Open letter[1]

to

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I would like to reply in a public way to your message that answered two of my previous messages (cited below). I hope that your answer, as well as my reply, would be useful for those people who became acquainted with your triality theory and, maybe, with the counter-examples to several triality results given by myself and two of my collaborators.

Z2G 06Oct2012:

To David Yang Gao and Changzhi Wu:

In your recent paper [GW] "On the triality theory for a quartic polynomial optimization problem" appeared in JIMO (doi:10.3934/jimo.2012.8.229) you assert:

"The so-called counter- examples constructed recently (see, for example, [13, 14]) actually address the same type of open problem for the double-min duality left unaddressed by Gao in 2003 [3]."

Please, explain why the counterexamples in [13, 14] are "so-called".

Also please cite the text in [3] containing the "open problem for the double-min duality".

It seems to me that [13] refers to a paper appeared before 2003 (more precisely, 1989), while [14] refers mainly to papers published after 2003 (more precisely, 2000, 2004, 2006, 2009).

So, for the papers published before 2003 you had to mention in [3] that those results are wrong, while in the papers published after 2003 you had to take into account your "open problem" and to not continue to publish wrong results.

Moreover, in the version of [GW] available on arxiv (arXiv:1110.0293v1 [math.OC] 3 Oct 2011) you say:

"On the other hand, a large number of "counterexamples" have been presented in several papers since 2010. Unfortunately, most of these counterexamples are either fundamentally wrong (see [30, 31]), or repeatedly address an open problem left by Gao in 2003 on the double-min duality [9, 10]."

I wish to know why "these counterexamples are fundamentally wrong"? Additionally, why do you not mention [10] in [GW]?

I hope to have an answer in a reasonable lap of time.

Z2G 07Oct2012:

To David Yang Gao:

Sir,

Incidently, today I found the web page `http://uob-community.ballarat.edu.au/~dgao/DualityCoffee.html` where my name is mentioned several times.

You say "Suggestions and comments from colleagues are sincerely welcome", but it seems that you are drinking your coffee solely.

I do not observe to be a way to write comments on what you are presenting.

[1] See the addendum on page 16.

I wish just to cite from your "discussion" the following text "In Gao and <http://en.wikipedia.org/wiki/Gilbert_Strang>Strang's paper, the external energy functional F(u) must be linear in order to obey conservation law and the internal energy functional W(e) must be objective (at least quadratic) in order to store energy", and to comment it shortly:

Are these hypotheses mentioned Gao and <

PS Please answer to this mail as well as to the mail sent to you yesterday. Without having answers to these emails I shall forward them (and any other mail sent to you) to all your colleagues.

G2Z 08Oct2012:

Reply To C. Zalinescu

I hope you now know why your counterexamples are "so-called" and are fundamentally wrong. But I need to give you further explanations so you can understand what is the canonical duality.

The "so-called" means that those "counterexamples" can be corrected mathematically and easily since they are related to the open problem I left in 2003.

I have told you before my these papers and if you are honest, you should cite at least one of them in your recent articles.

Now it is too late as this open problem has been solved.

I do have other mathematical mistakes and the corrections are important for people to see the intrinsic beauty.

The "fundamentally wrong" means your mistakes can't be corrected mathematically.

Those terminologies used in my works are basic concepts in continuum physics and are not hypotheses, just like the definitions of the open and closed sets in mathematics.

If you have ever read the first volume of the course by Landau and Lifshitz, you will understand what kind of mistakes you have made in your recent papers.

It is totally your freedom to send your emails to anyone you like, some of my colleagues already know who you are.

I copying (Bcc) this email to them as we are discussing your case for some times.

In the meantime, the following article by VI Arnold could be interested to you, especially the last sentence. http://pauli.uni-muenster.de/~munsteg/arnold.html

Enjoying!

DYG

• Before referring to your answer I wish to present a history of my triality experience.

In the year 2000 or 2001, I received the paper G00 [Gao, David Yang, Analytic solutions and triality theory for nonconvex and nonsmooth variational problems with applications. Nonlinear Anal. 42 (2000), no. 7, Ser. A: Theory Methods, 1161–1193] for a Zentralblatt für Mathematik review (which I had to do). I did not understand too much, but I was intrigued by the kind of results presented there. Because I had no time to read it attentively, I cited from the motivation of the paper; see www.math.uaic.ro/~zalinesc/papers3.php?file=zbl-gao-na.png. I proposed myself to come back to this subject later.

In 2006 I proposed my former student R. Strugariu to study your theory and indicated him several of your papers. After some time, he said that practically he is not able to understand what is done there. In this context we (yourself and I) had a change of messages as follows:

Z2G 02Apr2007:

In your paper D.Y. Gao, Canonical dual transformation method and generalized triality theory in nonsmooth global optimization, J. Global Optim. 17 (2000), 127–160, you refer

several times to Gao (1999), but in the reference list one can find Gao, D.Y. (1999a) and Gao, D.Y. (1999b). Which one is the right reference?

G2Z 02Apr2007:

Thank you for your message and your interests in my paper.

I checked my paper, the reference Gao (1999) cited in the paper is mainly for Gao, D.Y. (1999a) which is the book I published by Kluwer. The second edition will appear in 2008 by Springer.

By the way, attached is one of my recent papers which could be also interested to you.

Wbox-proof.pdf

Z2G 27May2008:

Some time ago I bought your book mentioned below and I began to read it.

I observe that there are several misprints and some statements or proofs which are not very clear for me.

Have you an erratum for this book?

G2Z 27May2008:

Thank you for your message and interesting in my work.

Yes, there are many typos and misprints in my book.

The publisher asked me to have second edition since the first one sold out.

The canonical duality theory now has many applications in analysis and optimization.

Some recent papers are posted on my web page.

As you can see that some nonconvex variational/boundary value problems can be solved to have analytic solutions.

Please kindly let me know your comments and suggestions so I can put in the second edition.

Z2G 28May2008:

Of course, I asked you about the errata in order to concentrate only on those misprints or flaws which you didn't observe.

I mention only the following (integrate the text in a latex file):

p.86, Th. 2.6.4: Of course, (2.99) means that $(\overline{q},\overline{p})$ is a super-critical point of $L.$ Why $\sup_{p\in\mathcal{E}_{a}^{\ast}% }L(q,p)=L(q,\overline{p})?$ What we know is that $\sup_{p\in\mathcal{E}% _{a}^{\ast}}L(q,p)\geq L(q,\overline{p}).$ If $\sup_{p\in\mathcal{E}_{a}% ^{\ast}}L(q,p)=L(q,\overline{p}),$ as $L(q,\overline{p})\leq L(\overline {q},\overline{p})$ (by 2.85) we obtain that $L(q,p)\leq L(\overline {q},\overline{p})$ for all $(q,p),$ that is, $(\overline{q},\overline{p})$ is a super-maximum. This is obviously false. In the second part it is obtained that $\Pi^{d}(p)\geq\Pi^{d}(\overline{p}),$ while in the first part was obtained that $\Pi(q)\leq\Pi(\overline{q}),$ even if the hypothesis is symmetric w.r.t. p and $q.$

p.86, Th. 2.6.5: The proof is based on the preceding result (even assuming the conclusion of Th. 2.6.4 is true, the proof is not clear).

