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A referee report on the PhD thesis of Mgr. František Dinnbier

The PhD thesis of Mgr. František Dinnbier entitled "Propagating star formation" is devoted to 3-dimensional hydrodynamical (HD) simulations. The author (and his coauthors) use a state-of-the-art code (Flash), perform calculations in a renowned supercomputing centre (IT4I) and study shells created in interstellar medium and confined by winds of massive stars. They focus on a fragmentation of shells and layers, in a plane-parallel & isothermal approximation, calculations of fragment masses and a possibility to form a next generation of stars.

They discover of a new mode of shell fragmentation, a collapse driven by coalescence. On the other hand, they rejected a possibility that this mode would create the observed regular patterns in interstellar medium (ISM). The resulting masses of fragments seem too *small* to create sufficiently massive stars and to propagate the formation further, which means the model presented so far is likely not the definitive one. The assumption of isothermal layers is actually contradicted by the observed densities of ISM and the respective masses of new stars (initial mass function; IMF), but this can be regarded as a critical assessment of the results.

Chapters 1 to 3 of the thesis serve as useful reviews. Chapters 4 to 6 (pages 65 to 107) summarize the original work of the author; together with appendices A and B (pages 125 to 154). A 23-page-long accompanying paper accepted to MNRAS is also attached.

I really enjoyed reading the review (Section 1), written in fluent English. Regarding Section 3.1, I kindly ask authors to present a complete set of HD equations which are actually solved, as detailed below. In Sections 5.3 and 6.1, there is another important issue, namely missing uncertainties of the quantities inferred from numerical simulations. It is then essentially impossible to rigorously compare the results with analytical models -- even though I assume the authors are well aware of inherent uncertainties of the models and approximations they use.

Nevertheless, the overall quality of the work is very good, and the work fulfills the requirements for a PhD thesis. Mgr. Dinnbier is also the 1st author of an accepted paper in a refereed journal. After a discussion of (moderate) issues described below, I beg to recommend the thesis to be accepted.

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Moderate issues (to be clarified during the defense):

Introduction:

Will Mgr. Dinnbier be a co-author of the paper Wunsch et al. (in prep.) mentioned in the Introduction?

No conference contributions are mentioned in this thesis.
Were the results already presented (by Mgr. Dinnbier) at some conferences?

Table 1.1:

What are the uncertainties of these numbers, \dot *M* and *v*_\infty in particular? They are never discussed.

page 16, paragraph 3, line 4:

The argument that energy distribution is 5/11 and 6/11 is unclear to me. Could you support it by a reference?

18, 2, 3:

$p = E/c$ is the linear momentum, L/c is actually momentum rate, $\mathrm{d}p/\mathrm{d}t$

18, 2, 5 and Eq. (1.6):

$\Phi = L/(4\pi R^2)$ is the *flux*, not radiation pressure, which is defined as $P_{\rm r} = 1/c \int_0^{4\pi} I\cos^2\theta\,d\omega$

18, 3, 3,:

"a red supergiant and then a Wolf-Rayer (WR) star" → Should be "Rayet", but more importantly, not all WR stars originate from RSGs.

29, 2:

The description of Sco-Cen OB association is fine, but its evolution is described only qualitatively and actual arguments are unclear (at least to me). What was a measured quantity and how the ages were determined?

50, 4, 4:

The three dispersion relations (V83, W10, and E78) give very different fragment masses -- by 2 orders of magnitude (!). What would be a general procedure how to decide which one is correct?

51:

Do you also account for small-scale turbulence (as a non-zero viscosity)? Are your simulations somehow affected by the *numerical* viscosity?

55, Eqs. (3.1) to (3.3):

This set of equations is not complete and not the one which

is actually solved. After carefully reading chapters 3 and 4, one can realise that the following terms are missing:

- **sink particles in Eq. (3.1);**
- **smoothing, the Ewald approximation in Eq. (3.2);**
- **radiation transfer, cooling and heating terms in Eq. (3.3);**
- **the Poisson equation or tree-code approximation, otherwise the system cannot be closed;**
- **for MHD problems, the induction equation would be also necessary.**

Could you rather present the complete set during the defense, please?
Is it “guaranteed” that the plasma is sufficiently neutral?
Is the system stable wrt. magneto-rotational instability (MRI)?

56, Eq. (3.4):

How can some stratification of the species can arise?
(All velocities are the same, aren't they?)

61, 4, 4:

"ionising photons can propagate only through the ionised medium" is a too strong statement. There is always a non-zero probability...

Section 3.2.2:

There is no radiation transfer equation explicitly written and there is literally no word (!) on opacities you use. Consequently, it is impossible to judge if the physical model is sufficient, or not. What is your source function? Is the description monochromatic or integral one? What potentially important processes aren't accounted for?

