CONFERENCE SUMMARY

SOME COMMENTS ON CLASSICAL NOVAE AND RELATED SYSTEMS

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ABSTRACT. I discuss aspects of physical processes in Classical novae and related systems, particularly in the light of what has been said at this colloquium.

I would like to start by congratulating the SOC and LOC for organising a most enjoyable and interesting conference, and thanking the speakers and poster-contributors on the very high level of comprehensibility that they have achieved. Perhaps I might add that the Colloquium dinner was excellent, and that the coffee-breaks were the most substantial and nutritious that I have ever enjoyed.

Several speakers had the temerity to suggest lists of objects which might have a nova outburst in the next few years, or decades. It would be interesting to have a sweepstake on this, with a bottle of champagne at the next nova conference, presumably in about 2002, for the person who correctly predicts the next nova outburst *in a system already known to be cataclysmic* (though excluding presumably already known recurrent novae). Since, as we have learnt from Evans, polycyclic aromatic hydrocarbons (PAHs) are a significant feature of nova outbursts, perhaps the next outburst will be recognised by its smell.

On a more serious level, what I found most impressive among many excellent reviews were three on areas which have not, until now, been sufficiently represented in the mainstream of classical nova research. I refer to reviews by Gehrz on infrared, by Hjellming on radio, and by Evans on dust. No doubt some of you will have been more aware than I, as something of an outsider, of the important developments in these areas; but at least my own eyes have been opened to the significance of these topics, and of the opportunities they present for more reliable data than heretofore, on for instance masses of nova shells, and their chemical composition: 'chemical' in the real sense and not the rather artificial sense commonly used by astrophysicists. We have heard of attempts to model the complicated chemistry that starts presumably with the formation of the simplest molecules such as SiC, CN but leads on a remarkably short timescale to such complicated entities as PAHs.

Other contributions that particularly impressed me had little in common except the initial 'B': Boyarchuk, and Barbara Hassall, on abundances, and Bianchini on activity cycles in the cool components of CVs. On abundances, I was relieved to learn that we

https://doi.org/10.1017/S0252921100069074 Published online by Cambridge University Press

do not have to worry about Fe-rich novae, but that NeMg-rich and CNO-rich novae really do seem to exist. One point that troubled me was that we seem to have some novae (*e.g.* GQ Mus) which are rich in CNO *and* NeMg. Wouldn't one expect just one, or the other? Perhaps I missed something there.

The subject of activity in the cool star was one that I was particularly glad to hear discussed, by Bianchini. I am not sure I was convinced by some of his multiple periodicities, but I am delighted that he should be investigating more deeply than most people the rôle of the cool component. I sometimes think this is the Cinderella of the CVs, with the Ugly Sister in the Roche Lobe next door demanding, and receiving, most of our attention with her extravagant outbursts. But the fact that the red dwarf, or in rare cases the red giant, is being forced to rotate many times, even a few hundred times, more rapidly than a single star of similar structure must surely make it a very active and presumably spotted object as is rather clearly seen in the much less drastic context of RS CVn and BY Dra stars. This activity will presumably modulate the mass transfer rate, on several timescales. For example, there appear to be slowly-migrating spots or spot clusters on RS CVn s, and similar migration in CVs, perhaps speeded up to months rather than years, for the shortest-period systems, might have something to do with the mass transfer rate. I would add here my routine remark that the belief, apparently deeply held in CV circles, that magnetic activity dies out in stars which have become fully convective is not founded on any serious observational basis. It is roughly true that the later the M dwarf the less activity it has, but then it is also bolometrically fainter; relative to its bolometric luminosity, its activity if anything increases to later types. The prototype flare star UV Cet has a mass of $0.11\,M_{\odot}$ (Popper 1980), and can hardly be anything but fully convective - yet it seems to be quite active. Thus it is nonsense to claim that a stellar dynamo *needs* to have field 'anchored' in a radiative core, though one can speculate that such a core may be more relevant for rapidly-rotating CV companions than for field M dwarfs.

The influence on a system of the cool component's activity is likely to be important in 'pre-cataclysmics' (close but detached WD/RD pairs) and 'ante-pre-cataclysmics' (wide binaries containing a red giant and an MS star), as well as in cataclysmics themselves and in 'post-cataclysmics' (presumably either single WDs or LMXBs). Among CVs themselves, for example, it seems conceivable that the magnetic field of the WD in a polar or intermediate polar may have actually been accreted along with mass (and despite some dissipation) from the cool component. This is moderately consistent with the fact that polars (AM Her s) are clustered strongly, and intermediate polars (DQ Her s) weakly, towards shorter periods than CVs as a whole, as if fairly substantial amounts of mass have to be transferred before the WD can become strongly magnetic. Magnetic WDs are *much* more common among CVs than among single WDs (Lamb and Melia 1987), and I wonder if the explanation can lie in the dynamo activity of the *red* star. Presumably a polar can evolve ultimately into a single magnetic WD, when all of the cool star has been accreted or dissipated, so that perhaps the strongest magnetic single WDs are actually the remains of AM Her s. The concept of a cool component being dissipated has been substantially reinforced by the recent discovery of the eclipsing radio pulsar 1957+20 (Fruchter *et al.* 1988), although it is not clear that a white dwarf could be as effective as a neutron star in dissipating a low-mass companion.

