E. Spyrakos-Papastavridis, J. S. Dai, P. R. N. Childs, "Confutation of the "Counterexample to Passivity Preservation for Variable Impedance Control of Compliant Robots"", *IEEE/ASME Transactions on Mechatronics, 2024.* Accepted for publication on 18 March 2024.

Confutation of the "Counterexample to Passivity Preservation for Variable Impedance Control of Compliant Robots"

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Abstract—**This paper reveals the incorrect argumentation, erroneous mathematical calculations, and misleading observations reported in the article titled "counterexample to Passivity Preservation for Variable Impedance Control of Compliant Robots" published in volume 28, issue 1 of this journal, in February 2023. All the calculations in the said article are either incorrect or redundant, and this paper apodictically reveals that the peculiar simulation results therein contained were produced from a failure to follow the implementation guidelines delineated in the original publication. This paper further generates a set of examples to confute the so-called 'counterexamples', thereby proving the erroneousness of the article's argumentation. Furthermore, numerous of the incorrect claims contained in the said article stand in stark opposition to results and theories reported in the robotics literature over the last three decades.**

THIS paper provides point-to-point refutations of the erroneous comments and calculations contained in all five pages of the article titled "counterexample to Passivity Preservation for Variable Impedance Control of Compliant Robots" [1], thereby invalidating all its arguments and exposing its want of substantiality to the reader.

I. Trivial Errors based on Incorrect Observations and Misconstrual of Information Described in the Existing Literature

A. Redundant Matrix, Vector, and Function Definitions

Article [1] commences with the following subjective – and totally redundant – paragraph:

"After equations (1:1) and (1:2) a summary of the model properties should be included, mentioning matrices that are diagonal, positive definite, etc. These properties are used throughout the paper and this is the logical place to provide this information: $\mathbf{M}(\boldsymbol{q})$ is a symmetric positive definite matrix, $\mathbf{C}(\boldsymbol{q}, \dot{\boldsymbol{q}})$ is skew symmetric, whereas $\mathbf{J}$, $\mathbf{N}$, $\mathbf{K}$, $\mathbf{D}$ are diagonal positive definite matrices."

It is unclear why it would have been *necessary* to repeat these matrix property definitions in our previous paper [2], when paper [3] – which provides precise definitions of these matrix properties – is already cited immediately before the introduction of equations (2:1) and (2:2). Hence, this comment is of no technical value whatsoever. Another preposterous sentence then follows:

"It should be explicitly stated in the problem formulation after equation (1:4) that $\boldsymbol{q}_d$ and $\boldsymbol{\theta}_d$ are constant reference signals. Equation (1:39) should also be placed in the problem formulation section in this paper rather than in the experimental results section. The paper is badly organized."

Since it has been clarified, immediately before the introduction of equation (2:4), that the controller in question – which merely serves as an example to highlight the original contributions in our previous paper [2] – is introduced and analysed in Tomei's paper [4], which is cited in [2], then there is no reason to repeat all these details.

Article [1] also makes another request for redundant information, as per the following comment:

"After equation (1:9) an expression for the storage function Q is missing. Also there is no discussion on how a storage function is chosen. The paper leaves details unspecified and this causes uncertainties for the readers. The paper should provide complete details rather than only partial information in this respect."

Since the storage function is already unambiguously defined through equation (2:7), which is only a few lines above equation (2:9), then there is no need for unnecessarily repeating the definition of the storage/energy function. Finally, the said article [1] points out that:

"In equation (1:12) there is no mention about properties of the matrices $\mathbf{K}_{\mathrm{P_{Mt}}}$, $\mathbf{K}_{\mathrm{D_{Mt}}}$, $\mathbf{K}_{\mathrm{P_{Jt}}}$, and $\mathbf{K}_{\mathrm{D_{Jt}}}$. Similar comments apply to equations (1:22) and (1:27). Once more the paper lacks details. The paper should mention that these matrices are diagonal with positive entries."

However, this is again a void argument, and presumably a result of the author jumping to conclusions prior to examining the whole paper: section IV. C in [2] commences by clarifying that

"To utilize *diagonal gain matrices* conforming to the stability conditions, the original controller design has been decentralized [39]".

It is worth noting that the above comment about positive entries is, in fact, incorrect, since the stability criteria can be satisfied even when some of the entries are negative – this is why section IV. C in [2] elucidates that the gains are tuned

"such that $\mathbf{K}_{\mathrm{P_{Mt}}}$ becomes the dominant term in $\mathbf{T}_{\mathrm{K}}$, as $\mathbf{K}_{\mathrm{D_{Mt}}}$ should also be the dominant term in $\boldsymbol{\Psi}_{\mathrm{K}}$."

B. Invalid Commentary Relating to the $U_g(\boldsymbol{q})$ Term

Subsequently, article [1] makes a starkly incorrect observation by stating that:

"After equation (1:3), $\boldsymbol{\tau}_g(\boldsymbol{q}) = -\left(\partial U_g(\boldsymbol{q})/\partial \boldsymbol{q}\right)^T$ is incorrect this should be $\boldsymbol{\tau}_g(\boldsymbol{q}) = \left(\partial U_g(\boldsymbol{q})/\partial \boldsymbol{q}\right)^T$."

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This constitutes a nonsensical comment, given that the definition provided after equation (2:3) is in fact $\boldsymbol{\tau}_g(\boldsymbol{q}) = \left(\partial U_g(\boldsymbol{q})/\partial \boldsymbol{q}\right)^T$.
One then encounters the misleading and nebulous statement:

"Several expressions have been copied from previous publications where slightly different models were used but this is not acknowledged."

Without the provision of additional information, it is almost impossible to decipher the above sentence – perhaps the author is referring to the papers that they co-authored with our group [5][6]? If this is the case, then it invalidates the comment made in article [1] about providing a "clear" definition of $U_g(\boldsymbol{q})$ – this is shown below:

"The paper also assumes that the gradient of $U_g(\boldsymbol{q})$ is a row vector. This convention is not unique (see Tomei's paper reference [1:32]) and therefore to avoid confusion the chosen convention should be explicitly stated in the paper."

Papers [5][6], which were also co-authored by the author of [1], do not explicitly clarify that "the gradient of $U_g(\boldsymbol{q})$ is a row vector" – it is unclear why the author failed to express any objections during the time that these papers were under preparation. Even more interestingly, perhaps, the article ([4]) adduced in [1] as a reference for a correct definition of $U_g(\boldsymbol{q})$'s gradient, does not explicitly state that $U_g(\boldsymbol{q})$'s gradient must be a row or column vector, nor does it mention the uniqueness of the convention; the reader is referred to equation (8) in [4].

C. The Invalid Assumption that Computational Power Standards have remained Immutable for Three Decades

[1] expresses concerns with regard to computational problems that may be encountered when using the approach proposed in [2], as revealed via the following anachronistic sentence:

"The definition of the induced matrix norm is not practical in computations and a useful expression in terms of singular values or eigenvalues should be given as given in Tomei's paper [1:32]."

This might have been a valid concern at the time when [4] ([1:32]) was published, namely 1991, although it is illogical to discuss issues of computational practicality/complexity when contrasting articles published approximately three decades apart from each other. In the year 2020 [2], it was perfectly practical to carry out matrix norm computations in real-time, even for multi-degree-of-freedom systems.

D. The Manifold, Self-Negating Observations

Amongst the numerous illogical sentences contained in [1], the following oxymoronic one is particularly conspicuous:

"In equation (1:6) and in the subsequent paragraph $\lambda_m(\mathbf{T_I}) \geq \alpha$ is wrong it should be $\lambda_m(\mathbf{T_I}) \geq \alpha$."

Furthermore, the sentence shown below:

"In equation (1:7) the terms involving $\mathbf{T_K}$ are incorrect, it should be $\mathbf{T_I}$",

is equally misleading, since there are no $\mathbf{T_K}$ terms in equation (2:7). In a subsequent section of [1], it is pointed out that

"Equation (1:46) is incorrect, **N** is missing, and the symbol $\mathbf{K_T}$ is not defined."

The above comment is incongruous for two reasons: firstly, simple inspection of equation (2:46) reveals the existence of **N** in the upper portion of the expression; secondly, $\mathbf{K_T}$ is defined in the sentence appearing before equation (2:46).

II. The Fallacy Surrounding the N Matrix

The erroneous critique in article [1] pertaining to the **N** matrix is not only revealing of the author's superficial understanding of nonlinear control theory, but it effectively opposes well-established results and theories that have been known to the robotics community for decades [7]-[11]. This is synoptically expressed in the following sentence excerpted from the said article [1]:

"After equation (1:8) the paper claims global asymptotic stability can be established using LaSalle's invariance principle when **N** is neglected. This is incorrect and indicates a lack understanding regarding Lyapunov theory. This also contradicts the results in Tomei's paper (reference [1:32]). The matrix **N** cannot be neglected when invoking LaSalle's invariance principle since $\boldsymbol{\Psi}$ should be positive definite. The matrix $\boldsymbol{\Psi}$ is not positive definite if **N** is neglected."

