Vacchini Replies: In his Comment [1] O'Connell objects that the master equation (ME) I recently derived in [2] (henceforth VME) describing the dissipative behavior of a Brownian particle interacting through collisions with a gas of Boltzmann particles is not acceptable. To reach this conclusion he refers to a ME derived in [3] and recently reproposed in another Comment [4], which is essentially the quantum optical ME in the rotating wave approximation (RWA), but with an explicit expression for the decay rate. He then comes to a comparison with VME inserting in it a harmonic oscillator (HO) potential. This modified ME is then compared to the high temperature limit of [3]: invariance under the transformation $x \rightarrow x\cos\theta + p\sin\theta$, $p \rightarrow -x\sin\theta + p\cos\theta$ or equivalently $a \rightarrow ae^{-i\theta}$, $a^\dagger \rightarrow a^\dagger e^{+i\theta}$ is now lost and a state having a canonical structure with the HO Hamiltonian is not a stationary solution. These remarks are correct, but the problems arising by the changes thus operated on VME could have been expected from the very beginning (apart from applying to other published ME, e.g., [5]). They are not an appropriate objection to VME, even though they are useful in clarifying a few issues. The key result was derived by Lindblad in a paper [6] he wrote just after his work on the generators of completely positive (CP) time evolutions. In [6] he considered the Brownian motion of a quantum HO and showed that in this case one cannot simultaneously satisfy the three requirements of CP, translational invariance (TI), and equipartition. This criterion is a meaningful way to compare various descriptions of quantum dissipation arising in different physical communities, as recently done in [7]. VME was obtained for the description of a free particle undergoing Brownian motion, so that TI is a central issue, and plays an essential role in the derivation. The physically relevant operators are $\hat{x}$ and $\hat{p}$, transforming in the usual way under space translations: a rotation in phase space has no physical meaning and invariance under this transformation is not an issue. Obeying CP and TI, the ME extrapolated to the case of an HO is not expected to lead to the canonical equilibrium solution: in fact, in [8] where VME is considered the authors look for the stationary solution. Deviations of the stationary solution from the canonical form was recently considered in [7], where these deviations are shown to be a necessary consequence of TI and vanish in the RWA. The ME given in [3] is obtained exactly in the RWA, thus granting CP but violating TI, as the very authors stressed in an immediately subsequent paper [9]. This does not mean that the derived ME is not acceptable; it simply describes different physics. The relevant operators are a, $a^\dagger$ and invariance under rotation in phase space is now an important issue, correctly taken into account in the RWA: one then has the canonical equilibrium solution. Note that the question about the physically meaningful equilibrium state is not necessarily obvious if one takes correlations properly into account [7]. VME has all the three *desired* properties: besides CP and TI it has the expected stationary solution $\exp(-\beta\hat{p}^2/2M)$. This exception for the free particle was already mentioned in [6]. The main point in [2], however, is the microphysical derivation, so that the result is not necessarily meaningful when extrapolated to another physical context. In the derivation the Brownian limit $m/M \ll 1$ is essential. O'Connell before making any comparison takes the high temperature limit *following Vacchini:* actually I never take this limit, typical of the HO treatment. In [2] Boltzmann statistics was considered, but this result has already been extended to quantum statistics, putting into evidence the role played by the dynamic structure factor [10]. What I consider was the limit of small momentum transfer, analogous to the Kramers-Moyal expansion leading from the *master equation* to the Fokker-Planck equation.

Though the objection raised against the validity of VME does not hold, the point raised leads to a clarification of some issues not always clearly spelled out. I would also like to recall that for the unmodified VME the expected canonical distribution is a stationary solution and to stress that I do not question the general validity of equipartition. One simply cannot expect that the extrapolation of a ME obtained for a specific physical model should work without flaws in another context.

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Bassano Vacchini
Dipartimento di Fisica and INFN
Università di Milano
Via Celoria 16
I-20133 Milan, Italy

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