Authors' response to the referees' comments

Manuscript title: Data-driven simulation and control
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We thank the referees and the associate editor 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 the comments of the associate editor

(a) The proposed methods can be used only in the noise-free case, which is the weakest point of the paper.

We consider the exact deterministic case, i.e., the data is assumed to satisfy an LTI system of a bounded order. This is clearly explained (even emphasized) 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, this feature of the manuscript is not a weakness—it is the logical first step in solving data-driven control problems.

Since the data-based simulation and control in the noise-free case have already been explored extensively in several works in References, the contribution of this paper relative to the works seems obscure.

The contributions of the manuscript, relative to the previous work in the field are clearly summarized in the enumerated list in subsection "Overview of the literature, outline of the paper, and summary of contributions" of the introduction. The above claim of the associate editor should be considered vague, and in our opinion it should be justified with specific comments showing

Which of our contributions are not original and where in the literature they are first made?

The associate editor and the referees point a number of references, however, the only specific comment about one of our contributions not being original is that Section 4 contains results from [8]. Our answer to this is: 1) we extend the result of [8] from specific responses (impulse response and free response) to *any* response of the system (data-driven simulation) and 2) we embed data-driven simulation in the new context of data-driven control. In our view these two facts justify Section 4 as having sufficient novelty, compared to the results of [8]. Therefore, we again do not agree with the comment of the associate editor. Unless otherwise proven by *specific comments* that are justified, the contributions of the manuscript are original and, in our opinion, the associate editor's claim has no foundation.

Note also that there exist several other "data-based" methods taking noise into account, such as adaptive control, iterative learning control, and so on.

We do not see the point of this comment. Adaptive control and iterative learning control are *model based* design methods, so they are only remotely related to the topic of the manuscript. The existence of advanced model based

control techniques that treat nonlinear stochastic problems does not disqualify work on exact noise free data-driven control.

(b) Notice that several important works closely related to this paper are missing in References. The following gives a collection of the related papers: . . .

The notion of "important" scientific work is of course subjective. However, some general considerations can be made. Three out of the five cited references are proceedings papers, which by itself should not duly emphasize importance. A commonly accepted indicator of "importance" is the number of citations. According to the *Web of Science* website, [A] is cited 5 and [B] is cited 0 times. For papers published 7–9 years ago, these figures do not suggest high impact.

In addition to the question about their "importance", references [A]–[C] are model based and therefore not immediately related to the manuscript. References [D], [E] are extensions of reference [3], cited in the manuscript. In our manuscript, we did not aim to be exhaustive and to cite all publications on data-driven control. We aimed instead at giving a comprehensive overview of the literature by sampling what we believe are the most important references of the major approaches.

In fact, [A] and [B] seem to be closely related to Section 6, . . .

The problems addressed in [A] and [B] are derivation of LQ(G) control from an impulse response representation of the plant. The contribution of Section 6 is an algorithm for LQ tracking that is *not* using *any* representation of the plant. Our problem, approach, and solution are different than the ones in [A] and [B]. This is a fact that for unfathomable reasons the AE refuses to acknowledge.

. . . [C] to Section 4, . . .

We thank the associate editor for pointing this reference to us. We were not aware of it and it is relevant for our research. However, we do not agree with the associate editor that the results presented by Li and Skelton coincide with ours in Section 4. Quoting from the introduction of [C]:

> Furthermore, when a real plant is assumed to be a linear system, the input/output crosscorrelation data coincides with the Markov parameters. In this paper, the input/output crosscorrelation data is employed in deducing the LQG controller.

According to our terminology, the problem of Li and Skelton can not be properly called "data-driven" because they use an impulse response representation of the system.

. . . and [D] and [E] to Sections 5 and 6.

References [D], [E] are extensions of reference [3] and we explain in the introduction our contributions relative to [3].

(c) The advantages of the behavioral formalism for the date-based simulation and control problems should be clarified.

