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Why the Binary Hypothesis Isn't a Panacea

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Abstract.
The 5.5-year cycle may lead to a breakthrough in our approach to η Carinae, but a binary interpretation has not achieved that status yet. So far the idea of a long-period companion star has had little impact on Eta's most important, long-standing mysteries. It may even be wrong; its strongest point at this time is a partial explanation of the X-ray behavior (~ 0.01 percent of the total luminosity), while it provides no clear model of the UV and visual-wavelength spectroscopic events. HST and RXTE observations of the 1997–1998 event were largely inconsistent with versions of the hypothesis that had been proposed in 1996–1997.

Here I sketch the current shortcomings of the long-period binary idea, some calculations and improvements that we need to make it really useful, and other viable hypotheses based on either a single star or a short-period binary system.

1. An Ambiguous Situation

A bandwagon careering downhill with great momentum and enthusiastic musicians can be a handsome spectacle, all the more exciting if we know that its wheels are flimsy and its brakes untested. My task here is to show why this meeting's bandwagon — the 5.5-year binary idea for η Carinae — needs major repairs and might even prove unserviceable. At least two little-noted alternative vehicles are available if needed: Eta may be a single star with a geyser-like thermal cycle, or a short-period interacting binary system. The 5.5-year binary idea is most promising to work on now because it seems relatively definite, but it is not well-developed as enthusiasts have implied. Meanwhile the alternatives, albeit vague because they're unexplored, have not been discredited.

In this discussion "primary component" will mean the star whose wind makes the observed broad emission lines. Considering its extremely unusual luminosity and current mass-loss rate, we can reasonably assume that this star was indeed the site of the Great Eruption seen 150 years ago. *The hypothetical companion object has not been detected.* For many background references, see Davidson & Humphreys (1997; hereafter DH97).

Here are five reasons for dissatisfaction with the long-period binary bandwagon; I will explain some of them in a little more detail later.

(1) Its wheels really are flimsy, i.e., supporting evidence is weak, but some advocates have nevertheless portrayed the hypothesis as being well-established. This particular type of exuberance can be a danger sign.

(2) The period is suspiciously long for an interacting binary. When various authors speculated about binary models for η Car before 1996, they envisioned close interacting systems with periods less than half a year. A 5.5-year orbit has an average separation of roughly 16 AU, which is 10 to 40 times larger than the primary star's radius. Tidal interactions or eclipse effects are likely only at separations less than $\sim$ 4 AU, i.e., only near periastron in a highly eccentric orbit if the period is as long as 5.5 years. Altogether, this picture seems more accidental than we'd like for a star that helped to inspire the idea of a general stellar-evolution-limiting instability at the top of the H-R diagram.

(3) Regarding X-rays and the stellar spectrum, which had not been observed well in previous spectroscopic events, the 1997–1998 event did not match the expectations — predictions, almost — that were stated or implied in 1997. Plausible explanations have been suggested afterward, but we cannot honestly credit the binary hypothesis with any successful, specific *predictions*. The data seem consistent with a stellar wind disturbance, whether or not a companion exists.

(4) The 5.5-year binary hypothesis has given only a partial interpretation of the X-ray observations, and is quite vague concerning the UV and visual-wavelength spectroscopic event. Pending serious theoretical work, it remains an *idea* or conjecture, not a full-fledged *model.*

(5) So far, this conjecture has served only with reference to the observations that inspired it. *It has not yet gone beyond them to produce valuable insights into the important and long-standing "η Carinae problem,"* whose central features are the 19th-century Great Eruption and the high, quiescent mass-loss rate. Nor has it helped us to understand the very peculiar Fe II fluorescent excitation mechanisms, strange dynamical structures in the ejecta, etc.

In brief: Damineli's (1996) identification of a 5.5-year cycle is a wonderful development that may lead to a breakthrough in our view of this mysterious object, but the binary idea does not yet constitute such an advance. Meanwhile, a probable 85-day period reported by Corcoran et al. (1997) may be of comparable significance but has not received much attention. With all the above qualms in mind, let me venture a skeptical assessment, emphasizing the need for specific analyses to make the hypothesis and its rivals more useful.