A. Attempts to Refute Results Reported in Existing Literature

Our previous paper [2] states, after equation (8), that global asymptotical stability can be proven when neglecting the **N** matrix, which clearly relates to equations (2:4) to (2:8) [4] that only consider motor-side feedback. Neglecting the **N** matrix is common practice in prominent literature, including Khatib and Siciliano's *Springer Handbook of Robotics* [11]; however, in Section III of [2], the **N** matrix is included for the purpose of conducting a stability analysis based on the proposed, passivity-preservation controller [2]. Therefore, [1]'s claim that **N** is necessary for a proof of global asymptotical stability is only criticising equation (2:8)—a result prevalent in most prominent robotics literature. As such, the claim of article [1]'s author is tantamount to refuting the majority of published research on this topic – this includes publications that have been cited thousands of times, in addition to the contents of pages 254 and 255 of the *Springer Handbook of Robotics* [11].
In a core part of the original publication [2], namely Section III, which introduced passivity-preservation control (or power-shaping signal control), the **N** matrix is included in equations (2:18) and (2:19); this renders all the argumentation in [1] completely irrelevant. However, [1]'s author attempts to refute this *rudimentary* point, by claiming that excluding the **N** matrix does not allow one to prove closed-loop stability; this claim is incorrect as demonstrated in some seminal publications [7]-[11], and in one of the most famous robotics textbooks [11], which have been collectively cited thousands of times. Textbook [11] is widely considered one of the most reliable sources of robotics knowledge and presents this specific result – the one that [1]'s author attempts to discredit – on pages 254 and 255. In fact, this comment is so absurd that if it were assumed to be valid, it would actually

invalidate the results presented in the majority of relevant publications – including some of the most well-known ones – on the topic of flexible-joint robot control. On page 2668 of article [12], after equation 16, it is verified that whether or not $\mathbf{N}$ ($\mathbf{D}$ in their case [12]) is included in the Lyapunov derivative function has no bearing on the asymptotical stability conclusions, which are valid in both cases. Even Tomei's famous paper [4], which is cited by [1]'s author, does not state or even imply that $\mathbf{N}$ is required for asymptotical stability. As a matter of fact, it implies the opposite, i.e. that stability can be proven when the frictional terms are eliminated! To be precise, Tomei's article [4] does not restrict its model to the linear $\mathbf{N}$ term, but rather considers a nonlinear vector of frictional forces.

B. Mathematically Disproving the Incorrect Claims in [1] regarding the Application of LaSalle's Invariance Principle

The erroneous statements regarding the applicability of LaSalle's Invariance Principle to the system comprising equations $(2{:}1)$, $(2{:}2)$, and $(2{:}4)$ in [2], are rigorously refuted in the following sentences. It is worth noting that the forenamed system is *autonomous*, since it is assumed that the gains, namely $\mathbf{K}_{\mathrm{P_M}}$ and $\mathbf{K}_{\mathrm{D_M}}$, remain immutable in equation $(2{:}4)$, as is unambiguously stated after equation $(2{:}6)$. Hence, this enables one to straightforwardly apply LaSalle's Invariance Principle to the *autonomous* system in question, which then leads to the conclusion that $\dot{\boldsymbol{\theta}} \equiv \mathbf{0}$ [7]-[11]. Inserting this result into the system's dynamical model yields:

$$-\mathbf{K}(\boldsymbol{q}-\boldsymbol{\theta}) = \mathbf{K}_{\mathrm{P_M}}\boldsymbol{\theta}_E + \boldsymbol{\tau}_{\boldsymbol{g}}(\boldsymbol{q}_d). \tag{1}$$

By then observing that $\boldsymbol{\theta} = constant$, since $\dot{\boldsymbol{\theta}} = \mathbf{0}$, and that $\boldsymbol{q}_d$, $\boldsymbol{\theta}_d$ are also constant (set-point regulation control), then the above expression can be rearranged as follows:

$$-\mathbf{K}\boldsymbol{q} = \mathbf{K}_{\mathrm{P_M}}\boldsymbol{\theta}_E + \boldsymbol{\tau}_{\boldsymbol{g}}(\boldsymbol{q}_d) - \mathbf{K}\boldsymbol{\theta} = constant, \tag{2}$$

thereby proving that $\boldsymbol{q} = constant$ and $\dot{\boldsymbol{q}} = \mathbf{0}$. Substituting these values into the link-side dynamics – whilst assuming "free motion" operation of the robot, i.e. $\boldsymbol{\tau}_{\boldsymbol{ex}} = \mathbf{0}$ – yields:

$$\mathbf{M}(\boldsymbol{q})\ddot{\boldsymbol{q}} = -\boldsymbol{\tau}_{\boldsymbol{g}}(\boldsymbol{q}) - \mathbf{K}(\boldsymbol{q}-\boldsymbol{\theta}) = constant. \tag{3}$$

Since $\mathbf{M}(\boldsymbol{q})$ is also constant, owing to its dependency on $\boldsymbol{q}$, it is then concluded that $\ddot{\boldsymbol{q}} = constant$. However, it was previously demonstrated that $\dot{\boldsymbol{q}} = \mathbf{0}$, and this consequently implies that $\ddot{\boldsymbol{q}} = \mathbf{0}$. It has therewith been confirmed that the closed-loop dynamics collapses to the well-known equilibrium point described in [2], as well as [4], namely:

$$\begin{bmatrix} \mathbf{K} & -\mathbf{K} \\ -\mathbf{K} & \mathbf{K}+\mathbf{K}_{\mathrm{P_M}} \end{bmatrix} \begin{bmatrix} \boldsymbol{q}_d - \boldsymbol{q} \\ \boldsymbol{\theta}_d - \boldsymbol{\theta} \end{bmatrix} = \begin{bmatrix} \boldsymbol{\tau}_g(\boldsymbol{q}) - \boldsymbol{\tau}_g(\boldsymbol{q}_d) \\ \mathbf{0} \end{bmatrix}. \tag{4}$$

Given that the above equation demonstrably possesses a unique solution [2][4], then this proves global asymptotical stability of the origin. Therefore, the above elicitation negates all the commentary contained in article [1] with regard to the requirement for the presence of the $\mathbf{N}$ matrix in the link-side dynamics, which, apart from being erroneous, also stands in stark opposition to the pertinent results reported in the robotics literature over the last three decades [7]-[11].

III. Major Mathematical Errors in Attempting to Confound the Power-Shaping-Signal Control Approach

This section highlights the most critical errors contained in article [1], which appear in the author's attempt to confound the PSC approach that constitutes the cornerstone of [2]. It is noteworthy that PSC has since proven an invaluable tool in the elicitation of the stability analyses contained in several publications [13]-[18], and it has also been recognised that the work in [2] was the first to address the problem of stability/passivity preservation for variable impedance control of flexible-joint robots [19][20].

A. The Three Fallacious, Non-Existing "Difficulties"

1. (Un)boundedness of the $\dot{\boldsymbol{\Theta}}_{\boldsymbol{PI}}$ Term

The first "difficulty" associated with the power-shaping signal (PSS) is purportedly – according to [1] – the following:

"The entries of $\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}$ are not bounded."

This is a trivially refutable argument, as is demonstrated in [13]: the finite encoder resolution of any robotic system implies that its minimum velocity readings/measurements possess finite values. Since the maximum values of $\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}$ are finite and known, then it can be said that the range of $\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}$ is *closed*, which consequently implies boundedness of this term. It is noteworthy that boundedness of $\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}$ is not even a prerequisite for the stability analysis (or the equilibrium uniqueness analysis because $\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}$ vanishes in the steady state) in [2], as is meticulously described in article [21]; the latter article is completely overlooked in [1], despite the fact that it is adduced numerous times in [2].

2. Unfamiliarity with "Recent" Literature

As a consequence of their unfamiliarity with the existing literature, the author of the said article [1] states that:

"$\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}$ is discontinuous and therefore the existence of unique solutions of the closed-loop differential equations is questionable. If solutions exist in what sense the solution is interpreted? Suitable references for Lyapunov theory for nonlinear differential equations with discontinuities should be provided."

"It is well known that the standard La'Salle's invariance theorem is not applicable to general time varying systems. This is explained in the relevant sections in [2, Ch. 8, p. 308] ... However, the paper does not explain how any of these results can be applied to the specific problem considered in this publication. The validity of several expressions in the paper has not been rigorously justified. This needs further explanations starting with the existence of equilibrium [2, Ch.4, Sec. 4.5]. For time varying systems the type of stability needs to be specified and the paper fails to give precise details for time varying systems."

