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Review of “Robustness of the Data-Driven Identification algorithm with incomplete input data”

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Review of version 1

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Reviewer 1 (Anonymous)

Reviewer

This article addresses some practical difficulties when applying the Data-Driven Identification (DDI) methods introduced by Leygue et. al., [18] and T. Kirchdoerfer et. al., [9]. The article is very well written and has a structure that is easy to follow and facilitates the understanding of its contribution. The topic is interesting and timely, and addresses important practical problems which are often omitted in publications. Hence, in my opinion, the present paper should be considered for publication, with at most a minor revision.

The article contribution is a practical guide for the implementation of DDI algorithms. It is centered on presenting some problems that the reader might encounter when using DDI techniques and some solutions/workarounds/preprocessing choices that result in robust results over several comparisons.

The DDI approach is a constitutive-law-free technique that assigns a mechanical state (stress and strain pair) and a material state (stress and strain pair) to each structural element. The mechanical state satisfies the equilibrium and compatibility conditions, whereas the material state for the element is the stress strain pair from the material states dataset which minimizes its distance to the mechanical state, this distance is defined in terms of an energy-like term and a complementary energy-like term. The inputs of the model are nodal displacements and nodal forces obtained from a non-homogeneous test and Digital Image Correlation (DIC). The outputs are the states of each element of the model and a material states dataset. The DDI model uses two parameters, one identifies the size of the material states dataset and the other the fourth order tensor used to compute the energy-like and complementary energy-like terms used in the error norm.

The authors discuss the some difficulties in DDI that might appear because of the following:

1. Net forces are measured instead of nodal forces.
2. There are some areas in which displacements are not known or noisy those zones are called clusters.
3. Displacement data are not available close to the edges.
4. The real contours are not properly captured in the edges and in holes.

The authors propose clear solutions and workarounds; for example

- Zero net force conditions in those boundaries where data is missing instead of naively ignoring equilibrium conditions on those nodes.

- The same net force condition is considered in the boundaries of holes instead of free edge conditions in those cases in which the real boundary of the hole is not close to the theoretical boundary.
- The grip boundaries displacements cannot be determined, those boundaries are replaced by boundaries away from the edges, in those boundaries it is assumed that the net force also corresponds to the cell force.

The authors present results in how their data handling and workflow leads to a more robust DDI (parameters and results).

The study is conducted using totally perfect synthetic data obtained assuming hyperelastic (Ogden model) behavior which is altered based on experimental observations to investigate robustness of the DDI.

I have some questions regarding some of the article content:

In page 11, in the paragraph which contains line 230: “In addition, C significantly contributes to the convergence of the method: the higher it is (to a certain extent), the lower the error is. Indeed, the distance defined by the norm 230 influences the mapping between material and mechanical states. By choosing a large value of C , the mapping based on strain values is favored, which is relevant since they are measured (and so reliable) unlike stresses which evolve during the convergence of the algorithm. Finally, N_* is more influential than C : without missing data, a bad choice of N_* will never be compensated by a good choice of C .”

As I understand it, the error function measures two types of errors, the error in terms of the energy-like term and the error in terms of the complementary energy-like term. I don’t see clearly what you mean by “to a certain extent” but if C is increased, the error from the complementary energy-like term is less relevant than the error in the energy-like term. Ignoring the complementary energy-like part is saying that the pairs mechanical state and material state will probably have the same strains but not the same stresses (we are giving preference to compatibility over equilibrium in our element state). If I am right, I would appreciate a more detailed explanation. If I am not right, I would appreciate clarification.

Authors The term “energetic norm” is not be the best choice in this context. The sole purpose of the norm defined in Eq. (3) is to measure distances between material and mechanical states within the DDCM/DDI framework. This norm has the form and the units of an energy density through the parameter C ([Pa]) nevertheless it is not related to any actual energy in the system. Through the amplitude of C , it is possible to weight this distance between the strain and the stress part. In the manuscript we have replaced “energetic norm” with “norm” where needed and we have added a sentence to clarify this issue after Eq. (3).