2. Concerning the Orbit

We must constrain the orbit parameters. Last year the most explicit evidence for a long-period binary was a particular "orbit solution," which, however, now appears to have been invalid.

Damineli, Conti, & Lopes (1997; hereafter DCL) identified 5.5-year-periodic wavelength shifts in some emission lines, and ascribed them to the primary star's orbital motion. I used their data to calculate two orbit models (Davidson 1997; hereafter D97). The first had an eccentricity close to 0.7, based only on Paγ and Paδ hydrogen lines as DCL recommended. Since no Paschen-line data were available for the brief time interval during the 1992 event when velocities changed rapidly, I also calculated a second orbit including DCL's He I λ6678 measures at that critical time. Its eccentricity was larger, $e \approx 0.8$, and periastron occurred near the times of past spectroscopic events. This version seemed promising because it allowed tidal interactions at periastron, and was commonly used as a working model in late 1997 and early 1998. Meanwhile, however, Augusto Damineli obtained new hydrogen-line velocity data, which turn out to be inconsistent with the $e \approx 0.8$ orbit! Using his up-to-date set of Paγ and Paδ observations, therefore, I've calculated a new orbit solution sketched in Fig. 1. With e slightly less than 0.7 and periastron passage in April 1998, it closely resembles last year's original Paschen-line solution ("Model 1" in D97).

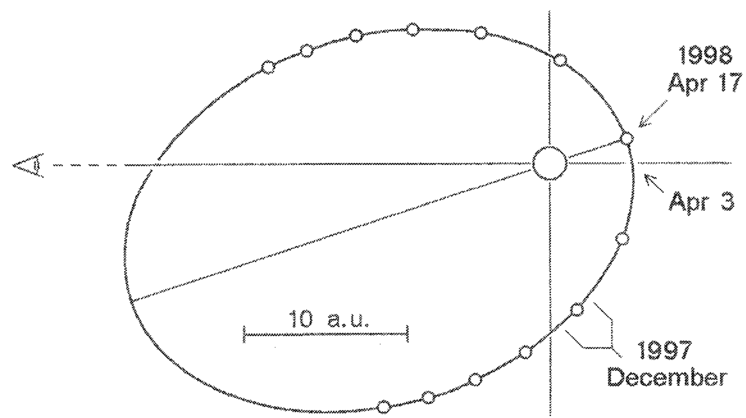

Figure 1. An orbit model based on Damineli's Paγ and Paδ velocities (which represent the primary, not the secondary star). Our viewpoint is on the left side, the large circle is the primary star, and positions of its companion are marked at 60-day intervals. The orbit plane would most likely be tilted $\sim 30°$ from our line of sight. *Caveat:* Reasons to doubt this particular model are given in the text!

Alas, *this orbit is unappealing with reference to the spectroscopic event.* Figure 1 shows no obvious reason for the sudden X-ray collapse in November 1997 (Section 3 below), even less reason for the recovery that started in February, and no evident cause for the effects seen in HST/STIS spectroscopy. During that time the two stars in this orbit would have been too far apart for tidal interactions. Their positions were then unfavorable for an apparent occultation or intra-system eclipse effects. Periastron would have occurred in April, long after the visible spectrum and X-rays had begun to recover. Conceivably the secondary star may have passed through the primary's dense circumstellar disk in November–January; I return to this idea in Section 4 below.

Our recent STIS data strongly suggest that the observed wavelength shifts do not simply represent orbital motion. Recall that the orbit solution in Fig. 1 used ground-based observations, seriously contaminated by bright ejecta within 0''.3 of the star. Since STIS's spatial resolution allows us to avoid the extraneous gas, now we have a larger number of measurable lines, each of higher quality than in any ground-based spectroscopy. These can be supplemented by a few UV Fe II lines observed with HST/GHRS in 1995–1996. Some preliminary results:

(1) The Balmer line peaks changed by only about 10 km s^{-1} between December and March, roughly a third as much as the orbit's prediction. (2) Broad Fe II lines show little or no perceptible wavelength variations in 1995–1998. (3) The March STIS data included Paschen lines beginning with Pa δ. Their apparent velocities increased progressively toward higher members of the series, with Pa θ differing from Pa δ by 50 km s^{-1}. This is almost certainly a radiative transfer effect in the wind, and there is no evident reason why just the third and fourth Paschen lines should represent the star's true motion. (4) Ground-based He I λ6678 wavelengths (DCL) do not match the Pa γ and Pa δ behavior. In summary, among a diverse set of emission lines produced in various zones of the wind, *only* Pa γ and Pa δ seem consistent with the proposed orbit that was based on them. The sawtooth shape of the observed "velocity curve" seems especially appropriate for an explanation based on radiative transfer effects in a cyclically varying wind, unrelated to orbital motion (see D97).

