

Ariadne's thread for a scientific apparatus

Written component master thesis

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Introduction

*Quizá yo he creado las estrellas y el sol y la enorme casa,
pero ya no me acuerdo.*

Jorge Luis Borges (1947) La Casa de Asterión.

This written component of my thesis entitled ***Ariadne's thread for a scientific apparatus*** is a documentation of the process of submitting a proposal for a scientific research project to a funding agency. It is complemented with interviews to several academics of different disciplines and to science writers.

The proposal itself is not included in the master's work but all the documentation is. The reason is that the proposal itself is unimportant for the artistic work. Instead, my interest is on the factors that surround scientific practice and that determine not the progress of science but rather the "fate of a scientist".

By "fate" I mean the set of conscious, unconscious and subconscious choices that a scientist makes, first by believing on his/her freedom as an independent intellectual entity. However, and this is my working hypothesis, *the existence of such freedom is at least questionable if not illusory*. Second, a scientist makes choices by incorporating interests that go beyond any scientific relevance. Nevertheless, these choices ultimately affect not only the scientist in question but feedback in the scientific institution, in turn affecting other fellows. Although such feedback might be of positive or negative outcome to individual scientists as a person, it is unfortunate that they do not work in the direction of scientific progress. In my opinion and experience as a scientist myself, the acting elements that feedback on a scientist can steer the individual contribution even towards unscientific practices, some of which are even unethical but still accepted on the basis of other interests. These alternative interests are largely

elements that mediate individual or institutional power and thus find their own justification on otherwise justifiable metrics.

A classic example is academic publishing, benefiting from the results of the old *publish or perish* attitude. However, this is the old school notion of success and the motto is forgotten, as I explain in the following lines. The need for publishing has been an ideal market opportunity for big editorials such as Elsevier, Springer, Blackwell and others. These editorials have taken major economic advantage from the scientific needs. This exploitation, regardless of the related economical issues, has given the opportunity to scientists to ensure a relatively high rate of publication. In this way, “knowledge” has been democratized and made accessible to pretty much any scientist. Therefore, the metric has shifted because, as a necessary consequence of such democratization, there has been the mediocratization of science: most research papers are published in low-impact journals and contribute little to the pool of knowledge (but are still costly). It is a law of entropy increase: whilst the publishing machinery extracts resources, the environment increases its disorder by producing research that is largely noise.

Enter the publication metrics and indices: they come to claim the good science, to distinguish the good science from the bad science and from the ugly science. With this invention of publication metrics a hierarchy of the scientist comes along. Now we become measurable again, not by our productivity but by our impact. Such impact is highly correlated to the journal in which the paper is published and as such there are good journals, bad journals and ugly journals. The publishers thus strive to increase their impact factor because the scientists want to increase theirs.

As the metric shifts, knowledge has been steered from reporting findings and ideas to publish anything that is publishable. Consequently, publishing on high impact journals becomes a necessity. There is a reversion here: high impact journals, seeking to maintain their impact, anticipate what topics are of general

interest (and controversy) and facilitate the channels that favour such topics. In turn, scientists anticipate high impact journals with potentially fashionable topics in order to favour their publications.

Those scientists that manage to publish once and again on high impact journals become “established” personalities and “have a say”. It cannot be denied that this makes use of creativity and they are very skillful (maybe also in scientific matters). It might be naively thought that climbing to a good position on the scientific society on this way necessarily implies that the science behind is of high quality. But that is as untrue as to claim that a suit designed by, say, Dolce&Gabbana is of high quality. It might be true that said suit has an original and appealing design, that is expensive, that is impressive, etc. but that says nothing of its quality. In as much as in any other cultural traits are fashion-driven, so it is science.

But there are also external politics that carry those of publication further because, today, we are also beyond publication metrics. There are links to other aspects. For instance, scientific funding has become the real new parameter because it brings resources to the universities and institutions, which liberates them from the burden of having to cover the required resources. But as with high impact journals, there is a limited opportunity and not everybody can be awarded with good or prestigious grants such as those from the European Commission (H2020) and other regional funding agencies such as NERC in the UK, the DFG in Germany, the FWF in Austria, CNRS in France, NIH in the US, and so on.

There is nothing wrong, in principle, in funding science, and it is obvious that not everyone can be funded, just because of the numbers if nothing else. Also, given the low quality of most scientific output, not everything should be funded. The issue is that there is a bottleneck that vices the process: there are too many applications to review from the side of the agencies and the decision for funding them or not is based on peer reviews. Either the reviewers are a fixed panel, are

external, or both. When there is a fixed panel, this is usually between five and twenty “experts”. They have the burden to review sometimes thousands of applications and, obviously, it is not possible for them to do so in great detail. Measures to pre-filter are applied, and these are not at all based on any scientific quality (which cannot be assessed at said stage). These measures are neither indicated in the guidelines, nor can be formally declared. What tends to happen is exemplified on the email conversation cited below (page 13), but which goes as follows. Proposals tend to be ranked by the number of high impact publications, such as Nature, Science, PNAS, Cell, and others alike. This reduces the pile to a manageable amount, which is the one that ultimately is reviewed. Effectively, the rest of the proposals are seldom evaluated (or done a posteriori superficially), which means that in practice there are hidden eligibility standards of proposals. Thus, younger scientists that have innovative ideas have little chance of being awarded these grants, because these ideas are hardly publishable on high impact journals since they have low impact. One issue is that there is no metric for innovation, only subjective and cultural metrics of success, which are taken as impact predictors. Curiously, only 2nd or 3rd generation of “new ideas” make it to Nature and Science, surprisingly, and rarely these reference original sources. The result is that less innovative science is awarded preferentially, and these are the ones that in turn become the committees that decide on other’s proposals, reinforcing the hidden use of the metrics.

If these were explicit guidelines for proposal submission (which of course would be unacceptable) it would save time and money. Ultimately, the taxpayer is paying for the hundreds of hours that scientists allocate to write proposals that are rejected on bases that are not even justifiable. Admitting it would be of course a major scandal. But we all know that the road to scientific success is to have a Nature paper, which will enable grants and academic positions, and a lot of prestige!

Recently (May 2016) on a conversation with the director of a major and prestigious scientific institution in Vienna, he confessed that, as a frequent evaluator for FWF grants he was often instructed to find reasons to reject proposals, even if he considered them to be worthy of being awarded, because the FWF currently is in an economic crisis and cannot fund the number of projects it is supposed to. This is a scandal, but is the lesser scandal because it just evidences (once more) the pathetic incompetence of politics of a system that pretends to be competitive. I have similar reserves, in this case by experience, from the Deutsche Forschungsgemeinschaft (DFG). Said German organization is overly risk-averse and funds only agendas that promise gradual scientific advances as deliverables.

My first research question is: **what effect do those politics have on the scientist?**

The answer is straightforward. We have been shaping generations of scientists that fight for a successful career where the target is to fulfill expectations based on sociological matters (fashion about some scientific topics, publish or perish attitudes, peer admiration, etc.), gauged by metrics that do not reflect scientific importance or advance and who, by the new standard of “responsibility”(of bringing funding to the institution) do gradual scientific increase to ensure fundability. Also it is our responsibility as scientist to train other generations, and to ensure they have successful careers.

The second research question is: **what kind of science are we constructing?** I believe we are constructing a labyrinthine structure of knowledge that has not clear or defined tendency or trend, with foundations that may be robust but with an architecture that is nonfunctional and at best aesthetical.

The third research question is: want to know what is pathetic? Scientists fucking know all that; scientists fucking acknowledge it; **why then, scientists fucking submit to it and don't do fucking anything (most of them at least)?**

Motivated by this line of thought I have engaged in a less emotional analysis to understand the situation. This has double-fold objective: to steam-out the tension (for myself) and to become aware of the interceding factors that are at play on me, as a scientist. It is nearly impossible to avoid these factors whilst staying in science because these are passive psychosocial forces. From an artistic perspective, outside from the circle of science, I can at least look back and see what is at play and what is at stake, for me and for science, and feel free to make a strong critique without risking being punished for it.

On this activist, whistle-blowing, artistic research my first realization was to understand the scientific society as an apparatus, *sensu* Giorgio Agamben. That is, an intricate network of relations that mediate power and knowledge relations in response to a need.

In the context of the sociology of science, I define the apparatus as formed by the mechanisms that govern the democratization and mediocrization of science, in a way as exemplified above. These mechanisms act by shaping a scientific identity through the imposition of interests and conflicts of interests (e.g. need for securing an academic position and the practices that are implemented on order to reach it). This is a process of subjectification: the individual scientist develops a seemingly idiosyncratic mind by aligning its interests in favour of those of the apparatus, in parallel to, or instead of, scientific questions. The subjectification of the scientist occurs because s/he is a human being and as such finds its joy in securing a quality of living and on the recognition and respect from the rest of the scientific community for her/his success. This might easily be in contrast to adhering to the accuracy of science and a genuine respect to the scientific method to ensure scientific progress, because the latter are mental constructs and on their own do not ensure happiness. Therefore it is natural that the alignment with the scientific apparatus occurs, because this process results from the biological, psychological and anthropological instincts that determine human behaviour and culture.

The second realization was that, another of Agamben's theory, *Homo sacer*, could be largely mapped to the situation that science is undergoing. By now I avoid the precise analyses, but the line of argumentation, on my mind is the following. In this framework Agamben defines the *state of exception* as a state of emergency where governments take the liberty to go above the law for a presumably good of the sovereign. In this way measures that were illegal become now legitimated for a common good.

