Krakow 22.11.2023

Review of the dissertation thesis by Paulina Sowicka titled

"Excitation of pulsation in hot pre-white dwarf stars from an observational point of view"

The PhD dissertation is in the form of a book manuscript, incorporating three chapters as already published papers. It begins with an introduction to stellar evolution, covering stars from the main sequence to PG1159 pre-white dwarfs. Additionally, it provides an introduction to stellar pulsations and describes the telescopes used for observations. This is followed by chapters on data reduction software, techniques employed during data reduction, and an introduction to the discrete Fourier transform analysis of time series data. The subsequent chapters consist of three publications: Sowicka et al. 2018, MNRAS 479, 2476 (paper 1), Sowicka et al. 2021, ApJL 918, L1 (paper 2), and Sowicka et al. 2023, ApJS 269,32 (paper 3). These chapters constitute the main body of the thesis, containing the analysis of time series photometry for 31 stars. The photometric time series data for this work were collected over nearly 9 years, predominantly by the author on various telescopes and with contributions from collaborators. Mrs. Sowicka has included statements from co-authors regarding their input, affirming her significant contributions to all three papers. This includes preparing telescope time proposals, organizing observing runs, data reduction, performing data analysis, and writing publications.

The work demonstrates the required depth of research and methodological rigor for a PhD thesis. Nevertheless, there are minor downsides in the dissertation, which were noted for the sake of formality during the reading process (**I do not require references to them from the author**).
In some parts, the description of the stellar evolution misses details or is misleading without providing additional information. For example:
Fig. 1.2-left shows a low-mass star where the second dredge-up is marked, while in the text on pg. 4, we have information "Low-mass stars do not experience SDU at the end of the He-burning..."
On pg. 5, a sentence about gravitational settling "...when gravitational settling cannot be prevented because the radiation-driven wind has weakened" is not clear.
Fig. 1.6 misses DQV stars, and on pg. 18, "...the GW Vir instability strip is not pure, i.e., not all stars within its borders show pulsations, in contrast to the two others..." namely DBVs and DAVs. The statement is based on the work by Fontaine & Brassard (2008, PASP, 120, 1043). However, this referee (and a couple of other SDSS co-workers) observation of some DB and DA suspects performed 3 years before Fontaine and Brassard (2008) publication has shown some non-pulsators within the DA,DB instability strips. Yet, it was not to the extent observed in GW Vir stars.
It is understandable that some details of the evolution were dropped due to a generalization of the description and after all, the introduction to PG1159 stars and the GW instability strip is written well.

Other minor errors and problems:
pg. 21. ACAM 4.2 telescope: "We used Sloan G filter and no binning" - where likely SDSS g filter was meant.
pg. 27, Fig. 2.1 "when dark frames need to be scaled" - perhaps the author meant: should they be scaled for the integration time of exposure frames?
On the same page (bias/dark frame subtraction) the explanation itself "(e.g., the matching ones cannot be used for some reason)" actually does not clarify anything.
pg. 31. "van Dokkum originally implemented" instead of Van Dokkum.
pg. 34. "...if the seeing becomes better" - perhaps, if seeing becomes smaller.

Additionally, the script listings presented in Listing 1 and 2 and the technical description of using the pipeline are elements that might be more suitably placed in an appendix or on GitHub (the latter being especially useful for others).
pg. 38. "it allows a subsequent check of each of the steps and an adjustment of parameters for a single file to improve the result." - it is not clear how was the improvement measured.
Paper 1: I'm missing spectral windows for two sests of the runs presented in Figures. 2 and 3.
Paper 2: It is also regrettable that the detection thresholds in the amplitude spectra plots of Fig. 2 are not provided.

Questions and comments for the author (responses to which are welcomed).

pg. 22. In most cases, observations were conducted without a filter or with blue filters. However, at the 10.4-m Gran Telescopio Canarias, observations were performed using either no filter or the SDSS r filter. As higher amplitudes are anticipated in a blue filter, could the author provide an explanation for choosing the r filter?

pg. 34. While the author utilized the PHOTOM package, there is no clear statement explaining the rationale behind this choice. Other packages, such as PROSE (Garcia L.J. et al., 2022, MNRAS, 509, 4817) and TEA-Phot (used by the author in Chapter 4), were available. Did the author find PHOTOM more suitable for the task, and if so, why was TEA-Phot used in Chapter 4 instead of PHOTOM?