Therefore *we do not know the orbit parameters after all.* This is a major setback for the hypothesis, for two reasons: (1) Until recently, the wavelength variations were cited as the only direct evidence that η Car is indeed a 5.5-year binary. Playing by normal rules, if what is regarded as perhaps the strongest evidence now proves invalid, then we must judge the idea to be weakened. (2) Without a specific orbit, a binary scenario allows theorists to "ascend into free-parameter heaven" as Jay Gallagher once said in a related context. This, also, weakens the hypothesis.

Each proposed orbit based on the observed wavelength shifts came with some unwelcome baggage, namely an implausible type of secondary star. In order to cause the primary to move with the supposed velocities, the mass ratio had to be close to unity, with a present-day mass $\sim 70 M_{\odot}$ for each component. The primary would be alarmingly close to the classical Eddington limit, while its companion would be in a fairly evolved state as explained in DCL and D97. Such an evolved star normally would not have a fast enough wind to explain the high X-ray temperatures (Corcoran et al. 1998). Now, however, if we abandon orbit models based on Paschen velocities, we are free to speculate on a more favorable type of secondary star. My guess is that a relatively normal, not-too-evolved O-type star, e.g., $\sim 30 M_{\odot}$, may be suitable, while the primary is $> 80 M_{\odot}$.

If η Car is a 5.5-year binary, can we establish the orbit parameters? Here are four independent approaches to the question. (1) Absorption lines of a hot companion star may be detectable in the far UV, and would have large orbital velocity shifts. Since existing low-resolution HRS data (Ebbets, Walborn, & Parker 1997) show no features obviously due to a second star, that object, if it exists, must be considerably less luminous than the primary. (2) Models of the X-ray "light curve" during the recent event may give some information (see the next section below). (3) The $\sim$ 85-day recurrence period should vary in a manner that depends on the orbit parameters (Davidson, Ishibashi, & Corcoran 1998). (4) STIS and RXTE data may indirectly constrain the periastron distance. The structure of the wind appears to have changed during the 1997–1998 event, since a "passive" (non-interacting) eclipse or occultation model, advocated in DCL and D97, cannot easily account for the full set of spectroscopic changes that I reported earlier at this conference. The simplest explanation in a binary context is that tidal and/or radiative effects disturbed the wind, requiring a separation less than 4 AU near periastron (D97). With a semi-major axis of roughly 16 AU based on the 5.5-year period, we thereby deduce an eccentricity $e > 0.75$. If this is correct then the orbit shape resembles last year's $e \approx 0.8$ model, except that its orientation (longitude of periastron) may be quite different — which is highly relevant to the X-ray event, point (2) above and Section 3 below.

The total observed length of a spectroscopic event is a serious embarrassment for any highly eccentric orbit model. In each proposed version (with tidal or radiative effects near periastron, or some sort of eclipse, or immersion of one star in the other's circumstellar disk), one expects a "spectroscopic event" to be limited to a time interval of only a few months. Half a year after periastron, for instance, the separation between components has grown larger than 10 AU. (See Fig. 1 where $e \approx 0.7$, and note that the recession would be faster for a more eccentric orbit.) According to Gaviola (1953), Thackeray (1967), and Zanella et al. (1984), however, some effects were still detectable more than six months after the central times of previous spectroscopic events; see Roberta Humphreys' comments elsewhere in this volume. The 5.5-year cycle seen in near-IR photometry, probably representing free-free radiation in the wind, is not concentrated within a few months of an event (Whitelock et al. 1994). So far, at least, *the binary idea has failed to account for a spectroscopic event's duration.*

Evolution of the hypothetical orbit deserves theoretical attention. Could η Car have been a close binary with a roughly circular orbit a few thousand years ago? Imagine, for example, that a giant eruption is triggered at periastron; mass loss at that part of an orbit tends to increase the subsequent apastron distance, causing both eccentricity and period to grow by fractions larger than the fractional mass loss, while the periastron distance is scarcely affected. If, on the other hand, long-term mass loss is not concentrated near periastron, then the periastron distance gradually increases. Because the system is near the Eddington limit, I suspect that mass *transfer* to the secondary is negligible.

