JOURNAL OF GUIDANCE, CONTROL, AND DYNAMICS
Vol. 30, No. 4, July–August 2007

Technical Comments

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Reply by the Author to T. H. Kerr

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DOI: 10.2514/1.30404

Kerr [1] makes reference to a method of determining the linear realization of a system of states whose covariance is known that differs from the method used in the Appendix of [2]. Although this is an important addition, the author too harshly criticizes the application of the somewhat simpler approach used in [2], and his final conclusion that the results of the analysis in [2] would, therefore, be of limited benefit is essentially unsupported.

Attempting to find fault with the method derived in the Appendix of [2], the author first claims that Eq. (A4) of [2] equates a function of one variable on the left-hand side to a function of two variables on the right-hand side. But this is not true. Equation (A4) is indeed an equation in one variable t, on both sides, since t_0 is clearly a fixed point in time. The remaining derivation is solid if certain conditions are fulfilled, one being that the covariance function (as a function of t relative to t_0) is differentiable at t_0 and the other that the covariance matrix of the states at t_0 is nonsingular. Stationarity is not a condition required at this stage, but the derivation would not change under this more restrictive and usually assumed condition. And, although the author refers to the method of [2] as "new," its application actually has precedence in the cited literature.

Kerr [1] offers two cases (one pathological) where the method breaks down. The first involves a covariance function that is not differentiable at t_0 (or, at the origin if we shift the coordinate origin to t_0 using the transformation provided by the lag, $\Delta t = t - t_0$). Because the covariance in this example is a function of the absolute value of Δt, its derivative with respect to t is undefined at t_0. Such covariance functions, while legitimate in some applications, were specifically not used in [2] because they would model the gravitational field with a singularity in free space, whereas, in fact, the field is analytic in free space (all derivatives exist); in fact, the covariance functions used in [2] are analytic. Therefore, while showing that this method to determine a linear state realization for the gravitational gradients may not be appropriate in other applications, the example in [1] does not show that the method is unusable in the problem at hand.

The second example considers a covariance function that is equally unrealistic for the gravitational field (because it implies covariances equal to the variance at nonzero lags), but it is actually more relevant than the first one by illustrating that the derived dynamics matrix of the state realization may result in insufficient or unrealistic dynamics. In fact, this was the situation encountered in the modeled state realization of the gravitational gradients, although it was not the dynamics matrix (as in the author's example), but the process noise covariance matrix that was nulled. The ad hoc solution of adding some appropriate "process noise" to the state dynamics matrix resulted in adequate modeling of the gradients as shown in the consequent covariance functions. One should also note that an error analysis of the kind conducted in [2], based on covariance propagation, is less sensitive to the strict shape of the covariance functions than to the scale, that is, the variances. As great care was taken to ensure proper scaling of the covariances, any possible mismodeling resulting from the use of an approximate state realization would be manifested mostly in the integral of the state (the gravitation states). However, even here, it was also shown to be entirely adequate for the time intervals of interest (time interval between gradient updates).

Kerr [1] advocates the use of other, perhaps more accurate methods to realize the gravitational gradients as linear state variables, and there is no argument against considering those (although any such method would still be approximate because we also know that the gravitational field is not linear); but his claim that the method used in [2] leads to weak or useless results in the analysis of gradient aiding of inertial navigation systems is unfounded. To make such a claim legitimate, one would need to demonstrate that the analysis is fundamentally sensitive to the modeling in question. The author did not, and, it is argued here that he could not, do this.

References

[1] Kerr, T. H., Comment on "Precision Free-Inertial Navigation with Gravity Compensation by an Onboard Gradiometer," *Journal of Guidance, Control, and Dynamics*, Vol. 30, No. 4, July–August 2007, pp. 1214–1215.

[2] Jekeli, C., "Precision Free-Inertial Navigation with Gravity Compensation by an Onboard Gradiometer," *Journal of Guidance, Control, and Dynamics*, Vol. 29, No. 3, May–June 2006, pp. 704–713.

Received 12 February 2007; revision received 12 February 2007; accepted for publication 16 February 2007.

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