Reviewer In page 2, in the paragraph which contains line 25: “Concerning material characterization, non-parametric approaches are proposed by [14] and [15] in which the strain energy function of a hyperelastic material is not presupposed but computed with splines, thanks to experimental simple tests. In [16], a decomposition of the strain fields obtained with Digital Image Correlation is made in order to compute the stress field without constitutive equation. In [17], experimental 30 dynamic measurements are used in the balance equations so that stress fields can be directly computed.”

The authors reference [14,15] in which the strain energy hyperelastic function is not presupposed but computed and [16,17,18] in which there is no constitutive equation at all. This is an interesting debate which is connected with “physics-based” data-driven learning. Somehow, in my opinion, we should give some physical background to the problem because otherwise the solutions are infinite, and probably of not much value. Just think of viscoelastoplastic materials. Can we know just from a tensile test and DIC the general behavior of a material? Or the minimum needed information?

The authors could add further discussion on that topic also cementing other possibilities left behind, e.g., using Data-Driven techniques to improve our knowledge or justify assumptions about the material behavior (it might be the case that we are just interested in understanding the relevant variables of our problem or the implications of doing different hypotheses, see for example [I], [II], [III] and [IV]).

In summary, after a minor revision, I think that the paper may be published.

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Authors The paragraph at the center of this comment has been rewritten to clarify the fundamental differences between [14,15] on the one side and [16,17] on the other. (See also comments by Reviewer #2).

Reviewer 2 (Anonymous)

Reviewer Data-driven approaches to computational mechanics are emerging methods that directly use experimental data, rather resort to empirical modeling of constitutive equations, to simulate equilibrium states of solid structures. An approach studied in this paper is one of such methods. The authors numerically investigate influence of choice of parameter values on output results. Also, incompleteness of data around boundaries of a structure is dealt with. In the concluding section the authors write some recommendations of these settings, which are drawn from only one problem instance. As a major concern, I am skeptical about the authors’ assertion that, with only one problem instance (which seems to be a linear problem), a specific setting of the method is universally recommendable.

Authors The recommendations made in the concluding section are indeed illustrated from a single problem instance (non-linear and finite strain) but are drawn from an extensive experience in using the DDI method with both synthetic and experimental data. Concerning the recommendations on the treatment of boundary equilibrium we believe that they are very general and can actually extend to other identification methods. They express the idea that one should not introduce equilibrium constraints that are not exactly verified in the experimental data (e.g. in sections 4.2.2 and 4.3.3). Concerning the recommendations on the DDI parameter values, experience tells us that they are valid at least for elastic behaviors. They may not apply exactly for non-elastic behaviors. The introduction and conclusion of the manuscript have been modified to reflect this.

Reviewer Also, the paper includes some inadequate expressions, as well as insufficient literature survey. Therefore, major revision is necessary before publication. The followings are more specific comments.

page 1, title of the paper: The word “altered” seems to be confusional. What the paper investigates

is dependency of the method on parameter values (not data) and treatment of incomplete data. Therefore, the title should be reconsidered.

Authors We agree with the reviewer the word “altered” might be confusional. We have therefore replaced it with “incomplete”, as suggested.

Reviewer page 2, 2nd paragraph: As data-driven computing in elasticity, approaches different from the one studied in this paper have been proposed. Those approaches should also been surveyed. Examples of such literature are the following:

- R. Ibañez, E. Abisset-Chavanne, J.V. Aguado, D. Gonzalez, E. Cueto, F. Chinesta, A manifold learning approach to data-driven computational elasticity and inelasticity, *Archives of Computational Methods in Engineering*, 25, 47–57 (2018). [hal-03653378](#).
- R. Ibañez, D. Borzacchiello, J. V. Aguado, E. Abisset-Chavanne, E. Cueto, P. Ladevèze, F. Chinesta, Data-driven non-linear elasticity: constitutive manifold construction and problem discretization, *Computational Mechanics*, 60, 813–826 (2017). [hal-01647641](#).
- P. Ladevèze, D. Néron, P.-W. Gerbaud, Data-driven computation for history-dependent materials, *Comptes Rendus Mécanique*, 347, 831–844 (2019). [hal-02417567](#).
- Y. Kanno, A kernel method for learning constitutive relation in data-driven computational elasticity, *Japan Journal of Industrial and Applied Mathematics*, 38, 39–77 (2021). [10.1007/s13160-020-00423-1](#).