3. The X-rays

Hot thermal X-rays, especially observed by Corcoran's group, give the binary scenario its main advantage; indeed, if we didn't know about the X-rays, then a single varying wind would seem better (simpler) for explaining the UV and visual-wavelength spectroscopic events. Note that I say "advantage" rather than "evidence." Colliding winds of two stars can produce an X-ray spectrum like the observed one, but this fact is not evidence against other possibilities.

Single-star explanations of the X-rays are ill-defined because practically no effort has been devoted to them, but here is one speculation that seems consistent with the data. A wide range of wind velocities has been observed for η Car, perhaps because rotation combined with the modified Eddington limit allows escape speeds to depend on position on the star. Thus we can postulate fast wind streams coexisting with slow ones, polar vs. equatorial to some extent but conceivably as amorphous as in the Solar chromosphere and corona. If wind zones squirm around above the stellar surface, the fastest outflows can encounter slow-moving material, producing shocks. We need high wind speeds ($>$ 2000 km s^{-1}) to explain the observed hard X-rays this way; but such fast, hot, low-density streams would be nearly invisible against the spectrum of the ~ 500

km s^{-1} bulk of the wind. The X-ray luminosity is only about a percent of the kinetic energy carried by the wind. In this picture a "spectroscopic event" may be either a shell ejection or some other general wind disturbance that quashes the hypothetical fast outflow streams.

I know of only two non-trivial predictions that were vindicated in a satisfying way by the 1998 event: Damineli's (1996) assertion that it would occur (which deserves a great deal of credit, because some astronomers — including members of at least one TAC — disbelieved it), and my own far less crucial guess written in August 1997 for a popular-level article (Davidson 1998), where I suggested that the X-ray behavior would "culminate toward the end of 1997, maybe in a final X-ray crescendo followed by a steep plunge" — but that correct hunch was not truly based on a specific model. In June to October 1997, when some of us informally discussed the impending X-ray event, we hoped that the alternative pictures in Fig. 2 would lead to a decision. We expected a binary colliding-wind event to be roughly symmetric in time, based on orbit models where conjunction occurred close to periastron (DCL; D97). A shell ejection event, on the other hand, would abruptly extinguish the X-ray flux, presumably followed by a slow recovery. There is no need to revisit last year's details here; Fig. 2 is an honest portrayal of the resulting expectations, and before the event no one expressed serious disagreement with them.

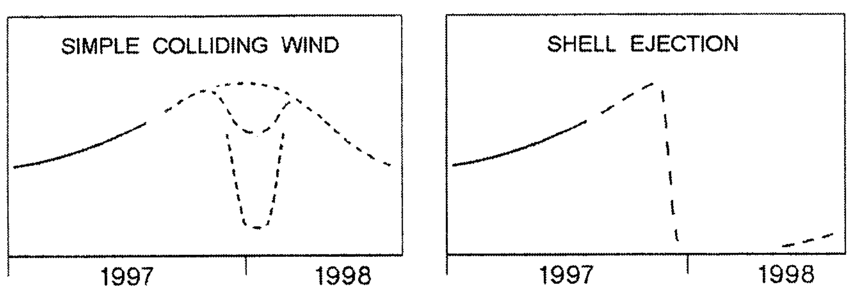

Figure 2. Two alternative patterns of apparent X-ray flux behavior that were expected for the 1997–1998 event.

