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A fast spectral method for the inelastic Boltzmann collision operator and application to heated granular gases

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Summary. This paper presents a fast Fourier spectral method for solving the inelastic Boltzmann equation with and without stochastic forcing. This work follows along the lines of the fast spectral method of Gamba, Haack, Hauck and Hu [14] to build a deterministic, fast solver for this equation, using a very general variable hard-spheres collision kernel for the cross-collision terms.

After an introduction of the model and the different numerical methods for solving it, the authors present their new method and an analysis of its computational complexity. This complexity is similar to the fast method introduced in [16], namely $O(AN^d \log N)$ for a moderately large A (importantly, independent on N). To obtain this result, they approximate a term which appears in the expression of the Fourier coefficient of the collision operator using Gauss quadrature for one part and spherical harmonics for the other part. They obtain two different methods, whether they decouple the collision operator in gain and loss term or not. The authors also present various numerical simulations to validate their methods, in the space homogeneous setting. The first test compares the temperature of numerical solution for the Maxwellian pseudomolecules case with the theoretical one, no other exact solution being known for that model. The authors show the good accuracy of the two methods in both two and three dimensions of the velocity space. They show in particular that the decoupling approach yields exactly the same results in 2d, but quite different (more accurate) in 3d. The second test concerns the numerical validation the so-called Haff's law, a physically relevant test which deals with the realistic hard spheres model.

Reviewer's opinion. **Not Publishable** in *JCP*: the reviewer requires **rejection**.

Critique. In the reviewer opinion, this work is not a true novelty, because it arrives more than three years after the fast spectral method for the Wu *et al.* paper [16] (which was in itself a refinement of a paper from the same group about multiple species Boltzmann equation). This method is almost the same than the one introduced in the manuscript, and the reviewer hardly sees then the relevance of such a work, in particular for possible publication in JCP, journal which "focuses on the computational aspects of physical problems. The scope of the Journal is the presentation of new significantly improved techniques for the numerical solution of problems in all areas of physics." The present manuscript hardly fits in this scope, especially given the fact that the numerical simulations presented are only space homogeneous.

Concerning the overall paper, the most important point for the reviewer is that the authors doesn't compare their methods with the one introduced in [16]. Comparing with the Classical Spectral Method from [11] is nice, and gives strong confidence regarding convergence of the new method, but concerning the accuracy and the computational times, [16] would be more relevant. Moreover, the reviewer thinks that, even if the derivation is different between here and [16], in the constant restitution coefficient case, both methods are the same (this fact is even almost said by the authors p. 4, l. 50). In that regards, the paper would gain a lot of interest to the reviewer if numerical simulations for the non-constant restitution coefficient-case were available.

The following are suggested modifications or improvements for this work:

1. P. 4 l. 70 the reviewer is surprised of the term “ambiguity” used for the strong form of the collision operator. The series of papers by *eg* Mischler, Mouhot *et al.* (JSP 2006 part 1 and 2, CMP 2009, DCDS A 2009) *eg* Alonso, Lods *et al.* (CMS 2011, CMP 2013 and SIMA 2015) asserts this expression quite clearly. More work on this topic is needed.

2. On p. 7, before (18), the reviewer would like to note that in the inelastic settings, the particles are macroscopic bodies (pollen, meteoroids, beads, ...) and not perfect molecules. This is the reason of the inelasticity of the collision, and as such, the collision kernel should be only of hard sphere type! The variable hard sphere kernel is not intended for granular gases, only molecular ones.

3. On p. 9, the computation of the temperature for the constant kernel is done, but the authors could also give some ideas of where the Haff’s law comes from (in particular the value of the coefficient C_0 in the simulations).

4. On p. 10, a reference about the fact that it suffices to take $R = 2S$ would be needed, because that doesn’t seems that easy for the reviewer. Same goes for the value of L latter in this page, because the inelastic case is slightly different on this regard that the elastic one.

5. When one deals with Fast Spectral methods, reference to the seminal paper of Mouhot and Pareschi, Math. Comp. 2005, would be necessary (for example on p. 11, before (36)).

6. The reviewer doesn’t understand AT ALL the assertion at the end of p. 11 saying that the “integrand in (39) is oscillatory on the scale of $O(N)$”. What does that means? More details (a lot !) are needed here!

7. On p. 12, expressions for the weight w_ρ and w_g are missing, as well as more details on the underlying numerical methods used here!

8. On the same page, l. 125, some more explication on the numerical complexity is needed, because the order of magnitudes are not the same than in the introduction section. The same goes for the storage requirement: numerical experiments showing that the new method is better on that regard than the classical one is needed.

9. What about the conservations properties of the new scheme? Are there any numerical simulations available?

10. Have the authors tried to take $e = 1$ namely the elastic case, in order to see if the BKW solution is obtained?

11. On p. 14, using spectral to compute the laplacian is nice, but what is the effect on the aliasing properties? Should one needs to change the truncation? this should be studied too, because the Laplace operator spreads in velocity, whereas Q concentrates, and the analysis on R and S is then broken.

12. On pp. 15–16, more explanations would be nice regarding the discrepancies between the decoupling approach in 2D and 3D. Moreover, [24] is not the good reference for the smoothing properties of the gain term, because this paper concerns only the elastic case. References toward on of the Mischler paper would be better (and the smoothing properties are by the way quite difference in the inelastic case).

Additional cosmetic suggestions and typos.

- On p. 5, l. 85, the expression for $(,_*)$ would improve the readability, as well as the one of J in (7).
- On p. 6, it seems to the reviewer that it is usually easier to derive the strong form of Q, starting from its weak form, rather than the contrary (due to the irreversible microscopic collision dynamics).
- Why does the author decide in the numerical section to use the BKW initial conditions? This has no physical relevancy in the inelastic case (and too much symmetries).