Referee report on the work

"Shell galaxies"

submitted by Mgr. Ivana Ebrová for the degree PhD

The presented Thesis deals with the phenomenon of "shell galaxies" – the objects that are likely a product of strong galaxy interactions. The author first briefly summarises observational evidence of shell galaxies and describes several hypotheses of the origin of shells. Among them, she accentuates the radial merger of two galaxies which is considered to be one of the most likely processes of formation of shells around elliptical galaxies nowadays.

In Part II, the author introduces a model of radial oscillations which offers a framework for description of the evolution of the shell structure. It is shown that within this model, a line of sight velocity distribution (LOSVD) of stars forming the shell should have a quadruple-peak profile, rather than double-peak as suggested by previous simpler models. Then the author describes how the LOSVD is, within this model, related to the potential of the primary galaxy and suggests that measurement of this quantity could be used to determine potential of real shell galaxies. The main author's research output is definitely concentrated in this Part of the Thesis. In spite of not instructive presentation of the methods and results, the hypothesis of quadruple peak spectral line profile presented here as well as suggested methods of its possible exploitation could be considered valuable and original scientific results of the author.

Part III of the Thesis describes numerical simulations of the formation of shells altered by continuous process of secondary galaxy disruption (in contrary to instant disruption considered in the previous Part) and by the dynamical friction. One important objection against this Part of the Thesis is that large pieces of the text are copied from the author's master degree thesis, i.e. it is not clear which part should be evaluated as the work performed within the PhD Thesis. Some work presented here was likely done by author's collaborator rather than by herself. Regardless of the authorship or originality of this part of the Thesis, I'd mention that the outcome of the Part III could be compressed into words that the author and her collaborators have performed some simulations of formation of shells, relevance of which is not much clear. It is likely that the dynamical friction together with gradual tidal decay of the secondary galaxy would alter the structure of produced shells, but it is not clear, whether it will be in an extent that invalidate the method of reconstruction of the primary galaxy potential based on the simpler model described in the previous Part.

Conclusions are not written sufficiently self-explanatory, i.e. this Section does not briefly summarise the main results in a way that does not require to read (and remember) the whole Thesis. The Conclusions are introduced with quite a strong, and I think inadequate, statement that the author has "developed a new method

to measure the potential of shell galaxies from kinematical data..." There is no suggestion of methodology which could be applied to real data given in the Thesis. What the author shows is that the LOSVD of the shells should have a quadruple peak profile and finds relation of LOSVD to the potential of the galaxy within an idealised model. The author herself admits that there are several sources of confusion, starting from the real fate of the process of the formation of the shells to the accuracy of the observational data available which is an issue not discussed within the Thesis.

I have made many hand-notes during two readings of the Thesis; rewriting them consistently would cover several sheets of paper which would be quite exhaustive. Nevertheless, many of my pieces of criticism have something in common: First, the author uses very often too vague and/or inaccurate statements or e.g. definitions of quantities used later on. Methods are not described properly and sufficiently enough so that the results could be verified independently. I have a strong feeling that the Thesis was put together in a hurry, perhaps without re-reading its final complete text. Some hint for this feeling is a missing reference on page 37, just below eq. (3). First five lines on page 56 are quite buggy as well. At several places I miss analysis of the obtained results and their implications for the real world. Major example of this drawback is the whole Part III of the Thesis which I comment on above. As another example let me mention e.g. Sec. 11.6 named "Comparison of approaches." While it shows some plots with lines constructed according to different methods, a discussion of quality of the individual methods is not present. Similarly I lack any discussion of the quality of different methods used to reconstruct galaxy potential from the simulated data in Sec. 13.3. Moreover, in this Section, there is not given any argument for selection of models presented in Figs. 30 – 32.

Several examples of not well tuned wording that degrade quality of the Thesis follow: Already in the Introduction, in Sec. 5, the author makes rather strong and perhaps inadequate confronting statement about the alternate models of the shell origin, namely the "weak interaction model" (WIM). While it is stated in the section that "the WIM has nice explanations for many phenomena related to the shells," in a subsequent sentence, the author states that WIM suffers form that it is not confirmed by the observations. This statement by itself is probably true, but it should be admitted honestly that the same could be written about the merger model. In this context, the formulation about the merger model "which we assume to be the true cause of the formation of the shell galaxies" sounds too strong.