Fig. 3 shows what really happened at 2–10 keV. These are RXTE net count rates obtained by Corcoran's group, reduced and analyzed by Kazunori Ishibashi. Except for the flare activity that became violent in September to November 1997, Fig. 3 looks like an awkward compromise between the two alternatives in Fig. 2! One can plot the data in a way that appears to resemble the binary version, especially by using a compressed time scale. However, if we recognize that the steep increase in early March 1998 coincided with the onset of a predicted flare event in the "85-day" sequence described by Corcoran et al. (1997), then *the underlying trend was like a shell-ejection scenario,* with an abrupt decline in November followed by a gradual recovery after January. Column densities indicated by the X-ray spectrum increased before December and decreased after January, more or less consistent with either interpretation.

My reason for mentioning last year's quasi-predictions is to emphasize that the 5.5-year binary idea visualized beforehand was *not* strikingly vindicated by the 1997–1998 event. I hasten to add that now, after the event, any scenario

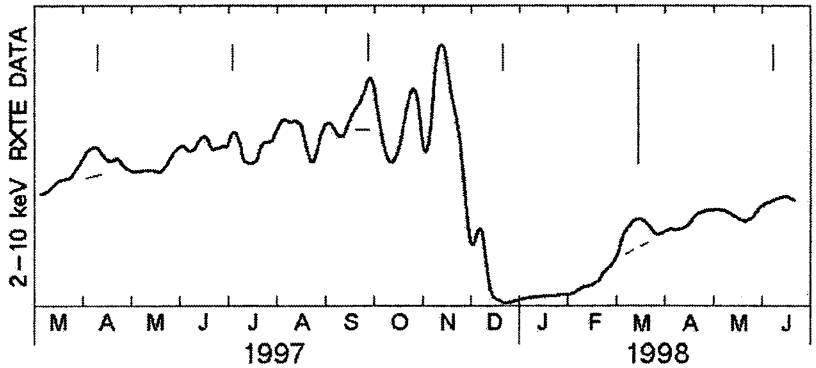

Figure 3. The observed X-ray event: RXTE data provided by Corcoran and Ishibashi. Vertical marks indicate expected times of X-ray flares at 85-day intervals (Corcoran et al. 1997); the apparent recovery trend after January was distorted by a flare in March.

can be modified to agree better with the RXTE data. For instance, since the periastron longitude that we assumed last year now seems unjustified (Section 2 above), binary enthusiasts seeking to model the X-ray behavior can now experiment more freely with the orbit parameters. *Something like a shell ejection may have occurred even in a binary system,* triggered by the periastron passage of the secondary; this is not an arbitrary additional hypothesis, since the STIS data indicate basic changes in the wind.

Note in Fig. 3 that X-ray fluctuations became more violent in the months before the event, but later the flux seemed steadier as it recovered after January (aside from the expected March flare). This behavior may be a hint that the shocked region was less dense after the event, since a shock tends to be more stable if it cools mainly by expansion rather than radiation. Anyway the temporal asymmetry intuitively seems more consistent with an outburst than with most binary encounters that have been proposed.

Intervening column densities N_H indicated by the X-ray data are smaller than we'd naively expect for a colliding-wind binary. In almost any such model, in late 1996 and early 1997 the X-rays should have been produced at distances roughly 10 AU from the primary star. Assuming a spherical wind with the parameters normally used for η Car, those distances would imply $N_H \sim 10^{23.5}$ cm^{-2}, about an order of magnitude larger than the observed values (Corcoran et al. 1997, 1998). Perhaps the star's mass-loss rate is only $\sim 10^{-4}$ rather than $10^{-3}\, M_\odot$ yr^{-1}. A more appealing possible explanation, consistent with other data, is that the wind is not spherically symmetric and we usually view the X-rays through a relatively low-density part of it, as in Fig. 4.

Finally, concerning the X-ray flux, we must bear in mind that the hard X-ray luminosity of η Car, of the order of 10^{36} ergs s^{-1}, is small compared to the UV and visual-wavelength luminosity ($\sim 10^{40.2}$ ergs s^{-1}), the kinetic energy flow in the wind (most likely $\sim 10^{38}$ ergs s^{-1}), or even the brightest individual emission lines in the wind (H$\alpha \sim 10^{37}$ ergs s^{-1}). Thus we should not expect the wind to be significantly ionized or heated by the observed X-rays. This statement includes X-ray photons below 1 keV produced by the same hot gas; additional soft X-rays from cooler shocks are likely but unobservable.