We are not sure to what the AE exactly refers to with the expression "behavioral formalism". We use the "behavioral language", which is a way of saying facts about dynamical systems. In our view, of primary importance is *what* we have to say and of secondary importance is *how* we say it. We believe that in the manuscript we "tell" something interesting and original and that the "behavioral language" provides a convenient way of conveying the message.

This point is very important since the same problems have already been considered in the existing works without taking the behavioral approach and the behavioral formalism may lead to a possible contribution of the paper.

The implication of the above comment is that the manuscript lacks novelty. We disagree with this. The contributions of the manuscript are clearly stated in the introduction and if the associate editor wants to dispute them, he should be specific about which of them are not new. See our reply to (a) above.

(d) The paper seems to be too long and to be unnecessarily complicated, which should be resolved.

See our answers to the related comments of the referees.

The authors should concentrate the paper on their own contribution. For example, a part of Section 4 seems to overlap with [8].

Section 4, extends a specific result of [8] (construction of impulse response and free responses directly from data) to the new concept of data-driven simulation (construction of an arbitrary response of the system directly from data). In addition we make a new link with data-driven control. The relation of Section 4 to [8] and [9] is clearly stated in the manuscript. Finally, the comment of the associate editor seems a bit strange in view of the fact that [8] and [9] are coauthored by us, so in the submitted manuscript we generalize "our own" work.

Since Section 5 is a counterpart of Section 4, it can be simplified.

We do not understand this comment. How can Section 4 be simplified?

Section 6 seems to contain unnecessary items and/or details, which can be removed.

See our answers to the related comments of the referees.

Answer to Referee 22057

Firstly, it seems that the only difference between what the authors call "classical approaches" to the simulation and output matching problems, and the proposed behavioral approach to these problems is a "classical" realization of the Hankel matrix (3) associated with a given data sequence.

We are not sure what the Referee means by "classical realization of a Hankel matrix". Realization normally refers to finding parameters of state space representation from the impulse response or the transfer function representation. Neither we have an impulse response or the transfer function representation of the system nor we compute state space representation parameters, so we do not see a link between realization and data-driven simulation or control. We are dealing with *general* data, so the relevance of this comment is obscure to us.

From an algorithmic point of view the proposed data-driven approach that amounts to determining g or G in (11) or (15) hardly differs from a realization approach that first finds an i/s/o realization (A,B,C,D) of the system (section 4) or of its inverse (section 5).

We disagree: the classical and data-driven approaches lead to conceptually and algorithmically different solutions. This is the main point of the whole research area called "data-driven control", so the comment of the Referee is of major importance for the whole area and not specifically for our manuscript. If correct, this comment will invalidate the reason for the existence of data-driven control as such. That would be fine by us, however, then the claim requires a precise statement (What do you meant by "from an algorithmic point of view two methods differ"?) and a proof of such statement.

What is more, Proposition 15 even explicitly mentions invertibility of the direct feedthrough matrix D of an (any) i/s/o realization among its conditions . . .

This is a comment that find us in complete agreement with the Referee. Indeed, it would be more consistent with the data-driven approach to provide a condition in terms of the data that avoids reference to the D matrix of an i/s/o representation. This has been done in the revised version.

. . . and therefore requires the construction of such a realization prior to applying Algorithm 5.

One can apply the algorithm without verifying the assumptions, so there is no need to compute D. Moreover, even if the condition is checked, one needed not compute a realization because only the matrix D is needed and it can be

computed directly from data; for example, by Algorithm 4.

One is therefore led to wonder what the data-driven merit of the paper is when i/s/o realizations need to be computed to verify conditions.

This comment is *unfair and misleading* because of the following reasons.

- Only one of our *six* data-driven algorithms is using in one of its conditions the parameter D of an i/s/o representation. Therefore, we believe that the Referee has no basis for generalizing such a statement to all six data-driven algorithms of the manuscript.
- Knowledge of the required parameter D does not necessitate the computation of a whole i/s/o representation. In fact, the algorithm can be applied without checking this condition.
- The condition based on D can be replaced by an equivalent condition in terms of the data.

