MONASH University

26 August 2022

To the Scientific Council:

It is my pleasure to provide the following report for the doctoral dissertation of Ms Zijia Cui titled

"Disk-planet interactions: formation of mean-motion resonances in a gaseous protoplanetary disk".

Understanding how resonance capture occurs and under what circumstances is currently one of the leading areas of enquiry in the fields of exoplanets and planet formation. Many outstanding questions remain, including the two addressed in this thesis:

1. Under what circumstances does capture into second-order resonances occur, given their quite delicate nature (compared to first-order resonances)?
2. Why is the period ratio distribution of systems of exoplanets relatively uniform (for small period ratios)? Why aren't many more systems in low-order resonant configurations given that planets almost certainly undergo convergent migration in their birth disks?

The candidate addresses these questions in a manner which is fully consistent with the level of inquiry at PhD level. She has made important contributions which have been recognized by the publication of two papers in the Astrophysical Journal, with the second paper representing a particularly significant advance on our understanding of the planet migration process and the period ratio distribution.

While I have various comments and questions which I set out below, I do not require that the thesis be altered in any way. Instead, I would be most grateful if Zijia responded to my queries and comments directly.

In summary, I consider the doctoral thesis of Zijia Cui 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.

Comments and queries:

As a specialist of dynamics and not of hydro simulations, I will restrict most of my comments and queries to the first study. I understand that another examiner has extensive expertise in the second area.

I realise that the two studies presented in this thesis have been published, and as such have passed the scrutiny of referees. However, if I had been the referee of the first paper, I would have raised the following concerns (*quotes from the thesis in italics*):

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1. In order to reach the 9:7 commensurability (assuming the system was not formed near or in-situ and started life wide of 2:1), the system must have migrated through 2:1, (5:3), 3:2 and 4:3. The disk torques would need to have been strong enough to achieve this, but weak enough for the system to stop short of 9:7. Thus my concern is that simulations were started just long of 9:7, with disk parameters tailored to stop the system passing through for at least some of the initial parameters. Taking into account any diminishment of the the disk surface density in the mean time, were these disk parameters consistent with having migrated through 4:3? A much more convincing study would have been to start (at the very least) long of 4:3 (if not 2:1 or 3:2).

   Having said that, I realise the reasons set out in the thesis for not doing this included the fact that the migration timescale needed to be extremely long for capture to occur (following eqn 3.2 in the thesis – $10^6 - 10^7$ yr at 1 AU – much longer than the lifetime of a disk it seems), and that this is prohibitively long from an numerical point of view. How much shorter would the migration rate need to have been at the time it passed through 4:3?

2. The second reason given for starting close to 9:7 is that *"second-order resonance capture is possible only if the eccentricity of the orbit is nonzero. To achieve that, the planets need to arrive at the resonance before the initial eccentricity will be damped completely, which means that the system can only migrate for a time comparable to the circularization time."* But the eccentricities will never be completely damped between 4:3 and 9:7 except in the sense that they reach their equilibrium values which are always non-zero. Since a system which is captured into 9:7 almost certainly must have passed though 4:3, unless some perturbing mechanism was in operation it must arrive at the boundary of the 9:7 libration region with eccentricities equal to their local equilibrium (or overstable) non-zero values, making the capture process entirely non-probabilistic when the problem is analysed self-consistently in this way.

   Thus while I appreciate the CPU reason for starting close to 9:7, it necessitated "guessing" the initial eccentricities, which in turn resulted in the observed phase dependence of resonance capture for the disk parameters studied (which themselves were chosen to make capture happen for at least some initial phases). As one of the main aspects of the study, I was not convinced by this result.

   In this respect, I am surprised that the work of Goldreich & Schlichting (2014) was not referenced. There they introduce the concept of "overstable librations" of the eccentricities, and show that it is the ratio of the migration to eccentricity-damping timescales which determines whether or not capture is possible (albeit for first-order resonances). I think this work is of great relevance to every aspect of the thesis.

3. *"The fact that the resonances are not as common as suggested by some of the simple models, tells us a lot about the way in which the planets migrate or to be more precise in which they do not migrate."*

   What about observational selection effects? Perhaps resonant systems are more common than have been detected so far! See, for example, Leleu et al 2021, A&A, 655, A66, Leleu et al 2022, A&A, 661, A141 and Leleu et al 2022, arXiv:2207.07456.

4. Brief descriptions of the NIRVANA and FARGO3D codes and their advantages and disadvantages for the purposes of this study would have been very useful. In particular, while a standard discussion of the fluid equations is presented, the details of how shear and viscosity are implemented in the codes is not discussed. For example, do these codes handle shocks? If so, how do they (which numerical scheme is used)?

Some minor comments and queries:

1. *"In the discussion above, we only consider the forces acting on the fluid due to the pressure and gravity, which is valid in the case if the molecular mean free path* $\lambda \to 0$*."*

   Shouldn't this be $\lambda \to \infty$?

2. *"The self-gravity of the disk is neglected in our simulations since the masses of the disks adopted in our work are sufficiently low."*

   How low is low? Conversely, for what surface densities (for a given stellar mass) is it important to take self-gravity into account?

3. *"To start the simulations, we initialize the equilibrium model of the disk and then put the planet as a perturber into the disk."*

   What is "the" equilibrium model? How is this done numerically?

4. *"To avoid the singularity of the gravitational potential at* $r = 0$*, the softening parameter is introduced..."*

   The softening parameter is used to avoid $r = r_p$, not $r = 0$!

5. *"...while b is the softening parameter which is adopted to mimic the vertical extent of the disk."*

   What does "mimic the vertical extent of the disk" mean in this context?

6. *"The second-order resonance capture has been investigated in the past in the framework of semi-analytic models and N-body calculations. Those studies have provided very useful guidelines and testbeds for our studies."*

   There are various instances in the thesis where such statements are made but no references are given.

7. In the thesis it states *"The reference frame is set to be rotating with the Keplerian frequency at the location of the super-Earth"* which I understand to be the outer planet, while in Cui+2021 it states *"We found it advantageous to adopt a rotating frame that corotates with the Keplerian angular velocity at the initial location of the inner planet."*

   Which frame of reference was actually used? A short discussion of the advantages of working in the rotating frame would have been useful.

8. Figure 4.1 (regarding "observations"):

   Since the results of the study depend sensitively on the mass of the inner planet, the histogram in this figure would have been much more informative (and pertient to the results of the thesis) if planet mass ratio had been indicated.

I would like to finish by congratulating Zijia on an excellent thesis and to wish her the very best for the next stage of her career.

Yours sincerely,

Dr Rosemary Mardling