First we have to define what is the "law" behind science, so that we can talk of a state of exception. We consider science as the systematic study of natural phenomena, of its entailing laws, and the predictions we can make, as a result of the application of the scientific method.

Clearly there is not explicit law system of science, but there are minimum ethical standards that act, even though not structured as a constitution, as equivalent to a law system. Some of them are managed properly by punishment, such as plagiarism. However, there are more subtle "laws" that are violated, as when there is a premature communication of sensational discoveries without a proper analysis, either statistical or a deciding experimental design accounting for controls. It is striking that examples of these abound and are often authored by respectable personalities published on big journals. This is an explicit deviation from the scientific method in favour of sensationalism. I equate this as a breakage of the foundational law (scientific method and logics) and the fact that this is accepted, institutionalized and applauded reflects a fundamental corruption of the mind of scientists.

And with this, I alarmingly concluded that science is on a state of exception.

We have fed ourselves to the mouth of the minotaur.

A note on the methodology

I originally intended to distil information from interviews that I carried out to several scientists and science communicators. I completed eight interviews with European and American scientists. Some excerpts are included at the end of the thesis.

Despite that the interviews were interesting, it became clear rather quickly that the information I was getting was not fulfilling my purposes, except in some cases only from a very tangential slant, and are the excerpts included at the end. The presence of a camera, the formality of an interview, the fact that statements are being recorded and registered, presumably put the interviewees on a compromising situation, and thus limited themselves to politically correct approaches.

I realized then that a better approach was to dwell into my email archives and sometimes provoke private conversations on the subjects of interest. These have taken precedence on this thesis because, unlike the interviews, the emails directly address the three central research questions above. These correspondences have been only minimally edited.

I first summarize a timeline of events to orient the reading. Then, the documentation follows, which include archives and emails between officers and colleagues. At the end, the interviews excerpts are included. Note that these interviews are not intercalated chronologically with the rest of the documentation in the central exchange.

Timeline of events

October, 2013		Al and Isidora Krstic interview Herbert Edelsbrunner
April 16, 2014		Al and Ondrej Merta interview Jackeline Stark
March 31, 2015		Al Interviews Meehan Crist
April 7	15:19	Ariadne writes to Theseus and Al on peer review
	22:51	Theseus answers
April 8	7:58	Ariadne replies
	8:12	Al comments
	8:52	Ariadne replies
April 10	11:08	Al writes again; end of thread
		Al Interviews Paula Sabbloff
April 11		Al Interviews Rhittu Chatterjee
April 16		Al Interviews Eörs Szathmáry
June 1st		Al Interviews Dominique Zimmermann
June 6		Al Interviews Marcus Werner
June 20		Al writes cover letter for ERC application
June 25	16:29	Proposal is submitted
	17:02	Email to Ariadne informing submission is completed
June 26	9:19	Ariadne replies and invites to peer review a paper
	9:32	Al replies, agreeing joint reviewing
	9:35	Ariadne replies
	9:37	Al replies; end of thread
September 16	17:14	Al is invited to the interview and receives comments from the panel and anonymous reviewers
October 3		Al's birthday
October 21	8:30	Interview at the ERC offices in Belgium
October 29	18:30	Rejection notice arrives per email
	19:02	Al forwards notice to Ariadne; end of thread

November 14	14:17	Al sends a letter to the panel
	17:49	ERC officer replies to Al
November 15	10:43	Al responds to ERC officer
	12:02	Al messages Ariadne and Theseus
	23:02	Ariadne replies
November 16	9:21	Theseus replies
November 19	10:05	Al writes back to Ariadne and Theseus
	17:06	Theseus answers
November 20	10:05	Al replies to Theseus; end of thread
March 8, 2016	22:02	Ariadne writes to Al and Theseus regarding Nature publications
	22:49	Theseus replies
	23:02	Ariadne writes back
March 9	19:02	Al writes back
	20:18	Ariadne replies
May 2	20:30	Al meets director of [REDACTED] institute
May 12	12:45	Al sends some links to Ariadne and Theseus; end of thread

Email Thread: On peer review

To: Theseus <Theseus@eap.gr>, Al Teleki
<Teleki.Al@gmail.com>
Date: 2015.04.07. 15:19
From: Ariadne <ariadne@aegean.gr>
Subject: Happy Easter

Science is deteriorating at an incredible speed.... now
Nature is introducing a fast track for expedite review of
your paper, if you pay, WTF!!!!

[http://links.information.nature.com/servlet/MailView?ms=N
DgyOTU3OTAS1&r=MTMzNDk2Mzk5MDM5S0&j=NjQzMDQ3NjY2S0&mt=1&r
t=0](http://links.information.nature.com/servlet/MailView?ms=N
DgyOTU3OTAS1&r=MTMzNDk2Mzk5MDM5S0&j=NjQzMDQ3NjY2S0&mt=1&r
t=0)

[http://www.the-
scientist.com//?articles.view/articleNo/42562/title/Fast-
Track-Peer-Review-Controversy/](http://www.the-scientist.com//?articles.view/articleNo/42562/title/Fast-
Track-Peer-Review-Controversy/)

On 7 Apr 2015, at 22:51, Theseus <Theseus@eap.gr> wrote:

Unpaid scientific "peers" in the classic system of review
also have their disadvantages: we have all received our
share of stupid and/or unjust referee reports. Let us
wait and see whether the papers published after fast-
track review are going to be significantly poorer (or
better) than those going through classic peer review, in
terms of citation counts or perceived quality and "true
impact". I totally agree there is a risk--but not a
certainty, of ending up with poorer papers. (Rejection
rates could also be compared between the two tracks, plus
also the subsequent impact of papers rejected then
published elsewhere. *Then* we will know whether this
system works or not).

Theseus

On 8 Apr 2015, at 07:58, Ariadne <ariadne@aegean.gr>
wrote:

I would be greatly surprised if your optimism turned out
to be justified.

Where money and the rat-race infiltrate, we are usually
in for some trouble.

Ariadne

On 8 Apr 2015, at 08:12, Al Teleki <Teleki.Al@gmail.com> wrote:

I was born an optimist :). However, while I am not sure this particular move is going to turn out bad, I totally agree that the culture of science is indeed deteriorating. Managerial attitude has come to dominate, and the rat-race is indeed omnipresent. This has so much become the norm that the current generation of young scientists no longer recognize that something is terribly wrong.

Al

On 8 Apr 2015, at 08:52, Ariadne <ariadne@aegean.gr> wrote:

I'm a born optimist too. But I'm pessimistic in this case. No one cares about science any more, or at least not the one ("basic research") that is published in journals. Rest assured applied science is not published (or not in any reproducible form). My cousin, who works in a pharmaceutical industry as a research chemist, says that only papers from 80s or earlier can be trusted when it comes to organic chemistry. From that point on, they cannot be reproduced. Also he cannot publish anything, even failed experiments or stuff that will never become medicine, just because the competition might deduce what they are working at by looking at these papers.

But my problem with the current rise of "commercializing" science, i.e. overpriced open access and all these paid services is that granting agencies should not allow this at all. Already the US and more and more the EU has its own repository for papers (Pubmed) so why not just publish in state owned "journals" for free? The cost of publishing and accessing data is soaring. There are some good initiative. I like Biology Direct's way of handling peer review. Also check out PeerJ, and its price policy. One basically buys life time possibility of publishing at that journal for a reasonable fee (200-300 Euro if I remember correctly. That should be the price range for an article, not 1500-2500 Euros).

And here is an economist (a radical one) take on science and bureaucracy:

<http://www.thebaffler.com/salvos/of-flying-cars-and-the-declining-rate-of-profit>

best wishes,
Ariadne

On 10 Apr 2015, at 11:08, Al Teleki <Teleki.Al@gmail.com> wrote:

It is a fact that most of the canonized journals are run by professional editors, NOT by practising scientists. The consequence is clear: most reputable scientists no longer play the major role in setting the standards of most fields. In spite of this, the very same scientists that blame the current system put a lot of pressure on young researchers when they are in hiring and promotion committees, sometimes simply counting how many papers this or that candidate has in "top-tier journals". This is understandable because time is limiting and most of us just tend to rely on "indicators" of scientific quality the same indicators that professional editors are proud to advertise!! In this sense, we have relinquished a large amount of responsibility to our profession.

I think there is solution to this Gordian knot: to come back to the most prestigious journals with the most rigorous standards ... those run by scientific societies that set the standards of a given field. Precisely what happened in the old days!!

Greetings from the middle of the desert.

Al

PS: Apparently Nature dropped the plan, the idea didn't stick (was evidently nasty)

<http://www.peerreviewneutrality.org>

Submission Cover Letter

Dr. [REDACTED]
European Research Council

Vienna, 20 June 2015

Dear Dr. [REDACTED] and Members of the Evaluation Committee

Please find enclosed my application for ERC Stg. As a theoretical biologist, I have chosen the opportunity that this scheme from the H2020 is offering. A central reason for my interest in this scheme is that it gives the opportunity to address subjects that are challenging. I have a mixed intellectual background: I come from the life sciences but have since long being interested in the arts, in its connection to science and otherwise.