The stability analysis delineated in our previous paper [2] relies on the *new Invariance Principle* introduced in [21], which is cited numerous times in [2]. The *new Invariance Principle* is applicable to systems with discontinuous control terms, as well as to *nonautonomous* (time-varying) ones;

several such examples are painstakingly described in [21], together with their concomitant proofs. In [2], the *new Invariance Principle* has been rigorously applied to the relevant analysis to establish closed-loop stability, as evinced by equations (49)-(51) in [2]. Thus, this contradicts the unwarranted claims in [1] that the *new Invariance Principle* has not been applied to the problem in question. Hence, the above erroneous claims about the validity of the stability analysis in [2] are in effect attempts at rebuking the *new Invariance Principle* introduced in [21]. However, [1] provides no proof/rationale to justify the reason why the author disagrees with the contents of the *new Invariance Principle* publication [21], which has been cited numerous times. Additionally, other LaSalle's Invariance Principle extensions to *nonautonomous* systems can be found in [22][23]. The forenamed points reveal the obliviousness of [1]'s author to more recent theoretical developments in the field. It is worth pointing out that all these erroneous statements in [1], regarding the validity of the *new Invariance Principle*, also contain numerous linguistic errors – as does the entirety of [1] – such as the one shown below [1]:

"However, the paper only "delineates" a proof. Concrete evidence showing how the New Invariance Principle and Barbalat's lemma have been applied to the specific model and controller in this paper is missing. The <u>*clams*</u> in the paper cannot be verified. This is unacceptable and the paper should have provided rigorous proofs."

The author of the said article [1] is clearly unfamiliar with the word "delineate", which, according to the Merriam-Webster dictionary, is defined as follows:

*"delineate (verb)**:** to describe, portray, or set forth with <u>accuracy</u> or <u>in detail</u>"*

However, the author of [1] assumes that the meaning of *delineate* is to provide a brief outline of an idea, and this constitutes an unacceptable oversight.

3. *The False Assertion Concerning the $\boldsymbol{I_d}$ Vector*

The author of the said article [1] contrives their own form of the $\boldsymbol{I_d}$ vector, as described in the following excerpt:

"The paper states that $\boldsymbol{I_d}$ is a unit vector. The standard definition of a unit vector is any vectors with a norm equal to 1. Therefore, there are an infinite number of unit vectors and this fact causes lack of clarity in the mathematical presentation. If the "unit vector" is a vector of ones, i.e., all entries are equal to one, then this should have been explicitly stated without the terminology "unit vector". The meaning of "unit vector" should be explicitly defined an expression for the symbol $\boldsymbol{I_d}$ is not provided."

The pleonastic paragraph shown above provides several otiose, unit vector definitions, which are, however, completely irrelevant, given that the term in question is lucidly defined immediately after (2:12), as follows:

$$\boldsymbol{I_d} = [1 \dots 1_n]^T \cdot rank\left(\dot{\boldsymbol{\Theta}}_{\mathrm{PI}}\right)^+, \tag{5}$$

where n denotes the number of joints and $\dot{\boldsymbol{\Theta}}_{\mathrm{PI}} \in \mathbb{R}^{n\times n} = diag\left(\dot{\boldsymbol{\theta}}\right)^+$. Also, $[1 \dots 1_n]^T$ is a vector comprising solely ones, while $rank\left(\dot{\boldsymbol{\Theta}}_{\mathrm{PI}}\right)^+$ returns the *Moore-Penrose pseudoinverse* of a scalar, which is even applicable to the 0 scalar, when $rank = 0$ [24]. The implication of these false assertions w.r.t the form of $\boldsymbol{I_d}$ is that equations 2, 3, 4, 5, and 12 in [1] are all invalid. By extension, this also renders all the provided simulation results [1] irrelevant, as they essentially pertain to a completely different control scheme that relies on an aleatory, contrived $\boldsymbol{I_d}$ term. In subsequent publications, e.g. [14], [15], [18], the $\boldsymbol{I_d}$ vector is referred to as a "rank-normalized all-ones vector". Despite the wording, however, the mathematical definition of this vector has always been correct [2], and this invalidates all the calculations in [1].

B. Oxymoronic Statements and Unjustified Comments regarding Vector Calculations

Prior to exposing their incorrect calculations, the author of the said article [1] commences with the oxymoronic comment shown below:

"In Theorem 1 and the relevant proof the paper does not explain what happens to $\boldsymbol{\tau}_{ex}$. The theorem statement lacks clarity and mathematical rigor. Moreover, even setting $\boldsymbol{\tau}_{ex} = \boldsymbol{0}$, (1:17) is incorrect."

The author of [1] initially complains that $\boldsymbol{\tau}_{ex}$ is not assumed to be zero, and in the subsequent sentence concedes that in order to obtain (2:17), it must be assumed that $\boldsymbol{\tau}_{ex} = \boldsymbol{0}$. There is then another erroneous statement about the penultimate term in (2:44), which is said to be incorrectly transposed:

"There are even notation errors in (1:44) in the transpositions of the second last term and transpositions errors in the term $\boldsymbol{\tau}(\boldsymbol{x})$ defined in appendix A."

The penultimate term in (2:44) is $\dot{\boldsymbol{q}}_{\mathrm{F}}^{\mathrm{T}}\boldsymbol{\tau}(\boldsymbol{x})$, where $\boldsymbol{q}_F \in \mathbb{R}^{2n} = [\boldsymbol{q}^T \quad \boldsymbol{\theta}^T]^{\mathrm{T}}$, as per the definition provided in sub-section II.B [2], while $\boldsymbol{\tau}(\boldsymbol{x}) = \left[\boldsymbol{\tau_g}(\boldsymbol{q})^T - \boldsymbol{\tau}_{\boldsymbol{gd}}^T \quad \boldsymbol{0}\right]^T$, as defined in Appendix A [2]. Since the actual, and desired, gravitational torque vectors are defined as $\boldsymbol{\tau_g}(\boldsymbol{q}) \in \mathbb{R}^n$ and $\boldsymbol{\tau_{gd}} = \boldsymbol{\tau_g}(\boldsymbol{q_d}) \in \mathbb{R}^n$, in sub-sections II.A and II.B of [2], respectively, then this implies that $\boldsymbol{\tau}(\boldsymbol{x}) \in \mathbb{R}^{2n}$. Thus, multiplying $\boldsymbol{\tau}(\boldsymbol{x})$ with $\dot{\boldsymbol{q}}_{\mathrm{F}}^{\mathrm{T}}$ is a perfectly valid mathematical operation, which leads to a scalar, thereby conforming to the requirements of the Lyapunov derivative function.

C. Fundamental Mathematical Errors that Invalidate the Entirety of the Contents of [1]

Equations (1)-(5) in article [1], as well as the concomitant commentary, are starkly erroneous, as is to be evinced through the straightforward calculations provided in the subsequent sentences. The said article [1] makes the unsubstantiated claim that

"The arguments provided in Appendix B are incomplete and the claim that (1:44) reduces to (1:17) is false."

Firstly, equation (1) in [1] is incorrect – its correct form would be the following:

$$\dot{\boldsymbol{\theta}}^T\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}\boldsymbol{I_d} = \dot{\boldsymbol{\theta}}^T\dot{\boldsymbol{\Theta}}_{\mathbf{PI}} \cdot [1\ 1\ 1 \dots 1\ 1]^T \cdot rank\left(\dot{\boldsymbol{\Theta}}_{\mathrm{PI}}\right)^+, \tag{6}$$

which could then be expressed as follows:

$$\dot{\boldsymbol{\theta}}^T \dot{\boldsymbol{\Theta}}_{\mathbf{PI}} \boldsymbol{I_d} = [1\ 1\ 1 \dots 1\ 1][1\ 1\ 1 \dots 1\ 1]^T \cdot rank\left(\dot{\boldsymbol{\Theta}}_{\text{PI}}\right)^+. \quad (7)$$

Clearly, then, the above expression would simplify to:

$$\dot{\boldsymbol{\theta}}^T \dot{\boldsymbol{\Theta}}_{\mathbf{PI}} \boldsymbol{I_d} = n \cdot rank\left(\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}\right)^+, \quad (8)$$

and observing that in this particular case, $rank\left(\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}\right) = n$, would lead to the result:

$$\dot{\boldsymbol{\theta}}^T \dot{\boldsymbol{\Theta}}_{\mathbf{PI}} \boldsymbol{I_d} = n \cdot \frac{1}{n} = 1. \quad (9)$$

Thus, equations (2) , (4) in [1] are erroneous. Further, equation (3) in [1] is incorrect, and should instead have been expressed as follows:

$$\dot{\boldsymbol{\theta}}^T \dot{\boldsymbol{\Theta}}_{\mathbf{PI}} \boldsymbol{I_d} = [1\ 1\ 0 \dots 1\ 1] \dot{\boldsymbol{\Theta}}_{\mathbf{PI}} [1\ 1\ 1 \dots 1\ 1]^T rank\left(\dot{\boldsymbol{\Theta}}_{\text{PI}}\right)^+ \quad (10)$$