By this comment we are led to wonder what the purpose of the Referee is: to give an objective evaluation of the manuscript, or to search for a reason on the basis of which the manuscript could then be rejected.

The paper seems to contribute little more than rephrasing the classical solution in a behavioral context, and it is for this reason that the merit of the proposed approach is not sufficiently clear.

We disagree. As explained in our answers above the proposed solution is not "rephrasing the classical solution". There are fundamental conceptual differences, which in our opinion have been sufficiently illustrated in the manuscript, and we regret that the Referee would not consider them adequate.

Secondly, the paper addresses the problem of input-matching (simulation) in section 4 and output matching in section 5 without emphasizing the symmetry between these two problems.

The first paragraph of Section 5 reads:

> Simulation can be viewed as "input matching": determine the output component of a system trajectory, corresponding to a given input component and initial condition. By reversing the roles of the input and the output, we have an "output matching" problem: determine the input component of a system trajectory, given an output component and initial condition. This is a control problem, which we call "output matching control".

The paragraph quoted above clearly states the existence of a symmetry between the simulation and output-matching problems. We believe that observing this symmetry is insightful and original in our manuscript. If the Referee has in mind any other "symmetry" we would be glad to know what he/she exactly refers to.

In addition, the two problems assume an input-output decomposition . . .

Yes, a global assumption in the manuscript is that an input/output partition of the variables is given. The same assumption is essentially used in our previous work [8], [9], [13], not to mention the rest of the data-driven control literature.

. . . that is an unusual starting point in the behavioral setting . . .

We agree that it would be nicer to relax this assumption, however, we do not know how to do this. (If we could avoid the assumption, then we would have done it in our previous work.)

. . . which moreover does not seem necessary

The behavioral setting provides a convenient language also when the variables have an a priori given input/output partitioning.

As an alternative a more symmetric problem formulation could be the following: ... This formulation captures both the simulation and the output-matching problem as a special case and does not require an input-output decomposition of w to start with.

Indeed, we solve two special cases of the proposed more general formulation. The special cases are useful (correspond to simulation and output matching control) and in the present manuscript we do not need an extra generality. In addition, and more importantly we do not know how to solve the proposed problem in its full generality. However, we thank the Referee for his/her perspicuous comment, which provides food for thought.

The solution approach would again involve (11) and uniqueness of g from w_0 and w'.

This is incorrect. The solution approach, based on (11), does assume that $w = \mathrm{col}(w', w'')$ is an input/output partitioning.

Thirdly, the simulation problem is certainly also if interest to autonomous systems and is not addressed at all.

The Referee is in our opinion, once again wrong. Computing the response of an autonomous system is equivalent to computing a free response of an open system, so Algorithm 1 addresses this issue. Note that the response y_{f} in Section 6 is a response of an autonomous system and is computed from data via Algorithm 1.

A major technical issue is the lack of definition of a causal behavior as is used in Section 5. Please define and discuss why this is a relevant property for the output-matching problem.

Causality is a well defined notion in the behavioral setting, see [Wil89, 3.1.5]. Since in out manuscript, we assume a given input/output partitioning of the variables, there is no issue about the use of causality in Section 5.

Please indicate the relation between $\mathbf{l}(\mathscr{B})$ and $\mathbf{n}(\mathscr{B})$ (if any).

The link between the order and the observability index is well known (generically $\mathbf{n}(\mathscr{B}) = \mathbf{l}(\mathscr{B})/\mathsf{p}$, however, in the general case the relation is more complicated). We do not see how the link between $\mathbf{n}(\mathscr{B})$ and $\mathbf{l}(\mathscr{B})$ would contribute to improving the clarity of the manuscript; in our opinion it is not essential for the presentation.

So, t-samples-long trajectories belong to $\mathscr{B}_t$, not to $\mathscr{B}$. (cf. pp. 5,6).