Throughout my career I have realised that being truly interdisciplinary is a challenging enterprise. I often find myself in a grey area between evolution and physics. My colleagues, including my supervisors, often recognise the value of my training, and my papers tend to be well received. However, it seems harder to raise funds since my ideas, employing deep concepts from physics and from evolution and the same time, are hard to assess. However, I believe that this is precisely the target of the ERC, and hope that in this case the ideas are found sound and interesting.

I refer to the subject that I address in my proposal as “Biopolitics of Science”. Please note that I use the term “biopolitics” in a technical way, following Foucault and Agamben. In essence, I propose to study and analyze from the philosophical, sociological and artistic perspectives the constraining and enabling factors at play in scientific research. Being a biologist from background most of my study cases (including preliminary interviews) are related to life sciences, chiefly from genetics and evolution. However, occasionally, I also invoke examples from other subjects such as physics and mathematics.

I believe that science is undergoing a crisis, because the scientists tend to be more compelled to consider sociological factors (fund raising, publication pressures, fashionable trends and controversies) rather than focusing on innovation. Furthermore, I am of the opinion that innovation is scarce, not because lack of ideas or talent but because most institutions are risk-averse. Although this is somewhat understandable, given the amount of money allocated by organizations such as the European Research Council and others alike, a detailed analysis is still relevant in order to have a more precise understanding of these dynamics.

I would like to comment that in my career I have successfully achieved a transfer of methodologies within science (e.g. between physics and evolution), but also between art and science. For instance, I have been committed to outreach activities where I have taken the chance to use media that is non-traditional to the scientific community. More relevant, I am about to complete the masters in Art & Science at the University of Applied Arts in Vienna, under the tutelage of Bernd Kräftner and Virgil Wiedrich.

I truly hope that you find my proposal interesting and relevant. Hope to hear from you in the next months.

Yours sincerely,

Al Teleki

Art & Science
University of Applied Arts
Vienna, Austria

Email Thread: Submission of the proposal and an invitation to peer review a paper

From: Al Teleki <Teleki.Al@gmail.com>
Subject: FWD: Proposal BioPol submitted
Date: 25 June 2015 17:02:41 CEST
To: Ariadne <ariadne@aegean.gr>

Dear Ariadne, the proposakl has been submitted.
Now back to real work.
Greetings from Transilvania.
Cheers, Al

> Begin forwarded message:
> **From:** European Commission <no-reply@ec.europa.eu>
> **Subject:** Proposal BioPol submitted
> **Date:** 25 June 2015 16:29:41 CEST
> **To:** Al Teleki <Teleki.Al@gmail.com>
>

Europa / Research / Participant Portal notification

Dear Proposal Participant,

The following proposal has been submitted to the Participant Portal Submission System:

Submitted by : Al Teleki (Teleki.Al@gmail.com)
Proposal acronym : BioPol
Proposal ID : 758062 (internal reference number: NOV-210201844)
Call : H2020-MSCA-Stg-2015
Type of action : MSCA-Stg-ST
Topic : MSCA-IF-2015-Stg
Call closure : 2015-25-06 17:00:00
Date of submission : 2015-25-06 16:29:39

You may return to this proposal via the [My Proposals](#) tab (<https://ec.europa.eu/research/participants/portal/desktop/en/proposals/index.html>) in the [Participant Portal](#). Please also note that you may reedit and resubmit your proposal before the deadline. In this case only the latest submitted version will be used during evaluation.

For your reference we included below information on the attachments uploaded to the submission system:

Part_B_-_BioPol_-_Al-Teleki.pdf (3403099 bytes, uploaded on 2015-25-06 16:28:56)

This email does not constitute a formal proof that the proposal submitted will be retained for evaluation.

With kind regards,
European Commission - Participant Portal Submission System team

This email has been auto-generated. Please do not reply to this account. Your email will not be read. For any inquiries please contact the NOV helpdesk (+32 (2) 29 92222 or DIGIT-H2020-NOV-SUPPORT@ec.europa.eu)

If you no longer wish to receive our notifications, you can easily change your settings in the Participant Portal > [My Notifications](#)

On 26 Jun 2015, at 9:19, From: Ariadne
<ariadne@aegean.gr> wrote:

Ok.

Do you feel like refereeing this madness? You don't have
to say yes!

Begin forwarded message:

From: "pre@aps.org" <pre@aps.org>
Date: 23 Jun 2015 4:43:55 pm GMT
To: Ariadne <ariadne@aegean.gr>
Subject: Review_request ARIADNE EM34071 **Asterion**
Reply-To: "pre@aps.org" <pre@aps.org>

Re: EM34071

Probability currents and negentropy production in nonequilibrium
open systems
by [REDACTED]

Electronic PDF-Attachment Referral from Physical Review E

Dear Ariadne,

We would appreciate your review of this paper, which has been
submitted to Physical Review E. The manuscript is included
as a PDF attachment. This message is the COMPLETE REFERRAL.
No hardcopy will be sent unless requested.

Comments from the editor specific to this manuscript:

A previous referee was unable to review this. We would appreciate
your help. Thank you for your assistance.

We prefer that you visit our referee web server at
<http://referees.aps.org> to return your report by web-upload.
Your referee id is 0438711. If you do not have a password
already, you can request one by clicking a button available at
the site. The password is not included in this message for
security reasons.

You may instead send your report by email to
pre@ridge.aps.org, or by postal mail or fax. If you use
email, either reply to this message or give as the subject
"Report ARIADNE EM34071 [REDACTED]".

Please include a complete Referee Response Form with your
report. In preparing your report, please consider the questions
in the Suggestions for Referees. All forms, contact
information, etc. are available at the referee web server.

Thank you for your help.

Yours sincerely,

Ronald Dickman
Associate Editor

Physical Review E
Email: pre@ridge.aps.org
Fax: 631-591-4141
<http://pre.aps.org/>

Physics - spotlighting exceptional research: <http://physics.aps.org/>

Attachments:
pre_em34071.pdf - Current paper [189.2kb]

=====

FORMS AND MEMOS:

=====

Please see the following forms:

<http://forms.aps.org/referee/referral-pr.pdf>
PR referee response form, included with referral (Copy included)
<http://forms.aps.org/referee/suggestionforrefs-pr.pdf>
Physical Review Suggestions for Referees
<http://forms.aps.org/referee/rvwstndrds-pre.pdf>
Review Standards - Notice to Referees - PRE

FORM: REFERRAL-PR-----
refresp-pr
revised 06/05

Physical Review - REFEREE RESPONSE FORM - Electronic mail version

Insert X in the parentheses in the response form as appropriate.

The preferred method for sending referee reports is via our referee web server (<http://referees.aps.org/>). If you do not have a password, you can request one by clicking a button available at the site. Your Referee ID is the six-digit number at the bottom of the first page of this referral, following your name. The referee web server allows you to upload your report and to respond to the questions on the Response Form by clicking boxes.

Alternatively, you may send your report by email, fax, or conventional mail. If you use email, give as the subject "Report" followed by your last name in upper case, the manuscript code number, and the last name of the first author, for example, "Report BROWN BA1234 Smith"; and include in the message the responses to applicable questions on the Response Form (a template is available at <http://forms.aps.org/referee/refresp-pr.asc>). If you do not receive an acknowledgment within one working day, please repeat the message or send the report via another mode. (For technical reasons, we cannot send automatic acknowledgments if the subject line contains "Re:", "re:", etc., so please avoid such subject lines.)

If you use conventional mail or fax, please include the Response

On 26 Jun 2015, at 9:32, From: Al Teleki <Teleki.Al>
wrote:

Sure, judging from the title the title and the authors it
seems it won't be hard to reject.

Want to delegate the review or wanna do a joint review?

On 26 Jun 2015, at 9:35, From: Ariadne
<ariadne@aegean.gr> wrote:

Send me your comments. I will notify the editor this is a
joint review.

When are you back to real Europe?

On 26 Jun 2015, at 9:37, From: Al Teleki
<Teleki.Al@gmail.com> wrote:

Next week. Bis dan.
Al

Interview Invitation and Reviewer Comments

From: [REDACTED] <[REDACTED]@erc.europa.eu>
Subject: Your EU proposal 758062 - BioPol Interview
Date: 16 September 2015 17:14:52 CET
To: Al Teleki <Teleki.Al@gmail.com>

Dear Dr. Teleki,

following our notification of February 10th, 2016 we are pleased to inform you that we sent out the official invitation letters via post.

For your information, please find attached a copy of the invitation letter.

Your presentation is scheduled for
Wednesday October 21st, 2015, 8:30 am
in the offices of the European Research Council
Place Charles Rogier 16
1210 Saint-Josse-ten-Noode (Brussels)
Belgium

You will first have the opportunity to outline your ideas in a 12 to max. 15 minutes presentation. Subsequently, there will be a 15 to 20 min round of questions and discussion. Although overall the reviewers supported your application, they had some critical remarks and questions, which you can find attached. You will also need to resubmit a version considering reviewer concerns.

In case your application has been submitted to another funding organization, we kindly ask you to keep us informed about the status of the application at all times. Unless otherwise informed, the European Research Council assumes that the application has not and will not be submitted to any other organization.

We look forward to welcoming you to Brussels and to hearing about your ideas.

Yours sincerely,

[REDACTED]

Attachments:
Comments on your proposal.