The assertion that the above expression reduces to equation (5) in [1] is equally fallacious – in reality, the correct form of the reduced version of the above equation would be:

$$\dot{\boldsymbol{\theta}}^T \dot{\boldsymbol{\Theta}}_{\mathbf{PI}} \boldsymbol{I_d} = (n-1) \cdot \frac{1}{(n-1)} = 1, \quad (11)$$

given that $rank\left(\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}\right) = n - 1$, when one of the motor velocity vector's ($\dot{\boldsymbol{\theta}}$) entries is zero. By extension, the $\dot{\boldsymbol{\theta}}^T \dot{\boldsymbol{\Theta}}_{\mathbf{PI}} \boldsymbol{I_d}$ term always yields unity, except in the case where all the entries of the motor velocity vector ($\dot{\boldsymbol{\theta}}$) are zero. When the said situation arises, $rank\left(\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}\right) = 0$, while the control law remains numerically sound, since $\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}$ is still invertible owing to usage of the Moore-Penrose pseudoinverse, which also ensures that $rank\left(\dot{\boldsymbol{\Theta}}_{\mathbf{PI}}\right)^+ = 0^+ = 0$. In view of these results, equation (2:44) always reduces to equation (2:17), in virtue of the PSC term's [13]-[18] ability to nullify any undesirable terms appearing in the Lyapunov derivative function, $\dot{V}$. This stands in stark contrast to the invalid claim made in [1] that the following expression does not hold:

$$\frac{q_e^T \dot{\mathrm{T}}_{\mathrm{K}} q_e}{2} + \frac{\dot{q}_F^T (\mathrm{T}_{\mathrm{K}}^{\mathrm{T}} - \mathrm{T}_{\mathrm{K}}) q_e}{2} - \frac{\dot{\theta}^T \dot{\Theta}_{\mathrm{PI}} I_D (q_e^T \dot{\mathrm{T}}_{\mathrm{K}} q_e + \dot{q}_F^T (\mathrm{T}_{\mathrm{K}}^{\mathrm{T}} - \mathrm{T}_{\mathrm{K}}) q_e)}{2} = 0. \quad (12)$$

It can be seen that by simply substituting the unity result described in equations (5) and (7), into the above expression, one is always able to guarantee that

$$\frac{q_e^T \dot{\mathrm{T}}_{\mathrm{K}} q_e}{2} + \frac{\dot{q}_F^T (\mathrm{T}_{\mathrm{K}}^{\mathrm{T}} - \mathrm{T}_{\mathrm{K}}) q_e}{2} - \frac{(q_e^T \dot{\mathrm{T}}_{\mathrm{K}} q_e + \dot{q}_F^T (\mathrm{T}_{\mathrm{K}}^{\mathrm{T}} - \mathrm{T}_{\mathrm{K}}) q_e)}{2} = 0. \quad (13)$$

Additionally, in cases where $\dot{\boldsymbol{\theta}} = \mathbf{0}$ and $\dot{\boldsymbol{\theta}} \neq \dot{\boldsymbol{q}}$, there could theoretically be residuals, i.e. uncancelled terms, in the above function. However, this is always preventable since impedance modulations can be suspended by the user, whenever $\dot{\boldsymbol{\theta}} = \mathbf{0}$, which then implies that $\dot{\mathbf{T}}_{\mathbf{K}} = \mathbf{0}$. Moreover, the $\mathbf{K}_{\mathrm{P_{Jt}}}$ gain can then also be *temporarily* nullified by the user, thereby rendering $\mathbf{T}_{\mathbf{K}}$ a symmetric matrix, leading to $\mathbf{T}_{\mathbf{K}}^{\mathbf{T}} - \mathbf{T}_{\mathbf{K}} = \mathbf{0}$; the user need also ensure that $\mathbf{K}_{\mathrm{P_{Jt}}}$ is nullified using a step function, which would also guarantee that $\dot{\mathbf{T}}_{\mathbf{K}} = \mathbf{0}$. Hence, the following statement in [1] is false:

> "Therefore, (1:44) does not simplify to (1:17) and in fact $\dot{V}$ is discontinuous. Standard Lyapunov theory requires V to be continuously differentiable."

Contrariwise, the control law and analysis provided in [2] guarantee that $\dot{V}$ is always a continuous function. The upshot is that the above calculations, i.e. equations (6)-(13) in this paper, dispel all the misconceptions that are manifest in [1], with regard to the validity of the mathematical proofs in [2]. The PSC approach allows one to considerably simplify the stability analysis process, and to procure theoretical results that are otherwise involute to generate using conventional, robot control methodologies.

IV. The Exceedingly Longwinded Discussion on the Eigenvalues of $\mathbf{T}_{\mathbf{K}}$ Based on an Erroneous Assumption

There is an exceedingly longwinded discussion on the third page of [1] that revolves around the topic of the $\mathbf{T}_{\mathbf{K}}$ matrix's eigenvalues. It is worth noting, prior to dismantling these (ir)relevant arguments one by one, that all the contents of the third page are invalid, void, and redundant, given that *Theorem 1* is expressed in [2] as reproduced verbatim below:

*Theorem 1: If the closed-loop system comprising (1), (2), and (12), satisfies $\lambda_{\mathrm{m}}(\mathbf{T}_{\mathbf{K}}) > \alpha$ and $\lambda_{\mathrm{m}}(\boldsymbol{\Xi}_{\mathbf{D}}) > 0$, **and $\mathbf{T}_{\mathbf{K}}$ is positive definite**, then this ensures the existence of a GAS equilibrium at $\boldsymbol{q}_{\mathbf{eq}}$.*

The fact that *Theorem 1* in [2] explicitly stipulates the additional condition that $\mathbf{T}_{\mathbf{K}}$ must be *positive definite*, essentially invalidates all the erroneous arguments contained in [1], with regard to the eigenvalues of $\mathbf{T}_{\mathbf{K}}$. Analogous errors were made by the author of [1] in a recently published Discussion article; the reader is referred to [25] for a *delineation* of these errors.

A. The Fallacious Counterexample that Serves as a Positive Example of [2]'s Theorems

Most of the misconceptions in the said article [1], with regard to the $\mathbf{T}_{\mathbf{K}}$ matrix, are conveniently synopsised in the paragraph below:

> "The paper does not explain how the condition given in equation (1:14) and in Theorem 1 holds. If no constraints are placed on the matrices $\mathbf{K}$, $\mathbf{K}_{\mathrm{P_{Mt}}}$, and $\mathbf{K}_{\mathrm{P_{Jt}}}$ the eigenvalues of $\mathbf{T}_{\mathbf{K}}$ can even become complex values. For the same reason, the results in Appendix A are incorrect, namely equation (1:41) is wrong since the matrix norm of a nonsymmetric matrix is not given by the maximum eigenvalue. The application of the contraction mapping theorem in terms of the largest eigenvalue of $\mathbf{T}_{\mathbf{K}}^{-1}$ is therefore invalid. The paper does not even mention how to ensure this inverse exists. The conclusion given in equation (1:43) is also wrong."

Firstly, it is nonsensical to claim that constraints should be placed on $\mathbf{K}$, which is a physical parameter and can therefore not be readily modified by the user, as this would entail tampering with the hardware. Also, sub-section IV. C of [2] provides very specific gain-tuning instructions to ensure satisfaction of the $\lambda_{\mathrm{m}}(\mathbf{T_K}) > \alpha$ condition, as per the sentence:

"Moreover, the generated gains should satisfy $\lambda_{\mathrm{m}}(\mathbf{T_K}) > \alpha$, which is achieved by assigning larger values to the $\mathbf{Q}$ matrix penalty terms associated with motor position states, as opposed to link position states, such that $\mathbf{K}_{\mathrm{P_{Mt}}}$ becomes the dominant term in $\mathbf{T_K}$, as $\mathbf{K}_{\mathrm{D_{Mt}}}$ should also be the dominant term in $\mathbf{\Psi_K}$."

Interestingly enough, the author of [1] follows the gain-tuning approach described in the above paragraph to generate a gain-set satisfying the criteria of *Theorem 1* [2] – this is lucidly described in the following sentence excerpted from [1]:

"If the link position gains are reduced to $\mathbf{K}_{\mathrm{P_J}} = \begin{bmatrix} 40 & 0 \\ 0 & 40 \end{bmatrix}$, then $\mathbf{T_K}$ becomes positive definite."

By reducing the values of the $\mathbf{K}_{\mathrm{P_J}}$ matrix, one effectively renders the $\mathbf{K}_{\mathrm{P_M}}$ values dominant in $\mathbf{T_K}$, as described in [2], thereby leading to satisfaction of *Theorem 1*'s conditions. Thus, the critique made in [1] about the gain-tuning approach of [2] is actually self-negating, and in fact serves as a positive example of the validity of *Theorem 1* [2].

