Xiao *et al.* Reply: We do not think the Comment by Duval *et al.* [1] addresses the main result of our Letter [2]. Our main result is that the density of quantum states in the weak field limit is modified from the usual form of $(2\pi)^{-d}$ to the expression of our Eq. (3). This is a quantum mechanical concept, and its application shown in Eqs. (5)–(11) is based on this understanding. In practice, it means that in the semiclassical limit, when replacing the sum over states to the integral over the $\boldsymbol{k}$ space, one should use the properly defined density of states,

$$\sum_{\boldsymbol{k}} \to \int \frac{d^d k}{(2\pi)^d}\left(1 + \frac{e}{\hbar}\boldsymbol{B}\cdot\boldsymbol{\Omega}\right). \tag{1}$$

On the other hand, the phase space volume in the Liouville theorem is a classical concept, and our discussion on it was just to motivate our main result.

The Comment also does not contradict any of our results in substance. Our claim is that the Liouville theorem on the volume conservation in the phase space of position and momentum is violated. Here "momentum" means the gauge invariant physical momentum. By direct calculation, we found that the Liouville theorem can be restored if one uses a modified measure of the phase space volume. The authors of the Comment agree with this result, but they pointed out an alternative route for reaching the same result apparently known in the largely mathematical and abstract field of noncanonical Hamiltonian dynamics [3].

Their only real objection is to our consideration of the "naïve definition" of the phase space volume from the very beginning, because "an abstract phase space carries no natural volume element" which "can be defined only through a symplectic form." We can understand such a point of view if one's scope is limited only to the mathematical object of symplectic dynamics devoid of physical meaning. However, the purpose of our Letter is to reveal a deep misconception that has prevented the proper application of semiclassical dynamics in solid state physics. Our phase space is not abstract, because the position and momentum are well defined physical quantities. We started our discussion with the naïve definition of phase space volume element, because that is what people naturally think of as the volume element. Our style of direct confrontation should be more effective in clearing out the misconception than by applying an abstract mathematical theorem.

Why is there such a misconception? Although the original Liouville theorem was restricted to phase space of canonical variables, our past experience often finds that it also applies for the physical position-momentum variables which are gauge invariant but noncanonical. For example [4], in the presence of a magnetic field, the physical momentum $\boldsymbol{k}$ for an electron is related to the canonical momentum $\boldsymbol{q}$ by $\boldsymbol{k} = \boldsymbol{q} + e\boldsymbol{A}(\boldsymbol{r})$, where we have taken $\hbar = 1$ and electron charge as $-e$. For a Bloch electron, $\boldsymbol{q}$ is the wave vector and also called crystal momentum. However, the Liouville theorem applies to the phase space of either set of variables. This can be explained by the fact that the Jacobian for volume transformation between the variables of $(\boldsymbol{r}, \boldsymbol{q})$ and $(\boldsymbol{r}, \boldsymbol{k})$ is unity. However, this is true only when the Berry curvature is zero. For another example, in the absence of a magnetic field, the physical position $\boldsymbol{r}$ can be expressed in the form $\boldsymbol{r} = \boldsymbol{R} + \boldsymbol{A}_n(\boldsymbol{k})$, where $\boldsymbol{A}_n(\boldsymbol{k})$ is the Berry connection related to the Berry curvature by $\boldsymbol{\Omega}_n(\boldsymbol{k}) = \boldsymbol{\nabla}_{\boldsymbol{k}} \times \boldsymbol{A}_n(\boldsymbol{k})$. In this case, $\boldsymbol{R}$ and $\boldsymbol{k}$ form a canonical set of variables, but the Liouville theorem applies whether one uses $\boldsymbol{r}$ or $\boldsymbol{R}$.

In summary, the Comment addresses an important, but not the main, result of our Letter, it does not contradict our results in substance, and the only objection is really on the style of approach. The concrete and rich physical results [Eqs. (3)–(14)] revealed in our Letter in fact conform with the general theory of symplectic dynamics, which is usually discussed in an abstract setting. It is good to make connection with the abstract theory, and we have done some, if not thoroughly, towards the end of our Letter.

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[1] C. Duval *et al.*, preceding Comment, Phys. Rev. Lett. **96**, 099701 (2006). The authors made a mistake in citing our result by including the distribution f into our density of states D.
[2] D. Xiao, J. Shi, and Q. Niu, Phys. Rev. Lett. **95**, 137204 (2005).
[3] See, for example, P. J. Morrison, Rev. Mod. Phys. **70**, 467 (1998).
[4] N. W. Ashcroft and N. D. Mermin, *Solid State Physics* (Saunders, Philadelphia, 1976), Appendix H.