Authors We have added the second and the fourth reference suggested by the reviewer to the discussion of the second paragraph in page 2 as these two contributions indeed propose alternative approaches to material behavior identification from experimental data.

Reviewer Unlike the method studied in this paper, some of them involve no parameter or have established methods (e.g., the cross validation) to determine parameter values.

page 4, eq. (5): It is strange that on the right-hand side summation is taken with respect to e and X , while the term on the left-hand side has a specific indices e and X .

Authors This equation simply gives the explicit form of the function being minimized. For the sake of rigor and to avoid any accidental confusions, we have modified the names of the summation indices in this expression.

Reviewer page 5, line 11; page 6, line 18: The authors write “reliability” of the data-driven method. I do not understand how the reliability of a data-driven method can be defined (in my view, it is impossible). On the other hand, in line 3 on page 8 the authors write “robustness”. These two words are used in different meanings in engineering.

Authors Agreed, reliability has been replaced with robustness where needed.

Reviewer section 4: It should be made clear how the data set was generated. It seems that linear isotropic elasticity was assumed and finite element analysis was conducted to generate data. Also, it should be stated clearly if (artificial) noise is added to the generated data (as in practical problems there always exists noise).

Authors The non-linear, finite strain, hyper elastic model used to generate the data is detailed in section 3. It is also specified in this section that “The discussion of the effect of noisy displacement values is beyond the scope of this paper and has already been partially addressed in the original DDI publication [18]”.

Reviewer page 11, line 2: “from Co” may be “from 10-6Co.”

Authors This has been corrected.

Reviewer page 11, line 7: The authors write that the reason why large r_* yields large error is similar to over-fitting. I doubt this. The method in this paper is clearly a local optimization method. My guess is that, with large r_* , the method converges to a poor local optimal solution, compared with the case $*$.

Related to the previous issue, dependence of the output results on choice of the initial point

should be investigated. Recommendations written in the concluding section may differ according to this choice.

Authors We actually wrote the opposite: “Conversely, a too small ratio (too many material states) produces an effect comparable to overfitting in regression: the behavior is no longer averaged sufficiently, which also leads to a significant error.” Let us however discuss this point: DDI regularizes the identification of the stress field through the material states which provide a coarse sampling of the material mechanical response. The intensity of the regularization is inverse to the number of material states. If one were to consider any balanced stress field to initialize the method with one material state per mechanical state, DDI would output the same field, independently of its relevance. It is only through the clustering of several mechanical states around one particular material state that the method promotes the fact that similar strains should yields similar stress. This clustering/regularization effect decreases as r_* decreases since more and more clusters are considered. We agree that the word “over-fitting” might be misleading and the observed effect is more related to an over-parametrization of the regularization provided by DDI which decreases its effect. We have therefore modified the corresponding paragraph to discuss more accurately the effect of the decrease of r_* .

Reviewer section 5: It should be reconsidered that the conclusions and recommendations drawn here are universally verified from the numerical results of only one problem instance.

Authors See discussion related to the first comment.

Review of version 3

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Reviewer 1 (Anonymous)

The paper is a very good work on data-driven hyperelasticity in which the finite element computations are included. In particular they adress in the paper the important issue of robustnes when some data is missing. The authors have addressed the comments of this reviewer and I think that the manuscript can be accepted as is.

I also want to apologize for the time it took me for this second review (which was quite straightforward). For some reason, it slipped through the cracks.

Reviewer 2 (Anonymous)

The authors have revised the manuscript according to the reviewers’ comments, and hence I think that the present manuscript can be published.

Editor’s assessment (Anna Pandolfi)

The revision of this paper has been very smooth. The observations of the two reviewers were significant and authors accepted them carefully, implementing some comment and clarification in the revised version. As often happens in many today reviews, reviewers suggested comparative literature to the authors, which often is not really necessary for the exposition of the research and for the understanding of the novelties of the paper. In this case, I have appreciated the objective analysis of the suggested references by the authors, that added to the revised version only the relevant literature.

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