B. The Illogicality of the Uniqueness Analysis Claims

It is irrational to claim that the contraction mapping results in [2] are invalid, since, according to the mean value theorem, equation (3) in [2] permits the inequality [4][7][8]:

$$\left\|\boldsymbol{\tau}_g(\boldsymbol{q}_d) - \boldsymbol{\tau}_g(\boldsymbol{q})\right\| \leq \alpha\|\boldsymbol{q}_d - \boldsymbol{q}\|, \tag{14}$$

which enables one to further establish that [4]:

$$\left\|\boldsymbol{\tau}_g(\boldsymbol{q}_d) - \boldsymbol{\tau}_g(\boldsymbol{q})\right\| \leq \alpha\|\boldsymbol{q}_E\| \leq \alpha\left\|\boldsymbol{q}_{F_E}\right\| < \lambda_{\mathrm{m}}(\mathbf{T}_{\mathrm{K}})\left\|\boldsymbol{q}_{F_E}\right\| \leq \left\|\mathbf{T}_{\mathrm{K}}\boldsymbol{q}_{F_E}\right\|, \tag{15}$$

where $\boldsymbol{q}_E = \boldsymbol{q}_d - \boldsymbol{q}$, and $\boldsymbol{q}_{F_E} \in \mathbb{R}^{2n} = [\boldsymbol{q}_E^T \quad \boldsymbol{\theta}_E^T]^{\mathrm{T}}$. The above result guarantees the uniqueness of equation (40) in [2], as well as the validity of equation (2:41) [2] – this is because $\lambda_{\mathrm{m}}(\mathbf{T_K}) = \lambda_{\mathrm{M}}(\mathbf{T_K^{-1}})$, in virtue of $\mathbf{T_K}$'s *positive definiteness*, which is central to *Theorem 1* [2]. Equally illogical is the implication that the inverse of $\mathbf{T_K}$, namely $\mathbf{T_K^{-1}}$, may not exist; it is well known from rudimentary linear algebra that positive definite matrices are always invertible.

C. The Numerically Absurd Inequality

The expression appearing after (1:13) is incorrect: α is said to be equal to 10.04804 on page 3 of [1], and after equation (1:13) it is stated that $\lambda_{\mathrm{m}}\left(0.5 * \left(\mathbf{T_K^T} + \mathbf{T_K}\right)\right) = 10.6150 < \alpha$, effectively implying that $10.6150 < 10.04804$, which constitutes an absurdity.

D. The Nugatory Nature of Appendix B

The elementary information contained in Appendix 2 of [1] – with regard to the conditions required to guarantee a matrix's positive definiteness – is of minimal use, as it can be found in the majority of linear algebra textbooks, in addition to lecture notes, and several other online sources. Additionally, Appendix 2 is nugatory in terms of the context in which it is introduced, as is testified by the following sentence [1]:

"The requirement $\lambda_{\mathrm{m}}(\mathrm{T_K}) > \alpha$ is not sufficient. This condition can be satisfied but $\mathbf{T_K}$ might not be positive definite (see Appendix 2 for definition and facts of positive definite matrices)."

Once again, the above statement completely overlooks the precise wording of *Theorem 1*, which perspicuously stipulates the following conditions for closed-loop stability:

*"… $\lambda_{\mathrm{m}}(\mathbf{T_K}) > \alpha$, $\lambda_{\mathrm{m}}(\mathbf{\Xi_D}) > 0$, **and $\mathbf{T_K}$ is positive definite**, then this ensures the existence of a GAS equilibrium at $\boldsymbol{q}_{\mathbf{eq}}$."*

This reinforces the fact that the contents of Appendix 2 in [1] are irrelevant, and inconsequential, in this context.

V. The Contrived Simulation Results based on an Overly Simplistic & Incorrect Dynamical Model

The "A counterexample" section of [1] contains an array of simulation results that purportedly serves the purpose of invalidating the PSC methodology proposed in [2]; it will, however, be demonstrated in the subsequent lines, that these results are the product of the author's failure to follow the controller implementation guidelines contained in [2].

A. Incorrect Coriolis/centrifugal Term in Dynamical Model

Prior to exposing the absurdity of the simulation results contained in [1], it is worth noting that the dynamical model – based on which these results have been procured – is actually erroneous per se. By examining equation (1) in [2], it is evident that $\mathbf{C}(\boldsymbol{q},\dot{\boldsymbol{q}})$ is assumed to be a matrix, as per the $\mathbf{C}(\boldsymbol{q},\dot{\boldsymbol{q}}) \in \mathbb{R}^{n\times n}$ definition appearing immediately after (2:2). The full dynamical model considered in [2] is expressed as:

$$\mathbf{M}(\boldsymbol{q})\ddot{\boldsymbol{q}} + \mathbf{C}(\boldsymbol{q},\dot{\boldsymbol{q}})\dot{\boldsymbol{q}} + \mathbf{N}\dot{\boldsymbol{q}} + \mathbf{K}(\boldsymbol{q} - \boldsymbol{\theta}) + \boldsymbol{\tau}_g(\boldsymbol{q}) = \boldsymbol{\tau}_{ex}, \tag{16}$$

$$\mathbf{J}\ddot{\boldsymbol{\theta}} + \mathbf{D}\dot{\boldsymbol{\theta}} - \mathbf{K}(\boldsymbol{q} - \boldsymbol{\theta}) = \boldsymbol{\tau}_m, \tag{17}$$

where $\boldsymbol{q} \in \mathbb{R}^n$ and $\boldsymbol{\theta} \in \mathbb{R}^n$ denote the link and motor positions, while $\mathbf{M}(\boldsymbol{q})$, $\mathbf{N}$, $\mathbf{C} \in \mathbb{R}^{n\times n}$ signify the symmetric inertia and damping, and skew-symmetric Coriolis matrices, respectively. Moreover, $\boldsymbol{\tau}_g(\boldsymbol{q}) \in \mathbb{R}^n$, $\boldsymbol{\tau}_{ex} \in \mathbb{R}^n$, and $\boldsymbol{\tau}_m \in \mathbb{R}^n$ represent the gravitational, external, and input torque vectors, respectively. Finally, $\mathbf{K}$, $\mathbf{J}$, and $\mathbf{D} \in \mathbb{R}^{n\times n}$ denote the diagonal, positive definite stiffness, motor inertia, and joint damping matrices. It is also stated twice, in Appendix B of [2], that the $\dot{\boldsymbol{q}}^T(\dot{\mathbf{M}} - 2\mathbf{C})\dot{\boldsymbol{q}} = \mathbf{0}$ property is assumed to hold true [25]. However, in Appendix 1 of [1], the author opts for a disparate link dynamics model as a basis for their incorrect simulations, which is expressed as follows [1]:

$$\mathbf{M}(\boldsymbol{q})\ddot{\boldsymbol{q}} + \mathbf{C}(\boldsymbol{q},\dot{\boldsymbol{q}}) + \mathbf{N}\dot{\boldsymbol{q}} + \mathbf{K}(\boldsymbol{q} - \boldsymbol{\theta}) + \boldsymbol{\tau}_g(\boldsymbol{q}) = \boldsymbol{\tau}_{ex}, \tag{18}$$

Evidently, the above expression assumes that the $\mathbf{C}(\boldsymbol{q},\dot{\boldsymbol{q}})$ term is a vector, i.e. $\mathbf{C}(\boldsymbol{q},\dot{\boldsymbol{q}}) \in \mathbb{R}^n$, which consequently leads to the impossible operation $\dot{\mathbf{M}} - 2\mathbf{C}$ and, by extension, to invalidation of the $\dot{\boldsymbol{q}}^T(\dot{\mathbf{M}} - 2\mathbf{C})\dot{\boldsymbol{q}} = \mathbf{0}$ property. As a disclaimer for the reader, it is worth noting that the dynamical model employed to produce the peculiar simulation results appearing in [1] is incorrect, and in no way conforms to the dynamical model considered in the original article [2]; nevertheless, equation (12) (in [1]) has been appositely factorised to ensure congruency with the theoretical results and models in [2], and subsequently used to produce the simulation results contained herein.

B. Counterexamples to the Confounding Simulation Results Generated in [1]

As was mentioned in the previous section, the remarks relating to $\mathbf{T_K}$'s positive definiteness are nonsensical, since they overlook the precise wording of *Theorem 1* in [2]. It is interesting to note, however, that even when using the incorrectly selected gains described in Appendix A of [1], a stable simulation can be produced, provided that the implementation guidelines described in sub-section V. D of [2] are followed. This conforms to the commonly known fact that Lyapunov stability proofs give rise to sufficient, but not necessary, conditions; thus, for nonlinear dynamical models, it might be possible to produce stable closed-loop systems, even when violating some of the theoretical stability criteria. Figs. 1-20 have been generated from a simulation model (Fig. 21) that is based on dynamical parameters identical to those shown in Appendix 1 of [1]. It is evident – in contradistinction to what is claimed in [1] – that the overly simple, and incorrect, double inverted pendulum model in [1] can be stably controlled using the scheme proposed in [2]; these results are showcased in the accompanying video attachment.