More importantly, the numerical realisation of the model of radial oscillations, used throughout the whole thesis is not properly introduced. The density profile of the shells depends upon the initial velocity distribution which is not determined at all. I further assume that all stars are released from the origin of the coordinate system, which however, is also not exactly given in the text. This incompleteness disallows independent verification of the results. Related to this is the artificial introduction of the surface density of the shells as an independent parameter to the model. This quantity, however, is uniquely determined by the initial velocity

distribution. By the way, it is not shown that the considered treatment of the shell surface density is consistent with the somewhat vague statement that the stars of the cannibalised galaxy occupy small volume in the phase space (first paragraph of Sec. 9). Considering further the surface density of the shell, it is not clear to me why the author decided to introduce such a quantity which is difficult to transform to the radial density profile needed to determine the line of sight velocity distribution. Following the incompleteness, I haven't found prescription how the quantity "intensity" (i.e. LOSVD) presented in several figures is calculated. While I have quite a good idea what this quantity should represent, I can't judge, whether its evaluation is done in a correct way. The explicit description would also be highly desirable in the context of discussion of (in)dependence of the LOSVD upon the shell surface brightness, which is one of its determining quantities (Sec. 9 and Figs. 14 and 15). The first time, the LOSVD is shown is perhaps in Figure 9. In spite of that there is not given any description how this Figure was constructed, it serves as a base for several very important statements, e.g. that the extremal line of sight velocity corresponds well to the maxima of the LOSVD (page 40). First, the word "well" makes the statement rather vague. Second, this result is not fully intuitive (intuitive idea against the presented profile is, that the shells are by definition high concentrations of stars with zero velocity, i.e. one may expect maximum of the LOSVD at $v = 0$) and, therefore a thorough discussion, better with some simple examples would be highly desirable. There is some discussion on this point later in the Thesis, but it is still not perfect (see my comments on the definition or description of the method how the LOSVD is calculated above) and, moreover, the reader is let for quite long time with the feeling of standing on water. At this point I would also suggest to consider LOSVD integrated over some range of radii which is likely what could be compared to real observations. The quadruple-peak profile is likely to be smeared then out depending on the width of the integration/observation slit.

The page 40 suffers from more inaccuracies of similar kind: let me mention the sentence "More precisely, for inward stars, points A and B are little closer to the centre as indicated if Fig. 9..." In my opinion, the quantification "little closer" even without being said with respect to what is far from being "more precise". Still on page 40 bottom, the formulation "separation in velocity between peaks" could be interpreted in several different ways. Slightly further, in Table 2, there is used quantity $r_{*,\max}$ which seems not to be defined anywhere. Is it the same as $r_{\rm s}$?

Only vague definition of $\Sigma_{\rm sph}$ is given in Section 14.1 (the reader can only guess what the "surface density of the sphere" is meant to be); afterwards, very loose argumentation is given for the chosen integration area in the integral of eq. (59) which, in the end, seems to be evaluated incorrectly. I think that the result should be $\Sigma_{\rm los} = 2\Sigma_{\rm sph}\Delta R^2$. In this Section, I also feel confused by the figures of the "intensity" (which is not properly defined, of course) vs. R. From the text I guess (but cannot find it to be stated explicitly) that the line of sight coincides with the axis z of the coordinate system sketched in Fig. 34. If so, then the coordinate R

should be perpendicular to the shell surface, i.e. I would expect the "intensity" which is likely something like brightness to have maximum at $R \approx r_{\rm s}$, which is not what we can see in Figs. 38 and 39. Their meaning is, therefore, completely obscured.

In Part III. the Multiple Three-Body Algorithm and its implementation is, again, not sufficiently exhaustively described and, therefore, the results presented here cannot be verified independently. Figs. 47 – 49 used to describe the secondary galaxy decay are not easy to read. I'd suggest to plot some quantity which evolves more smoothly, e.g. the orbital energy of the secondary galaxy in the potential of the primary

A mistake is probably in the caption of Fig. 31 where I assume should be $v_{\rm s,eq42-slope}$ instead of $v_{\rm s,eq22-slope}$.

To summarise: I think that the author have declared an ability for scientific work, though there is a lot of room for improvement of presentation of the results and critical attitude to them. The amount of work done within the Thesis is not great but some valuable results have been achieved. I recommend the submitted text to be accepted as the doctoral Thesis provided the author improves the presentation of the results during the defence.

Praha, 9.11.2012 Ladislav Šubr