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Discussion on: "Adaptive Extremum Seeking Control of Fed-Batch Bioreactors"

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1. Comments on the Main Result

The paper by Titica, Dochain, and Guay presents a nice alternative to the non-model based extremum seeking design in [1]. The idea of the paper is to simultaneously perform nonlinear model-based set point regulation and model-based set point adaptation. This idea sounds elegant, however, in its execution one has to face considerable complexity and cannot avoid having to introduce an exogenous probing signal to generate persistent excitation.

The paper is quite hard to read because all of the ingredients of the design are presented simultaneously–set point regulation, state estimation, parameter estimation, and probing. I think I would have had an easier time following the development if the solution to the set point regulation problem for a known set point was presented first. This may be a standard result contained somewhere in the literature but I am not familiar with it. I am aware of a related design in [2], Section 3.4.2, where the substrate feed rate S_0, rather than the dillution rate u, is used as a control input. With S_0 as the control (and u constant), the system is in the "pure-feedback form" and the design follows the standard backstepping idea where S is viewed as the "virtual control" at the first step of backstepping. In the present paper, the system (1), (2) has the control input u appearing in both equations of the nonlinear model, so the idea of the non-adaptive design is not completely obvious. It would be helpful if the authors clarify it.

Just as S_0 may replace u as the regulating control input, it may also replace it as an extremum seeking input. This choice was not pursued in [1] and it may be obvious to a chemical engineer why. It is not obvious to me and it would be useful if the authors comment on this, as well as on the possibility to adapt their entire design to using S_0 for control.

It is not completely clear why the authors use a state estmator $\hat{y}$ for y, which is measured. Is it to avoid some algebraic loop that would otherwise arise in the problem? What are the consequences of having to choose the observer gain high, as indicated in (37)?

The Lyapunov function in (17) contains an expression that does not necessarily look positive definite. Is $1+\theta_s S+\theta_i S^2$ always positive for physical values of S and the parameters?

In (29) the control contains division by $S_0 - S$. Is this quantity likely to always remain away from zero during the transients?

The authors are being very honest in discussing the difficulty in guaranteeing persistency of excitation a priori. However, in implementation, it is to be expected that similar rules as in linear adaptive control apply. I am a bit surprised that the authors take five spectral components in the probing signal in (63), whereas I would expect two sinusoids to be sufficient for three parameters. Besides, what is the benefit of choosing the A_i's as random numbers?

2. Simulation Comparison with the Non-Model based Extremum Seeking Design of Wang, Krstic, and Bastin [1]

There is enough merit in the idea of the approach by the authors that it could stand on its own, without having to "outperform" the previous approach

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(presumably for the purpose of the review process). Especially, when the authors themselves state that the performance of the scheme in [1] "could be improved by further adjustments of the design parameters". I have yet to see an application of extremum seeking where a good choice of design parameters won't yield convergence in just a couple of periods of the probing signal.

I am however perplexed by other issues arising in the simulation comparison. First, in the text below (64) the authors make it sound like [1] requires the measurement of the inaccessible quantity μ. This is not the case, in [1] the biomass outflow rate X_u is used for extremum seeking feedback. I do understand what the authors *mean* to say – that, since they optimize μ in their paper (without having to measure it), they employ the same variable for driving the extremum seeking scheme of [1] – however, this is not how the text sounds.

Second, to my surprise, the authors do not actually use the version of the algorithm proposed in [1] for the Haldane model, but the version proposed for the simpler, Monod model. It was shown in [1] that extremum seeking alone is not sufficient for the Haldane kinetics and that the system gets attracted to the undesirable equilibrium $X = S = 0$. It was also proposed there to use locally stabilizing full state feedback, filtered through a washout filter to eliminate equilibrium bias, to make the desired equilibrium attractive and achievable by extremum seeking. In light of this, it is not surprising that the authors get the poor result shown in Fig. 6. What is surprising is that the authors used the algorithm for the Monod model on the Haldane model.

3. Conclusion

In summary, the authors are to be commended for their effort in this and their other recent papers in expanding the set of applications of extremum seeking beyond those in [3] and for putting a major effort into quantifying the conditions for parameter convergence.

References

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