G2Z 28May2008:

Thank you very much for your message.

>p.86, Th. 2.6.4: Of course, (2.99) means that $(\overline{q},\overline{p})$ is

>a super-critical point of $L.$ Why $\sup_{p\in\mathcal{E}_{a}^{\ast}%

>}L(q,p)=L(q,\overline{p})?$

>What we know is that $\sup_{p\in\mathcal{E}%
>_{a}^{\ast}}L(q,p)\geq L(q,\overline{p}).$
This holds only on $q \in \mathcal{U}_k$
>If $\sup_{p\in\mathcal{E}_{a}^{\ast}}L(q,p)=L(q,\overline{p}),$ as $L(q,\overline{p})\leq L(\overline{q},\overline{p})$ (by 2.85) we obtain that $L(q,p)\leq L(\overline{q},\overline{p})$ for all $(q,p),$ that is, $(\overline{q},\overline{p})$ is
>a super-maximum. This is obviously false.
$(\overline{q},\overline{p})$ is a super-maximum on $\cal{U}_a \times \cal{E}^*_a$
not on $\cal{U}_k \times \cal{S}_s$
The admissible spaces $\cal{U}_k$ and $ \cal{S}_s$ are fundamentally important in non-convex analysis.
>In the second part it is obtained
>that $\Pi^{d}(p)\geq\Pi^{d}(\overline{p}),$ while in the first part was
>obtained that $\Pi(q)\leq\Pi(\overline{q}),$ even if the hypothesis is
>symmetric w.r.t. p and $q.$
You are right, there is a mistake here, i.e., instead of $\geq$,
$\Pi^{d}(p)\leq\Pi^{d}(\overline{p}),$
Please feel free to let me know if you have any other questions.
I am preparing the second edition.
Your careful reading and comments are sincerely appreciated.

G2Z 11Nov2008:
Attached is a new paper just published, which could be interested to you.
By the way, if you have a book project, I will be more than happy to recommend for the Springer Advances in Mechanics and Mathematics `http://www.springer.com/series/5613`
QJMAM-Gao-Ogden.pdf

Z2G 19Nov2008:
Thank you for the paper.
Recently I came back on your book "Duality Principles in Nonconvex Systems: Theory, Methods and Applications".
On page 242 you consider the closed linear operator Λ. I suppose that this is also the case in Th. 5.1.17 because no additional assumptions are made there on Λ; am I wrong?
I ask this because I know the conclusion of Th. 5.1.17 for Λ having its domain the entire space ($\cal{U}$).

Z2G 27Nov2008:
As I said you recently, I came back on reading your book.
In a message on May 28 (2008) I mentioned Th. 2.6.5. Are you so kind to send me a (more) detailed proof of this theorem? One says that one uses the super-critical point theorem (this seems to be Th. 2.6.4), the super-Lagrangian theorem (I didn't identify it; probably giving also the number could be useful) and Theorem 2.6.4. In fact in Theorem 2.6.4 one assumes that Π and Π^d are Gateaux differentiable at $\bar q$ and $\bar p$, respectively, assumption which is not made in Theorem 2.6.5.
I hope to receive your answer.

Z2G 10Aug2009:
Probably you recall that I asked several questions about your book some time ago.

Your answers were not satisfactory for me, so I (and two other colleagues) continued to think about those problems; the result is the attached paper.

svz.pdf

G2Z 10Aug2009:

Thank you for your message and paper.

I am currently on travel and will be back to US nest week.

I will read your paper carefully and get back to you soon

G2Z 13Sep2009:

Thank you again for your message and the paper.

Sorry for this seriously delay in response due to very busy in travels, preparing for launching a new journal and a society of global optimization.

The questions raised in your papers are very important, which tell me that my writing should be more careful in order to avoid misunderstanding.

Also I have realized from my recent years activities in the mathematical communities that there is a big gap between mathematicians and engineers/scientists.

In this email, I only give briefly answers to your questions. Details will follow when I have more time.

1. The function L(x,y) in Proposition 1 is a indeed a Lagrangian form associated with a quadratic d.c programming primal problem. Since without input term (linear term), the solution must be trivial (i.e. no out put). Therefore, the primal function f(x) = \sup_y L(x,y) has to be defined on a feasible space U_k such that the primal problem is a mathematical problem. This requires that

\inf f(x) > -\infty \forall u \in U_k

or

\sup f(x) < + \infty \forall u \in U_k

Otherwise, the unconstrained primal problem does not make any physical sense! I think I discussed this with you before.

In geometrically linear dynamical systems (\Lambda is a linear differential operator), the d.c. function f(x) is called the total action. the double-min type duality has been studied by both J Toland and G. Auchmuty. However, both of them did not have restrictions on U_k therefore, difficulties such as \infty - \infty have to be handled as I indicated in my book (page 94).

Actually, the bi-duality was discovered first in geometrially nonlinear systems (nonlinear \Lambda).

When I was writing the book, I realized that this theory reveals a fundamental property for convex Hamilton systems: i.e. the double-min and double-max happen periodically.

Recently when I discussed this with Prof. Ivar Ekeland, he eventually understood that the least action principle is indeed a misnomer for convex Hamilton dynamics.

For quadratic d.c. optimization problems (in static systems), since \lambda does not depend on time, the double-min and double-max can not happen simultaneously. But for nonconvex d.c. optimization problems, the double-min and double-max could happen simultaneously if the primal problem has more than two critical points.

This leads to the triality theory.

2. Regarding Example 1 and Example 2.

Since the primal function is a quadratic d.c. function

f(x) = 1/2 < a, x>^2 || b ||^2 - 1/2 || x ||^2

therefore, for min-primal problem

\inf f(x) \forall x \in U_k

the feasible space U_k has to be a set such that on which the function f(x) is convex. Only on this set, the double-min duality is for sure true ! Otherwise, the primal problem does make any sense, i.e. \inf f(x) = - \infty

Similarly, for max-primal problem

\max f(x) \forall x \in U_k

the U_k is the set that f(x) should be concave. This leads to conditions for your given parameters a and b .

The misunderstanding is that in Example, you let U_k = X

This is wrong.

3. Regarding the nonlinear operator \Lambda and Example 3.

Geometrically nonlinear system is well-defined in continuum mechanics. As I indicated in many my papers and book, choosing the right geometrically nonlinear operator is the key to solve nonconvex problems, which is one of three components of the canonical duality theory (canonical dual transformation, complementary-dual principle, and the triality theory).

In mathematical physics, \Lambda(u) is a geometrical measure, or strain, which has to satisfy certain rules.