This is a typo (missing subscript t). It is corrected. Our thanks to the Referee for pointing this out.

Is [6] an extension of the present paper to cover approximate solutions?

No, [6] is about identification, not data-driven control.

A "rank-revealing factorization" has not been defined. As a consequence I have not been able to understand section 4.3 and Algorithms 2 and 3.

The definition is added. Again many thanks to the Referee for the helpful comment.

In any case, Algorithms 2,3,4 seem rather trivial extensions of Algorithm 1 and Proposition 6 and do not need as much space as presently.

We agree that once the general concept of data-driven simulation has been understood, Algorithms 2, 3, and 4 are relatively straightforward extensions of Algorithm 1. This, however, does not mean that Algorithms 2, 3, and 4 are not important. In addition, they are used in the rest of the manuscript so we believe that their explicit statement is needed.

If the Referee agrees that Algorithms 2, 3, and 4 are important, then the statement that they follow “trivially” from our general concept of data driven simulation (Algorithm 1) just proves how powerful the general concept is. Also it proves that the manuscript is clearly written making the derivation look “trivial”. We are pleased to hear that all referees find “trivial” a manuscript solving a nontrivial problem. To our ears, this statement sounds like supporting the insightful and simple approach we are pursuing.

I dislike section 4.5 as signals are defined as mappings $u : \mathbb{Z} \mapsto \mathbb{R}^{\mathsf{m}}$. In section 4.5 $u(t)$ is a matrix not a vector.

We agree. This issue has been corrected in the revised version of the manuscript.

As already mentioned, the assumption on D in Proposition 15 disqualifies the main motivation of the paper.

In the revised version of the manuscript, the assumption on D in Proposition 15 is expressed in terms of the data. The "main motivation of the paper" (there is more than one, in our opinion) is now standing on firmer ground.

Section 6: By taking Φ positive semi-definite, the results of section 4 and 5 (with 0 error) can be recovered as a special case of Problem 17.

Yes, we made this observation in the second paragraph of Section 5:

> The output matching problem is a special (singular) case of the output tracking problem, when the input does not enter into the cost function and the desired output can be achieved exactly.

Our solution of the LQ tracking problem, however, depends on the assumption $\Phi > 0$, so that the singular cases are not covered.

The displayed equation in Algorithm 2 seems incorrect.

We believe that it is correct. (Please be specific what is wrong.)

The minor comments are taken into account.

Answer to Referee 22058

the current manuscript is not constructed in such a way that its original contributions are visible in view of the works in the literature

The contributions of the manuscript, relative to the previous work in the field, are clearly summarized in the enumerated list in subsection "Overview of the literature, outline of the paper, and summary of contributions" of the introduction.

1. The contributions of Section 4 should be clearly compared with the previous works by the authors. The basic idea to solve the equation of type (11) seems common with [8]. The problem of finding the impulse response from the data has been solved in [8, Theorem 4].

In the introduction, we state that the results of Section 4 are an extension of the results of [8] and [9]:

> Data-driven simulation of free response and impulse response was used in [8] for deriving balanced input/state/output representations from data. In its full generality the data-driven simulation problem was first formulated and solved in [9]. In Section 4, we review the results of [9], because they are fundamental for the solution of the data-driven control problems, considered in Sections 5 and 6.

Note that [9] which overlaps with the content of Section 4 is an IFAC conference paper of ours. IFAC encourages authors to publish extended version of their conference papers in IFAC journals. The relation of [9] to the submitted manuscript was indicated on the time of submitting the manuscript. In addition, Section 4 is a revised and extended version of [9], it is embedded in a more general setting, and it is a prerequisite for solving the data-driven control problems. The manuscript is related to our previous work [8], [9], [13]; however, it applies our main result, called the "fundamental lemma" to data-driven control.