https://doi.org/10.1017/S0252921100069074 Published online by Cambridge University Press

It occurred to me that another possible similarity between CVs and cool stars might be seen in the bipolar flows which Drew described in, for instance, SU UMa and YZ Cnc. Although the temptation is to blame this on the intense radiation field of the hot WD, we know that a class of cool stars, the T Taus (Lada 1985) manage much the same kind of flows without the benefit of a hot source. The process is likely to be due to a hydromagnetic acceleration, although the hotter radiation field of an accreting WD might allow the outflow to achieve higher terminal velocities. I wonder whether a magnetic field on the WD, too weak to disrupt the the disc yet still very strong as magnetic fields go, could spin up and throw out a proportion of the gas that is flowing in through the disc. Just because a CV is not an AM Her or a DQ Her, it does not follow that it is non-magnetic. Pringle (1989) has discussed another mechanism, also hydromagnetic, for explaining bipolar T Tau flows; he invokes dynamo action in the strong differential rotation at the star/disc boundary layer.

We heard relatively little about the physics of accretion discs, no doubt because they are more relevant to dwarf novae and their outbursts than to classical novae. Despite the warmth of his argument, I am afraid I was not convinced by Shaviv that accretion discs can really be as small (5 - 10 WD radii, if I understood correctly) as his models suggested. I was a little surprised not to hear that recently the death of viscous- driven Keplerian accretion discs has been announced; at least, that is how I interpret the fascinating work reviewed recently by Spruit (1989), in which he shows that a stationary spiral shock-wave pattern can also transport angular momentum outwards while material flows inwards. I did not have time to digest fully this fascinating work, which is backed by very sophisticated simulations on a Cray computer. One or two important questions still seem open. However, it seems reasonably clear that such spiral-shocked discs will nevertheless produce much the same energy output in much the same wavelength bands, so we will not have to tear up all previous attempts at modelling the accretion flow.

I was glad to hear Kraft, in his introduction, telling us that he has always found the evolutionary question, of where CVs come from and how they evolve on the long term, to be particularly interesting. Of course, we will only understand such issues by observing on the relatively short term so far allotted to human experience; but I hope Kraft's remarks can be an excuse for me to dwell on some of the long-term issues. It is now generally assumed that the 'common envelope' mechanism of Paczyński (1976) is the means by which CVs are formed from initially wide binaries. I have no doubt that this is largely correct, although I continue to think that the 'star-planet' scenario of Livio and Soker (1984), can also play a rôle. But it certainly cannot be the case that all, or even most, episodes of late-case-C RLOF lead to the formation of close binaries; Barium stars (McClure, Fletcher and Nemec 1980; Boffin and Jorissen 1988) appear to have to be a much more common outcome. Certain symbiotics, *i.e.* those with WD rather than MS companions, have presumably also avoided large angular momentum loss. Possibly this can be explained by appealing again to surface activity on the cool component, this time a giant, prior to RLOF. An enhanced wind, *i.e.* the natural wind of a cool giant enhanced by tidal friction and consequential rapid rotation, may mean that the giant reduces its mass, perhaps by a factor of 2, before it fills its Roche lobe, and this can easily suppress the tendency of such RLOF to become drastic (Tout and Eggleton 1988), in those systems where the initial mass ratio was not far from unity.

Unfortunately it is difficult to predict how much wind enhancement is to be expected in a binary, quite apart from the fact that even single-giant winds are not easily quantified.

There is a remarkable triple star, 4 Dra (Reimers, Griffin and Brown 1988), which may be capable of shedding considerable light on the genesis of CVs. This star contains a CV in a 1700d orbit with an M giant. An immediate consequence is that the supposed wide-binary precursor of the CV cannot in fact have been especially wide since otherwise the outer binary would have been disrupted. Given that some mass has almost certainly been lost from the system, which is likely to mean that the *outer* period was originally shorter than 1700d, the initial period of the CV precursor can hardly have been longer than 100d (Eggleton, Bailyn and Tout 1989). Such a short period makes it only marginally possible for the WD's precursor giant to have ignited helium in its core. It seems clear from this one example that birth-processes do not necessarily favour massive white dwarfs in CVs, although several selection effects appear to favour the *detection* of CVs with massive WDs, as we have heard from Ritter. I wonder if it is possible for delicate UV spectroscopy to reveal the velocity amplitude of the CV in its 1700d orbit: since the M giant's orbit is well determined, it should be necessary only to measure the change in γ-velocity of the CV between epochs about 2 years apart. I suspect that the CV would be found to have quite a low total mass, perhaps $0.7\,M_\odot$.