4. A Thick Circumstellar Disk?

Few afficionados will be surprised if η Car has a thick circumstellar disk of slow-moving ejecta (see refs. in DH97). Therefore, last December when Kazunori Ishibashi and Mike Corcoran said that the X-ray flux had just crashed (Fig. 3), one of my first reactions was to sketch Fig. 4. This was a conscious attempt to mimic the appearance of a shell ejection event in a binary context, possibly leaving most of the stellar wind unaffected.

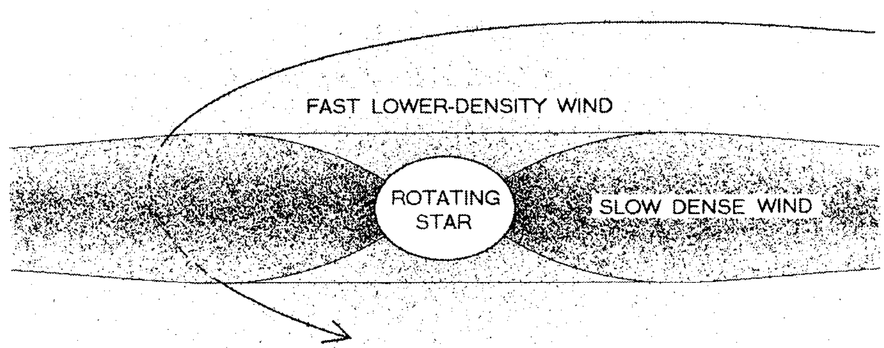

Figure 4. Various observational and theoretical hints suggest that η Car may have a dense circumstellar disk. Can a spectroscopic event be caused by the immersion of a companion star in the disk?

Here is a typical binary-with-thick-disk scenario. Suppose that a hot companion star, producing many more photons above 20 eV than the primary does, is responsible for the highest ionization stages seen in the ejecta (e.g., Ne^{++}, Fe^{++}). It may cause much of the ionization and heating in the outer parts of the primary's wind. Further suppose that its orbit intersects the primary's circumstellar disk near periastron. When the secondary star moves into this dense disk, so does the colliding-wind X-ray source region; the shock becomes very unstable, causing X-ray production to shift to lower photon energies, while at the same time the intervening column density increases dramatically. Result: The 2–10 keV X-rays practically disappear. Meanwhile the hot star's 20–40 eV ionizing photons are absorbed in the disk, explaining the disappearance of doubly-ionized emission lines in ejecta a thousand AU away, and conceivably accounting for a general decrease of ionization in the primary wind.

Anyway that is one story; but it shouldn't be taken too seriously until real calculations of ionization and excitation have been achieved. Problems and loose ends include: (1) The X-ray recovery after January seems a little inconsistent with this scenario. If the X-ray emission zone was then on the far side of the disk from our point of view (having passed through the disk), why didn't the intervening column density remain very high? Did the X-ray source somehow move completely clear of the projected disk in February–March, or did it disrupt the disk, or what? (2) If the orbit intersects the disk at some relative tilt angle, then the secondary star should pass through the disk again at an opposite node 180 degrees later in the orbit. With the orbit in Fig. 1, for instance, another event would have occurred in mid-1998. We can avoid this objection by placing the intersection point very close to periastron, so the other one occurs much later near apastron; but in that case, is there some dynamical reason for the special orientation? (3) If a hot companion star strongly affects the primary wind's heating and ionization, then the highest-excitation emission — notably the helium lines such as λ6678 — should occur in the wind's less dense outer zones. But the observed He I lines probably originate in an *inner* wind zone, in fact the wind-acceleration region, since their absorption minima are seen at velocities around -350 km s^{-1} rather than -500 km s^{-1}. These deep-seated helium features became much weaker during the event; which seems reasonable if the event was a basic change in the wind, but I do not see an explanation based on the disappearance of a companion star into a circumstellar disk at a distance of 3 AU or more. (4) UV absorption in such a disk mainly involves photoionization, since dust grains cannot exist so close to both stars. When gas (not dust) absorbs far-UV photons, then extra ionization occurs, leading to high-excitation emission lines. Therefore we should avoid saying carelessly that "the secondary star was temporarily hidden in a disk" as though the results were obvious. Ionization calculations are required, and maybe gas dynamics too.

