The authors have satisfactorily addressed all of my comments except one, which concerns the potential application to quantum metrology. I acknowledge that some of my previous remarks were not well-founded, which may have obscured the main point. Below, I clarify the aspects on which I agree with the authors and those on which I do not, and I attempt to illustrate the core of the issue using a simpler example.

> We emphasize that, for chaotic dynamics, the QFI at times beyond the Ehrenfest time closely approaches its long-time average value, as shown in Fig. 6(a-c).

I fully agree with this statement.

> In this sense, a single evolved chaotic state suffices for phase estimation with high precision in any spin direction, yielding $\Delta\theta \sim \sqrt{3}/N$ corresponding to $F_Q = N^2/3$.

This statement is technically correct, but it should be emphasized that achieving this level of precision in phase estimation requires **precise knowledge of which specific state (among all possible ones) is being measured**.

> This directional flexibility not only enhances robustness but also eliminates the need for precise time control or fine-tuned Hamiltonians-one only needs to ensure that the system evolves into a chaotic state.

I disagree. Merely knowing that the system evolves into a chaotic state is not sufficient to construct a proper estimator that achieves the stated precision; see an example below.

> However, the key advantage is that no specific measurement direction is required, which significantly simplifies experimental implementation.

Even if the same measurement is optimal for all states, precise knowledge of the actual state is still required to construct a valid estimator.

I would also like to emphasize that, leaving aside the ongoing discussion (in which I clearly disagree with the authors on certain points), the article itself does not contain any explicit errors related to this topic. In particular, the question of whether achieving the claimed precision requires precise knowledge of the prepared state (in this case, precise control over timing) is not addressed in the manuscript.

I strongly encourage the authors to consider the argument presented below and, if they find it convincing, to clarify the applicability of their results in the context of quantum metrology.

Nevertheless, since the article also includes results that extend beyond the metrological setting, I am inclined to recommend the manuscript for publication in its current form.

To clarify my concerns, let me introduce a simplified, caricatured example that captures the essence of the problem. Consider a machine that randomly produces states:

$$|+\rangle = \frac{1}{2}(|0\rangle + |1\rangle), \quad |-\rangle = \frac{1}{2}(|0\rangle - |1\rangle), \tag{1}$$

with equal probabilities $p(+/-) = \frac{1}{2}$. **Are these states useful for measuring rotation around the z axis, generated by $\exp(i\theta\sigma_z)$?**

The answer to this question depends on whether, in each subsequent iteration, we know which state was drawn.

For each of these states individually, the QFI equals $F_Q[|+/-\rangle, \sigma_z] = 4$.

The rotated states are respectively

$$|\theta,+\rangle = \frac{1}{2}(e^{i\theta}|0\rangle + e^{-i\theta}|1\rangle), \quad |\theta,-\rangle = \frac{1}{2}(e^{i\theta}|0\rangle - e^{-i\theta}|1\rangle). \tag{2}$$

Moreover, in both cases, the same measurement (in basis $\{|+\rangle, |-\rangle\}$) is optimal for estimating θ, leading to the following probabilities:

$$\begin{aligned} p(+|\theta,+) &= \cos^2\theta, \quad p(-|\theta,+) = \sin^2\theta \\ p(+|\theta,-) &= \sin^2\theta, \quad p(-|\theta,-) = \cos^2\theta, \end{aligned} \tag{3}$$

where by $p(i|\theta,j)$ I denote the conditional probability of obtaining result i, if the state has been drawn as j, and rotated by angle θ.

However, in order to construct a valid estimator from the measurement outcomes, we need to know which of the states was drawn at the beginning. Indeed, without this knowledge (i.e., after averaging over $j=+,-$), all information about θ disappears:

$$\sum_{j=\{+,-\}} \frac{1}{2} p(+|\theta,j) = \frac{1}{2}, \qquad \sum_{j=\{+,-\}} \frac{1}{2} p(-|\theta,j) = \frac{1}{2}. \tag{4}$$

That is the crucial distinction between the two cases.

In the first scenario, the state is drawn from an ensemble with given probabilities, and we have access to the information about which specific state has been selected. In this case, the metrological potential is quantified by **the average of the QFI calculated for each individual state**:

$$\sum_{j\in\{+,-\}} p(j) F_Q[|j\rangle, \sigma_z], \tag{5}$$

which takes the value 4 for this example.

Alternatively, if we only know that the state has been drawn from an ensemble but lack knowledge of which specific state was selected, the metrological potential is quantified by **the QFI calculated for the averaged (i.e., mixed) state**:

$$F_Q\left[\sum_{j\in\{+,-\}} p(j) |j\rangle\langle j|, \sigma_z\right], \tag{6}$$

where the formula for QFI of the mixed state needs to be used. Indeed, for the above example, the average state is simply $\sum_{j\in\{+,-\}} p(j)|j\rangle\langle j| = \frac{1}{2}\mathbb{1}$ and it carries no information about θ, and QFI is equal 0.

I hope the above example helps clarify the misunderstanding that arose in previous correspondence. In the main text, the authors use the average of the QFI calculated for each state, which corresponds to a scenario where the exact state — and thus, in practice, the precise timing of the evolution — is known. This assumption significantly limits the practical applicability of their results in a metrological context. I suggest that this limitation be acknowledged in the conclusions.
