Referee report

The submitted paper "Transport coefficients in standard Kappa distributed plasmas" by Jwailes et al. (hereafter referred to as the manuscript), discusses transport coefficients such as the electric conductivity, based on the standard Kappa (SK) distribution originally introduced by Olbert (1968). The manuscript compares these results to those obtained using the modified Kappa (MK) distribution (Livadiotis 2015) and the associated Maxwellian distribution (MD).

The manuscript appears to be a follow-up to a previous paper by the same authors, "Transport coefficients in modified Kappa distributed plasmas" (hereafter referred to as Paper I). In Paper I, the authors extended earlier and simpler treatments of the Boltzmann collision term by employing the five-moment approximation for the Boltzmann collision integral. Considering three types of collisions (Coulomb, hard-sphere, and Maxwellian molecule collisions), they derived in Paper I expressions for the effective collision frequency in plasmas described by Kappa and Maxwellian distributions, and used those to formulate the relevant transport coefficients.

There is ongoing debate regarding the correct interpretation of Kappa distributions, particularly concerning the definition of temperature. One side argues for the MK with a κ-independent temperature, equal to that of the associated MD (recovered in the limit $\kappa \to \infty$). In contrast, the other side favours the SK with a κ-dependent temperature greater than that of the corresponding MD. While Paper I specifically discussed results based on the MK, the present manuscript focuses on the transport coefficients in the SK formalism. The authors highlight both qualitative and quantitative differences in the coefficients across the different distributions (SK, MK, MD) and collision types, and offer physical interpretations for these differences.

The manuscript is generally well written in terms of language and presents some new results; however, it lacks clarity regarding the scientific novelty and necessity. Additionally, in my opinion, some sections are overly lengthy and technical, which may make it difficult for readers to appreciate the results, some of which are not sufficiently highlighted. Before the manuscript can be considered for publication, I recommend a major revision. Below, I list my major and minor concerns regarding the content of the manuscript, as well as technical corrections.

Major Points

1. It appears that the manuscript does not clearly distinguish between new contributions that build on Paper I, and work that was already done in Paper I. In its current form, the manuscript gives the impression that it contains a novel derivation of the relevant transport coefficients using the five-moment approximation and SK formalism for the underlying particle velocity distribution (see the abstract or Section 3). But is this really the case? To my understanding, the transport coefficients based on the SK were already implicitly derived in Paper I. Therefore, this aspect of the manuscript would not provide a genuine scientific advance, yet it represents a significant portion of the manuscript.

In Paper I, the zeroth-order function for the five-moment approximation (Eq. 20) is given by

$$f_{\rm s} = \frac{n_{\rm s}\,\eta(\kappa_{\rm s})}{\pi^{3/2}\,w_{\rm s}^3}\left(1 + \frac{c^2}{\kappa_{0_{\rm s}}\,w_{\rm s}^2}\right)^{-\kappa_{\rm s}-1}, \quad \eta(\kappa_{\rm s}) = \kappa_{0_{\rm s}}^{-3/2}\frac{\Gamma(\kappa_{\rm s}+1)}{\Gamma(\kappa_{\rm s}-1/2)}. \tag{1}$$

Written in this form, the mathematical distinction between the MK and SK is determined solely by the form of $\kappa_{0_{\rm s}}$ (assuming that the temperature in Eq. 1 is independent of $\kappa_{\rm s}$). Therefore, Eq. (1) serves as the general form for both the MK and SK (see Eqs. 20 and 22 from Paper I, compared to Eqs. 1 and 4 in the manuscript). The MK is then defined with $\kappa_{0_{\rm s}} = \kappa_{\rm s} - 3/2$, while in the SK formalism, $\kappa_{0_{\rm s}} = \kappa_{\rm s}$.

Thus, throughout the derivation of the effective collision frequency and transport coefficients, $\kappa_{0_{\rm s}}$ remains an algebraic constant (see appendix of Paper I). The entire derivation can be carried out using the general form in Eq. (1), and only at the end, once the coefficients are obtained, is the explicit form of $\kappa_{0_{\rm s}}$ specified, depending on whether the MK or SK case is considered. Consequently, the SK formalism is already fully encompassed in Paper I, with the present manuscript differing only in the final substitution for $\kappa_{0_{\rm s}}$. Similarly, any discussion about the transport coefficients approaching the MD-based results in the limit $\kappa \to \infty$ is already contained in Paper I as well, regardless of the specific Kappa distribution type.

