I recommend a major revision of the paper before it can be considered for publication. That is so because it contains only a (rather elementary) mathematical analysis of the works by Lorenz and Lilly, without consideration of the underlying physical processes, and also because it is rather poorly written.

The paper is meant to 'revisit' the works by Lorenz and Lilly, which have led to the well-known conclusion that the range of deterministic weather forecasts has an ultimate limit of something around two or three weeks. That conclusion has been amply confirmed by numerical experiments performed with models of increasing spatial resolution and physical realism (see, *e.g.*, Zhang *et al.*, 2019).

The authors' main point is that the works by Lorenz and Lilly, although they lead to a similar conclusion, are actually very different from their very starting point, and that further study is necessary as to the predictability of the atmospheric flow.

As just said, the paper is poorly written, with lengthy developments of secondary interest, useless repetitions and inclusion of elementary mathematical material that should not be necessary. This confuses the reader and I had actually some difficulty in even following the logical thread of the paper.

From what I understand, the significant part of the paper begins with the introduction of the eddy turnover time $\tau(k)$ (Eq. 5). That quantity is introduced with a reference to Vallis (2006), without appropriate explanation as to its physical significance nor on how it has been determined. The only indication in the paper is that $\tau(k)$ is *the time for a parcel with velocity* v_k *to move a distance of 1/k, with* v_k *being the velocity associated with wavenumber k*. (ll. 298-299). More information would be necessary, be that only to refresh the reader's memory. I simply note that, since v_k is defined as the velocity associated with wavenumber k, the variations of v_k with k contain the same basic information as the spectrum of kinetic energy, which is considered later in the paper. That should be mentioned explicitly.

The authors then proceed to estimate predictability times by integrating the turnover time over two different grids in spectral space. They find (Eqs 9a-b) that the integral on the exponential grid (Eq. 1a) is finite while the integral on the linear grid (Eq. 1b) is infinite. The former being sparser for large values of the wavenumber k, it is obvious that the corresponding integral will be smaller. From a physical point of view, what should be considered there is how fast an uncertainty at wavenumber k propagates to larger scales, and how the propagation relates to the turnover time. That should determine on which kind of discretized grid an integral of the turnover time can be physically significant. Although I presume that has been done by other authors, that basic question is not even mentioned, nor is any reference given about it. The authors totally miss here a critical point.

Another point (subsection 3.4.2) is relative to the coefficient $2^{-2/3}$, which is present in both Lorenz's and Lilly's approaches. The authors show (Table 1 and subsection 3.4.2 together with the associated Figure 4) that this coefficient is not defined by Lorenz with any real accuracy. But they do not really mention how it comes into Lilly's approach through Eq. (5) and the hypothesis of a -5/3 power law for the KE spectrum. Again, additional explanations may, be necessary there.

Actually, the point I have found of most interest in the paper is the fact that Lorenz, although he used in Lorenz (1969d) a linear nonturbulent model, found a predictability time of about the same magnitude as Lilly, who used a nonlinear turbulent model. That fact, which is certainly of great interest, is not further discussed in the paper, but I accept it could be considered as going beyond its scope.

On a different aspect, the paper is full of elementary developments in basic calculus, with which most readers can be expected to be already fully familiar (for instance the development from Eq. (18) to Eq. (21b)). And there is certainly no need to *review the concept of the Jacobian* (l. 393) (the Jacobian turns out to be no more in the present case than a scalar derivative, and not a full determinant as is usually meant by the word).

By eliminating those useless developments as well as many equally useless repetitions, the length of the paper as it stands could be substantially reduced.

I make a final remark. There is some truth in the existence of at least a practical limit to deterministic weather forecasts. Is the fact that Lorenz and Lilly have reached the same conclusion, with the same approximate value for the limiting value, purely accidental, or is there a common basic truth in the two approaches ? That question should, if not discussed, at be least explicitly mentioned.

I suggest a major revision of the paper, with inclusion of a physical discussion in the approaches of both Lorenz and Lilly, particularly on the concept of eddy turnover time, and elimination of lengthy and useless developments and repetitions.

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