Of course we can imagine other variants of the binary-and-disk idea. Maybe the encounter disrupts the disk; maybe the orbit is in the same plane as the disk; maybe the orbit is not very eccentric after all, and intersects the circumstellar disk at a radius much larger than 4 AU; etc. In any case we should refrain from invoking a disk as a *deus ex machina* without suitable calculations. (One might even consider an undetected binary companion to be one d.e.m. itself while the unproven disk is another. Few good stories need one, let alone two.)

In a binary system, a circumstellar disk may provide essentially the same observable effects as a shell ejection event; in either case the secondary star and the X-ray emission region are suddenly immersed in denser gas. For reasons noted above, my own impression is that the 1997–1998 event looked more like a wind outburst (or at least disturbance) than an affair with a disk, but this opinion is tentative, unsupported by calculations.

5. Other Considerations

This final section is a potpourri of important considerations not mentioned earlier. First, the 5.5-year cycle itself is often cited as evidence for a binary, because a single star allegedly cannot have a regular periodicity that long. Characteristic dynamical times for η Car and its wind should be days, weeks, or possibly a few months. However, a *thermal* timescale of several years is not unreasonable if it refers to some outer layers of the star. We don't understand Eta's big 20- and 5-year eruptions seen around 1840 and 1890, but thermal timescales probably figured in their physics. A good set of eruptive thermal oscillators can be found not far from this meeting — and some of the Yellowstone Park geysers are famously periodic! (A geyser–LBV physical analogy was often proposed before Eta's 5.5-year cycle was known; see Humphreys & Davidson 1994.) Alternatively, 5.5 years might be a local timescale for angular momentum diffusion, e.g., maybe the time required for rotation of the outermost layers to reach a critical value, triggering a shell ejection which then carries away those layers.

X-ray flare events in η Car recurred at an interval of 85 days during 1996–1997 (Corcoran et al. 1997). Had this periodicity been discovered before the 5.5-year cycle, most astronomers would have guessed that η Car is a binary with an average separation of about 2 AU, quite suitable for interactions in this nearly-Eddington-limited case. Since research trends have a sort of momentum, the resulting 85-day-binary bandwagon would have continued even after the 5.5-year timescale emerged. With that thought in mind, we should not dismiss the idea of a short-period binary merely because the 5.5-year bandwagon happened to start first!

Ishibashi has noted several serious objections to a close binary colliding-wind model: column densities N_H within 3 AU of the primary star should be much larger than the values observed in the X-ray spectrum, N_H shows no dramatic short-period variations, and there is preliminary evidence that the 85-day period may have lengthened in 1998 (which is OK in other models, see the next paragraph below). I reluctantly agree that a short-period binary model now appears harder to design than either a long-period binary or a single-star model; but it may yet be possible, a challenge for inventive theorists.

The circa-85-day timescale has been unjustly neglected in most recent discussions. This value is very suitable for rotation, and seems possible for pulsation of the primary star. However, the period is observed in the X-ray source, not the stellar surface. In most colliding-wind explanations, the interval between X-ray flares indirectly reflects the star's rotation or pulsation and should vary with position in the orbit, becoming longer after periastron (Davidson et al. 1998). Therefore *variations in the 85-day period may provide information about the orbit parameters* if Eta is a long-period binary. (Such variations would not be clear evidence against a single-star shell ejection, however, because surface rotation and pulsation can be affected by a temporary readjustment of the star's outer layers. Obviously we need quantitative theoretical analyses.)

A triple-star system (e.g., as proposed by Livio & Pringle 1998) that combines an 85-day period with an eccentric 5.5-year orbit would almost certainly be unstable. At periastron in the long-period orbit, the separation distances and angular speeds of the two orbits would be comparable to each other, which looks to me like a recipe for disaster; at least we would expect the longer orbital period to vary. Admittedly it would be fun if the ratio of the two underlying periods should turn out to be an integer! Another gonzo thought: might 5.5 years be the *precession* period of a circa-85-day orbit or disk? My impression is that the primary star would have to rotate implausibly fast, but I am not sure, and someone may enjoy exploring this type of picture.

