To what may it concern:

The report on the PhD thesis by Ayush Moharana

To: Scientific Council, Nicolaus Copernicus Astronomical Center of the Polish Academy of Science

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I have read the PhD. dissertation thesis of Mr. Ayush Moharana named "Comprehensive study of low-mass Compact Hierarchical Triples using Eclipsing Binaries" and find it to be an appropriate and interesting research work. The techniques that were used by Mr. Moharana to analyze these stellar systems are state of the art and produced new information on all the analysed objects. Some findings presented in the thesis appear to be quite useful also for the wider stellar community.

The form of the presented thesis incorporates a general introduction, which is well-written and understandable, some of the parts would even be suitable for some textbook on astrophysics. The presented methods are here described properly, and the reader is introduced to what is going to follow in the next sections.

The selection of the already published papers is well-substantiated and set to a proper broader context for the particular study. Hence, I think this is now quite a common approach in the present-day theses. Thus, overall I find this thesis is certainly adequate for a PhD degree and commend Mr. Moharana for his work. The thesis in its entirety shows that the candidate is able to conduct independent research in stellar astrophysics, and his abilities are based on solid astrophysical knowledge.

I also list here several minor issues. These are given later below. However, all of these are fairly minor, some of them should be taken into account for some future studies or publications. But these are not at all sufficient to be an issue regarding his qualifications for a doctorate.

I would have several questions if I were present at his defense. These are marked as "*Please comment on that.*" for the particular questions. The reaction to the rest of the issues is up to the author. Nevertheless, the issues are fairly minor and should be only taken as comments or recommendations for some future studies.

Individual comments:

Last paper in the list on page "iv" - missing year of the publication.

I do not fully agree with the statement on page 4 that the Corot was the first satellite on that. A few years before there was the MOST satellite launched in 2003.

Page 12: Lodov > Lidov

Page 13 - It should be mentioned that the ZLK cycles are very long, therefore we study the CHT systems. And also what is missing to mention is that the system should be non-coplanar to ZLK cycles to work.

Page 16: is > are. Several English grammar issues.

Chapter 2 page 17: Six methods for detection of triples are a bit questionable. Can we really be sure, that the third light fraction detected is coming from a bound tertiary? Optical double cannot be easily ruled out. On the other hand, the eclipse depth variation is completely missing. Despite the fact that such a method directly led to confirmation of a third component in the system. Please comment on that.

Chapter 2.3 line 3: term "minima" rather to replace by "eclipse timing" or similar.

Page 22: "the angle between the angular momentum vectors of the inner and outer orbits." Is not defined in the text.

I am not very sure I understand the eq. 2.9. What are the first sum of terms? c_0 + c_1*E + c_2*E^2 + c_3*E^3 Why the E^3 term was also included? It was not mentioned before. Please comment on that.

Page 25, paper page 55, sect 4.1. The detrending is always rather tricky. What if there is some real long-term variation? How does the author deal with that? Please comment on that.

Page 27, paper page 57: Surprising that SU Ind resulted in eccentric orbit. With such a short P this is record breaking. Is it really a conclusive result? Why are the secondary eclipses missing in the ET diagrams? Eccentric orbit would be clearly visible there.

Page 26, paper page 56 figure - Quite surprising level of scatter of data points. On the level of 0.05 mag is really a bad photometry, sometimes even better is provided by some photometric surveys.

Rather remarkable scatter of times of eclipses at the level of 200s is really high. Some explanation of cause is missing here.

Page 27, paper page 57: It is not clear what does the negative values of "l3" luminosities mean. It was not discussed in the text. This appears to be a non-physical solution.

In general, the value of the third light "l3" was not properly discussed in the text. For example, for TYC8504-1018-1 the l3 is larger, error bars low. Does it mean that there is some additional hidden component? What if the suggested parabolic trend in fig.6 in fact shows some small part of the longer orbit with periods of years? Would this be possible? Please comment on that.