1. *Stable Set-Point Regulation Simulations: Gain-Set A*

The first set of simulations entails supplying the manipulator with constant link-position references, identical to those considered in [1], i.e.:

$$\boldsymbol{q}_d = [0.1 \quad -0.1]^{\mathrm{T}}. \qquad (19)$$

It is worth noting that the equation used in [1] to perform link-to-motor position reference conversions, namely (1:19), is incorrect, since the undefined $\boldsymbol{q}_q$ variable should actually be equal to $\boldsymbol{q}_d$ – any other permutation of this equation is simply erroneous and incongruous w.r.t controller presented in [2]. For the simulations contained herein, the following, correct, link-to-motor position reference conversion equation is used:

$$\boldsymbol{\theta}_d = \boldsymbol{q}_d + \mathbf{K}^{-1}\boldsymbol{\tau}_{gd}. \qquad (20)$$

Hence, the control scheme implemented in the simulation considered herein is the following:

$$\boldsymbol{\tau}_m = \mathbf{K_{P_M}}\boldsymbol{\theta}_E - \mathbf{K_{D_M}}\dot{\boldsymbol{\theta}} + \mathbf{K_{P_J}}\boldsymbol{q}_E - \mathbf{K_{D_J}}\dot{\boldsymbol{q}} + \boldsymbol{\tau}_{gd} - \frac{1}{2}\dot{\Theta}_{\mathrm{PI}}\boldsymbol{I}_d\left(\boldsymbol{q}_e^T\dot{\mathbf{T}}_{\mathbf{K}}\boldsymbol{q}_e + \dot{\boldsymbol{q}}_F^T\left(\mathbf{T_K^T} - \mathbf{T_K}\right)\boldsymbol{q}_e\right), \qquad (21)$$

where $\mathbf{K_{P_M}}$, $\mathbf{K_{D_M}}$, $\mathbf{K_{P_J}}$, and $\mathbf{K_{D_J}} \in \mathbb{R}^{n\times n}$ signify diagonal, positive definite, motor and link, active stiffness and damping matrices, respectively. Moreover, the exact same gain-sets as those considered in [1] are used herein, to ensure fairness, and these are dubbed gain-set A (GSA) and gain-set B (GSB). GSA comprises the following values [1]:

$$\mathbf{K_{P_M}} = diag([210 \quad 210]), \qquad (22)$$
$$\mathbf{K_{D_M}} = diag([5 \quad 5]), \qquad (23)$$
$$\mathbf{K_{P_J}} = diag([160 \quad 160]), \qquad (24)$$
$$\mathbf{K_{D_J}} = diag([4 \quad 4]), \qquad (25)$$

Fig. 1 illustrates that the link positions converge to the desired set point references represented by equation (19), while the corresponding link and motor velocities converge to zero (Fig. 2). In contrast to [1], the $\mathbf{K_{P_J}}$ gains are modulated in "real-time" to ensure saturation prevention, and this is performed using a step function, i.e. the gains are either decreased to zero or increased to 160 instantaneously, which ensures that $\dot{\mathbf{T}}_{\mathbf{K}} = \mathbf{0}$, whilst also yielding $\mathbf{T_K^T} - \mathbf{T_K} = \mathbf{0}$, thereby *momentarily* nullifying the PSS. This approach harmonises with the guidelines provided in sub-section V. D of [2]. Fig. 3 illustrates the $\mathbf{K_{P_J}}$ activation/deactivation instances, and Fig. 4 displays the corresponding PSS values, whose magnitudes are very reasonable when compared to the total torque input values contained in Fig. 5.

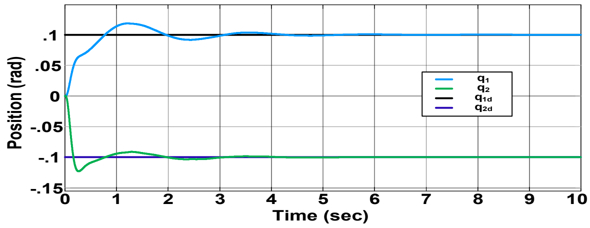

Figure 1. Link position responses during set-point regulation (gain-set A).

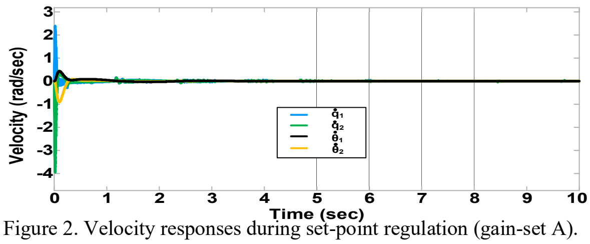

Figure 2. Velocity responses during set-point regulation (gain-set A).

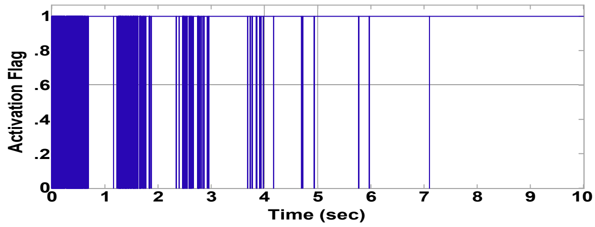

Figure 3. (De)activation of $\mathbf{K_{P_J}}$ gains during regulation (gain-set A).

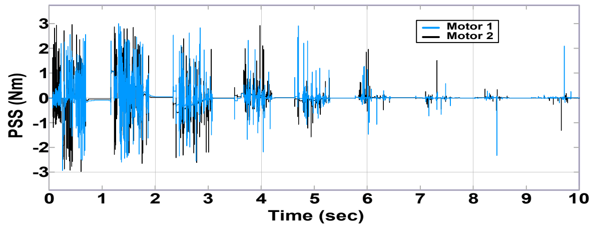

Figure 4. PSS magnitudes during set-point regulation (gain-set A).

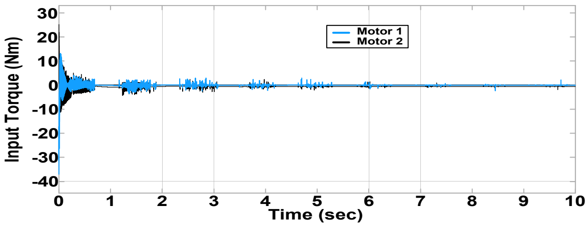

Figure 5. Total input torque values during set-point regulation (gain-set A).

2. Stable Set-Point Regulation Simulations: Gain-Set B

In contrast to GSA, GSB actually satisfies the conditions specified in *Theorem 1* [2], and is composed of identical gains to those pertaining to GSA, but with the following exception:

$$\mathbf{K}_{\mathrm{P_J}} = diag([40 \quad 40]). \tag{26}$$

Thus, usage of GSB ensures that $\mathbf{T_K}$ is positive definite, i.e. $\lambda_{\mathrm{m}}\left(0.5 * \left(\mathbf{T}_{\mathrm{K}}^{\mathrm{T}} + \mathbf{T_K}\right)\right) > 0$; additionally, it satisfies the condition $\lambda_{\mathrm{m}}\left(0.5 * \left(\mathbf{T}_{\mathrm{K}}^{\mathrm{T}} + \mathbf{T_K}\right)\right) > \alpha$, as well as the more conservative condition of *Theorem 1* [2], namely $\lambda_{\mathrm{m}}(\mathbf{T_K}) > \alpha$. Once again, in stark contrast to the preposterous results presented in [1], using GSB together with equation (21) produces a stable closed-loop system. Figs. 6 and 7 verify that the link positions and velocities stably converge to the desired equilibrium, as per *Theorem 1* [2].

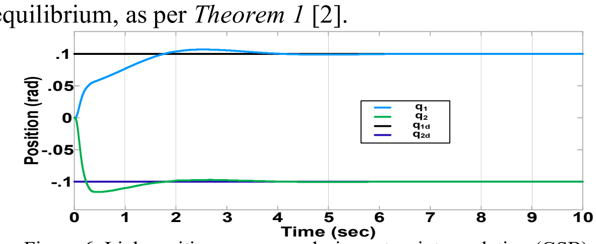

Figure 6. Link position responses during set-point regulation (GSB).

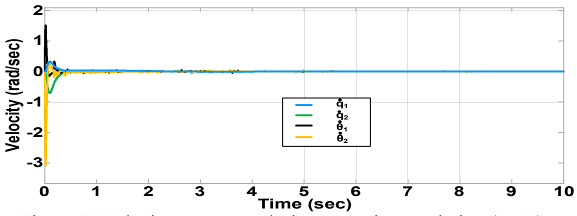

Figure 7. Velocity responses during set-point regulation (GSB).