Section 6.3 in my book discussed certain well-known canonical strain measures. Whence this Gateaux differentiable operator is chosen correctly, the variation of the primal function leads to the virtual work principle in geometrically nonlinear system (Equation 17 in Gao and Strang's paper).

Also, the objective function P(x) in optimization, or the total potential (action) P(u) in nonconvex system theory, has to satisfies certain fundamental rules, including the Objectivity and Isotropy.

Objectivity is also refereed as /material frame indifference /in continuum physics, which is a fundamental law to All nature systems.

Section 6.1 in my book discussed briefly these rules.

However, the function defined in your Example 3 is not objective and I don't understand the associated optimization problem. Therefore, the canonical dual transform may not apply.

4. Example 4. The function \Xi (u, v) = - 1/2 v^2 + 1/2 u^2 - u^4 is neither a Lagrangian form not the total complementary energy. the canonical measure Lambda(u) can not be zero for any given system!

5. Example 5 and 6.

Again, please think a meaningful primal and dual problems.

6. Examples 7,8,9, 10, 11.

Again, the functions listed in these examples are neither Lagrangian forms or the total complementary function as required by the canonical duality theory.

The canonical duality theory is developed from reality and reveals natural behaviors. I understand that many mathematicians may have difficulties to understand this theory due to my writing style and my way thinking. Many of my mathematician friends told me these and a group of mathematicians in Tsinghua University are working on a "mathematical version of the canonical duality theory" by translating my book in their language.

Like you all, they confused and argued with me at the beginning. However, whence they understood, they all are very happy to see the magic of this theory, including Professors Ivar Ekeland and Ray Ogden.

We are having more very exciting results coming out.

Attached is a new paper on the canonical duality in global optimization. One of main results of the triality theory is Theorem 7, which shows that the NP-hard integer programming is equivalent to an unconstrained dual problem in continuous space.

I am now working with two computer scientists on developing a deterministic method for solving this dual problem.

Recently my this research project has received several big grants from NSF and US Airforce, and I hope you can visit me some day so we can have good discussion.

Please feel free to let me know if you have any question.

Thank you all again for good questions.

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• After reading your answers to my messages, I concluded that, in your opinion, in your book there are only "typos and misprints". Moreover, Springer asked you to publish a new edition (see G2Z 02Apr2007, G2Z 27May2008, G2Z 28May2008); by the way, did you publish the second edition of your book? Furthermore, your answers were not satisfactory (for me); maybe I am wrong, but the readers of this text will be able to judge themselves and reach a conclusion. Instead of giving convincing arguments you use the name of well known mathematicians such as Professors Ivar Ekeland and Raymond Ogden to testify that your theory is "magic". (Indeed, your theory is magic because it is not based on usual logic.) We (M. Voisei and I) followed your hint and asked their opinion on the subject considered in four of our papers (those we had at that time) containing counterexamples to your results. I. Ekeland agreed with our conclusions (**"I think you are right in your counterexamples"**, 12Oct2009) while R. Ogden answered **"I don't have time to look at these papers"** (which is quite surprising because you have at least two papers written together containing triality results; see also P8 and P9 below).

As a result of studying your book and papers I wrote (solely or together with two collaborators) the following papers (ordered by submission date):

P1. *A counter-example to "Minimal distance between two nonconvex surfaces"*, Optimization, `http://www.tandfonline.com/doi/abs/10.1080/02331930903531535#preview`, submitted 28.06.2009, first report - 14.11.2009 (refers to a paper published in Optimization, 2008).

P2. *Counter-examples in bi-duality, triality and tri-duality*, DCDS-A, `http://www.aimsciences.org/journals/pdfs.jsp?paperID=6521&mode=full`, submitted 29.06.2009, first report - 06.07.2009 (refers mainly to the book (2000) and a paper published in a volume, 2009).

P3. *A counterexample to a triality theorem in "Canonical dual least square method"*, COAP, `http://www.springerlink.com/content/n314j87mv18311x8/`, submitted 07.09.2009, first report - 05.10.2009 (refers to a paper published in COAP, 2008).

P4. *Counterexamples to a triality theorem for quadratic-exponential minimization problems*, MMOR, submitted 11.09.2009, first report - 15.04.2011, not yet final decision (refers, mainly, to a paper published in MMOR, 2008).

P5. *On three duality results*, submitted to JIMO on 24.09.2009, first report - 17.05.2010 (refers to papers published in JIMO, 2007, 2008, 2009); rejected. See also `http://arxiv.org/abs/1008.4329` and `http://www.math.uaic.ro/~zalinesc/papers3.php?file=reports-answers.pdf`.

P6. *Some remarks concerning Gao–Strang's complementary gap function*, Applicable Analysis, `http://www.tandfonline.com/doi/abs/10.1080/00036811.2010.483427#preview`,

submitted 31.12.2009, first report - 28.01.2010 (refers to a paper published in QAM, 1989)

P7. *Counterexamples to some triality and tri-duality results*, JOGO, http://rd.springer.com/article/10.1007/s10898-010-9592-y, submitted 23.10.2009, first report - 06.05.2010 (refers to papers published in JOGO, 2000, 2004, 2006, 2009).

P8. *On a result about global minimizers and local extrema in phase transition*, QJMAM, submitted 26.02.2010, first report - 19.10.2010 (refers to a paper published in QJMAM, 2008); rejected. See also http://arxiv.org/abs/1101.3534 and http://www.math.uaic.ro/~zalinesc/papers3.php?file=qjmam-answers.pdf.

P9. *A non-convex variational problem appearing in a large deformation elasticity problem*, ZAMP, submitted 13.04.2010, first report - 08.12.2010 (refers to a paper published in ZAMP, 2008); rejected. See also http://arxiv.org/abs/1202.3515 and http://www.math.uaic.ro/~zalinesc/papers3.php?file=zamp-ans.pdf.

P10. *On the paper "A study on concave optimization via canonical dual function"*, JCAM, submitted 08.04.2010, first report - 27.08.2011 (refers to a paper published in JCAM, 2009); rejected. See also http://arxiv.org/abs/1112.2130 and http://www.math.uaic.ro/~zalinesc/papers3.php?file=jcam-ans.pdf.

P11. *On two triality results*, Optimization and Engineering, http://link.springer.com/article/10.1007%2Fs11081-010-9134-y, submitted 08.04.2010, first report - 08.12.2010 (refers to 2 papers published in OE, 2009).

