Authors’ response to the referees’ reports

Manuscript title: Data-driven simulation and control
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We thank the referees for their relevant and useful comments.

In this document, we quote in **bold face** statements from the reports. Our replies follow in ordinary print.

Answer to referee #1

Main comments:

The simulation and control algorithms only consider the deterministic data. No measurement noise is allowed, which restricts the application of the algorithms only to systems with negligible measurement errors.

We indeed consider the exact deterministic case, i.e., the data is assumed to satisfy an LTI system of a bounded order. This choice is clearly explained (even emphasised) in the abstract and the introduction of the manuscript. Admittedly the assumption that the data is exact makes the problem not practical for “real life” applications where the data is coming from a nonlinear system and is possibly noisy. However, this is the simplest case which solution is a basis for more realistic approximate and stochastic data-driven control problems. Note that, for example, in subspace identification first are solved deterministic identification problems, which correspond to the data-driven control problem of the manuscript. Therefore, we believe that this feature of the manuscript is a logical first step in solving data-driven control problems.

A restriction of the output matching control is that the D matrix must have full rowrank.

The D matrix being full row-rank is a fundamental limitation of output matching control, i.e., if D is not full row-rank, then it can be shown that there exists a reference signal y_{r}, such that the output matching control problem has no solution. This restriction is a feature of the problem per se and do not result of a relaxation used in solving that problem.

The proposed algorithms are not compared with the existing data-driven LQR solutions, e.g. (Favoreel 1999).

We fail to understand in what sense should a comparison be made. In the absence of noise *any* data-driven LQ algorithm must theoretically give the same answer (because the solution of the LQ control problem is unique). We show this for the algorithms presented in the paper. The same holds for any other data-driven LQ algorithm (in particular the one of (Favoreel 1999)). Our aim is not to single out the “best” (in whatever sense) *computational* method but to show a *conceptually* new approach to data-driven control. For this purpose it is enough to state that the algorithms are different but solve equivalent problems.

Since Sec. 4 is a review of (Markovsky et al. 2005b), it can be shortened to provide only the necessary technical background.

(Markovsky et al. 2005b) is a conference paper, which contains only part of the results presented in Section 4. In particular,

- Computation of zero initial conditions response (Section 4.4 of the manuscript) is not present in the conference paper,
- Computation of the impulse response (Section 4.5) is not present in the conference paper, and
- The general simulation problem (Sections 4.1 and 4.2) is presented in the more general setting of nonzero initial conditions (which is crucial for the data-driven control problem), while in our conference paper it is presented for zero initial conditions.

We believe that the above list of differences is sufficient to justify the content of Section 4. In addition, *all* algorithms presented in Section 4 are used in the rest of the paper, so that shortening Section 4 will unavoidably make the presentation incomplete. If the referee believes that a particular part of Section 4 is unnecessary for the presentation, then we would be glad to known which one and why.

Detailed comments

... only the comparison between the second and the third tracking approach are given.

Section 6.4 presents two simulation examples. In the first one (LQ regulation) we do compare the three methods presented in the manuscript.

"σ denotes the backwards ...". "Forward" should be the right word here.

The referee is mistaken by the "+1" in the definition of σ: $\sigma(w)(t) = w(t+1)$, which suggest "forward shift". Actually the "+1" produces a shift of one step *backwards* in time, as the referee can convince himself from the following table

t	$\cdots$	-2	-1	0	1	2	$\cdots$
w	$\cdots$	$w(-2)$	$w(-1)$	$w(0)$	$w(1)$	$w(2)$	$\cdots$
$\sigma(w)$	$\cdots$	$w(-1)$	$w(0)$	$w(1)$	$w(2)$	$w(3)$	$\cdots$

Fer detailed explanation of basic signal operations, see [OW96, Section 1, Subsection "Transformation of the independent variable"].

In algorithm 4, $\operatorname{col}(I_m, 0_{m(T_r-1)\times p})$ **on the RHS of Eq (15) are not compatible in the columns, if** $m \neq p$**.**

Thank you for pointing this out to us. The column dimension of the matrix in the RHS is `m` instead of `p`. We have corrected three typos.

In Proposition 15, the condition $\mathscr{B}_{0,1} = R^m$ can be directly inferred from Proposition 10. Besides, this condition is not intuitive. The condition that D has full row rank is better in this sense, as derived in the proof.