Page 31, paper page 61: sect 6.1.4 HD60637, it was not discussed why there is the discrepancy between the two solutions by the author and previous study by Helminiak(2019). The difference

is significant, out of the error bars. Why? It should tell us something about unrealistically small error estimates on both studies maybe. It should be discussed in more detail.

Why was only the LTTE taken into account for the one positive detection system? Are the dynamical effects small? It should be clearly stated or estimated its level, based on the P1^2/P2 one can see whether this is the case or not. Please comment on that.

Page 42: The discussion about the problematic derivation of values of "i" & "Omega" from LC&RV (hence to have the complete information about the orbit). It should be mentioned that these can be derived using visual orbit. Interferometry would be added as one possible source of information. At least derive the predicted angular separation of the prospective double, this information is missing for the analysed systems.

Page 45 - I am not sure I understand properly the eq.3.23 but it seems like the method "velocity space disentangling" is only shifting the profiles for the velocities of the particular component, right? In the correct method like Korel by Hadrava, or other disentangling methods, the added value is also independently fitting the relative strengths of individual spectra contributions. Because in eclipsing binaries when one of the component is (partly) being eclipsed this can play an important effect and cannot be easily ignored. Then the relative strengths of individual component's lines are different. This method is probably ignoring this. Please comment on that.

Page 53+54, paper page1910+1911, figures 1&3: Is there some specific reason why the RV&LC curves are shifted in phase that their eclipses are at phases 0.3 and 0.8?

The paper on pages 51-66 is very nice. In deep analysis, going into detail with the available data and revealing all the basic properties of the two systems. Good work!

The one detail missing there is some estimation of angular separation of the double for future interferometric detection. This would reveal the absolute orientation of the orbit(s) in space, hence the "i", "Omega" values for precise numerical integrations and future evolution of the orbits.

And another idea, which should also be tested, is the following. Discussing about possible orbital orientation and rate of inclination change - some lower estimate on the change of "i" can be done via comparing the older photometry from surveys ranging back several years and derive its inclination independently (with much less precision). But comparing this value with the current TESS LC modelling value can give us some rough estimate whether the "i" is constant or some maximal rate of change during that period. Please comment on that.

BD44 shows emission. This aspect of the spectra was not discussed. Should be analysed. Was this emission caused only by surface activity? Or is it somehow time-dependent? What about some circumstellar matter in the system? Can we rule out the possibility of that? It can influence the whole analysis.

Paper on pages70-90: Very nice analysis using all available means, yielding reliable parameters of the bodies and their orbits. The only issue: Why for CD-58 system is the value of the third light

fixed to zero? It should be non-zero, I guess. TESS has usually the quality to detect third light at the level of 1% of the total light. And also, for the other systems the third light uncertainty is at the level of 0.1% or even better from the other tables in that paper.

The statistical properties of the sample of CHT systems are interesting. However, still the numbers are rather low. In the paper by Tokovinin in 2008 (2008MNRAS.389..925T) there are also presented the distribution of parameters for the triple and quadruple systems. This was even before the launch of the Kepler and TESS missions. Can you comment on that? Do some comparison between your findings and how these stars fit into these statistics? It seems like the shape of the distribution is different. But of course, the discoveries made by Kepler and TESS are biased because still most of the detected triples are rather short-periodic and eclipsing. Please comment on that.

The statistics of CHT should also be compared and discussed with the findings by Borkovits(2016) by Kepler timing. This study shows the mass ratio q3 to comparable equal masses rather than q3 similar to M12. Can you comment on that, are the data or method somehow biased?

Page 94: I do not fully agree with the statement that the author derived... "dynamical evolution and stability of the system". As was already mentioned, the Kozai-Lidov cycles can play a role, hence the future fate of the systems should be properly modelled. But this cannot be done without knowing the precise orbital architecture of the whole system, which is unfortunately still not the case here.

Summing up, I consider the doctoral thesis of Mr. Moharana 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.

Written in Prague, CZ
On 17 July 2024