• In one of your papers you say: *The triality theory has been challenged recently by Voisei and Zalinescu in a series of more than seven papers either published or submitted. ... As a reviewer for most of these papers, the first author of the current paper indicated in his referees reports that these counter-examples are not new but simply address the same type open problem for the double-min duality left in 2003 [13].*

Of course, I think it is normal for the authors of a criticized paper to be made aware of the existence of a critical paper, but the final decision concerning that critical paper should not be based entirely on the opinion of those authors. It seems that you are quite influent (and that is easily understood because, as seen from your web page http://crnballarat.com/elements/element-1/bios/david-yang-gao/, you are:

"Editorships

1. Editor-in-Chief: Encyclopedia of Duality in Engineering Science, Springer, 2006-

2. Co-Editor-in-Chief of Book Series:

(1) Advances in Mechanics and Mathematics (AMMA) (with Ray W. Ogden), Springer, 2002-present

(2) Modern Mechanics and Mathematic (MMM) (with Martin Ostoja-Starzewski) Taylor & Francis, 2002-present

(3) Optimization and Control of Complex Systems (with H.D. Sherali), Taylor & Francis, 2008-present

Member of Editorial Board:

(1) Mathematics and Mechanics of Solids, SAGE Press, 2011-present

(2) Advances in Material and Mechanics, Springer/High Education Publisher, 2010-

(3) Numerical Algebra, Control and Optimization, AIMS Press, 2011-present

(4) Discrete and Continuous Dynamical Systems-B. AIMS Press, 2001-2011

(5) Journal of Global Optimization, Springer, 1999-present;

(6) Journal of Industrial and Management Optimization. AIMS Press, 2004-present

(7) Optimization Letters. Springer, 2005-present.
(8) Electronic Journal of Technology in Mathematics, 2006-present.").

In my opinion **you were a referee for the papers P3, P4, P5, P7, P8, P9**; probably you were a referee for P11, and one of your collaborators was a referee for P10.

Now I wish to discuss your answer **G2Z 08Oct2012** and some assertions from `http://uob-community.ballarat.edu.au/~dgao/DualityCoffee.html`.

Related to G2Z 08Oct2012:

You say: *The "so-called" means that those "counterexamples" can be corrected mathematically and easily since they are related to the open problem I left in 2003.*

I learned about the so called open problem formulated in [Gao, D.Y., Perfect duality theory and complete solutions to a class of global optimization problems, Optimization, 52 (2003), 467–493] from reports on the papers P4 (first report - 15.04.2011), P5 (first report - 17.05.2010), P7 (first report - 06.05.2010). In this paper **you do not formulate any open problem**; and this can be easily checked by anyone who reads your paper. In that paper you refer several (three) times to open problems, but those open problems, as you mention, were formulated by Ivar Ekeland. Moreover, in my message Z2G 07Oct2012 I asked you <please cite the text in [3] containing the "open problem for the double-min duality">, but you could not cite that text. Maybe, the term "open problem" has different meaning for you and me. You know that there are the so-called false friends (words that have the same spelling but different meaning in two languages). If you are speaking Chinese and I am speaking Romanian surely we don't understand each-other. Why do you publish in mathematical journals and not in physics journals (for example)?

I suppose that the part of Remark 1 in that paper (Remark 1 is indicated in one of your reports) is the following:

> 'In this case, the bi-duality theory developed in [17] proves the double max duality relation (38), as well as the double min duality relation
> $P(\overline{x}) = \min_{x\in\mathcal{X}_r} P(x) = \min_{y^*\in\mathcal{Y}_r^*} P^d(y^*) = P^d(\overline{y}^*)$, (40)
> under certain additional constraints.'

(The reference [17] mentioned above is your book.)

Probably you want to say that your results can be corrected mathematically. If so, **why did you publish wrong results after 2003**? In fact, normally, you would have had to take those "additional constraints" into consideration and write them explicitly in the subsequent papers, that is, in those published after 2003. The counterexamples given in my papers show that those "additional constraints", if used in the statements, are not adequate.

Related to the text "*... or repeatedly address an open problem left by Gao in 2003 on the double-min duality [9, 10]*" quoted in Z2G 06Oct2012 my opinion is that you are completely wrong: We do not "repeatedly address an open problem left by Gao in 2003" for the simple reason that there is not an open problem formulated by you in that paper, but just a remark that for having relation (40) one needs "certain additional constraints". In economics one uses an hypothesis (axiom) called suggestively "no free lunch". In fact, in our papers, we address these so-called "theorems" that are clearly stated in your papers (published after 2003).

You say: *I have told you before my these papers and if you are honest, you should cite at least one of them in your recent articles.*

It is not very clear, but it seems that you say that you told to me about your "open problem" before my papers on your results were written. And I am not honest because I did not cite your 2003 paper in any of my papers. As seen above I learned the first time about your "open problem" on 06.05.2010; did you send me other mails related to your papers than those mentioned at the beginning of this letter? At that moment (06.05.2010) all the 11 papers were already submitted; moreover, you recalled your "open problem" after reading my papers as a referee. So, it seems that **you are the person who is not honest**.

You say: *Now it is too late as this open problem has been solved.*

Please indicate the precise reference(s), or, even better, send me the paper(s) in which this open problem is solved. I shall read it (them) attentively.

One of the referees of the paper P4 (whose first reports were received on 15.04.2011) mentions the paper [D.Y. Gao and C.Z Wu: On the triality theory in global optimization]. The referee cites from the abstract: "The method is based on the elementary linear algebra but it solves an open problem on the double-min duality left seven years ago". When has this paper become public? I found the following on `http://arxiv.org/abs/1104.2970` : On the Triality Theory in Global Optimization, David Y. Gao, Changzhi Wu, (Submitted on 15 Apr 2011 (v1), last revised 20 Feb 2012 (this version, v2)). Did you solve your "open problem" in this paper? If yes, where was it published? Is it the paper from JIMO mentioned in Z2G 06Oct2012? As I said above, I shall read the paper where the "open problem" is solved attentively.

You say about me (see above) that I am not honest. I have to point out (once more) that **you are not honest**. This may be proved by the fact that **you delayed** as much as possible writing (submitting) your reports on my papers in such a way to pretend that you solved the so-called open problem, and this before my counter-examples could be published. Excepting the papers P1, P2, P6, I had to repeatedly ask about the status of the papers because I did not receive any answer for long periods of time.

You say: *The "fundamentally wrong" means your mistakes can't be corrected mathematically.*

Those terminologies used in my works are basic concepts in continuum physics and are not hypotheses, just like the definitions of the open and closed sets in mathematics.

If you have ever read the first volume of the course by Landau and Lifshitz, you will understand what kind of mistakes you have made in your recent papers.

Indeed, I did not read "the first volume of the course by Landau and Lifshitz" and I do not promise to read it. Of course, it would be good to know physics. Unfortunately, I did not read enough important courses in mathematics. I do not pretend to know PDE, Stochastics (even though I read some courses). There are lots of persons that live under the impression that they know a subject if they read a text on that subject. Why do you pretend to solve problems in "analysis and optimization", "nonconvex variational/boundary value problems"? (See G2Z 27May2008.)

You extrapolated some of your results (maybe correct in that context) from "continuum physics") to domains of mathematics. You should have remained with your results in physics and our trajectories would not have touched. By the way, you use mathematical notions as Gâteaux differentiability, but I am sure you do not understand that notion. To paraphrase you, had you ever read a first course in Analysis, you would have understood the kind of mistakes you make in many of your papers and book.

