# Response to "Comment on 'Note on the relation between thermophoresis and slow uniform flow problems for a rarefied gas'" [Phys. Fluids 22, 049101 (2010)]

Shigeru Takata[a)]
*Department of Mechanical Engineering and Science, Graduate School of Engineering, Kyoto University, Kyoto 606-8501, Japan*

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This is a response to the Comment by Sharipov[1] on our recent paper.[2] The Comment[1] asserts that the thermophoresis is not considered in the theory[3] [for short, (S1-1)] and that we assert its nonexisting error [for short, (S1-2)]. The Comment[1] also states that our previous papers[4,5] imitate his works[3,6] [for short, (S2)]. The present response will show that the Comment is not appropriate.

We first summarize main facts to be noted:

(a) Numerical data in Ref. 2 show that the formula for thermophoresis given in Ref. 7 is not correct. The cause of error is explained in the last paragraph of Sec. IV of Ref. 2.
(b) The formula[7] was derived by Sharipov as an application of the theory.[3]
(c) The estimate (33) of Ref. 3 does not hold, so that the theory[3] does not justify the reciprocity for unbounded domains. It is mentioned in Sec. 7 of Ref. 5.
(d) References 4 and 5 present original ideas and consequences such as a set of Green functions, their pointwise reciprocity, the pointwise Onsagar–Casimir relation, etc.

These facts will be the base of the detailed response below.

In Ref. 2, we explained that the incorrect formula of Ref. 7 is caused by the inconsistent assumption of Ref. 3 for the far field [see (a) above]. The inconsistency can arise when the linearization is made around a local Maxwellian in Ref. 3, which is pointed out as an erroneous assumption in Sec. 7 of Ref. 5. An inconsistent assumption leads to an incorrect result, which is of general nature. This inconsistency is likely to be overlooked. In fact, the validity of the incorrect formula[7] has been claimed in his later papers.[6,8]

The facts in (b) and the previous paragraph show that the thermophoresis has been considered by the theory[3] without noticing the inconsistent assumption. Thus, the assertion (S1-1) is not appropriate. It just shows that the scope of the theory[3] has not been considered seriously from the very start of the series of papers.[3,6–8]

As to the assertion (S1-2), the Comment[1] asserts that the paper[3] has no error under the assumption (iii) of Ref. 1. However, because of the flaw mentioned in (c) above, this assertion does not apply even when the above consistency of assumption is ensured. The flaw is concerned with the basis of the theory[3] *for unbounded domains*. It should be noted that we stressed this point in Sec. 7 of Ref. 5 and made an affirmative statement on the theory[3] for bounded domains to avoid unnecessary confusions. Thus, the latter half of the third paragraph of the Comment[1] is converting the subject and is not appropriate.

Reference 3 states that the estimate (33) of Ref. 3 holds if the particle, momentum, and energy fluxes through the control surface ($\Sigma_g$ in Ref. 3) are finite and if the assumption (iii) of Ref. 1 is satisfied. However, this estimate is not valid. There are counterexamples. 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 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 Stokes flow or Fourier temperature field). However, Eq. (33) means a much faster rate of $r^{-2}$ and thus is not a correct estimate of the far field. Thus, the estimate (33) of Ref. 3 cannot be obtained from the above assumptions.

The above counterexample is mentioned in Sec. 7 of Ref. 5 [see (c) above], but the Comment[1] does not give any evidence to disprove it. Thus, the reciprocity of Ref. 3 is not justified by the discussion of Ref. 3 in the case of unbounded domains.

In Sec. 7 of Ref. 5, we also mentioned that our estimate eventually makes the reciprocity of Ref. 3 survive in a certain class of situations. This is why, for instance, Eq. (5.42) of Ref. 7 agrees with the counterpart in our paper.[4] Obviously, however, the agreement does not mean that the theory[3] is correct for unbounded domains.

As is clear from the above discussions, the theory[3] has a flaw even in the case where the Comment[1] asserts its validity for unbounded domains. Thus, the assertion (S1-2) is also not appropriate.

Next, as to the assertion (S2), we need to give brief remarks on the feature of Refs. 4 and 5 [see (d) above]. First, we introduced a Green function approach in Ref. 4, which is original and is free from the entropy production argument. Thus, Ref. 4 is in its nature different from the works[3,6] of Sharipov. One of main outcomes of Ref. 4 is a general expression of mass, momentum, and heat fluxes through the

[a)]Also at Advanced Research Institute of Fluid Science and Engineering, Kyoto University. Electronic mail: takata@aero.mbox.media.kyoto-u.ac.jp.

 

boundary in terms of the Green function. The Green function approach naturally leads to a reciprocity in a way of point correspondence. In Ref. 5, on the basis of this reciprocity, the Onsager–Casimir relation is established in a way of point correspondence. This is not found in the literature. From such a detailed reciprocity, the global type reciprocity is obtained. As explained earlier, Ref. 3 does not justify this reciprocity for unbounded domains. Our approach[5] also copes well with the problem of possible divergence of entropy production. This problem has not been considered before. As is seen from these facts, our papers[4,5] present new ideas and consequences. Thus, the assertion (S2) (the last sentences in the fourth and last paragraphs of the Comment) is not appropriate.

As to the comments in the sixth paragraph of Ref. 1, we do not see the reason for citing Ref. 6, because it does not contain new result of the thermophoresis. Rather, it claims the validity of the results in Ref. 7 (see the last five lines of Sec. IV of Ref. 6). As explained in (a) above, the result of thermophoresis in Ref. 7 is not correct. Thus, the assertion in the Comment[1] that the correct formula for thermophoresis follows directly from Eq. (36) of Ref. 6 is not correct. The reason why Ref. 6 did not detect the error is that it is a consequence of stopping the consideration of the far field behavior. Thus, the essential part of discussion for unbounded domains is not correct or is lacking in his two theories.[3,6] By contrast, the far field behavior is properly considered in Ref. 4. Thus, the correct formula (3) of Ref. 2 is directly obtained from the general expression of momentum flux in Ref. 4.

In conclusion, as is clear from the present response, the Comment[1] is not appropriate. The theory[3] has been related to the thermophoresis. It has a flaw even when the assumptions in the Comment[1] are satisfied and the consistency of assumption at a far distance is ensured. We also gave brief remarks on new ideas and consequences of our previous papers[4,5] to avoid any preconceptions caused by the Comment.[1]


[1] F. Sharipov, "Comment on 'Note on the relation between thermophoresis and slow uniform flow problems for a rarefied gas'," Phys. Fluids **22**, 049101 (2010).

[2] S. Takata, "Note on the relation between thermophoresis and slow uniform flow problems for a rarefied gas," Phys. Fluids **21**, 112001 (2009).

[3] F. Sharipov, "Onsager–Casimir reciprocity relations for open gaseous systems at arbitrary rarefaction. I. General theory for single gas," Physica A **203**, 437 (1994).

[4] S. Takata, "Symmetry of the linearized Boltzmann equation and its application," J. Stat. Phys. **136**, 751 (2009).

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[8] F. Sharipov, "Comments on 'On the theory of thermal polarization of bodies in a rarefied gas flow'," J. Non-Equilib. Thermodyn. **21**, 164 (1996).