Paper 2: Can the author provide further insights into the bottom panel of Fig. 2, where the amplitude spectra of the variable and comparison stars are displayed? I'm assuming this represents the Fourier transform (FT) of instrumental data for both the variable and comparison stars. Frequencies at 45, 55, 97, and 112 c/d in the figure's bottom panel appear in both the comparison star (grey area) and the variable FT. The problem I see is that without FT of the differential light curve, it is challenging to determine if these signals are real or will be detactable after dividing the variable by the comparison light curve. A discussion on this aspect seems to be lacking in the paper, and the presence of claimed frequencies in the comparison star's amplitude spectra raises some concerns. On the other hand, in the FT of the differential light curve (depicted in Fig. 1 from the GTC telescope), two frequencies at 97 and 112 cycles per day are distinctly observable. Consequently, it can be concluded that the star is pulsating.

Paper 3: Can the author provide clarification regarding an issue with Table 2? It is observed that there are three stars lacking luminosities, whereas the footnote on page 15 mentions the absence of GAIA data for only two stars. Additionally, could the author explain why there is no luminosity (L) listed for NGC6765?

Comments (answers are not required):
pg. 26. "Bias frames" - While the median method for creating bias (or dark) frames is widely used and has its advantages, a master bias calculated this way has slightly larger noise (sigma) than a master bias calculated from the mean of bias sets. Therefore, a method resulting in slightly lower noise uses the combination of both the median and the mean. Refer to "https://mwcraig.github.io/ccd-as-book/01-06-Image-combination.html."

pg. 40, Fig 2.5. The procedure for removing extinction from differential light curves by fitting a Dmag vs. airmass line might be risky when a star is variable. It is conceivable that observations could end up at, let's say 1.155 (see Fig. 2.5) airmasses, causing the line's inclination to change. However, the variations induced in this way would likely be long-term and not affect short-period FT analysis, such as those observed in PG1159 stars.

Paper 3: In the classification of DOV, PNNV, and PG1159, a broader confusion is evident. Stars with log g greater than or equal to 7 are labeled as WDs, constituting a clasical but artificial division between pre- and WDs. One can note that within this log g range, certain PG1159 stars also fall.

Paper 1: In the context of the current era marked by numerous publications and fast paper preparation, I find Mrs. Sowicka's statement (the last one in the summary and conclusions section) particularly significant:

"The example of VV 47 shows that it is possible derive credible model fits even if based on inadequate data, in addition even being in agreement with values determined using other methods. Careful analysis and interpretation of observational data should therefore prevail over the temptation to claim potentially exciting results on a poor base."

Despite above minor problems, the work is properly organized and written. Based on the results, Mrs. Sowicka demonstrates the ability to conduct demanding observations, data analysis and deriving independent conclusions.

Key achievements by Mrs. Sowicka include:

Proving that the candidate for epsilon-driven pulsations, VV 47 star (the central star of the planetary nebula), is not pulsating. This conclusion is drawn not only from Mrs. Sowicka's observations but also from the reanalysis of raw CCD data used by Perez G. et al. (2006) which led them to derive VV 47 variability.

Discovering pulsations in the nitrogen-rich PG1144+005 star, initially considered an exception within nitrogen-rich PG1159 pulsators which confirms the nitrogen dichotomy among PG1159 stars (i.e. nitrogen-rich PG1159 stars are pulsating while nitrogen-poor are non-pulsators, however, this conclusion was somewhat weakened later in Mrs. Sowicka's paper 3, where she summarizes known N-poor PG1159 stars that seem to pulsate).

Conducting extensive observations of multiple PG1159 stars between 2014-2022, leading to the estimation of a new pulsator fraction among PG1159 stars at 36%.

Constructing a theoretical HR diagram for PG1159s based on photometric and GAIA data (and derived bolometric corrections) and the most recent list of 67 known PG1159 stars, including their physical parameters and information on their variability.

Finaly, successful applications for telescope time on various telescopes, including the 10.4-m Gran Telescopio Canaris, further underscore her accomplishments.

In a summary I consider the doctoral thesis of Mrs. Paulina Sowicka to be a valuable contribution and to meet the criteria prescribed by the law for a doctoral dissertation. Therefore, I request that this dissertation be admitted to a public defense.

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