Assuming that the results in your book and in Gao–Strang's paper are correct under

the implicit hypotheses (which are included in the *basic concepts in continuum physics*), you should have got correct conclusions for the problem you study in [Gao, David Y.; Yang, Wei-Chi, Minimal distance between two non-convex surfaces. Optimization 57 (2008), no. 5, 705–714]. However, as shown in P1, the solution is not provided by the critical solutions of **the Gao–Strang total complementary function**, as you like to call the function Ξ.

You say: *It is totally your freedom to send your emails to anyone you like, some of my colleagues already know who you are. I copying (Bcc) this email to them as we are discussing your case for some times.*

I shall use this freedom of mine. I hope those colleagues of yours will be among the persons who will receive this letter.

And who am I? And who are you?

Every person dealing with mathematics can learn something about me. To ease this activity, I indicate my web page `http://www.math.uaic.ro/~zalinesc/`. One can also find me on Google scholar (see also `http://scholar.google.co.uk/citations?user=MVFC2agAAAAJ&hl=en`), and of course, on MathSciNet (see also `http://www.math.uaic.ro/~zalinesc/papers3.php?file=cz-mr-cit.png`) and Zentralblatt für Mathematik. I can add that it seems several people appreciate me because I read mathematics quite attentively. Unfortunately, I had to write some critical notes, generally, because, after writing to the corresponding authors, they did not admit they made mistakes (and you are one of those authors). However, even though I am not very proud with those papers, I consider them very useful for the mathematical community. For the health of mathematics it is important to point out the wrong results in the literature. In this sense on the web page `http://www.millersville.edu/~bikenaga/math-proof/counter/counter.html` one can read:

> 'A **counterexample** is an example that disproves a universal ("for all") statement. Obtaining counterexamples is a very important part of mathematics, because doing mathematics requires that you develop a *critical attitude* toward claims. When you have an idea or when someone tells you something, *test the idea* by trying examples. If you find a counterexample which shows that the idea is false, that's good: Progress comes not only through doing the right thing, but also by correcting your mistakes.'

What about you? From your web pages (`http://www.math.vt.edu/people/gao/`, `http://uob-community.ballarat.edu.au/~dgao/index.html`, `http://crnballarat.com/elements/element-1/bios/david-yang-gao/`) you give the impression to be among the greatest scientists in the world and an encyclopedic person. Probably it is disappointing for you (and for me) that you are the person that cites mostly your works (see for example `http://www.math.uaic.ro/~zalinesc/papers3.php?file=gao-mr-cit.png`).

You say: *In the meantime, the following article by VI Arnold could be interested to you, especially the last sentence.* `http://pauli.uni-muenster.de/~munsteg/arnold.html`.

Thank you for that link. It is an interesting text. However VI Arnold discusses about teaching mathematics. Your papers are not about teaching, but research papers solving mainly NP-hard problems. The key words in the paper [D. Y. Gao and H. D. Sherali, Canonical Duality Theory: Connections between nonconvex mechanics and global optimization] are: Duality, triality, Lagrangian duality, nonconvex mechanics, global optimization, nonconvex variations, canonical dual transformations, critical point theory, semi-linear equations, NP-hard problems, quadratic programming.

Related to `http://uob-community.ballarat.edu.au/~dgao/DualityCoffee.html` :

You say: *In Gao and Strang's paper, the external energy functional F(u) must be linear in order to obey conservation law and the internal energy functional W(e) must be objective (at least quadratic) in order to store energy.*

I mentioned this text in my message Z2G 07Oct2012, but you did not comment it. However, I cite from Gao-Strang's paper the following text (see page 497, lines 2–5):

> 'Let $\Pi : \mathcal{U} \times \mathcal{E} \to \overline{R}$ be the total superpotential
> $\Pi(v, \epsilon) := W(\epsilon) + F(v)$. (46)
> **Theorem 1** If $\Pi(v, A(v))$ is Gateaux-differentiable, the critical points of $\Pi(v, A(v))$ are the solutions of the governing equations (34).'

I know that the linear functionals (if continuous) and the quadratic functionals are real-valued and Fréchet (hence Gâteaux) differentiable (of any order). So, why do you take $\Pi : \mathcal{U} \times \mathcal{E} \to \overline{R}$ (envisaging so non finite values for Π)? Moreover, why do you ask $\Pi(v, A(v))$ to be *Gateaux-differentiable*?

In fact, this shows that, similar to your so-called "open problem" from Gao's 2003 key paper, you recalled (much time after the publication) that the functions F(u) and W(e) must verify "certain additional constraints".

You say: *Dually, in Voisei and Zalinescu's paper, they choose quadratic functions for F(u) and (piecewise) linear function for W(e) as counter-examples, which lead to their conclusions including: About the (complementary) gap function one can conclude that it is useless at least in the current context.*

Some common misunderstandings on certain important concepts, such as objective function, cost function, energy function, and energy functional, etc, will be discussed.

As I mentioned above, had you not extrapolated those results to non physics problems I would not have read those papers. You mention again that

Over the past 20 years consistent research, Gao and Strang's original work in finite deformation mechanics has been extended to general problems in nonconvex systems and a so-called canonical duality theory has been developed. Extensively applications of this theory in mathematics and sciences have shown that Gao and Strang's work provides a unified framework for a large class of nonconvex/nonsmooth/discrete problems in mathematical physics, global optimization, and complex systems, and the complementary gap function lays a foundation for the canonical duality theory and the associated triality theory [1].

So, you testify that you extended "Gao and Strang's original work in **finite deformation mechanics**, where "Those terminologies used in my works are basic concepts in continuum physics and are not hypotheses", to domains of mathematics, where "Those terminologies" are not basic, and so they become "hypotheses" which have to be mentioned explicitly.

You say: *The mathematical review was written by C.O. Horgan, who is an expert in both solid mechanics and applied mathematics, well-known in the community of engineering science;*

While C. Zalinescu is an expert in abstract analysis and (theoretical) optimization with reputation in finding people's mistakes and no mistakes and flaws in his own papers.

I apologize for not knowing who C.O. Horgan is, but I doubt that he read attentively your paper (one of my professors said once: Who does not read eulogizes). If he had really

been so enthusiastic about your paper he would have used it in his research. However it seems that is not the case (see http://www.math.uaic.ro/~zalinesc/papers3.php?file=mr-gao-strang.png).

However, what do you think about the opinion "the fact that the authors of [2] are far form being experts in convex analysis is obvious" of one of the referees of P6? ([2] is Gao-Strang's paper.)

Be sure that I am not hunting "people's mistakes". I have explained above my critical papers. Excepting those related to your results, my (critical) papers treat subjects in which I have my own results. And, unfortunately, I make mistakes, too (see the errata of my book Convex analysis in general vector spaces). By the way, why don't you have an errata for your book?