Therefore, if my understanding above is correct, I recommend that the authors clearly indicate that the actual derivation was already carried out in Paper I, where only the MK case was discussed, and clarify that the current manuscript builds upon this by focusing on the discussion and interpretation of the SK case.

2. Accordingly, the abstract should not focus on the method, but instead place the current work in context by motivating its necessity and clarifying how it builds on the previous study.

3. As another consequence, Section 3 is lengthy and technical, without adding new results compared to Paper I beyond the explicit insertion of the κ_0 expression for the SK formalism. In my opinion, many of the equations included are unnecessary for the subsequent discussion and interpretation of the differences in the SK- and MK-based results. I suggest that the authors keep only what is essential for the discussion and refer to Paper I for details.

4. Section 2 is also relatively long and discusses points that have been already addressed by other authors in previous works, for example by Lazar et al. (2015) and Lazar & Fichtner (2021), where the authors refer to the MK and SK as Kappa A and Kappa B, respectively. Some of the points in the text, as well as the overview in Table 1, were also addressed in earlier works such as Scherer et al. (2020). It also reiterates points already mentioned in the introduction regarding the differences between a Maxwellian and Kappa distributions, or the MK vs SK debate. Table 1 could be omitted or at least put into an appendix, while the main text could be reduced to highlight only the main differences, citing earlier works for further information. Alternatively, Section 2 could be omitted by placing the reference to the MK vs SK debate only in the introduction.

5. Figure 10 presents an interesting result, where the electric conductivity based on the MD is smaller than that based on the SK, but larger than the MK-based graph. The authors explain this behaviour by pointing out that the MK has an increased core compared to the associated MD, therefore providing a state with more frequent collisions between particles, which lowers the conductivity of the plasma.

Did the authors compare these findings and interpretations with earlier works cited in the manuscript? Do previous results show the same ranking of the three distributions involved? For example, I plotted the electric conductivity presented in one of the cited works, Husidic et al. (2021), and if I did not make a mistake, both the graphs based on the MK and SK yield larger values than that based on the MD.

If that is the case (this should be double-checked; the authors may also wish to check this for expressions in other cited works), it would suggest that the simplified approach of using a Krook-type collisional operator does not adequately capture differences between the different types of Kappa distributions. More precisely, the simplified approach does not sufficiently account for the increased thermal core of the MK, but rather the suprathermal tails dominate the trend of the electric conductivity. This would actually further motivate the use of more accurate expressions for the Boltzmann collision integral, such as the five-moment approximation utilised in Paper I.

6. The current introduction has a long first paragraph containing basics from plasma theory, which should be known to most readers. The paragraph about observed non-Maxwellian particle distributions in space plasmas should include a few more citations regarding the observations and modelling.

   The authors discuss in the introduction earlier transport theory studies that used the MK, and point out how the SK differs from the MK. But why do we require a form of transport coefficients based on the SK? Are there any works in other aspects of space plasmas, where the SK formalism has shown an advantage over the MK formalism? Is the use of the SK a more realistic approach? Does the MK still have its right to exist?

7. The thermoelectric coefficient derived in Paper I is independent of the κ-parameter and thus identical for both Maxwellian and Kappa distributions. This is mentioned both in Paper I and the present manuscript, but not further addressed. In particular, the manuscript does not acknowledge that this result stands in contrast to previous studies cited within, such as Jiulin (2013), Guo & Jiulin (2019), and Husidic et al. (2021), where a clear κ-dependence of the thermoelectric coefficient was found.

   Is it not surprising that in the five-moment approximation, although more physical than the simplified methods of previous works, the κ-dependence in the thermoelectric coefficient does not appear? Could this be a limitation of the five-moment approach, with κ-dependence potentially emerging if higher-order moments or alternative methods are used? In my opinion, this issue deserves direct discussion in the manuscript.

Minor Points

1. The current title of the manuscript is rather generic and does not clearly highlight how this work differs from previous studies with similar titles on transport theory in Kappa-distributed plasmas. I suggest that the authors consider a more specific title.

2. Lines 47 and 53: *All of the reviewed studies...* and *Thus, all reviewed works...*: Strictly speaking, these statements are not entirely accurate, since the manuscript also cites Paper I along with the studies that used simpler approaches.