Reviewer 1

Mr. Teleki's proposes to study sociological aspects of science, in particular in relation to the role and impact that the individual scientist can have and the constraints that s/he experiences by numerous elements that are intrinsic to academia but not necessarily scientific. The question has not been previously posed as such, at least not properly on a sociological framework, and in that respect, they are welcome.

Now it has. I am glad the reviewer has welcomed it.

In this sense, I find many aspects of the proposal interesting

Good, so there is an overlapping agreement.

even though some other are puzzling and/or obscure (explained later on this review).

If they would be clear this would be a publication, not a proposal, wouldn't it?

The author strongly roots his proposal on Giorgio Agamben's works by making use of two principal sources, the short monograph "What is an Apparatus", and the longer book "Homo sacer". While both sources are genuine and recognized it is largely unclear when or what aspects are only metaphoric and when they are more literal.

It seems to me that we have exactly the same concern. Thus in a way there is no concern, because that is one of the aspects that I would like to clarify during the research. However, some aspects are clearly metaphorical, such as when I refer to the "death of a scientist" I do not mean the death of the person, but the transformation of the intellectual activity into an ambition for a career (that is defined in the text). In that sense, a "camp" (a scientific institution or university) is not literally a camp. But I think that this example is self-explanatory. Other elements, such as the notion of an apparatus, I am directly applying on a more literal way, not metaphorical. However, I would argue that there are no metaphors here but language tools, and in that sense I am using analogies to make these "metaphors" precise. I.e. victims of camps were killed as ideas in scientific institution are killed.

Given the central role it plays in the proposal, the notion or definition of "apparatus" should be given, both in general and in the specific context of the work.

I define a scientific apparatus as "The network of interacting parts that constitute power relations in the scientific sociology, where by the role of the different actors are not restricted to scientific issues but subject to other factors that may or may not work for scientific interests."

I did express myself clearly. I am aware it was defined, but the definition is vague because the "different actors" are not properly defined, and only, at best, some of them are exemplified. My following comment holds.

On this line, although I understand that the "scientific apparatus", as the author calls it (following Agamben's notion), is necessarily an intricate network of relationships between individuals, institutions, and interests from the public, government, and private sectors (etc), it seems to me that, first, the attempt to include all these and other elements at once is too

ambitious; second, the considered elements seem to constitute a list that is arbitrary; third, the considered elements are obvious channels of influence on a scientist (and channels for a scientist to be influential in the two senses indicated by the author), and while I do not disagree on their relevance, it seems to me that there can be other less conspicuous elements playing an active role that is equally important but less straightforward to recognize.

The purpose of the project is not to characterize the apparatus. Instead the different examples show how different interests that are not scientific influences the perception of what science is about (e.g. p.7: "science is what scientist do, and that is not necessarily research adhered to the scientific method"). The notion of the apparatus is used as a framework to understand and conceptualize the mediation of these factors. I have clarified that point in the new version.

The problem that I see (which remains) is that although the "practical" or exemplified elements of the "scientific apparatus" are described in detail, there remain many other "power relations" that are tacit and implicit. Yet, the candidate still talks about "the scientific apparatus" as a well-defined and concrete object.

On the good side, the way in which these elements are presented in the diagram of Fig. 2 is very interesting –although it still suffers from (at least) the three problems mentioned in the previous paragraph. I agree that the scientific method cannot be taken in isolation from other "forces", as you call it in analogy to "market forces".

Well, it is not an analogy. There are actual market forces acting on the different steps of the scientific method.

I think you are correct but only partly. The "other" forces need not to be economical, but yet equally as present. In that sense, they are only analogous to market forces.

This diagram is a very helpful illustration of how the actors involved in the sociology of scientific research are mediated through the scientific method. Thus, this diagram should be used more actively in the explanations. For example, you quote Callon (1994) "p.399: any good has intrinsic properties linked to its form or its physical characteristic, and these properties determine its ability to become a marketable product for the purposes of commercial transaction". While the case of scientific publications is a clear example of a "marketable product" that targets a specific step in the scientific method, other ones are less clear. For example, the marketability of a scientific result is arguable, since the step from that to a product is quite big. It is even harder to grasp how a "hypothesis" is marketable. What could we deduce from the diagram of the "hacked" scientific method?

*Maybe I wasn't clear enough (partly because of space). The marketability of, say, hypotheses result via other, more subtle mechanisms. For instance, there are fashionable hypotheses that sound cheeky (Arsenic DNA, Cold Fusion, Faster-than-light Neutrinos, Kin-Group Selection Controversy, etc. see refs 5,16,22,3 resp.) but that *literally* have no scientific ground. Yet, they make a great headline and cover on Nature or Science (or others). Here the benefit to the author is not economical (to the journal it is, in the long run), but it does give a lot of publicity. However, the scientific method is "hacked" because, in it, the report is unscientific, defeating its*

own purpose. What is being marketed is the name of the scientist, for, as Richard Dawkins says, “the admiration of the colleagues”. This in turns increases “respectability” and the likelihood to get grants etc. even if said reputation is partly based on unscientific but published-in-good-journals papers.

An aspect that could be discussed in more detail (and again regarding the metaphors) is the notion of the “state of exception”. In Agamben’s discourse, the concept refers to the sovereign’s right to go beyond the established law in as long as it is justifiable in terms of the public good. I do not see how scientific institutions transcend the laws of science, as you claim, even metaphorically. Certainly there is a constant challenging of the known limits of science, and that is what science is about. But what you mean is at least unclear (if not wrong) from your proposal.

Ok, I see your point, and that has been taken care of in the revised version. To summarize and clarify: I did not mean “laws of science” as literally “violating gravity” or anything like that (but see below). I was meaning to imply that there are tacit “laws” in the scientific society regarding what is a good scientific question, regarding what is scientific (mis)conduct, regarding what qualifies as fundable and non-fundable science, regarding career plans and steps, etc. The “state of exception” is coming not from scientific institutions literally going beyond the laws of science, but from the authority they have attributed to themselves to allow going beyond the “genuine laws” (defined as genuine scientific motivations, or as you correctly say “that is what science is about”). What is alarming is that under this state of exception, there have been actual claims of literally breaking the laws of physics (e.g. the neutrino story), or of genetics (the arsenic DNA story) etc. So although not literally breaking the laws of science, by breaking the laws of scientific society there has been a pretension of breaking the laws of science. To further add to your comment “Certainly there is a constant challenging of the known limits of science, and that is what science is about”, that is my point: that is what it should be about, but it is not about it anymore, or at least it is so only rarely. That is the state of exception.

Also, I believe that this concept has to be developed more gradually. The author makes some initial parallels between scientific society a country or city, and rather fast jumps to the conclusion of the state of exception. Although it is true that there some intermediate steps are laid out, the metaphor should be made more precise, not only by pointing the parallels but also the aspects on which they differ. These differences are a potential source of understanding much richer than the parallels themselves.

That is the point of a grant proposal.

The connection between the two approaches, the bottom-up (WP1) and top-down (WP2), seems to be missing. I was expecting that WP3 would connect these two, but it wasn’t the case. There should be an attempt to relate how scientific labour, which concentrates on the scientific method, is interrelated more intimately with the scientific apparatus. I mean, the proposal does states that “Extra scientific interests [...] anchor at each specific step of the scientific method [...]” and although some examples are given, these extra scientific interest are discussed either in terms of the power they excerpt on the scientific society as a whole, and how an individual scientist is subject to such power elements. But how these connections of the power structures to the scientific method relates to the state of exception of science, as discussed in WP2, remains unclear.

I am a bit lost regarding the metaphor with the labyrinth and the erratic construction of scientific theories.

That is the point of the labyrinths. To get lost in them.

Or for amusement, in the back pages of the newspapers or old video games.

I do not see what purpose this metaphor is serving. Seems to me more of a poetic and subjective appreciation than anything else. I would suggest leaving this section out (that would also free some space on the proposal where some other aspects can be developed in more detail). Alternatively (which seems harder) I suggest making more extensive use of said metaphor (although it is not clear how at this stage).

Understood. The metaphor does serve as an abstraction and to dramatize the elements, not for scientific purposes but, we could say, to evaluate the urgency of the situation.

The proposal presents some fragments of some interviews that have been conducted as part of a preliminary pilot study. On the one hand, they seem to be a rich and interesting source of information. On the other hand, I am worried that the fragments are cherry-picked instead of systematically and properly analyzed and reported. It is mentioned that eight interviews to “*scientists and science communicators of different disciplines were conducted*” however, there is no mention of the criterion by which the interviewees were selected. Also, is the author intending to carry out more interviews? This is implied but not clearly stated. If so, how many? A plan for such resource should be presented, outlining the kind of information that is aimed at.

I agree the interviews are not that informative at the moments, as they were more explorative. Nevertheless, they do serve as preliminary evidence to reify the questions and hypotheses of the proposal. Regarding the other concerns, I have updated the information accordingly.

I would like to thank the referee for the useful and insightful concerns.

Reviewer 2

The project addresses an important challenge in sociology of science and has the potential for ground-breaking contributions concerning biopolitics and how they can drive scientific research. The proposed research aims to develop a theory by using sociological techniques in order to further our understanding the nature of scientific research in contemporary society.

I thank the referee his observations and that he considers the subject important.

This is a very ambitious project with an innovative approach for theory development. It has a medium-to-high-risk/high-gain balance.

The proposed research seems feasible given the PI's expertise.

Some concerns are:

The plans for dissemination of the scientific results via peer-reviewed publications are not explained in sufficient detail.