You say: *Gao and Strang's paper was published 20 years ago in Quart. Appl. Math., (ranked as an A-class applied math journal by ARC, and founded by W. Prager, a well-known applied mathematician in mechanics).*

While Voisei and Zalinescu's this paper is published recently in Applicable Analysis (ranked as a B-class pure math journal).

You are right. But G. Strang is a very well known (American; even more: from MIT) mathematician, while I am a much less known (Romanian) mathematician. You know very well that, for publishing a paper, it is important to have (very well) known persons among the authors; it is also important the institution the authors are coming from (and you exploited these facts). However, it is much more important that the results in the paper be used, correct, and useful than the place where the paper is published (Gao–Strang's paper is an example). I am convinced that G. Strang is not proud of this paper. In fact I think that you asked G. Strang to be the co-author of this paper, in order for this paper to be more convincing (mostly to convince the editors that your paper is a good one since it was authored by a very well known mathematician).

The experience with QJMAM (P8) and ZAMP (P9) papers shows that the probability that P8 be accepted to Quart. Appl. Math. was zero (even if the event was not an impossible one).

You say: *Duality exists in any system at each scale. In physics, each particle has an anti-particle. It seems to be true also in social and complex systems.*

And you found a duality between the review of Gao-Strang's paper and our article on Gao-Strang's paper similar to the duality beautiful & ugly. This is not a surprise because you discovered previously (and pointed out) many other dualities (see http://www.math.vt.edu/people/gao/art/index.html). Also you discovered "Triality in Nonconvex Analysis and Global Optimization (see below), Triality in Music, Triality in Religions, Triality in Science" (see http://www.math.vt.edu/people/gao/triality/triality.html).

You say: *local mistakes in Zălinescu and his co-workers will be ignored (since there are many, especially in the most recent paper published in DCDS-A, p. 1456, for example).*

Why ignore the mistakes of Zălinescu and his co-workers? I think that it is useful (and healthy) for the mathematical community to point out those mistakes.

You say: *As we know now that for the total potential energy P(u) = W(e(u)) + F(u) studied by Gao and Strang in [2], the stored energy W (e) must be an objective (at least quadratic) function and the external energy F(u) should be linear.*

I tried to find a word containing **objectiv** in Gao–Strang's paper without success.

You say: *In this case, the system is not conservative and traditional variational methods do not apply (see below).*

I tried to find a word containing **conserv** in Gao–Strang's paper without success.

You say: *So far, four of total five examples listed by Voisei and Zălinescu in [4] are (globally) opposite to the total potential studied by Gao and Strang. Apparently, they know how to use duality to find these counter-examples, but the target (function) is wrong. Therefore, their results could be useful for problems in imaginary world, but useless for problems in real world.*

So, you consider that what we (myself and my collaborators) do is *useful for problems in imaginary world*. I accept that this could be possible (true). I ask you to give concrete examples of use of your theory in real problems. If I recall well, in your talk at ICCOPT 07/MOPTA 07 (Hamilton, Canada, 2007), you mentioned that your results are applied by Boeing. Please, be more specific.

Because you are both a scientist and a mathematician, it is clear that you can write a version of your (Gao-Strang's) paper in the mathematical dialect.

So, **please translate Gao-Strang's 1989 paper in the mathematical dialect**, in such a way those people living in the imaginary world have, at least, a small idea about what is happening in the real world. Because by your *Open question:* $G \cap G^* = \emptyset$? $G + iG^* =$? you suggest the set of complex numbers, I wish to recall that the introduction of the imaginary numbers was very useful not only for mathematics, but also for "science". By the way, does "Open question" mean "Open problem"? I ask this because in the paper [J. Liu, D. Y. Gao, Y. Gao, Canonical duality for solving nonconvex and nonsmooth optimization problem, Optimization and Engineering 10 (2009), 153–165] you formulate two conjectures and give their proofs (for the second you say: "This theorem is a particular application of the general triality theory given in (Gao 1999, 2003a, 2004; Gao and Strang 1989)"); hence for you theorem and conjecture are synonym. By the way, did you develop a triality theory in Gao and Strang 1989?

In your papers you have statements of the types
$(\alpha = \beta) \iff (\gamma = \delta)$ and $(\alpha = \beta) \wedge (\gamma = \delta)$.
Of course the first proposition is much weaker than the proposition $(\alpha = \beta) \wedge (\gamma = \delta)$. But I am almost sure you do not see (understand) the difference (maybe in quantuum physics the previous propositions are synonyms, to not say equivalent).

I exemplify the above remark by the text below found in the paper (arXiv:1104.2970v1 [math.OC] 15 Apr 2011) (see pages 4 and 7, respectively):

> '**Theorem 3 (Triality Theorem [3])** Let $(\overline{x}, \overline{\varsigma})$ be a critical point of $\Xi(x, \varsigma)$.
> If $G(\overline{\varsigma}) \succeq 0$, then $\overline{\varsigma}$ is a global maximizer of Problem $(\mathcal{P}^d)$, the vector $\overline{x}$ is a global minimizer of Problem $(\mathcal{P})$, and the following canonical min-max duality statement holds
> $\min_{x \in \mathcal{X}_a} \Pi(x) = \Xi(\overline{x}, \overline{\varsigma}) = \max_{\varsigma \in \mathcal{S}_a^+} \Pi^d(\varsigma)$. (17)'

and

> 'In this case, the Triality Theorem 3 holds in its strong form.
> **Theorem 4 (Tri-Duality Theorem)** Suppose that $\overline{\varsigma}$ is a critical point of the canonical problem $(\mathcal{P}^d)$ and $\overline{x} = [G(\overline{\varsigma})]^{-1} f$.

If $\overline{\varsigma} \in \mathcal{S}_a^+$, then $\overline{\varsigma}$ is a global maximizer of Problem $(\mathcal{P}^d)$ in $\mathcal{S}_a^+$ if and only if $\overline{x}$ is a global minimizer of Problem $(\mathcal{P})$, i.e., the following canonical min-max statement holds:

$\Pi(\overline{x}) = \min_{x\in\mathbb{R}^n} \Pi(x) \Longleftrightarrow \max_{\varsigma\in\mathcal{S}_a^+} \Pi^d(x) = \Pi^d(\overline{x})$. (27)'

Last remarks.

a) In the paper mentioned above (and in many others papers) you say *The notation sta*$\{^*\}$ *stands for solving the stationary point problem in* $\{^*\}$ (see page 2), and you consider (some lines below)

"$\Pi^d(\varsigma) = sta\{(x,\varsigma) \mid \forall x \in \mathcal{X}_a\} = F^\Lambda(\varsigma) - V^*(\varsigma)$, (5)".

