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Reply to the Comments of Sharipov on "Symmetry of the Linearized Boltzmann Equation"

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Abstract This is a reply to the comments by Sharipov on the papers published in J. Stat. Phys. **136**:751 (2009) and in J. Stat. Phys. **136**:945 (2009). The present reply will show that the comments do not apply to the papers.

This is a reply to the comments by Sharipov [1] on our recent papers [2, 3]. In the last two paragraphs of Sect. 7 in [3], we pointed out two flaws in the theory [4, 5] for unbounded domains. His comments state that our conclusion in [3] on this part is completely wrong. The present reply will show that his comments do not apply to our papers [2, 3].

Before going into details, it should be emphasized that we restrict ourselves to a monatomic single-component gas without external force because the theory [4, 5] contains the flaws even in this simplest setting. Actually, one of the flaws causes the incorrect formula (5.39) of [5] for thermophoresis. The other makes it impossible to justify the reciprocity by the discussion of [4] for unbounded domains. These issues are clearly explained in the last paragraph of Sect. 7 of [3].

Point 1: Incorrect estimate (33) of [4] and its influence
In the second paragraph, the comments mention the footnote 3 of [2] and assert that (33) of [4] is not a key estimate because it is used only for unbounded domains. However, this is converting the subject. The footnote 3 is put to the sentence about unbounded domains. Actually, the estimate (33) is the key to justify (35) in the discussion of [4] for unbounded domains. Without (35), the reciprocity (56) of [4] cannot be obtained.

In [4], the estimate (33) is derived from two assumptions. (i) The solution approaches a uniform perturbed Maxwellian. (ii) Particle, momentum, and energy fluxes through the control surface (Σ_g in [4]) are finite. However, this estimate is not valid. There are counter-examples to it. For instance, consider a slow uniform flow past a sphere, which satisfies the assumptions. In this case, it is known that the solution approaches the far-field perturbed

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Maxwellian with the rate of r^{-1} as $r \to \infty$, where r is the distance from the center of the sphere (also remember the Stoke flow or Fourier temperature field). However, (33) of [4] means a much faster rate of r^{-2} and thus is not a correct estimate of the far field. Thus, (33) of [4] cannot be obtained from the above assumptions.

The above counter-example is mentioned in Sect. 7 of [3], but the comments do not give any evidence to disprove it. Thus, the reciprocity (56) of [4] is not justified by the discussion of [4] for unbounded domains. This influences the results in [5] for unbounded domains.

In Sec. 7 of [3], we mentioned that our estimate makes (35) [not (33)] of [4] survive in a certain class of situations. Thus, as we assured it in [3], some results for unbounded domains in [5] may survive if they are in that class. This is why there are equations of [5] that agree with the counterparts of [2]. As is seen from the above discussion, however, the agreement does not mean that the paper [4] is correct. The paper [4] does not justify those equations. By the way, the examples of known relations in [2] are typical problems studied by different authors. Thus the last comment in the third paragraph [1] is not understandable.

As is clear from the above discussions, the theory [4] has a flaw for unbounded domains.

Point 2: Inconsistent assumption on a far field and the thermophoresis

In [4], linearization is made around a local Maxwellian. Thus, the assumption (i) mentioned above restricts the form of that local Maxwellian at a far distance (e.g., remember the classical result by Grad [6]). Inconsistent assumption by the lack of this observation causes the incorrect formula (5.39) of [5] for thermophoresis. In this case, the inconsistency came to the surface in such a way that a non-Maxwellian term is missing. Thus the reasoning for the error of (5.39) in the third paragraph of the comments ("careless omission" of non-Maxwellian term) just tells the consequence of the inconsistent assumption.

Finally, in the fourth paragraph, the comments assert a generality of the paper [7]. It is, however, a consequence of stopping the examination of the far field. Thus, the inevitable estimate of the far field is not correct or is lacking in [4, 7]. As a result, for instance, [7] claims the validity of the results in [5] (see the last five lines of Sec. IV of [7]). As mentioned above, the result for thermophoresis in [5] is not correct. Thus, the comment that the last equation on p. 777 of [2] follows directly from [1] is not consistent with the facts.

In conclusion, as is clear from the present reply, there are two flaws in the theory [4, 5] for unbounded domains. The comments do not give evidence that denies the flaws. The paper [7] lacks the inevitable estimate of the far field. Thus, the comments [1] are not appropriate.

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