Mechanisms for project management and oversight of progress are not described in sufficient detail.

Although the assessment is very general, I tried to consider the issues as much as possible. Dissemination plans have been expanded accordingly. I have also added further material to monitor progress, such as milestones, Gannt chart etc.

Reviewer 3

This ambitious project is purely theoretical, aiming at providing new modelling tools to better understand major questions in sociology of science, such as the effects of metrics in the choice of scientific questions, ethics and scientific conduct and the interconnections between the relations of power between reviewer and reviewee. Motivation for the project stems from the correct identification of gaps in scientific objectives or difficulties at formally defining a framework to address big open questions. These difficulties revolve around the fact that classic sociological models generally do not consider natural factors as equivalent to anthropological ones, as relatively recently accounted by Callon. Even if the theoretical exploration of those key questions is certainly not a virgin field, there is indeed a need for further work on these issues, especially in the context of unprecedented occurrences of misconduct. The project proposes that analogy between biopolitics and sociology of science could provide new tools and concepts allowing significant progress in those questions.

In this case, the present project could lead to significant insights and tools for future studies and help to give a more general sense to accumulating data and evidence for a more fair system of publication policies and resource allocation. On the other hand, the fact that such analogy has not yet yielded to a revolution in the field could indicate that, even if interesting conceptually, it does not solve all limitations of present situation.

I thank the referee for his supportive attitude. I do not expect that this work will "yield a revolution". (If Kuhn did not do it I cannot expect more.) However, I do expect that to contribute to the identification of certain aspects, moving on from unknown unsolved issues to known unsolved issues. I think that there is a crisis and we need, at least, to acknowledge and understand it.

The project manipulates several difficult concepts and is a bit obscure in several places.

I tried my best to clarify at several points, especially following the more detailed comments of Reviewer 1. I hope it is clearer now.

Email Thread: Rejection of the proposal

From: Al Teleki <Teleki.Al@gmail.com>
Subject: FWD: NEW TASK: Your EU proposal 758062 - BioPol rejection
Date: 29 October 2015 19:02:21 CEST
To: Ariadne <ariadne@aegean.gr>

> Begin forwarded message:
> From: European Commission <EC-NO-REPLY-GRANT-MANAGEMENT@nomail.ec.europa.eu>
> Subject: NEW TASK: Your EU proposal 758062 - BioPol rejection
> Date: 29 Oct 2015 18:30:29 CET
> To: Al Teleki <Teleki.Al@gmail.com>
>

Europa / Research / Participant Portal notification

Dear Coordinator,

We regret that we have to inform you that your above proposal has been rejected. The Rejection Letter is available on the proposal page at the Participant Portal. Log on at the Participant Portal, go to My Area > My Proposal(s) and click the Follow-up (FO) button.

Regards,
Participant Portal Grant Management Service

Please do not reply to this message

This message has been automatically generated by the Grant Management System of the European Commission. For security reasons it does not contain any attachments or direct URL links.

If you no longer wish to receive our notifications, you can easily change your settings in the Participant Portal > [My Notifications](#)

On 29 Oct 2016, at 19:19, From: Ariadne <ariadne@aegean.gr> wrote:

Fuck.

Email Thread: Letter to the panel

From: "Al Teleki" <Teleki.Al@gmail.com>

Date: 14 November 2015 14:17:03 CEST

To: "[REDACTED]" <[REDACTED]@erc.europa.eu>

Dear [REDACTED],

I wrote the attached letter to the members of the panel.

I would like you to forward it to them. But, you may take a look first to it (as I mention your name) to make sure Im not putting you on any compromising position. If you think I do, let me know and I can fix that.

Thanks and have a lovely weekend!

Best regards,

Al

Dear members of the panel,

First of all let me express my gratitude for having invited me to Brussels for the interview, and thank you very much for the time you invested in reviewing my project and listening to my presentation.

I would like to briefly comment you regarding the evaluation I have kindly received from Dr. [REDACTED] on the morning of Wednesday 21 of October.

I am very obliged to you being *fascinated* by my ideas and even considering them of a *revolutionary* potential, using the adjectives that Dr. [REDACTED] expressed on your behalf.

As it is natural, I am sad that I was not awarded the grant. While I submit to your decision, as making the selection is your duty and you have your parameters to decide what fits the scheme, I must express my disagreement with some of your arguments. Or at least how I understood them from what Dr. [REDACTED] expressed. First let me summarize and paraphrase the reasons as I understood them:

1. The panel was not confident that I could fulfil my goals since the concrete steps were not convincing.
2. The methodology was not convincing.
3. The definitions of “biopolitics” were not convincing.
4. My statistical skills are not solid enough to pursue this project.

Regarding (1) I have to sadly accept the fact that I was not proficient enough to express or articulate my ideas. Yet, I would like to understand the actual reasons.

Regarding (2-4) I have serious concerns. In general lines, I do not accept them as genuine reasons for rejecting my proposal. Personally, I believe that (2) and (3) are not even scientific issues but are rooted sociological vices of the scientific functioning (proof of concept of this very same proposal). Reason (4) is simply unfair assessment. I explain myself better in the following paragraphs.

As an interdisciplinary researcher, this is not the first time I have to deal with ‘field biases’ (intrinsic and extrinsic). There are two aspects playing here. In working at the interface of two fields there is always a big space to point out the faults and disregard the merits of taking the first steps. Moreover, suspicion is common because it is a costly endeavour to convince two or three communities of a given approach, even if it bridges established fields (as in my case). Sadly, this has little to do with the science behind but rather with the sociology of the scientific fields. Not only the questions, approaches, methods, practices, etc. but also the specific ‘rituals’, behaviours, prejudices, taboos and, in general,

social dynamics are largely field-related, and unfortunately what is natural thinking for one field can be even disturbing for another field.

Large part of these dynamics is the semantics. Let me start with the panel's concern regarding 'biopolitics': it means different things in different fields. I did clarify that on my talk. Unfortunately I am not the one who coined the terms, and yet I did explain the different meanings (sociology, philosophy and art) as well as what I meant by it and what exactly I wanted to do. If the notions were not convincing, that is beyond my control, because the term has been for decades now and is not up to me (that would be arrogant) to pretend correcting the usage on different fields. I defined "biopolitics" as Foucault and Agamben did, as well as uses in art by Da Costa. Also I clearly stated how I intend to use it methodologically. Hence any lack of agreement regarding what biopolitics is, is just a consequence of the interdisciplinary nature of my work and of the panel. Of course you might have simply disliked my approach, but that is a very different argument from not having provided a convincing *definition* of 'biopolitics'. A definition is what it is.

Regarding (4), I understood that you don't consider that my statistical skills are solid enough to pursue this project. I must say that I am shocked and stunned about this. I think it does not take much effort to realize by taking a look to my papers (if not by my studies and to the people I have worked with) that if there is something I am not lacking are the mathematical skills. I might lack experience in other areas, but if anything colleagues have pointed out is my strong mathematical abilities and approach. This is so true that it took me years to learn to decrease the level of my talks in order to make them understandable to the rest of my colleagues. Regarding my work, not only I have *developed* and *applied new concepts* of statistical to genetics (something rather innovative) but also I have been using quite solid maths from different theories (incl. statistical mechanics, dynamical systems, probability theory, stochastic processes, game theory, analysis, statistics and lately machine learning and neural networks -in press) applied to population genetics and other fields ever since my first publications in 2003 (when I was a master student of applied mathematics specialising on stochastic processes). I have introduced new techniques, new concepts and developed new methods. That is something that few do in evolutionary biology; most researchers inside and outside population genetics rarely care about maths and are, largely, happy with simulations or simple toy models. But if my previous work has shown something, is a strong commitment to build a formal basis for understanding evolution, motivated and reconciled with biological facts. I am sorry to deny your assessment and, without any belligerent tone, point out that on this matter your judgment has been incorrect, premature and superficial.

What seems to be happening is that I am too tamed by the biologists (another sociological issue) and been too condescending to my colleagues by putting a strong effort in

explaining what my models and equations mean rather than focusing only on the maths. In fact, this is something that as a peer reviewer I strongly criticize from publications such as PRE, where too often researchers working on the interdisciplinary area of biology and physics only focus on the maths without explaining and exploring the actual biological implications, meanings and consequences of their models. But what others do is beside the point. For my part I truly regret intending that the rationale of my talk was as understandable as possible by the interdisciplinary panel (as requested by the foundation in the invitation). In any case, I remind you that theoretical evolutionary biology has its own techniques that differ substantially from those of physics (c.f. my papers) and having focused on the mathematical aspects would have made it all even less understandable, since the members of the panel that would be acquainted with these methods were very few.

Aside from these issues regarding my specific evaluation, I would like to express a worry: I consider that there was an overrepresentation of physicists on the committee. Although I agree that representatives from that field should be present, I also think that its overrepresentation might put in disadvantage other fields, partly because, I presume, I was too close to physics and to biology (as the panel was), but also off enough from both fields as to be severely criticised on the basis of technical issues that are largely irrelevant for my project, as argued above. And I do believe it had such effect in my case for two reasons. (i) It implies that the majority, although probably got very clearly what my aims and objectives were, were most likely unacquainted with the standard methods and ideas of population genetics which I strongly rely on, making it hard to judge how much is innovative in my project and how much is existing. (ii) At the same time, this creates an underrepresentation of biologists. As a discipline, biology is arguably much more varied than physics, and even within the field of 'evolution' the views are so varied and opposed as to be alien to one another. For instance, to say something, evolutionary game theoreticians rarely deal with population genetics, or population geneticists rarely deal with evolutionary ecology, and so on. While I have no concerns with the particular representatives of evolutionary biology in the panel, I just want to point out that even this mix between population genetics and group selection can be considered an interdisciplinary approach (since group and kin selection is typically discussed by evolutionary ecologists, not by population geneticists), and as such, the set of tools differ substantially.