In an earlier version of the paper we had the condition of Proposition 15 given in terms of D. A referee, however, objected that a condition in terms of a state space representation of the system is not consistent with the data-driven spirit of the paper. (Of course, D is a parameter of the system that is not known.) We agreed with the comment of that referee and gave the condition in terms of the behaviour $\mathscr{B}_{0,1}$, therefore making the condition easier to deduce from the data (e.g., via Proposition 10). We believe that we are right in adopting that formulation, since indeed it is more consistent with the data-driven approach.

In the statement of Algorithm 5, since zero initial conditions are also assumed, why not replace u_{ini} and y_{ini} with zeros of appropriate dimensions as expressed in Algorithm 3?

It is not true that in Algorithm 5 zero initial conditions are assumption. An initial trajectory w_{ini} is given as an input to Algorithm 5 and specifies (in general) non-zero initial conditions. See also the data-driven output matching problem formulation (more specifically, the third bullet of Problem 13).

In the definition of $\tilde{\mathscr{O}}$ in Sec 6.2, the zero matrices $0_{p\times n}$ should be $0_{m\times n}$.

Thank you for pointing this out to us. We have corrected this typos.

In the simulation example 2, it is stated that $T_{\text{ini}} = 2$. But $w_{\text{ini}} = (1,1)$, containing only one sample.

Yes, we did not specify correctly w_{ini}. In the simulations we used the initial trajectory $w_{\text{ini}} = \left(\left[\begin{smallmatrix}1\\1\end{smallmatrix}\right], \left[\begin{smallmatrix}1\\1\end{smallmatrix}\right]\right)$. Thank you for the remark.

In the simulation results, it is argued that "the compared methods compute the same optimal trajectory". Does this mean during the first 30 samples?

In Experiment 1, the optimal trajectory w^* is defined over the time interval $[1,2,\ldots,30]$. The three methods compute the same optimal trajectories, say w_1^*, w_2^*, w_3^*, respectively, which means that

$$w_1^*(t) = w_2^*(t) = w_3^*(t), \qquad \text{for } t = 1,\ldots,30.$$

In Experiment 2, the optimal trajectory w^* is defined over the time interval $[1,2,\ldots,60]$. The second and the third methods compute the same optimal trajectories, say w_2^* and w_3^*, respectively, which means that

$$w_2^*(t) = w_3^*(t), \qquad \text{for } t = 1,\ldots,60.$$

We believe the above explanation is unnecessary as the support of w^* is clearly stated in the paper and, in addition, it is obvious from the plots in Figures 3 and 4.

Step 1 of Algorithms 1 to 5 is generally a rank-deficient least square problem, because the matrix $\begin{bmatrix} U_p^T & Y_p^T & U_f^T \end{bmatrix}^T$ usually has much more columns than rows. In what sense should they be solved? With minimum 2-norms?

Step 1 of Algorithms 1 to 5 is actually a rank-deficient *least norm* problem. This means that if there is an *exact* solution then there are infinitely many solutions. The corresponding propositions state under what conditions there is a solution. Part of the statement of the proposition is that under the given assumptions *any* solution (e.g., the minimum 2-norm solution) can be used for deriving the simulated trajectory of the system. In the algorithms we leave the choice of the particular solution up to the implementation. In our software implementation, we use the minimum 2-norm solution but this need not be the case.

Answer to referee #2

The authors may want to point out that one important difference, between the approach presented in this paper and the IFT and VRFT methods cited in the Introduction, rests in the role of the initial conditions.

Done as follows (see the last paragraph of Section 1):

> The control criterion, considered in Section 6 of the paper, is over a *finite horizon*, so that the initial conditions play an important role. In iterative feedback tuning (Hjalmarsson *et al.* 1998, Hildebrand *et al.* 2004) and virtual reference feedback tuning (Campi *et al.* 2002) approaches, instead, the objective is to achieve a desired operating regime of the plant. In this case an infinite horizon cost is used, and therefore the initial conditions are not relevant. This difference is also reflected in the fact that in the approach of this paper one designs a control input while in iterative feedback tuning and virtual reference feedback tuning the aim is to tune the parameters of a feedback controller.

We thank the referee for pointing this comparison out to us.