3. Line 49: The acronym "BGK" is not defined.

4. Lines 97, 115: The acronyms SK and MK are introduced but not used elsewhere in the text. They should either be removed or consistently used throughout the manuscript. I recommend following the standard practice of using the acronyms after their introduction, which would make the text easier to read.

5. Eq. (3): Is it correct to use the species subscript s also for v?

6. Line 99: *thermal velocity*: Since this is not a vector quantity, would "thermal speed" be more appropriate? Furthermore, it should be noted that the term can be ambiguous. For example, some authors (e.g. Olbert, Vasyliunas, Huang) refer to Eq. (2) in the manuscript as the most probable speed, while "thermal speed" is reserved for other expressions (see textbooks on thermodynamics and statistical physics).

7. Lines 106–108: It would be helpful to include one or two citations where an interested reader can find details.

8. Line 109: Strictly speaking, as κ increases but remains finite, the Kappa distribution does not fully reach the MD. It would be more precise to state that the MD is recovered in the limit $\kappa \to \infty$.

9. Line 119: The use of the term "invariant" is unclear here, since κ_{0_s} changes as κ_s varies.

10. Line 146: The authors could consider rephrasing for more clarity, for example "...both the MK and SK exhibit suprathermal (or enhanced) tails compared to the Maxwellian distribution".

11. Figure 1 and others: The units or normalisations used for the plotted quantities are not specified. Even indicating "arbitrary units" would improve clarity.

12. Figure 1: The authors should consider whether the figure is necessary, as Fig. 2 seems sufficient to illustrate the trends and differences between the MK, SK, and MD.

13. Figure 2: The authors may wish to consider using a logarithmic scale for the ordinate axis, as in Lazar et al. (2016), to better illustrate the differences in the high-energy regime between the distributions.

14. Lines 157–163: It would be helpful to provide citations to support the statements.

15. Line 190: The quantity $\mathbf{M}$ is not defined when it is first introduced in Eq. (11).

16. Line 193, *coordinate space*: In this context, velocity (or momentum) components are also considered coordinates. The authors may wish to consider a clearer formulation.

17. Lines 293–301: It is somewhat confusing to read that the authors start with a drifting SK, but then immediately neglect the drift.

18. Figure 3: It would be helpful to explicitly indicate the κ-dependence so the figure is understandable on its own (or refer to an equation in the caption).

19. Figure 3: Since $\kappa > 3/2$ is required, it might be clearer not to plot the κ-dependence for values below this threshold.

20. Figure 10: For consistency, the authors should clarify in the caption and main text that this figure refers to Coulomb collisions.

21. Tables 2, 3, 4: The authors should consider moving these tables to an appendix, as the information may interrupt the flow of the main text and is largely available from the discussion.

22. The authors should ensure that all references include correct and appropriate links. For example, the link for Lazar & Fichtner (2021) does not direct to the actual source on the Springer website, and other links such as Livadiotis (2018) are broken.

Technical Corrections

1. Line 14: A comma is missing after "e.g." (assuming American English conventions).

2. Line 14: The word "particles" should be "particle".

3. Line 18: An "and" is missing before "the external forces".

4. Line 26: The word "non-uniform" should be "nonuniform" for consistency with the remaining text.

5. Line 33: An "a" should be inserted before "powerful class".

6. Lines 37/38: The citations should be separated by a comma, not a semicolon.

7. Line 43: An "a" should be inserted between "to" and "weaker".

8. Line 56: "In this approach, a new transport theory is...", "is" should be "was".

9. E.g. lines 111 or 115: Equation numbers are sometimes given without parentheses and sometimes with. For consistency, they should be used with parentheses. I also suggest to use the abbreviation "Eq." in the middle of sentences instead of writing out "equation".

10. Line 143: Should this read "particle velocities"?

11. Line 215, *...in the s particle reference frame, ...*: "reference frame of particle s" would be clearer. Additionally, in case specific labels such as "e" or "p" are used for s instead of numbers, the "s" should not be in italics.

12. Line 350: I believe "reduced" should read "reduction".

13. Line 351: Typo; it should be "collisions" instead of "collision".

14. Line 377: The comma in "...on, the..." should be removed.

15. Line 378: There is an extra "the" in the sentence.

16. Long paragraphs: Some paragraphs, such as the first paragraph in the introduction and the second paragraph in the conclusions, are rather long and could be divided for improved readability, especially in a potential two-column format. But this is largely a matter of style.