In fact, these imbalances are reflected in that the concerns of the panel are mostly of technical nature and to a large extent and in my opinion, somewhat superficial.

I hope you understand my point of view regarding your arguments for the decision to reject my proposal. I truly wish the future candidates, especially those dealing with

interdisciplinary subjects, that the decisions of their projects are less tinted with the (almost inevitable) dogmas and sociological dynamics of the different scientific disciplines.

For myself, I continue to be disappointed that truly interdisciplinary attempts are (non-intentionally) punished, and see myself, after several failed efforts to secure funding for exciting and innovative research, forced to undertake a more conventional, gradualist and less ambitious approach to science, as the 99% of us do. But I do understand that not everything can be funded and that the foundation has its own interests.

Lastly, once again, I thank you truly for having given me the chance to discuss my ideas with such a distinguished and qualified panel.

Best wishes for you and for the successful candidates.

Al Teleki

Art & Science
University of Applied Arts
Vienna, Austria

On 14 Nov 2015, at 17:49, [REDACTED]
<[REDACTED]@erc.europa.eu> wrote:
Dear Al,

many thanks for your email and your kindness to consult me in this regard.

First of all, with your statement you are not putting me in any compromising position. Thanks for your thoughtfulness.

I spent some thoughts on the issue and discussed the situation with my colleagues. We want to be honest and I have to tell you, that the Commission generally does not forward feedback from the applicants to our review panels with regard to funding decisions. This is a general rule and has nothing to do with your request. I am sorry about this, but I can do nothing for you in this regard.

However, your feedback is very helpful for me as a program manager and my colleagues in the Commission. Since we are responsible for setting up a review panel, we will definitely take your feedback into account when it comes to future review panels. Your perspective on the disciplinary representation is very helpful and will be considered for the next funding round. I appreciate your thoughts on that and will discuss your feedback with my colleagues.

I am aware, that my answer might be disappointing for you. Nonetheless, I appreciate your feedback and will keep it in mind.

Many thanks for your efforts and all the best!

[REDACTED]

Dr. des. [REDACTED]
European Research Council

phone: +49 511 8381 297
fax: +49 511 8381 4297

On 15 Nov 2015, at 10:43, Al Teleki <Teleki.Al@gmail.com> wrote:

Thank you [REDACTED].

I understand your points, and I am glad that at least this can help you as a program manager. I am obviously disappointed -doubly now- on how the Commission manages these issues. And of course my opinion on the competency of the panel in their quality of reviewers is at this point rather low. (I am not judging their scientific competencies though.) Either they have been overly superficial in their judgment, or the reasons for rejecting the project are not what they are saying, at least not fully, and therefore are not fully honest.

Anyway, this is none of my business anymore, and I should not keep bothering you from doing your work!

All the best to you, and thanks for your support!

Cheers, Al.

From: "Al Teleki" <Teleki.Al@gmail.com>
Date: 15 Nov 2015 12:02:17 CEST
To: Ariadne <ariadne@aegean.gr>
Cc: Theseus <theseus@eap.gr>

Hello Ariadne, did you see what I forwarded regarding the (rejection) of my proposal? :(

On 15 Nov 2015, at 23:02, Ariadne <ariadne@aegean.gr> wrote:

Yes I can tell you what happened

On 16 Nov 2015, at 9:21, Theseus <Theseus@eap.gr> wrote:

Al, indeed the proposal was well written, let's see what they write. Even if they are not writing much in H2020.

On 19 Nov 2015, at 10:05, Al Teleki <Teleki.Al@gmail.com> wrote:

Hi Ariadne & Theseus, I have a plan.
Don't know how far it will get me, or how feasible it is.
Need your opinion.

First of all, they won't write too much in the ERC report, it will be a generic letter, saying "sorry you were not shortlisted and thanks for your effort... blah blah".

The truth is that they applied an anti-ethical selection procedure that was not informed. To narrow down the number of applications to consider for step 2, they only took into account those candidates that had prior publications in Nature and/or Science. Clearly this leaves much out. The point here is not only on the nastiness of the criterion, but rather that these journals do not represent homogeneously all fields. For instance, maths and theoretical biology (amongst others) would be less represented than, say, neuroscience or synthetic biology.

What we require is to be able to prove that this criterion has been applied. That of course is the hard part because they will probably not admit it. Here I have a question: is there any way to know (at least afterwards) which applications were considered for step 2 and which ones not? A simple statistical test will provide the answer to this hypothesis. This we could of course support with any statement that they give, and maybe even convincing someone on the panel to admit it (much harder).

Once done that, a letter should be addressed to the commission, but also to higher instances, accusing the commission of "discrimination" for not allowing all areas to be equally represented, for not having a meaningful criterion of excellence. Also, we could accuse them for working for the interests of the above mentioned journals rather than working for the scientific excellence and

European needs. In addition an editorial on a competing journal such as PLoS ONE etc. could be published. This would of course put the face of the ERC in a very shameful situation. Here we will be in position to demand more transparency on the evaluation process, not to mention the reconsideration of countless proposals, etc.

If we are ignored we can still proceed with legal actions by suing the commission for being incompetent, biased, discriminatory, having conflicts of interest with private corporations (i.e. Nature Publishing Group), and any other approach that a lawyer could find fanciful to tackle and that Brussels would not like.

What do you guys think of this?

Best, Al

On 19 Nov 2015, at 17:06, Theseus <Theseus@eap.gr> wrote:

Dear Al,

Indeed some of the panels are using this kind of evaluation; even when they shouldn't. but if they decided to go this way, I'm afraid they have ground to do it. The reason is that even if questionable, it is in their own legal possibilities :(

A very important point for me is that there are simply too many applications still (2920)... and this is also due to the lack of national funding. The evaluation process for this number of proposals is simply too expensive if done by the most competent evaluators

Ariadne was member of the ERC panel and might comment on this as well.

Cheers,
Theseus

On 20 Nov 2015, at 10:05, Al Teleki <Teleki.Al@gmail.com> wrote:

Thx for the feedback. I guess I'm pretty screwed with this :S What to do. Between us, I didn't want to disclose it so openly, but it was Ariadne who told me about the selection method over dinner. As she mentioned (also to you), Procritus was in the panel, and that is how we know what happened. Anyway, its not a secret, Procritus is listed (although not easily accessible), but that's irrelevant.

Email Thread: On high impact journals

From: Ariadne <ariadne@aegean.gr>

Date: 8 Mar 2016 22:02:17 CEST

Subject: Do you two think we should invite authors of Nature paper on [REDACTED] group, even if not space enough at first meeting?

To: Theseus <Theseus@eap.gr>, Al Teleki <Teleki.Al@gmail.com>

Dear Theseus & Al,

I agree with Theseus on the [REDACTED] case and I remember that we discussed these things. @Al: There are some (better to say many) in this community who continue to oversell wrong claims (genuinely based on a LACK of selectivity) - some (better to say many) of them were even educated by myself in the proper use of the [REDACTED] tool I wrote - and were explicitly warned by myself. In 2006 or 2007 a young girl from the **Asterion** group (coauthoring this paper) came to my lab and spent several months in **Troy**. While I personally was convinced that she is a honest young scientist, I was not so sure about her boss. In a way, I finally felt urged to accept coauthorship in a paper in PNAS because - there is some human side in science. Yes, the paper (PNAS) looks convincing and, yes, it could be like in their interpretation - but the number of controls **Idas** performed in my lab (before 2004 when our "listed" paper in Nature came out) - and the number of controls young scientists of today usually carry out is somewhere like 10:1. A good guess is that you can forget about approximately 90% of the literature in this field produced after 2000 - when the neoliberal revolution started to overtake science.

I'm not reading **Asterion** anymore - I was also contacted by a journalist but I didn't respond. I have the understanding that this is systemic - so I don't blame either **Asterion** nor anyone of his generation.

It looks to work like this: The first small lie is really, really difficult for a typical young scientist today - he is typically conscious about and so he feels bad. But he finally does it, because he believes that many around him - his competitors at least - do it as well. Should he risk his career for doing time-consuming controls while the others don't do? It is much more important to shoot out the paper - and if he has luck - he even gets it out into Nature and Science. Now he

smells fame first time - smells good. The media response is great - it feels great to be seen as important. He gets his first award. Even some money included. Wife says: You are on the right track. Only he himself knows that all is based on a little lie. Little later he has increased his level of what we call excellence today, expected to become "more excellent", "more closer to the so called elite". The next little lie becomes easier. The next even more, and so on... This is why I believe that the "corporate psychopath" in science is a notorious liar selected by competition and the Darwinian principle. Cf: <http://topdocumentaryfilms.com/i-am-fishead-are-corporate-leaders-psychopaths/>

Genetically - people with a high level of testosterone and a low level of oxytocine receptor (OXTR) seem to be programmed to become more successful in a neoliberal society - they combine a good self-esteem with a low degree of empathy. They are less scrupulous - our apoptotic system wants this. This type of personality is certainly not a good scientist, if science is about finding the truth. But science is not just about truthfinding. For most it is just a means to make a living. Like any other job. The most "successful" have "an idea" and start to write a paper before doing any experiment. Then they do something - not much - just to get some display items of "real data" demonstrating their idea to be operative - no controls at all - if any they select the ones needed. This is not a statement, my friends. I know that it is exactly like this. I KNOW IT.

