss this later. But I will think about it. OK, that's a good point.

**Kaper**: I have a remark from the observational point of view. For some stars, like 10 Lac for instance, you have some resonance lines, which, if you didn't know the intensity scale, you would say are very broad and saturated. But if you would see it in intensity, then you see it is (I don't remember the flux numbers per second) above zero intensity.

**Heap**: I want to confirm what Lex says. Figure 3 of my paper shows the profile of CIV 1550 in the spectrum of 10 Lac. The absorption trough is absolutely flat but it has a residual intensity of 25 percent! The observation was obtained with the GHRS holographic gratings, so there is no possibility that gratting scatter has filled in the absorption trough.

**Puls**: This is another very important point. I pointed this out also in Munich. I don't know how difficult it is really to reduce the spectra correctly. But the degree of blackness may be a really important problem. So what you see here from the observations done by Morton and Underhill, in this very high resolution mode of Copernicus (when you take the values literally), is that there is a very small offset of the order of 0.5 percent or so.

**Kaper**: You already pointed out that what is also very remarkable in some of the O star ultraviolet spectra is this what I call persistent component, like in 15 Mon. You also have this similar structure in spectra of $\lambda$ Ori, and in some time series I have seen that indeed there are things moving towards it. I've never seen things moving through it, really. But maybe Ian and Raman have.

**Puls**: Yes.

**Prinja**: It's quite difficult to see it.

**Kaper**: Yes, it's very hard because, of course, low intensity gives also low signal-to-noise.

**Prinja**: With IUE, it's too difficult.

**Puls**: When you just have this constant feature in some way, what goes through it are all these small shells. You would need really a very high resolution to find this–and, I think,

[high] temporal resolution and signal-to-noise as well. Maybe that's just a feature of our present models, but that it is present requires that you have sort of more regular structure in the outer wind part. We don't know at present, to what extent this feature corresponds to reality, but all our present calculations imply that in the outer part the structure looks more regular. If you have more regular structure outside, you will have this additional feature here and maybe it helps us in the explanation of the DACs, and maybe it makes life a bit easier, I would say.

**Howarth**: There's such a feature in $\zeta$ Oph for sure.

**Kaper**: I have a question about these scattering lines. The fact that this emission rises when you get blackness is just the conservation of photons, I suppose. And you made this remark in your paper, that you had some problems with the conservation of photons, but isn't it...

**Puls**: No. no, no. It was never [a problem]. It was just mentioned in our paper [Puls, Owocki & Fullerton 1993]. In this paper, we investigated three different approximations to get more physical insight. And we had one consistent treatment. It was just pointed out that [the formal integral] is also a good method to check the photon conservation. Then in all our *consistent* models, photon number was perfectly conserved; only in our *toy* models, it was not.

**Kaper**: Okay. Thank-you.

**Hubeny**: In your hydro simulations–

**Puls**: Achim's.

**Hubeny**: –you don't have any microturbulence at all?

**Puls**: No, no, no microturbulence. Nothing. I mean, that's important news. You don't need it. And you don't want it.

**Hubeny**: Oh, sure. And a technical detail: Why do you still need Monte Carlo? Because I would have guessed if you have anyhow several structures, you should [already be solving for multiple resonances], even for singlets. So what makes the difference if you need it?

**Puls**: You want to compare to the observations.

**Owocki**: Why didn't you just use generalizations of the original POF code?

**Puls**: Ah. Why did I do it with Monte Carlo? There's simply one answer. Because Monte Carlo is much more effective for this problem, and in some way, [for] the doublet formation, a bit easier. You always have a large number of coupled resonance zones. If you double the number of resonance zones, you have to solve for certain matrices and that makes life–rather, computing time–expensive. So with Monte Carlo, we reproduced all our analytic singlet results. We actually used our analytical treatment to understand things, because with Monte Carlo it is always sort of difficult. But if you have understood all the basic physics, then you can go back to Monte Carlo, since it's more efficient.

**Lamers**: Did you include in your calculations of the line profile the fact that there may be an underlying photospheric line?

**Puls**: We didn't include it–this is the reason why I didn't overlay the observed spectra with the theoretical one. These results are brand new, and although it's only one [program] statement to include this, I haven't had the time. [But I agree], it's really important. This can give additional bias.

**Lamers**: I think that's the answer to Ian's question–why you don't see the red Sobolev jump. If there's any photospheric line at that wavelength, you suppress this.