On the next page you find that

"$\Pi^d(\varsigma) = -\frac{1}{2}\left\langle G(\varsigma)^{-1}f, f\right\rangle - V^*(\varsigma)$. (13)"

In my imaginary world an action (of *solving a stationary point problem*) cannot be equal to a real number, but in your real world this is possible, probably using the *Conservativity and Objectivity* principles.

b) In the paper arXiv:1104.2970v2 [math.OC] 20 Feb 2012 (`http://arxiv.org/abs/1104.2970v2`, which you declare to be the second version of arXiv:1104.2970v1 [math.OC] 15 Apr 2011, even if it has another title and many other differences besides the introduction of a new section) you introduce the set $\mathcal{V}_a$ defined by $\mathcal{V}_a = \{\xi \in \mathbb{R}^m | \xi = \Lambda(x)\ \forall x \in \mathbb{R}^n\}$. In *my imaginary world* the set $\mathcal{V}_a$ is empty excepting the case in which Λ is constant. I suppose that you had the intention to take for $\mathcal{V}_a$ the set $\{\Lambda(x) \mid x \in \mathbb{R}^n\}$ $[= \{\xi \in \mathbb{R}^m | \exists x \in \mathbb{R}^n : \xi = \Lambda(x)\}]$, that is the image of Λ. But in *your real world* the quantifiers $\exists$ and $\forall$ do coincide!

Probably the paper arXiv:1104.2970v1 is that indicated by a referee as being the paper where you solved your "open problem". As I asked above, please indicate the precise reference for the published paper (having a DOI) containing the solution of your (so-called) open problem (to read it attentively).

I do not intend to continue to discuss with you. It seems that we do not speak the same (mathematical) language, even if we use many, many common words. I encourage you to write in non-mathematical journals. Probably among physicists you are considered a very good (if not the greatest) mathematician of our times and among mathematicians you are considered (maybe, but I doubt) a very good physicist.

October 29th, 2012
Constantin Zălinescu

Addendum (September 26th, 2014)

After making public my open letter to you on October 29th, 2012, besides [WP0] `http://uob-community.ballarat.edu.au/~dgao/DualityCoffee.html`, to which I referred in that letter, you made public several texts trying to prove that myself and my collaborators are doing mistakes in our counter-examples, and myself I am not honest. I exemplify by the following links:

[WP1] `http://uob-community.ballarat.edu.au/~dgao/Lecture1.html`
[WP2] `http://uob-community.ballarat.edu.au/~dgao/Cafe.html`
[WP3] `http://uob-community.ballarat.edu.au/~dgao/Lecture7.html`
(accessible until March 2014),
[WP4] `http://www.isogop.org/organization/david-y-gao/triality`
[WP5] `http://www.isogop.org/organization/david-y-gao/canonical-duality`
[WP6] `http://www.isogop.org/organization/david-y-gao/super-duality-triality`
(accessible until September 1st, 2014),
[WP7] `http://sitevm1.ballarat.edu.au/dgao/Lecture1.html`
[WP8] `http://sitevm1.ballarat.edu.au/dgao/Cafe.html`
[WP9] `http://sitevm1.ballarat.edu.au/dgao/Lecture7.html`
[WP10] `http://sitevm1.ballarat.edu.au/dgao/DualityCoffee.html`
(accessible now, September 26th, 2014).
I mention that I have copies of all these web-pages.

Moreover, in several of your talks you revealed "Some fundamental and conceptual mistakes in recent papers by C. Zalinescu and his co-workers".

Of course, you are free to try to convince that what you published is correct. The attentive readers can judge by themselves if you are right or not. However you confessed in [WP6] "*Actually, I even forgot my this problem left in 2003 [1,2] due to busy life during those years*". The normal behavior of a scientist is to accept when he/her did mistakes and to try to correct the corresponding results.

In the sequel I don't try to convince you to accept that what I published concerning your results is correct. Somewhat related to this aspect I quote from [E. T. Jaynes (1967), Foundations of Probability Theory and Statistical Mechanics]:

> "You cannot base a general mathematical theory on imprecisely defined concepts. You can make some progress that way; but sooner or later the theory is bound to dissolve in ambiguities which prevent you from extending it further. Failure to recognize this fact has another unfortunate consequence which is, in a practical sense, even more disastrous : Unless the conceptual problems of a field have been clearly resolved, you cannot say which mathematical problems are the relevant ones worth working on; and your efforts are more than likely to be wasted."

In this sense see your definitions for *objectivity;* I traced a history of objectivity in your works in `http://www.math.uaic.ro/~zalinesc/papers3.php?file=submit-1012268.pdf`.

My main aim (objective?) in this Addendum is to discuss (quite briefly) about honesty based on what you wrote on your web-pages.

In [WP4] and [WP10] one can read, respectively:

"1) Copying Gao's published idea/results without citing his papers",

"1) Copying other people published idea/results without citing the original papers".

Your argument for this allegations is found in the text "*Unfortunately, their counterexamples are not new, which were first discovered by Gao in 2003 [1,2], i.e. the double-min duality holds under certain additional condition (see Remark 1 on page 481 [1])*".

My rapid answer to this is:

On one hand, it is true that we copied results from your works in all those 11 papers; in fact we quoted those results in order to make the reading easier. So the readers have the possibility to verify that our examples are effectively counter-examples. Of course, the sources of all those quotations were mentioned.

Even more, I copied (in fact I quoted) from your messages and your (initial) web-page [WP0] in my open letter to you (see the first part of this text).

On the other hand, I don't see to which idea you refer. The idea to give counter-examples can not be found in your papers even if you sustain that "*their counterexamples are not new, which were first discovered by Gao in 2003 [1,2]*". There are no examples in "Gao 2003 [1,2]"which might be considered counter-examples for a certain result (of you or another scientist)! But **in your opinion, the fact that a result needs "some additional conditions" represents not only an open problem, but also a counter-example**!

The only correction I have to do with respect to what I said in my open letter refer to:

"P4. *Counterexamples to a triality theorem for quadratic-exponential minimization problems*, MMOR, submitted 11.09.2009, first report - 15.04.2011, not yet final decision (refers, mainly, to a paper published in MMOR, 2008)."

In fact I didn't mention that the first reports (received on 15.04.2011) were included in the EiC's rejection message:

"From: "Sven Leyffer" <leyffer@mcs.anl.gov>

To: "Constantin Zalinescu" <zalinesc@uaic.ro>

Subject: MMOR: Your manuscript entitled Counterexamples to a triality theorem for quadrat

Ref.: Ms. No. MMOR-D-09-00165

Counterexamples to a triality theorem for quadratic-exponential minimization problems

Mathematical Methods of Operations Research

Dear Dr. Zalinescu,

Reviewers' comments on your work have now been received. You will see that they are advising against publication of your work. Therefore I must reject the manuscript as is but I encourage you to resubmit the manuscript.

.....