With the proviso that close-binary and especially single-star ideas also deserve consideration, we can imagine a "best bet" 5.5-year binary scenario with these features: (1) The primary star rotates fast enough so that its equator is precariously close to a modified Eddington limit; the equatorial wind zones are slow and easily subject to disruption. (2) The companion star has $M < 60 M_\odot$, preferably $< 40 M_\odot$, so it has not evolved too far. Therefore it is hot enough to cause the observed high-excitation emission lines in ejecta blobs about a thousand AU away, and has a fast enough wind to account for the hard X-ray spectrum via colliding winds. (3) The orbit is somewhat more eccentric than that shown in Fig. 1, and oriented differently. (4) Near periastron a mixture of tidal, gas-dynamic, and radiative effects abruptly cause a general disturbance in the dense primary wind. This may be a shell ejection event or some other temporary structural change. (5) Consequently, the gas density suddenly increases around the secondary star and the colliding-wind region. The situation then resembles the immersion-in-a-dense-disk sketched in Section 4 above. Conjectural result: a "spectroscopic event." As usual, we need a series of heavy-duty theoretical calculations to show whether this picture makes sense.

Finally, the most important point: Even if a long-period binary model is correct and we find its parameters, that does not "solve the η Carinae problem." *The unique astrophysical role of η Car is based on its Great Eruption, its unusually dense wind, remarkable structures in its large-scale ejecta, and peculiar excitation processes in the ejecta* (see DH97). For years we have wondered how Eta's basic instability works, and whether it has implications for very massive stars in general. The 5.5-year cycle, while a fascinating puzzle, will be significant in this context only if it is responsible for the instability, or if it gives valuable clues to the stellar parameters, or if it is a clue to some essential phenomenon that we have not imagined yet. The most obvious evolutionary effect of a companion star in a highly eccentric orbit is to limit the size of the primary star. Periastron interactions may trigger an eruption of the primary, but most likely cannot account for the essential underlying eruptive instability. If, on the other hand, a single-star model proves to be correct, then the existence of a 5.5-year cycle will be a definite clue to the structure of the outer layers. In either case, unless we have all failed to recognize some basic trick, *the "Eta Carinae problem" still concerns mainly the primary star and has not been solved.* It deserves a greatly increased amount of theoretical attention.

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pp. 373 - 374

Panel Discussion on the Cause of the 'Great Eruption'

Bruce Balick, Moderator

The panel members were the six speakers in this session — Dave Arnett, Jon Bjorkman, Joe Cassinelli, Kris Davidson, Joyce Guzik, and Icko Iben. To start the discussion Bruce Balick asked each of them to state briefly what they think was the cause of Eta's Great Eruption 150 years ago.

...

Davidson: ... let's try not to multiply hypotheses prematurely. Joyce Guzik's and Jon Bjorkman's remarks obviously make a lot of sense, especially in combination. For many years we've known that Eta is close to the classical Eddington limit and is cool enough for an appreciable number of opaque Fe^{++}, Fe^{+}, and H^0 ions to form in the atmosphere, while its outer layers may float precariously on deeper high-opacity layers around $T \sim 10^{5.5}$ K. This sounds like an eruption waiting to happen! With rotation it becomes even more unsteady.

Joyce Guzik's comment that the unstable layers contain little mass doesn't invalidate this picture; consider, for instance, the analogy of a geyser eruption, which begins at the top of a column of hot water but then propagates downward to release a much larger mass.

Rotation may give us an additional "recovery" timescale following an eruption. When some outer layers are suddenly expelled from the star, the outermost surviving layers may expand, so they rotate slowly for a while. But they gradually get spun up by the inner part of the star, perhaps in a characteristic time of 5 to 500 years. Can this, rather than a thermal timescale, determine the time interval between eruptions or even between spectroscopic events?

No one has ever done an adequate theoretical analysis of Eta's full set of circumstances. Do all those likely instabilities trigger an eruption, or should they merely produce an unusually dense long-term wind? If a companion star is present, does it really matter? Does it trigger an eruption, or is a second star necessary for the basic instability? Does η Car represent a state of crisis that every very massive star passes through? These long-standing questions remain unanswered.