**Howarth**: It'll shrink a bit, but it won't go away. It doesn't help so much.

**Puls**: But you thermalize also at the inner part. I forgot to mention this. In all our models we always assumed pure scattering. However, there is thermalization at very low velocities.

**Lamers**: Then I have another question. In your fit of $\zeta$ Puppis, it looks very nice throughout most of the spectrum.

**Puls**: You mean, my SEI fit?

**Lamers**: Yes. You didn't fit the phosphorous V lines and they were awfully wrong. [laughter]

**Puls**: Yes, I know this. That's a very good point.

**Lamers**: You didn't mention it, but everybody saw it. [laughter] So next time it's better if you point it out.

**Puls**: OK, good. It's really a good point. When Adi [Pauldrach] and I wrote the first version of this paper [Pauldrach et al. 1994, A& A, **283**, 525], we didn't want to discuss the differences of this P V line *compared to observations*, since the formation of this line itself is a detailed problem, which in our paper was thouroughly explained. However, Rolf [Kudritzki] then told us, "Point this out, that this is a real difference, because people will say, 'Why didn't you explain that very clearly?"' [laughter]

**Owocki**: You should listen to your boss!

**White**: [To Puls] Have you thought about what the effect would be if you made the model not a spherically symmetric model but sort of shuffled things in radial phase? A spherical model is reasonable for the absorption part because there's not that much of the sphere subtended, but not so for the emission part.

**Puls**: Right. It's really important to include these effects. We thought about this last year or so. It will change the topology of the resonance layers, which is necessary for the reconstruction of all these re-emission processes. We planned to calculate this, but I think it will not change the conclusion that one needs more structure downwards in the wind. When you have structure in the upper wind part, you will always obtain these more stationary profiles. So I think my major conclusions will not be affected, but maybe the fits will change qualitatively.

**Owocki**: Directly to that point, I'd like to say that, to the degree that you can get high signal-to-noise observations of the emission part, I think you can constrain the lateral scale, perhaps, of the structures in the wind. Even though it's smaller than the [absorption] component, the [emission] variability that one sees as shells move [through] the wind is still enough that, with good signal-to-noise, you should see it vary. So if you look at those observations and sit on the emission part of the profile and put an upper limit on how much it varies, you have a constraint on the azimuthal size of the structures.

**Massa**: Good luck! [laughter]

**Owocki**: In principle, I'm saying.

**Massa**: I just sent a proposal in this year that got killed; it was to do just that. To try to figure out the solid angles.

**Owocki**: What? For HST?

**Massa**: Yes.

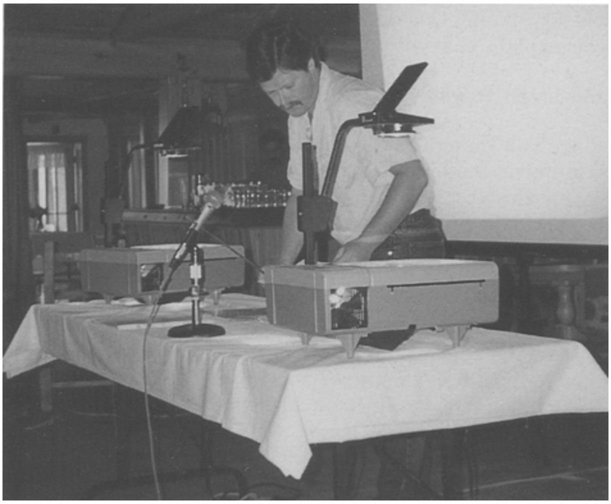
**Owocki**: Well, you know the problems of getting time with HST.

**White**: Well, just one more comment and maybe a suggestion. When you do the Monte Carlo simulations, it might be a useful diagnostic–even though you can't observe it–to look at the spatial distribution of where the photons are coming from as a function of frequency. You may be able to get some physical insight as to what's going on at the blue edge, what's causing the broadening, and so on. For instance, when you have an optically thick line, presumably the continuum from the star is completely absorbed, and all the structure that you're seeing in the absorption trough is emission structure.

**Puls**: Yes, that's definitely it.

**White**: So, it would be interesting to see where that comes from and get a better feeling for it.

**Puls**: I think we did this. As I pointed out, we didn't start with Monte Carlo. In my view, we have a fairly good answer to all these questions in this paper by Stan, Alex and myself. And this we're actually doing also in our Monte Carlo investigations. But that's a good comment.



Joachim Puls