76, 1, 6:

Acceleration relative errors 10^-5 to 10^-4 are OK, but is it really sure they cannot lead to some artifacts, patterns or even instabilities? Do these errors accumulate in the course of time or not?

83, 1, 3b:

In the models presented in Section 5.2.2, you use a very simple cooling by setting the temperature, and no heating. What is the real cooling mechanism (radiative transfer, convection, ...)?

Table 5.1 and Figures 5.2, 5.3, Table 5.2:

There are no uncertainties of any quantities estimated from your 9 numerical models. It is then impossible to decide whether the differences between previous analytical models (E78 in particular) and your model are significant or not.

Moreover, the procedure mentioned on p. 86 "The data are binned to reduce the noise." is never described in detail and seems a bit dangerous in this context.

101, Eq. (6.3) and 103, Eqs. (6.7a to f):

Again, fits of simulation data seldom have zero uncertainties! Nobody then knows the real scatter of the simulation data and actual usefulness of the fit.

101, 4, 1b:

"... indicating a good agreement." ← The same problem, without any uncertainties the comparison is useless.

101:

There is no direct comparison with observational data in your work. What (types of) observations could constrain your model?

106, 1b:

Further work can be more elaborated.

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Minor comments (language, typography):

There are missing dashes everywhere (in the printed version) which makes reading unnecessarily difficult.

It would be useful to carefully distinguish -, --, --- and $-$.

Citations often have wrong brackets (\cite{} vs \citep{}).

There are missing spaces (\,) between numbers and units.

Pages 4-9 consist of long paragraphs without any \subsection's or \paragraph's, which isn't a nice structure of the text, even though the sequence is logical.

page 1, Sec. 2.2.4:

Sedond → Second (a typo in a subsubtitle)

Section 1.2

Was the review on stellar feedback mechanisms prepared on the basis of original references? Or did you start with another review?

page 23, paragraph 2, line 14:

"object is pointed" → "object is point-like"

40, 3, 3b:

"crutial" → "crucial"

42, 2, 3 and later:

"dimensionless" → "a dimensionless"

"on another" → "on other"
"on the first sight" → "at the first sight"
"in Section 2.2.1" → "later in Section 2.2.1"

43, 1, 1 and later:

"collapse in direction z" → "collapsing in z-direction"
"in P_ext" → "of P_ext"
"we see" → "we saw"
"in respect" → "with respect"
"define" → "defining"
"one yields" → "one obtains"

48, 2, 1:

"In previous section" → "In the previous Section"

48:

The description of dispersion relations is a bit tedious...

49, 2, 3 and later:

"With comparison" → "In comparison"
"Fig. 2.3", "A → 0", or "near 0" should not be split

50, 4, 6:

"mention" → "mentioned"

51:

"It is a nice theory..." is a subjective statement

sometimes, indefinite articles are missing, e.g. "homogeneous field", "from very high growth rate", "by acceleration"

The description of the KH instability is relatively too short and **g** is not necessary for the instability to operate; only the shear.

"become" → "becomes"

In the whole Section 2.3, some references are missing.

55, 1:

"Flash code" → "the Flash code"?
"Particular unit" → "A particular unit"
"routine doing anything" (Wow!) → "routine doing nothing"

56, 2, 1 and later:

"in grid" → "on a grid"
"by piecewise parabolic method" → "by a piecewise..."
"fitting cubic polynomial" → "fitting a cubic..."
"there is special treatment" → "there is a special..."

57, 2, 1 and later:

"spans" → "ranges"
"by minimum" → "by a minimum"
"$min(\Delta{\bf r})$" → "${\rm min}...$" as a function
"is minimum" → "is the minimum"

63, 1, 1:

"righ" → "right"
"cast" → "casts"
"Current version" → "The current version..."
"implementation" → "an implementation"
"significantly larger volume" → "a significantly larger volume"
"in dense ... layer" → "in a dense ... layer"
"for formation" → "for the formation"
etc.

Most indices in Eqs. should be \rm not \it.

64, 1, 2:

"unstable" → "unstable."
"grav. force" → "gravity force"

106, 3, 3:

"possible" → "capable"
"a constant" → "constant"
"accreting" → "accretion"

Bibliography:

It is better to switch names/surnames, at least for the 1st author.

Landau & Lifshitz (1959), Sedov (1959), Spitzer (1978) are incomplete references.

Miyama et al. ... "Simulations ." → "Simulations."

123:

E78 line is typeset with excessive spaces.

147--152:

The Eqs. should be typeset more carefully to avoid overlaps and overfull boxes.

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With kind regards,

doc. Mgr. Miroslav Brož, Ph.D.

February 20th 2017 in Prague