2. The significance of Section 5 is questionable, and this part can be significantly reduced. Though the construction of the paper is such that the simulation and control problems are juxtaposed, the section seems too long for its contents. It would be possible to move Algorithm 5 in Section 4 because it is based on the same principle as Algorithm 1 although the equations (11) and (17) are seemingly different.

We agree that the two algorithms are close. In fact, once one has understood the main idea of constructing responses from data all the rest follows naturally from it and may look trivial.

The statements of Problem 12 and Subsection 5.2 can be omitted.

Problem 12 makes Section 5 parallel to Section 4. In addition, although we call it "classical", we have never seen Problem 12 explicitly stated in the literature from our point of view — symmetric to the simulation problem. Therefore, we consider this feature of the manuscript to be original and insightful.

Example 16 is not informative because it would be a common knowledge that a nonminimum phase plant may exhibit unbounded control signals for the output matching problem.

We understand the referee's comment, and the reason behind it; however, let us point out that we prefer to include this example anyway, because it shows clearly that the data-driven solution leads to the same solution obtained by the classical method.

3. The presentation of Section 6 should be significantly changed. First of all, the paper should be more articulate on the contributions of previous works, especially [3-5].

We did this in the introduction, where we stated the contributions of the manuscript relative to the previous work in the literature. We thought and we still think that it would be prolix to repeat the same considerations elsewhere in the manuscript.

The reference [4] also uses the projection to compute the optimal control, but the current manuscript does not well summarize the work in the introduction.

Reference [4] is based on a given impulse response representation of the system. According to our definition this is not solving a general data-driven control problem.

the references [3] and [5] also computed a basis of the behavior. The last sentence in Section 1 is not clear on this.

As stated in the introduction we propose different algorithms compared to the ones of [3] and [5]. In addition, we relate the problem of deriving such algorithms to the data-driven simulation problem. This aspect is not present in [3] and [5] and is a unique feature of our work.

Second, although the section includes the three approaches for solving the linear quadratic tracking problem, the first two approaches apparently contradict the motivation of Section 4, i.e., to find a solution without deriving an explicit representation of the behavior. The first two approaches should be omitted or stated quite briefly.

The first two approaches are presented in order to make contact with the "classical" methods. We believe that the manuscript will be less clear without then. However, we see the point of of the referee's comment.

4. There are a couple of phrases or words which are not precisely defined. Though one can guess the meaning, it would be desirable to state the definitions. They are; observability matrix of $\mathscr{B}$ with T_r block rows on page 9, rank revealing factorization on page 10, and augmented observability matrix of $\mathscr{B}$ with T_ini and T_r block rows on page 17.

We wholeheartedly agree with the Referee and we thank him/her for pointing this out. We made the terminology more consistent in the revised version of the manuscript.

5. In Subsection 6.2, A and C are not defined.

A note that they are parameters of a state representation of the system is added.

6. I do not understand what it means by "algorithm is exact," which appears in the introduction.

We mean that the algorithm solves the exact problem. The statement is clarified.

Does it mean data is exact or model is exact? In abstract, the authors use "exact data."

The data is exact for certain LTI model of a bounded order (i.e., a bound on the order is given). A clarification is added. Our thanks to the referee for pointing out this unclear statement.

7. On page 3, the definition of σ appears two paragraph earlier than needed.

We do not see what is the problem with this. The definition of σ is given in what to us appears to be its logical place.

8. On page 9, line 8, Lemmas 1 should be Lemma 1.

Corrected.

Answer to Referee 22059

The paper does not proceed beyond redefinition of well known concepts of system identification and control.

The contributions of the manuscript, relative to the previous work in the field, are clearly summarized in the enumerated list in subsection “Overview of the literature, outline of the paper, and summary of contributions” of the introduction. In order to support his claim, the Referee should argue against these contributions. We regret that this serious critique is made in such an offhand way, and that it is not supported by factual and detailed reasons.

There is no elaboration of algorithms and error analysis is absent.

