Review of: "Optimal Latency Compensator for Improved Performance of Teleoperated UGVs on Soft Terrains"

Wei Zhan[1]

1 Texas A&M University - College Station

Potential competing interests: No potential competing interests to declare.

The research topic of this paper is interesting.

The sentence in the abstract, "Our study revealed a latency threshold of 0.72 seconds is critical for maintaining a stable UGV operation," is misleading. After reading the paper, I think the conclusion only applies to the specific UGV system studied in this paper. The conclusion was made based on the simulation results of a specific system. If the system structure or even the parameter values are different from the ones studied in this paper, then the analysis needs to be repeated, and the critical value may be different from 0.72 seconds.

GA was first used in the title of Figure 1 without a definition.

There is a math error in equation (3). The second row of the 2x2 matrix should be [1/b 1/b]. If this equation was the one used in the simulation, then all conclusions must be re-evaluated.

The cost function defined in Equation (12) is problematic in the sense that the numerator and denominator can both be 0, in which case the function is not well-defined.

We all know that a system with delay may become unstable if the delay is too large. The analysis in this paper basically verifies that with a specific system using simulation. I don't see anything new in that.

In the case of varying delays, the authors assumed that the distribution is uniform. I think it is more reasonable to use a normal distribution. The delay is usually around some average value, so the probability of a delay far away from the average value should be very low. I am curious if the conclusion will be different if a normal distribution is used instead of the uniform distribution.

The last sentence in Section III is confusing. I don't think you can claim that variations in delay have a detrimental impact on performance and stability. Instead, a large constant delay or delays with significant variations have a detrimental impact.

The authors considered the completion time and the estimated mission success rate. It would be an interesting optimization problem to maximize an objective function such as W1* estimated mission success rate + W2* completion time, where W1 and W2 are weights.

I question the advantage of a remote operator's perceived slippage compared with the slippage estimation calculated locally by the controller. The latter can completely avoid the communication delay.

What happens if the lower left block (optimal backward predictor) of Figure 1 is implemented in the slave robot and the u)m(t) is transmitted to the master robot? Will the result be similar to or different from the result in this paper?

The manuscript is acceptable for publication if the concerns in the review are addressed.