The fact that a deeply convective cool component is expected to lose mass at a drastic (hydrodynamic) rate if its mass exceeds ~ 0.7 of its companion's mass (Paczyński 1965) is relevant not only to the possibility of common envelope evolution in initially wide binaries, but also to CVs themselves. We have heard more than once that observed masses in CVs have to be treated very cautiously, because of the complexity of spectral behaviour, especially in the WD and its surrounding accretion flow. But one of the better cases, I believe, is EM Cyg in which the mass ratio (RD/WD) is 1.25 - 1.4 (Stover, Robinson and Nather 1981), while the cool component's mass is 0.7 - 0.8 $M_\odot$. This seems to argue for at least very rapid thermal-timescale RLOF, and possibly hydrodynamic RLOF. Yet the behaviour of the system does not suggest more rapid mass transfer than several other CVs where the WD is probably more massive, perhaps by a factor as much as 2. It seems to me that a possibility is that the cool component is so active that it is able to transfer mass in a 'directed wind' focussed primarily through L1, but originating from a surface which is fractionally *inside* its Roche Lobe. Some degree of mass transfer by wind rather than directly by RLOF seems to me to be a strong possibility in CVs, even if the mass ratio is not the unusually adverse one seen in EM Cyg. Shara mentioned the observation by Sion *et al.* (1989) of a rapidly expanding shell, suggestive of a former nova outburst, around V471 Tau, the well-known 'precataclysmic' (perhaps that description is now obsolete?) binary in the Hyades. Both components of V471 Tau are similar to those of EM Cyg in mass, but the orbital period is nearly twice as great so that the system is well detached. The cool component has many resemblances to the cool components of RS CVns, and is presumably losing mass by wind, some of it sure to be accreted by the WD. So I do not think that the possibility of CN outbursts should surprise us, and by the same token it would not be surprising if EM Cyg were also detached, though much closer to RLOF. The distance between the photosphere and the Roche Lobe does not need to be restricted to a few times the pressure scale height of an atmosphere in equilibrium, since the strong surface activity which I believe is inevitable will make for a quite different and much more extended distribution of density.

Following Tutukhov, I return to the issue of whether, *i.e.* in what ranges of initial masses, mass ratios and periods, a wide binary can undergo common envelope evolution to end up as a close binary. Although it is not difficult to see how a binary like V471 Tau can have resulted from such a process, and then evolve further by magnetic braking to become something like EM Cyg, I have long felt (Eggleton 1983) that a superficially similar but actually rather different 'pre-cataclysmic' AA Dor (Kudritzki *et al.* 1982) is much harder to understand. The SDO component is apparently of too low a mass to have come from anything other than a *sub*giant, and the other component is probably of too low a mass to be a nuclear-burning star. AA Dor is a problem both for the common-envelope scenario, which requires a massive enough companion to blow off the giant's envelope, and for the star-planet scenario, which requires a more extreme precursor giant and hence a substantially more massive SDO star. It is not clear to me that we understand yet, even in broadest outline, how this, and therefore other 'pre-cataclysmics', can be formed.

The end-point of CV evolution seems to me to be as problematic as its starting-point. I don't think we have yet heard a definitive answer to the question of whether, and in what circumstances, a CV can evolve by 'accretion- induced collapse' (AIC) to an LMXB. That such evolution should occur seems very attractive; and yet if NeMg WDs, which are presumably about the most massive WDs to be produced, are able to reveal themselves by showing NeMg enrichment in their ejecta, this seems to me to argue that they are *losing* mass in the long term, and thus not capable of exceeding the Chandrasekhar limit. In fact, since at least one LMXB, V616 Mon, has a primary mass in excess of $3\,M_{\odot}$, and possibly twice that (McClintock and Remillard 1986), it is clear that AIC cannot be the only mechanism that produces LMXBs; and by Occam's razor one might claim that therefore AIC is an unnecessary mechanism. I dare say we have not heard the last of it, however. Several astrophysical situations seem to succeed in defying Occam; for example, we have heard from Webbink of how the very small class of recurrent novae seems to split into two quite radically different physical groups, with nuclear outbursts on WDs in one group, and accretion events on main sequence stars in the other, giving outburst behaviour that is embarrassingly similar. In the words of our Chairman, Dr Friedjung, 'more investigation is necessary'.

In conclusion, I would like to thank all speakers for the high quality of their presentations, and all participants for discussion which, while often vigorous, has always been friendly. I am also grateful to ESA for some financial support.

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