The only option one has is to self-exclude from the "stream to the elite". I can do it, because I have an old professorship contract at an **Athenean** university and so I can afford self-exclusion. Younger colleagues can't however - so I don't blame them. Most of them sense that it is wrong. But they are full of fears that if they don't lie they will lose their future. The extreme 1% criminal psychopath doesn't have any fear at all - they just do it, let something happen and enjoy like mistreated children that others have to suffer, not themselves.

This is the Poised Realm Matrix we are living in, gentlemen.

All the best,
Ariadne

On 8 Mar 2016, at 22:49, Theseus <Theseus@eap.gr> wrote:

I class scientists into the Good the Bad and the Ugly based on Fyenman's criteria. "Science is about not deceiving oneself. If one can manage that, one doesn't deceive others. "

The GOOD scientist manages not to deceive himself and thus not to deceive others.

The BAD one deceives themselves and in doing so accidentally deceive others.

The UGLY ones don't deceive themselves, but deceive others on purpose!

I think you criticize the latter. Al and I do NOT I think fall into the final camp. Not all scientists who want a Nature paper fall into camp 3 Ariadne.

No idea what you mean by the first small lie? Lie about what? How? Neither Al or I were proposing to lie about anything. I am not tempted to not include proper controls. Wanting to write a Nature paper, having an IDEA is not a bad thing. A bad thing is to make up the data, or to decieve.

You mean that there won't be a Nature paper because Nature is corrupt? thats a separate issue.

Cheers,
Theseus

On 8 Mar 2016, at 23:02, Ariadne <ariadne@aegean.gr>
wrote:

Dear Theseus,

I have NOT said or indicated that you were doing anything
in a bad way. I just wanted to highlight some dangers
that seem to be on the road, characterized above.

The statement

> Then they do something - not much - just to get some
> display items of "real data" demonstrating their idea
> to be operative - no controls at all - if any they
> select the ones needed. This is not a statement, my
> friends. I know that it is exactly like this. I KNOW
> IT.

is striking; it means that THIS TYPE OF BEHAVIOUR IS
SPREADING. I just want to signal very clearly that I am
NOT GOING to go down this line, ever, and I do not
recommend it to anybody whom I would like to continue to
respect.

By the way, these journals are fashion dictators, and I
am definitely not fashionable, this has come clear by
now. You see, I follow my interests, and occasionally I
submit to those journals.

The trouble is that writing Nature papers is more a
matter of yielding to fashion dictators than anything
else.

It was not always like this, but it is now.

That is all.

Cheers

Ariadne

On 9 Mar 2016, at 19:02, Al Teleki <Teleki.Al@gmail.com> wrote:

Well, that is just survival of the journal at the expense of social dynamics.

What can you do when the journal is coupled with an economic system!!!?

I mean science is not about science anymore, but about what scientists do, which is not always science!

The "incorruptible" alternative is that funding agencies endow journals and scientific publishers with money to compensate the spendings. Or the strategy of open source.

But anyway, I insist that we do what we gotta do and eventually we might have the opportunity to fix it from the inside.

Al

On 9 Mar 2016, at 20:18, Ariadne <ariadne@aegean.gr> wrote:
Yes!

Ariadne

On 12 May 2016, at 12:45, Al Teleki <Teleki.Al@gmail.com> wrote:

To complement this old discussion...
I guess you've seen it.

<http://www.theguardian.com/science/2013/dec/09/nobel-winner-boycott-science-journals>

[Applause]

Al

Extract of the interviews

It was certainly amusing to carry out the interviews. However, this is the anticlimax of this thesis (see Review comments). As mentioned in the Introduction, and unlike with the email threads, the interviews take a very polite approach. Even then, I included here excerpts that relate to the central subject and which are direct and honest accounts from the three interviewed subjects.

In the first interview Herbert Edelbrunner discusses the fashions in mathematics and in science. Although discussing mostly about the (educated) public perception, it does expose some elements that stir what is a fashionable subject. These are, as a matter of fact, the situations where a smart careering scientist can efficiently exploit. (There is nothing wrong in exploiting an opportunity, but that is different than treasure hunting for those fashionable opportunities which I regard as blindly rat racing on the labyrinth of ignorance.)

We see how in the second interview, Eörs Szathmáry addresses more directly the issues of this thesis, there is, at least, a neat exposition of some of the problems that I am denouncing here. An interesting aspect is how this senior researcher deals with different aspects of the problematic. Even if he is exposing clearly several issues, it is evidenced that the only way we deal with these issues at the moments is by accepting them and “living with it”, a clear state of exception where we as individuals are unable to change anything.

In the third interview, Marcus Werner, a scientific communicator and writer, shows another, bottom-up, perspective. Werner, who “failed as a scientist” partly because he did not submit to the interests of the Principal Investigator, exposes the tensions and conflicts of interest to which PhD candidates are. This complements the first Email Thread that points to the contrary: choose a famous supervisor, do what he expects and publish in Nature. (Ergo: a good career.)

Herbert Edelsbrunner, Professor of Mathematics

Interviewed by Isidora Krstic and Al Teleki.

IST Austria, Klosterneuburg, Austria. October 2013.

Video Interview. Colour, stereo sound.

Transcription courtesy of Tatiana Labeikovsky.

Synopsis

We start by discussing the scientific subject of Suspended Animation. We then talk about what kinds of topics in science go mainstream and get public attention, and the specifically what subjects in mathematics catch the interest on the media. We discuss what are experiments in mathematics. We then talk about why are some conjectures and hypothesis more interesting than others. The subject of aesthetics in mathematics is then tackled and compared to software tools. This leads to the question of what determines success both in the social sense and in the scientific sense. Finally, we talk about how much into the 'unknown' are scientist venturing.

Excerpts

AT: In your experience, is there some example in Mathematics where [...] something goes mainstream?

HE: I think that to go mainstream it needs to capture people's imagination [...] I think that's a big part of it. To generalize, I think there are two aspects to your question: what makes a scientific insight usable for the general public and what makes it a topic for coffee-house discussion and so on, which are different things.

AT: But somehow there always need to have components of both. I mean, if it is something that is coming from within science, of course...

HE: Often it does not have both...

AT: (sigh)... why?

HE: For example, I think in Mathematics what captures the interest of the public the most is a few singular names, who are enlarged to the level of superheroes or something.

AT: In the public...

HE: In the public opinion. There's a few questions, there's a few problems in Mathematics: 1, 2, 3. Not more than that. And if you solve that... And then if something happens there, then all of a sudden everyone is an expert and everybody is interested. If anything happens on the level lets

say “below this”, nobody cares. I guess I wanted to say that to capture the public interest has nothing to do with applicability, necessarily. Of course, if there are applications and there are consequences on everybody's lives, [...] perhaps, it's a mixture of the two.

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AT: You commented [...] that Mathematics is a very esthetic science. What is this “esthetics” in mathematics?

HE: Yes, that's all the time in a mathematician's mind... or in many. I think it is because Mathematics is created. It's not a science in the sense that I look out what's there, but it's rather looking inside into the structure, the information in the structure, so we build it ourselves, we create categories or objects, concepts, and arrange them in certain ways, and you notice very soon that if you are not careful about what you are doing it becomes very bizarre, large, unusable, and then nobody cares anymore. Only the beautiful designs stay and it is easy to convince other people, to say it this way.

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HE: [...] the ability to convince the general public or experts of something, I think that's the notion of success, which implies, by the way, that your success is only as good as other people's opinions around you. If they... it's an interesting thought experiment to think what if those 100 or 200 thousand number of people once of a sudden change their mind about you? That changes your reality entirely...

AT: change their mind in which way?

HE: From thinking that you are genius or from thinking that you are a fraud, for example, which happens sometimes if certain facts come out... it happens also in science... so that's a drastic change. Politically, I think the landscape is very rugged because they go from positive to negative very fast, or the other way around. [...] And sometimes there are some interesting earthquakes happening there that are changing a lot of things, including the definition of success.

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HE: [...] we are very focused in our own area and very fine-tuned on distinguishing what's impossible but possibly reachable, likely and unlikely, because that's somehow drives us in our profession.

Eörs Szathmáry, Professor of Evolutionary Biology

Interviewed by Al Teleki.

Parmenides Foundation, Pullach (Munich), Germany. April 16, 2015.

Video Interview. Colour, stereo sound. 47:47

Transcription commissioned to Stephanie Märzluft.

Synopsis

ES introduces himself and explains his research lines. We talk about his particular approach of being a “professional generalist” in his work, and discuss how his background shaped him in such a way. Discusses about how to deal with the flood of scientific information and how this results on a decrease of the average scientific quality. We discuss the impact of the current sociology of science on younger scientists, how the latter are trained to satisfy expectations and not scientific curiosity. We talk about the need to be humorous and not stiff. The interview turns to the risk of failure in scientific theories, and to the fact that risk, as in business, is inevitable. ES suggests that a role of certain established scientists should be to inform politicians that risk is inevitable if innovation is desirable. AT Asks what enabled him to take a risky career path. Finally, they discuss the roles of other scientists (peer reviewers and colleagues).