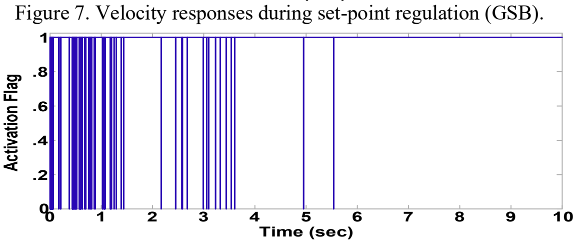

Figure 8. Activation/deactivation of $\mathbf{K}_{\mathrm{P_J}}$ gains during regulation (GSB).

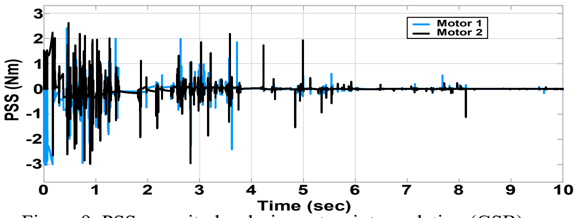

Figure 9. PSS magnitudes during set-point regulation (GSB).

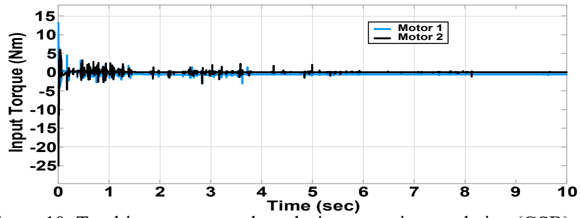

Figure 10. Total input torque values during set-point regulation (GSB).

Fig. 8 shows the $\mathbf{K}_{\mathrm{P_J}}$ activation/deactivation instances that ensure saturation prevention, Fig. 9 illustrates the concomitant PSS torque values, and Fig. 10 displays the total torque input values. Collectively, Figs. 6-10 corroborate the efficacy of the scheme proposed in [2] w.r.t set-point regulation control, whilst simultaneously controverting the curious, set-point regulation results presented in [1].

3. Stable Trajectory-Tracking Simulations: Gain-Set A

As another disclaimer for the reader, it is crucial to point out that the trajectory-tracking section in [1] is completely out of touch with the contents of [2], which merely revolve around set-point regulation control in conjunction with online impedance gain modulation. It is unequivocally stated in sub-section V. B of [2], that a *set-point generator* – inspired by the one proposed in [26] – was utilised to conduct the pertinent practical experiments. Thus, the results in this sub-section of the paper do not align with the contents of the original publication [2], and only serve as a means of debunking the fallacious, and irrelevant, trajectory-tracking simulations appearing in [1]. Using the link-position trajectory generator presented in [1], which is the following:

$$\boldsymbol{q}_d = \begin{bmatrix} 0.1 + 0.05 \cdot sin(\omega_0 t) \\ -0.1 - 0.05 \cdot cos(\omega_0 t) \end{bmatrix}, \tag{27}$$

in combination with equations (20) and (21), $\omega_0 = 1.5708$ rad/sec [1], and GSA, yields a stable simulation, as verified via Figs. 11-15. Figs. 11 and 12 reveal stable convergence to the equilibrium point (*Theorem 1* [2]), Fig. 13 displays the $\mathbf{K}_{\mathrm{P_J}}$ (de)activation instances, while Figs. 14 and 15 verify that the input torques produced by (20) are perfectly feasible and bounded, in marked opposition to the results in [1].

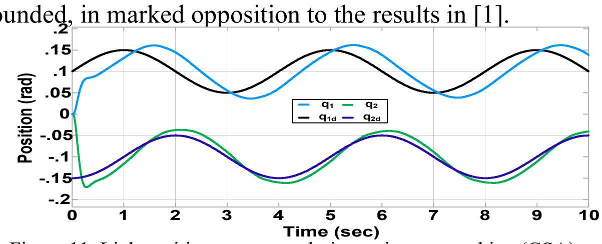

Figure 11. Link position responses during trajectory tracking (GSA).

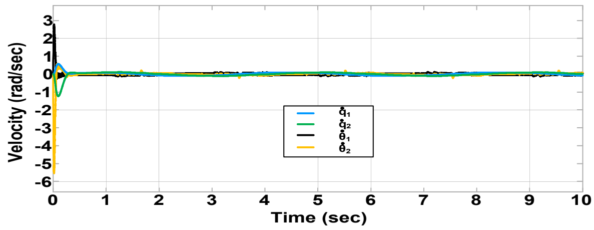

Figure 12. Velocity responses during trajectory tracking (GSA).

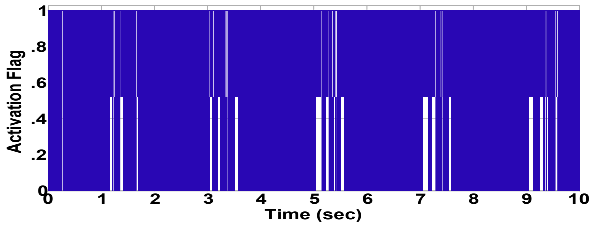

Figure 13. Activation/deactivation of $\mathbf{K}_{\mathbf{P}_\mathbf{J}}$ gains during tracking (GSA).

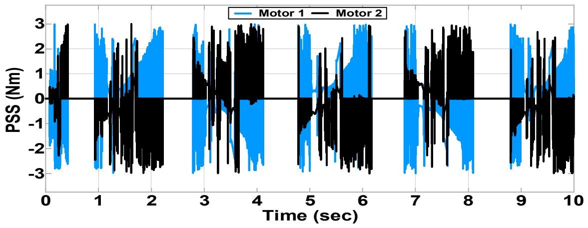

Figure 14. PSS magnitude during trajectory tracking (GSA).

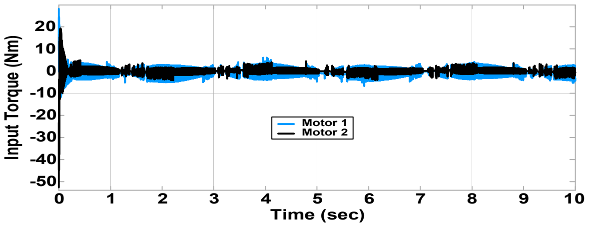

Figure 15. Total input torque values during trajectory tracking (GSA).

4. Stable Trajectory-Tracking Simulations: Gain-Set B

The simulations described in the previous sub-section were executed anew, but this time using GSB instead of GSA. Although Fig. 16 demonstrates tracking control performance degradation w.r.t Fig. 11, it still verifies that the simple (and incongruous) double inverted pendulum model [1] can be stably controlled using the scheme proposed in [2]. It is worth elucidating that the suboptimal gain values pertaining to GSA and GSB were selected in [1], and are not a product of the work herein described. Further, Fig. 17 confirms boundedness of the velocities for the task at hand; Fig. 18 contains the $\mathbf{K}_{\mathbf{P}_\mathbf{J}}$ gain deactivation instances; Figs. 19 and 20 validate that the input torques generated using equation (20) evolve within perfectly reasonable limits, as opposed to the odd results reported in [1].

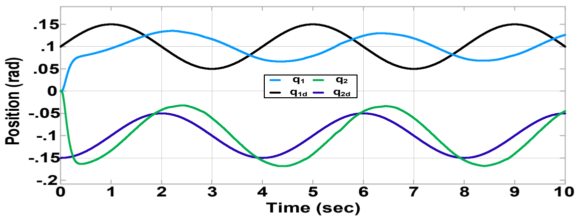

Figure 16. Link position responses during trajectory tracking (GSB).

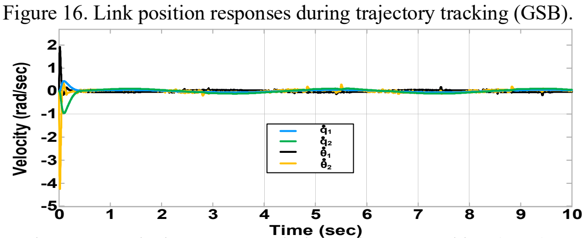

Figure 17. Velocity responses during trajectory tracking (GSB).

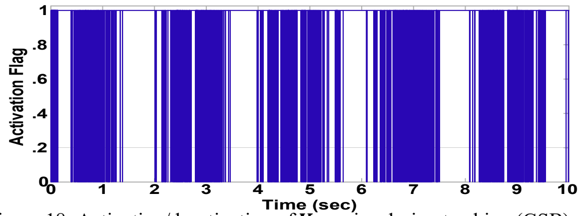

Figure 18. Activation/deactivation of $\mathbf{K}_{\mathbf{P}_\mathbf{J}}$ gains during tracking (GSB).

