Reply to "Comment on 'Quantum linear Boltzmann equation with finite intercollision time'"

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Hornberger and Vacchini [Phys. Rev. A **82**, 036101 (2010)] claim that the specific collisional momentum decoherence, pointed out in my recent work [Phys. Rev. A **80**, 064104 (2009)], is already described by their theory. However, I have performed a calculation whereby I disprove the authors' claim and refute their conclusion that my recent work had no advantage over theirs.

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In their Comment [1], Hornberger and Vacchini (HV) claim without a direct proof that the "surprising collisional decoherence effect" (SCDE), central to my new quantum linear Boltzmann equation (QLBE) [2], "is fully accounted for in the QLBE" of the authors [3]. If this claim is correct then my work [2] is superfluous. However, it turns out that the HV QLBE completely ignores the SCDE.

Let us study SCDE—a robust *single collision quantum effect*—in helium gas (molecule mass $m = 0.67 \times 10^{-23}$ g, temperature $T = 300$K, density $n_g = 10^{19}\ \mathrm{cm}^{-3}$). Consider a test particle of mass $M = \mu m$, where μ is, say, 100. Suppose a cross section $\sigma = 10^{-14}\ \mathrm{cm}^2$ with hard collisions so that the momentum transfer be of the order of $\sqrt{mk_BT}$. The mean intercollision time becomes $\tau \sim 10^{-9}$ s. Suppose our test particle is in pure state at rest and choose the following isotropic coherent momentum spread for it:

$$\Delta P = \sqrt{Mk_BT} \sim 10^{-17}\,\mathrm{g\,cm/s}. \tag{1}$$

Suppose a collision happens to such initial state. According to Eq. (5) of Ref. [2], this single collision *completely* decoheres one component $P_\parallel$ of the test particle's postcollision momentum. In reality, there will be a residual coherence $\Delta P_\parallel^{\mathrm{res}}$ due to the eventual finiteness of both the molecule's coherence length and the intercollision time. In our example the latter one dominates. A finite τ leads to an uncertainty $\hbar/\tau$ of the energy balance (4) in Ref. [2], which allows for a residual postcollision coherence:

$$\Delta P_\parallel^{\mathrm{res}} \sim \frac{\hbar}{\tau}\mu\sqrt{\frac{m}{k_BT}} \sim 10^{-21}\,\mathrm{g\,cm/s}. \tag{2}$$

This means a reduction of the precollision coherence (1) by four orders of magnitude in a single collision. Such sudden drop cannot be resolved by kinetic equations, yet a QLBE may qualitatively account for it by an extreme high value of the coefficient D_{xx} of momentum decoherence.

The HV QLBE [3], as well as Ref. [4], predict the momentum decoherence rate $D_{xx}(\Delta P)^2$, where

$$D_{xx} = \mathrm{const} \times \frac{1}{\mu\tau Mk_BT} \tag{3}$$

is fully classical, $\hbar$ is not involved. During a period τ, the predicted ratio of momentum decoherence of the chosen initial spread (1) takes this simple form:

$$D_{xx}(\Delta P)^2\tau = \frac{\mathrm{const}}{\mu} \ll 1. \tag{4}$$

The coherence (1) looks preserved for times $\sim\mu\tau$ extending over many collisions. The SCDE [2] is not at all accounted for by the old QLBE.

My work [2] derives a new expression (23) for the coefficient D_{xx}, which contains the quantum factor const $\times$ $(\tau k_BT/\hbar)^2 \sim 10^8$ with respect to the old D_{xx} (3). With $\mu = 100$, my QLBE predicts a decoherence ratio $\sim 10^6$ instead of $\sim 10^{-2}$ in expression (4). This is a heuristic signal of the SCDE, to indicate that the average drop of the coherence $\Delta P = \sqrt{Mk_BT}$ is extremely big on the time scale τ. Once this fast transient behavior is over, my QLBE is expected to faithfully treat the dynamics of the residual momentum coherence in or about the range (2). That is the subject of further investigations.

My QLBE [2] involves new heuristic considerations, like the finite time phenomenology of scattering theory. Items (i)–(iv) of the HV criticism [1] may well hold while the criticized "unfavorable properties" are the price I consciously paid for the SCDE be accounted for. Item (v) is conceptional, but HV's concept of quantum-classical correspondence should have been made precise, otherwise the related criticism cannot be checked. Fortunately, the remaining two can be since they criticize physical predictions. The lack of Gibbs stationary solution (vi) is physically plausible in my theory where the test particle never ceases to interact with the molecules. (I perceive that HV argue against such extension of conservative scattering theory.) The corrections with respect to the Gibbs stationary state will all (but Lamb's) disappear in the diffusion limit [2] of my QLBE, too. Criticism (vii) finds unphysical that D_{xx} grows above all bounds when $n_{\mathrm{gas}} \to 0$. Now, the growth of the SCDE is counterintuitive but real: ΔP^{res} tends to zero, this needs few molecules only and a very long τ. Thus real is the growth of D_{xx} as well. Obviously, my QLBE model of the SCDE will break down before $n_g = 0$ and $\tau = \infty$ because, e.g., the residual coherence ΔP^{res} might get influenced by the molecules' coherence length.

I have proved that, contrary to the HV Comment, the HV QLBE does not describe the SCDE [5]. I also showed that my QLBE does. HV's criticism has thus been put into a different perspective. To get SCDE accounted for, I needed more heuristics than before.

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[2] L. Diósi, Phys. Rev. A **80**, 064104 (2009).
[3] B. Vacchini and K. Hornberger, Phys. Rep. **478**, 71 (2009).
[4] L. Diósi, Europhys. Lett. **30**, 63 (1995).
[5] HV hold a vision of collisional momentum decoherence that somehow differs from mine. Their vision was likely to prevent them from recognizing the SCDE in its full capacity. Perhaps they deny that the residual coherence after a single collision is so little as it in fact is. Perhaps they agree. It is difficult to determine because the HV Comment does not criticize the SCDE itself.