Excerpts

AT: How are you managing today with the flood of information, orders of magnitude larger [than a few decades ago]?

ES: Badly. I am sorry to say. And I am probably not the only person moaning about this problem.[...] You know, but what I really think retards science is that there are too many things that we cannot follow and this proliferation of papers and journals also, I am afraid to say, mostly resulted in the proliferation of junk, right? So, the danger is then that valuable results are buried by a heap of junk and you cannot read everything so there will be difficulties to find them and then of course this can also lead to inadvertent repetitions of previous works because you don't know, do you publish it again and again. [...] So, in several cases the group should have published one paper, instead of three or four and that would be a substantial and important contribution to science where as individually it is just building up this load of, this mountain of publications that honestly I think we don't need. It would be much better without them.

AT: So, in average the quality of research papers have decreased, compared to twenty years ago.

ES: Absolutely! They have decreased. You know there is this joke about intelligence that the total population is increasing but the sum of intelligence is constant. You had this, I think also, in science, unfortunately. And this is also complemented by the distortion of the sociology of science, how science is being managed and run. I am not the first to complain about, for example how certain journals are edited, I mean, even Nobel prize winners are in the club complaining about it; and to some extent rightly so. And then the grant systems how they work and so on, I think that there is deterioration.

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AT: Do you consider that science should be actually protected from economic interests, from political interest?

ES: You know, science is there to acquire knowledge and knowledge is useful but not immediately useful. It reminds me of the famous story, it is a true story. It is when Bob May an eminent theoretical ecologist, who was a physicist originally, he became the chief scientist of Britain that means that you are the adviser to the Prime Minister on scientific matters that is the chief scientist. Then in the House of Parliament there was a conservative MP that told him "I would like to give you an advice that you should invest only in that type of science that gives you economic results, you know, in the foreseeable future". And then Bob May says "thank you very much Sir really appreciated. May I reciprocate with an advice on my own to you? Do you play at the stock market? Oh yes, of course I do. Ok, so, I suggest you buy only shares that are going up?" If you put it like this it is trivial. [...] Of course there is a cumulative knowledge, just like there is cumulative adaptation in biological evolution. But you know that comes at the expense of many failed trials or trials that worked as a good idea for a while, but well this also happens in science.

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AT: What is the real damage that there is too much irrelevant science been produced?

ES: I think that there is a lot of low quality science produced. This is not an offence necessarily *vis a vis* the scientist. But we have discovered that the system itself is not really good for the maintenance of quality because there are other weights in the goal function that are probably higher.

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Marcus Werner, Science Writer

Interviewed by Al Teleki.

Amsterdam.

Audio Interview. Stereo Sound.

Transcription courtesy of Tatiana Labeikovsky.

Excerpts

AT: So I'm interested first how do you come to be a journalist, or, science writer; you were originally a biologist...

MW: Yes, that's right. Well, it's actually quite simple. I more or less failed as a biologist. I graduated and I first had jobs in commercial biology, like an ecological adviser, but that wasn't my thing. And then I tried to get back into research but I knew that I was actually too late, because I was from the generation that you didn't publish in your masters, and the people after me were already publishing papers. So, since they already had papers I knew that I didn't have a chance. I tried to get some PhD positions but I didn't succeed. I was without a job, I didn't want to become a teacher, and I always had the ambition to write. My father is a writer too. I grew up very much in an atmosphere of literature, and stuff like that. Because I was always very enthusiastic about popular science at a certain point I thought: I'd like to do that.

AT: You had finished your master's when you say you failed as a scientist; what do you mean by that?

MW: I mean I just didn't succeed in getting a PhD, you know, continuing in research.

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AT: I think that having broad interests is kind of a handicap for an academician. I hate to say it but I think it's good once you are senior, it's very well seen, but if you're junior then it's bad.

MW: That's probably right.

AT: I don't know what are the reasons for this.

MW: Well, I *do* know. I think that, and I've noticed that, the reason why I didn't get jobs, is because [...] I think that I always overestimated the level that they were looking for. I think that, actually, these project leaders and stuff, they are looking for somebody that you can mould, you know? Somebody that is going to execute *your* ideas in the field. I don't know, that was my impression; it may sound arrogant but that's always what I did. I remember when I had a chance to do a PhD on elephant seals in Antarctica, [...]. But I over-prepared myself for this because I spent about 4 weeks studying like mad, and I made a whole plan of the project I was going to do, I knew everything. I think that something went wrong there, that they thought: This Marcus, we're going to send him to Antarctica and he's going to do all kinds of stuff that we don't want him to do, or something like that. I think in retrospect that something like this was happening [...].

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AT: To what extent do you think it is necessary for what you do, being a writer, to have a method that is different from actual method used in science?

MW: It's because of the nature of the matter too. I don't find things myself because Science Journalism is based on talking to the scientists and then writing up what they say [...]. I very quickly understood that [...] these newspapers they have own editors and they [...] are going to cover what's published in Nature, in Science, the in big mainstream science news, so I very quickly understood that you should not try to compete in this terrain. What they are looking for is somebody who is going to write on subjects that they won't think very quickly it will sell. From the beginning, when I started in Science Journalism, I used to look at journals like Journal Theoretical Biology and Proceedings of the Royal Society, and TREE for instance; most *general* science journalists don't know about these journals, they only think about Nature, Science and PNAS[...]

AT: But from what I'm understanding is that people will always get this distilled from Science and Nature?

MW: No, because there's people like me who are looking at the other journals.

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AT: So what are your criteria to decide what could be potentially a good story?

MW: The big problem is that I pitch the stories to what *I think* that the head editor is going to take from me, because for me it's a lot of work, and if I pitch stories that are not going to be taken it's throwing away money because they are not going to pay me, for the research, they don't. So I kind of have this gut feeling about what I know that the head editor likes; it's *bad*, it's very bad, but I know there are certain things that the head editor will pick up.

AT: Why do you think it's bad?

MW: This is *bad*, because sometimes there is a mismatch between what the head editor thinks is good and what the general public will be interested on.
[...] In science journalism, the only thing people are interested in... the papers... look, general papers print what people want to read, because the most capitalistic institute possible is the newspaper. Because everything the newspaper does is dictated by the readers. Of course, papers make errors, that's why some papers don't do so well, because they don't really feel what... that's how papers work! Papers are only there because people buy them. And they are there because advertisers know that people buy the papers... and papers are there to present the advertisements to the people. So what papers do is try to get a feel for what people want to read. And for this reason, nowadays, in science journalism is all spectacular stuff. No interest at all in theoretical subjects.

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AT: [...] from what I understand, the role newspapers are playing is just to reinforce a preconceived idea that people have. Enriching it with some examples. If the example is correct or incorrect, it really doesn't matter.

MW: No! Exactly.

AT: It will be tinted and selected accordingly.

MW: Yeah, exactly, it's a very depressing conclusion, I think...

AT: What impact do you think this has...

MW: Perception...

AT: At a higher level... perception but... what happens when a politician in Belgium reads this thing and...

MW: I know! That's the problem.

AT: He's the one that is meeting with Parliament...

MW: That's a huge problem! It worries me a lot. That's very worrying. [...] I'm convinced that this is a huge problem, a huge problem, because all the decision makers get their information... get wrong... distorted information.

AT: Absolutely.

MW: The animal welfare issue is a case in point. Nobody knows a fucking thing about real animal welfare. You have to do research on it, good research, and you have to consider it from the point of view of the animals, but nobody does that. [...] People... they are deceiving themselves...

MW: [...] But there's more, there's a lot more. We talked about bees, that's something also an extremely interesting subject for the general public. This strange thing that these worker bees forgo their own reproduction, and they're feeding the offspring of the queen, why? Well, it turns out that it is because because of this haplo-dyploid system... bla-bla-bla... I don't even know the details, I forgot in the meantime, but I know that it it's all related to individual fitness, and people like that, too. People are always being interested in animals. I mean, why Lascaux, the caves, why did they paint these fucking things? That's what people do.

AT: So there is a natural interest in these things, it's very human to be interested.

MW: Of course, why are all these nature series so popular? I mean they are extremely popular nowadays, probably for the wrong reasons...

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Conclusion

Although the presentation of these interviews are more conservative than the emails above, the former constitute an additional body of evidence that there is big gap between how science is performed, exposed and perceived.

It cannot be denied that the interests at play are, in fact, steering research in ways that are more sociological than scientific. That is acceptable for an economic market (including, at least partly, the artistic) because it only relies on cultural factors. But science has its own way of justifying its existence, which is understanding and predicting nature. The moment we are not doing this anymore, but only using science as an argument to become a “rock star” we are breaking the basic tenets of science, violating and manipulating elements of the scientific method and, consequently, justifying our actions on a sociological basis. This is not unlike breaking the law. And doing so on the justification of the sovereign, that is our scientific community, puts indeed science at a state of exception where we are not required to respect the “scientific law”, under the grounds of other sociologically justifiable factors.

We keep running inside a labyrinth of ignorance.

*Ein labyrinthischer Mensch sucht niemals die Wahrheit,
sondern immer nur seine Ariadne*

Friederich Nietzsche (1882)