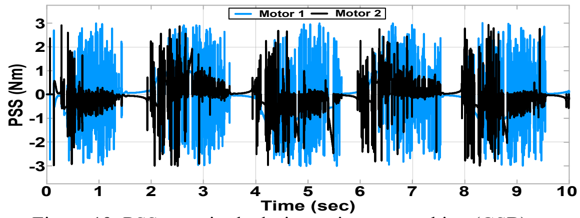

Figure 19. PSS magnitude during trajectory tracking (GSB).

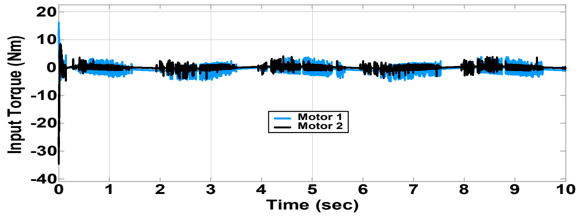

Figure 20. Total input torque values during trajectory tracking (GSB).

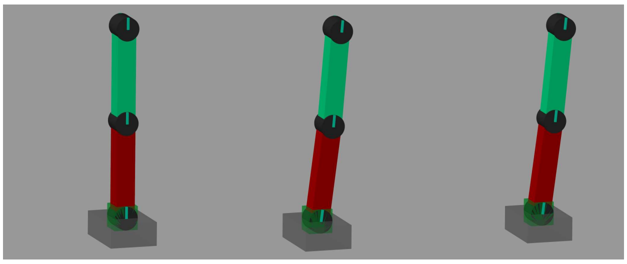

Figure 21. The Simscape model used to verify the simulation results.

VI. Conclusions

This paper has – through rigorous mathematical calculations, sound argumentation, and an abundance of numerical results – apodictically overturned the fallacious contents of [1]. In doing so, it has exposed the ineffectual and circumlocutory arguments in [1], its numerical absurdities, miscontrual of results and theories that have been well-known to the robot control community for decades, as well as the manifold erroneous mathematical calculations. Moreover, this paper has unequivocally demonstrated – via an array of numerical results – that the contrived simulation data presented in [1], whose motive may have been to cast doubt on the feasibility of the control method presented in [2], was simply a product of that author's failure to follow the pellucid implementation guidelines in [2].

References

[1] G. Medrano-Cerda, "Counterexample to Passivity preservation for variable impedance control of compliant robots," *IEEE/ASME Transactions on Mechatronics, vol. 28, no. 1, pp. 598-602, 2023.*

[2] E. Spyrakos-Papastavridis, P. Childs, J. S. Dai, "Passivity preservation for variable impedance control of compliant robots," *IEEE/ASME Transactions on Mechatronics*, *vol. 25, no. 5, pp. 2342-2353, 2020.*

[3] M. Spong, "Modeling and control of elastic joint robots," *ASME Journal of Dynamical Systems, Measurement, and Control, vol. 109, pp. 310–319, 1987.*

[4] P. Tomei, "A Simple PD Controller for Robots with Elastic Joints," *IEEE Transactions on Automatic Control, vol. 36, pp. 1208-1213, 1991.*

[5] E. Spyrakos-Papastavridis, G. A. Medrano-Cerda, N. G. Tsagarakis, J. S. Dai, D. G. Caldwell, "A Compliant Humanoid Walking Strategy based on the Switching of State Feedback Gravity Compensation Controllers," *IEEE/RSJ International Conference on Intelligent Robots and Systems, pp. 3630-3636, 2013.*

[6] E. Spyrakos-Papastavridis, G. A. Medrano-Cerda, N. G. Tsagarakis, J. S. Dai and D. G. Caldwell, "Gravity Compensation Control of Compliant Joint Systems with Multiple Drives," *IEEE International Conference on Robotics and Automation, pp.4960-4966, 2013.*

[7] A. de Luca, B. Siciliano, L. Zollo, "PD control with on-line gravity compensation for robots with elastic joints: Theory and experiments," *Automatica*, *vol. 41, no. 10, pp. 1809–1819, 2005.*

[8] L. Zollo et al., "Compliance Control for an Anthropomorphic Robot with Elastic Joints: Theory and Experiments," *ASME Journal of Dynamic Systems, Measurement, and Control, vol. 127, pp. 321-328, 2005.*

[9] C. Ott, A. Albu-Schäffer, A. Kugi, S. Stramigioli, G. Hirzinger, "A Passivity Based Cartesian Impedance Controller for Flexible Joint Robots - Part I: Torque Feedback and Gravity Compensation," *IEEE International Conference on Robotics and Automation, pp. 2659-2665, 2004.*

[10] C. Ott et al., "On the Passivity-Based Impedance Control of Flexible Joint Robots," *IEEE Transactions on Robotics, vol. 24, no. 2, pp. 416-429, 2008.*

[11] B. Siciliano, O. Khatib, *Springer Handbook of Robotics*, 2 ed., 2016.

[12] A. Albu-Schäffer, C. Ott, G. Hirzinger, "A Passivity Based Cartesian Impedance Controller for Flexible Joint Robots - Part II: Full State Feedback, Impedance Design and Experiments," *IEEE International Conference on Robotics and Automation, pp. 2666-2672, 2004.*

[13] E. Spyrakos-Papastavridis, J. S. Dai, "Minimally Model-based Trajectory Tracking and Variable Impedance Control of Flexible-Joint Robots," *IEEE Transactions on Industrial Electronics, vol. 68, no. 7, pp. 6031-6041, 2021.*

[14] E. Spyrakos-Papastavridis, Z. Fu, J. S. Dai, "Power-Shaping Model-based Control with Feedback Deactivation for Flexible-Joint Robot Interaction," *IEEE Robotics and Automation Letters, vol. 7, no. 2, 2022.*

[15] E. Spyrakos-Papastavridis, J. S. Dai, "Stable Flexible-Joint Floating-base Robot Balancing and Locomotion via Variable Impedance Control," *IEEE Transactions on Industrial Electronics, vol. 70, no. 3, pp. 2748-2758, 2022.*

[16] E. Spyrakos-Papastavridis, J. S. Dai, "Flexible-Joint Humanoid Balancing Augmentation via Full-State Feedback Variable Impedance Control," *ASME Journal of Mechanisms and Robotics, vol. 13, 021014, 2021.*

[17] E. Spyrakos-Papastavridis, J. S. Dai, "A Model-Free Solution for Stable Balancing and Locomotion of Floating-base Legged Systems," *IEEE/RSJ International Conference on Intelligent Robots and Systems, pp. 3816-3822, 2020.*

[18] E. Spyrakos-Papastavridis, J. S. Dai, "A Variable Impedance Scheme based on Power-Shaping Signals and Partial Knowledge of Link-Side Dynamics for Flexible-Joint Robot Interaction and Tracking Control" *IEEE/ASME Transactions on Mechatronics*, *vol. 29, no. 1, pp. 588-601, 2024.*

[19] F. J. Abu-Dakka, M. Saveriano, "Variable Impedance Control and Learning—A Review," *Frontiers in Robotics and AI, vol. 7, 2020.*

[20] Y. Michel, C. Ott, D. Lee, "Safety-Aware Hierarchical Passivity-based Variable Impedance Control for Redundant Manipulators," *IEEE Transactions on Robotics, DOI: 10.1109/TRO.2022.3174478, 2022.*

[21] I. Barkana, "Defending the Beauty of the Invariance Principle," *International Journal of Control, vol. 87, no. 1, pp. 186-206, 2014.*

[22] Y. Orlov, "Extended Invariance Principle for Nonautonomous Switched Systems," *IEEE Transactions on Automatic Control, vol. 48, no. 8, pp. 1448-1452, 2003.*

[23] N. Fischer, R. Kamalapurkar, W. E. Dixon, "LaSalle-Yoshizawa Corollaries for Nonsmooth Systems," *IEEE Transactions on Automatic Control, vol. 58, no. 9, pp. 2333-2338, 2013.*

[24] R. Penrose, "A generalized inverse for matrices*," Proceedings of the Cambridge Philosophical Society*, vol. 51, no. 3, pp. 406-413, 1955.

[25] E. Spyrakos-Papastavridis, J. S. Dai, P. R. N. Childs, N. G. Tsagarakis, "Closure to "Discussion: Selective-Compliance-Based Lagrange Model and Multilevel Noncollocated Feedback Control of a Humanoid Robot"," *ASME Journal of Mechanisms and Robotics, vol. 14, no. 5, 056001, 2022.*

[26] J. E. Slotine, W. Li, "On the adaptive control of robot manipulators," *The International Journal of Robotics Research, vol. 6, pp. 49-59, 1987.*

[27] E. Lutscher, E. C. Dean-Leon, and G. Cheng, "Hierarchical force and positioning task specification for indirect Force-Controlled robots," *IEEE Trans. Robot., vol. 34, no. 1, pp. 280–286, 2018.*

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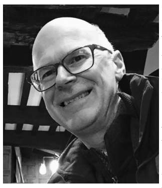

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