We are not sure what “elaboration of algorithms” means. The algorithms are not studied from a numerical linear algebra point of view in this manuscript. This may be done in future work. We also do not understand how criticizing the manuscript for failing to do something it never was intended to, can impinge on the validity of its contribution.

Compare your results, if any, to the vast body of literature available under the keywords adaptive control, iterative learning control, iterative feedback tuning, system identification, and identification for control. Some basic references are given below.

Adaptive control and iterative learning control are *model based* paradigms, so they are not directly related to the topic of the manuscript. Iterative feedback tuning is a data-driven control method; however, it is optimization based while we consider subspace type algorithms. This is an essential difference. System identification, in particular identification for control, are vast areas. The suggestion of the Referee to compare our results with those in system identification is not specific enough to be considered seriously. None of the listed references is on the topic of data-driven control. Consequently, we do not understand the nature of this remark and we would appreciate hearing in what sense the suggested comparison should be done.

Answer to Referee 22060

The authors should have shown the superiority of the proposed approaches over the pre-existingones in the references [3].

Superiority in what sense? For our purpose—namely to show a *conceptually* new approach to data-driven control—it is enough to state that the algorithms are different but solve equivalent problems. Our aim is not to single out the "best" (in whatever sense) *computational* method. Comparison of the methods from a numerical efficiency or statistical accuracy (in the noisy case) point of view may be useful and can be done in another place.

Also, in numerical study the performance of their approaches should be compared to the pre-existing ones.

We regret to say that we do not understand just what kind of "performance" has the Referee in mind: numerical efficiency; numerical robustness; accuracy in the presence of noise; or something else? All these comparisons can be done and they may be useful to suggest which algorithm is "better". However, we fail to see how such comparison would improve our manuscript. In addition, the algorithm of [3] is presented abstractly. Extra work is needed in order to be translated to standard numerical linear algebra operations and this work may lead to different implementations. Finally, any comparison based on simulation is by itself questionable because it may only suggest some evidence, but it does not and cannot prove anything.

3. Is the paper clearly written and well organized? The Referee can't find why it is important to use the behavior language in the paper. The authors distinguished between input and output signals.

The behavioral setting provides a convenient language even when the variables have an a priori given input/output partitioning. We also want to point out that the main issue is *what* is presented, not *how* is presented.

Throughout the paper the state-space representation (A,B,C,D) is used. It seems just confusing.

The data-driven solutions do not make reference to state-space representations, which are used only in some proofs (not "throughout") or in order to make link with known concepts and methods.

4. What is the contribution of the paper? It is obscure.

The contributions of the manuscript, relative to the previous work in the field, are clearly summarized in the enumerated list in subsection "Overview of the literature, outline of the paper, and summary of contributions" of the introduction. We regret to say that in the absence of concrete remarks, based on the above mentioned list, we fail to see the validity of this statement of the Referee.

The discussion around Algorithm 1 seems the key point of the paper. However, without noise, it seems obvious from the fundamental lemma of authors' previous work.

Yes it is. Does this make the solution of the data-driven control problems less valuable?

Algorithm 1 simply means the redundant linear equations which has an exact solution because of the no-noise case.

We do not understand this statement.

No brand-new theoretical issue can be seen there.

If "here" refers to Algorithm 1, we agree. The conceptual contribution of the manuscript is in applying Algorithm 1 for data-driven control. We state this clearly in the introduction of the manuscript.

As told in item 2 above, critical is a lack of attempt to relate the proposed approaches to existing approaches.

As answered before, the contributions of the manuscript are clearly summarized in the enumerated list in subsection "Overview of the literature, outline of the paper, and summary of contributions" of the introduction. The items of

this list compare the results of the manuscript, among others, to [3]. We do not understand how more effectively we could have related our approach to the existing ones, and we would appreciate to hear this Referee's suggestions on this issue.

Eq. (9) is wrong.

Corrected. We thank the Referee for pointing this mistake.

References

[Wil89] J. C. Willems. Models for dynamics. *Dynamics reported*, 2:171–269, 1989.