Reviewer #1: The authors indicated in the beginning that the main goal of this note is to give a counterexample to the Triality Theorem in [2]. In fact, the authors spent more than a half of this paper (from page one to page 5, line 22) to prove the first two statements (14) and (16) in the triality theorem. From line 24 to line 32, the authors show that for one dimensional problems, if the Hessian H(y) of the dual function does not vanish, both statements (15) and (16) in the triality theorem also hold. By Example 1 and 2, the authors show that if the Hessian H(y) = 0, these two statements do not hold. But this reviewer doesn't think these two cases are counterexamples. The reason is very simple: if H(y) = 0, the critical point y is not a local extremum point. It is my understanding that the triality theorem provides only

sufficient extremality conditions for global and local extremum points. Therefore, the two examples are trivial and should not be considered as counterexamples to the triality theorem.

By Example 3, the authors show that for n > 1, the statement (16) may not hold. But this case has been already indicated in Gao's previous paper published in 2003. Since the authors have already published similar papers in other journals, the publication of this paper is not recommended.

...."

Compare the report of Reviewer #1 with your report `http://www.math.uaic.ro/~zalinesc/papers3.php?file=MMOR.pdf` provided by yourself. Because you made public your report, I make public our answer to your report (see `http://www.math.uaic.ro/~zalinesc/papers3.php?file=ans-rev1-mmor.pdf`).

As the MMOR's EiC suggested, we resubmitted the same manuscript on 27.04.2011 (see `http://www.math.uaic.ro/~zalinesc/papers3.php?file=mmor-04-01-2013.png`); it is quite strange the fact that one rejected the paper but it was suggested to resubmit it (without asking any change). If someone is interested I can provide the files approved for submission by me on 11/09/2009 and 27/04/2011, that is the manuscripts MMOR-D-09-00165 and MMOR-D-11-00075, respectively.

For this reason I mentioned "not yet final decision".

In fact you delayed submitting your report and you sent it to MMOR after you published on arxiv your manuscript `http://arxiv.org/abs/1104.2970v1`. So we see your honesty and morality in action! Delaying as much as possible your reports was the rule.

But your honesty and morality can be seen very well from the apotheosis of your Lecture 7 (see `http://www.math.uaic.ro/~zalinesc/papers3.php?file=dyg-lecture7-14-01-05.pdf`). The Notes included in this lecture are relevant:

"Notes:

1. The review report [MMOR] was written by Gao on Voisei and Zalinescu's paper P4, which was submitted on 27 April 2011 and published online on 22 Jan. 2013 as an Original Article. But the counterexample to the double-min duality was originally discovered by Gao in 2003 [1,2] and these two papers were not honestly cited in P4. Also, the conditions for the counterexample has been solved and posted online `http://arxiv.org/abs/1104.2970` on 15 April, 2011. Specifically, the triality theory has been proved for general quartic-exponential functions published online `http://arxiv.org/abs/1210.0180` on 30 Sep. 2012. Interested readers should contact with the editor of MMOR to understand why P4 can be published.

2. The review report [JOGO] was written by Gao, while the reports [JOGORP2] and [JOGORP3] were written by other experts chosen by JOGO editor (I don't know who wrote [JOGORP2] but [JOGORP3] was written by one of Zalinescu's close friends, who sent me for comments before he submitted to J. Global Optimization). As we can see that all these three reports indicated that the counterexample to the double-min duality was first discovered by Gao in his 2003 papers [2,3]. However, based on these reports, Voisei and Zalinescu deleted the second paragraph in their original submission [JOGO-09-1308] (i.e. a correction of this theory is impossible without falling into trivia.), but still refused to cite Gao's paper [1,2] in their revision [JOGO-VZ]."

– You say that "The review **report [MMOR]** was written **by Gao on** Voisei and Zalinescu's **paper P4**, which was **submitted on 27 April 2011**".

In this report (made public by yourself: http://uob-community.ballarat.edu.au/~dgao/MMOR.pdf, presently http://sitevm1.ballarat.edu.au/dgao/MMOR.pdf) it is written explicitly: "**Referee's Report on Manuscript Number: MMOR-D-09-00165**", while our **submission from 27 April 2011 has the reference number MMOR-D-11-00075**! I don't wish to explain again and again why we had no any motivation to cite your papers from 2003. But I point again that you rediscovered these papers of yours just after being acquainted with our works (see again your confession: "*Actually, I even forgot my this problem left in 2003 [1,2] due to busy life during those years*"). So, in the period 2003–2009 you continued to publish false results!

– I agree with your suggestion "Interested readers should contact with the editor of MMOR to understand why P4 can be published"; even more, the interested readers could ask why one needed so much time to take decisions for both submissions (see again http://www.math.uaic.ro/~zalinesc/papers3.php?file=mmor-04-01-2013.png).

– I wish to point out that by your high honesty you put online our paper in MMOR (http://uob-community.ballarat.edu.au/~dgao/MMOR-Published.pdf). Probably you have special rights because you are associate editor for some Springer journals as well as co-EiC for series of books at Springer. Of course, I am happy that you put online this paper; in this way several people can read freely the paper and to see not only our arguments and counter-examples, but also the acknowledgement where it is mentioned that the first submission was on 11/09/2009.

– Another proof for your honesty and morality is putting online not only your report [JOGO] (http://uob-community.ballarat.edu.au/~dgao/JOGO.pdf), but also the other two reports [JOGORP2] (http://uob-community.ballarat.edu.au/~dgao/JOGORP2.pdf) and [JOGORP3] (http://uob-community.ballarat.edu.au/~dgao/JOGORP.pdf, presently http://sitevm1.ballarat.edu.au/dgao/JOGORP.pdf). Because, probably, they are (or will be) no more available on your web-pages, they can be downloaded at the links http://www.math.uaic.ro/~zalinesc/papers3.php?file=JOGO.pdf, http://www.math.uaic.ro/~zalinesc/papers3.php?file=JOGORP2.pdf, http://www.math.uaic.ro/~zalinesc/papers3.php?file=JOGORP.pdf.

– You say that you received [JOGORP3] directly from its author (a close friend of mine), but how did you obtain [JOGORP2]? Note that we received [JOGORP3] included in the EiC's message. Moreover, those reports were confidential. In fact I am satisfied that you made public all the reports because in this way I feel free to make public our answer to these reports: http://www.math.uaic.ro/~zalinesc/papers3.php?file=answer-JOGO.pdf. I am not able to identify a friend of mine who could write [JOGORP3]. In fact our impression was that the referees have corroborated their reports since there are common ideas such as: our examples are not counter-examples since we have not considered other assumptions which may be specified elsewhere or we do not understand the triality theory, or that the counter-examples are not new etc.

– Thank you for putting online our initial submission [JOGO-09-1308] (http://uob-community.ballarat.edu.au/~dgao/JOGO-09-1308.pdf) as well as our JOGO paper (http://uob-community.ballarat.edu.au/~dgao/JOGO-VZ.pdf); see again http://www.math.uaic.ro/~zalinesc/papers3.php?file=dyg-lecture7-14-01-05.pdf.

I end this addendum observing that **you have very high standards of correctness, honesty and morality; congratulations!**

September 26th, 2014 C. Zălinescu