Answer to referee #3

1. Traditional model-based approaches of simulation and control are well known, and it is not so necessary to compare data-driven approaches with model-based approaches.

We believe that the comparison is beneficial for readers (supposedly most of the IJC readers) who are familiar with model-based approaches but not with data-driven approaches. The parallel between the familiar notions of model and initial conditions and the less familiar data-driven equivalents of data trajectory w_{d} and initial trajectory w_{ini} helps to better understand the meaning of the data-driven simulation and control problems. Drawing parallels with familiar concepts is our style of presenting new material and we do find it effective.

> “Does teaching consist in putting questions?” Indeed, the secret of your system has just now dawned upon me. I seem to see the principle by which you put your questions. You lead me through the field of my own knowledge, and then by pointing out analogies to what I know, persuade me that I really know some things which hitherto, as I believed, I had no knowledge of.
>
> (Socrates as quoted by Xenophon in “The Economist”, Chapter XIX)

By omitting the detailed explanations of existing results, the originality of this paper becomes clear.

The statement of existing results is reduced to the necessary minimum for giving the background information and for making the link with the existing literature. The originality of the paper is discussed in the introduction and the contributions of our work relative to the traditional approaches and alternative data-driven approaches is clearly stated.

2. The following paper is a recent one in the area of data-driven approaches. It should be included in References. Its treatment of data is similar to that of the paper under review.

We have added the paper of Park and Ikeda in the reference list.

3. The idea of the data-driven output matching algorithm in 5.3 looks similar to that of Ikeda et al. 2001. What is different should be discussed.

This comment is closely related to comment 5. Please refer to our answer to comment 5 for more detailed explanation.

1. In (Ikeda et al. 2001) there are no conditions under which the data-driven output matching problem is solvable. We provide such conditions in Proposition 15.
2. In (Ikeda et al. 2001) there are no explicit algorithms for solving the data-driven output matching problem. We provide such an algorithm (see Algorithm 5).
3. In (Ikeda et al. 2001) only the SISO case is considered. Our treatment is for MIMO systems.
4. We view the data-driven output matching problem as an "inverse data-driven simulation problem" therefore bringing a new insight in the problem.

4. At the bottom of page 14, it is said that "... that avoids the instability problem, pointed out in Example 16." However, the linear quadratic tracking problem treated in Section 6 is a finite-time problem, although stability is an infinite-time problem. To resolve the instability problem, the authors need to consider an infinite-time quadratic tracking problem.

We agree: the notion of stability is well defined only in infinite horizon problems. We rephrased the sentence as follows:

> In the next section, we consider a more general tracking problem—follow a reference trajectory w_r by trading-off errors in both the input and the output. In the infinite horizon case, the latter problem avoids the instability issue, pointed out in Example 16.

5. The data-driven linear quadratic tracking problem considered in 6.3 looks similar to that of Fujisaki et al. 2004. What is different should be discussed.

The contributions of our work relative to the previous work in the field are clearly summarised in subsection "Overview of the literature, outline of the paper, and summary of contributions" of the introduction. We find this summary sufficiently clear, but answering to the question of the referee we will give more explanations here:

1. Theoretically we derive *sufficient conditions for existence of solution of the data-driven LQ optimal control problem.* Such conditions are *not* given previously in the literature. In particular, the approach of Fujisaki et al. is not rigorous as the authors are not specific when the problem is actually solvable. (Admittedly, existence of solution is the first basic theoretical question in approaching a mathematical problem.) The sufficient conditions given in our manuscript come from (Markovsky *et al.* 2005a, Willems *et al.* 2005) and we do give credit to this work.
2. We propose *different algorithms to the ones Fujisaki et al. may have suggested in their work.* Note that contrary to the related work of (Favoreel 1999), in (Fujisaki et al. 2004) (as well as in the follow up work of Fujisaki) there are *no* explicit algorithms. Deriving algorithms for a theoretical piece of work may not be a trivial matter. We consider our algorithmic approach as a significant step forward compared to the work of Fujisaki et al.
3. Finally, we *relate data-driven control to the data-driven simulation problem.* This aspect is not present in the literature and is a unique feature of our manuscript.

References

[OW96] A. Oppenheim and A. Willsky. *Signals and Systems*. Prentice Hall, 1996.