A Personal View on Reviewing

Kai-Uwe Carstensen

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contact at kai-uwe-carstensen.de

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Foreword: What's up, doc?

Some time ago, I read the autobiography of <u>Oliver Sacks</u>. I've always liked his honest, clear, case-study style of writing, and although I lack the corresponding competence in probably all relevant respects (last but not least because of English not being my mother tongue), I will try to do what he did all his life: simply write down what one is concerned with or interested in, just starting at the beginning and then seeing where it leads (although I will not write THAT much, of course).

At the moment, and therefore for this whatever-it-is (an essay with small, strangely-titled sections (?)), this is about reviews and reviewing. I like to read non-scientific reviews, as you can find the best and worst of man's intellect in a few lines of text. With growing scientific experience, I more and more wondered why scientific reviews—against expectations—often tend to the latter.

Most of the following will concern the scientific reviewing of journal articles. One can compare submitting to a journal with going to a doctor. With luck, everything works fine and you are treated well, to everybody's satisfaction. More often than not, however, once you enter the doctor's office, you turn into a 'you know nothing Jon Snow'-nobody who is treated like a dotard, and who has to elicit the doctor's sense of urgency for a treatment (instead of being cured on account of the (obvious) symptoms). In both cases, either you are satisfied, or you have to go elsewhere. Yet in reviewing, you cannot even complain.

If you happen to get the same proofreading services spam as I do, you might know the dramatic description of that situation and its ubiquity (here's a passage of an email I got today, cited without reference, of course):
<drama>

> [t]hen the email message you had been awaiting for months arrived from the editor of the top-tier journal to which you submitted your latest paper, and his or her response to your work was not at all what you expected. Instead of commending you for writing such an excellent article and informing you that it would be published in the very next issue of the periodical; instead of even a less positive but nonetheless workable request to revise certain aspects of the paper so that it could be accepted or at least seriously considered for publication, he or she simply rejected your submission. It was a moment of personal and professional deflation, one in which you began to think that the time and energy you invested in the article might have been wasted, and perhaps one in which you felt utterly alone, as though you were clinging with breaking nails to the bottom of the academic or scientific totem pole.

> First and foremost in such a challenging situation is the need to recognise the consoling reality that you are definitely not alone. Rejection from publishers is a far more common experience among authors than acceptance is, and each one of those authors, no matter how confident and experienced, feels overwhelmed to some degree by a wave of disappointment when a rejection letter arrives.

</drama>

Getting serious again, I myself have been a reviewer oftentimes. Besides that, almost all of my articles have been finally accepted, so there is no categorical cause for resentful behavior. Accordingly, although I am mostly involved in what I am going to write about, this is not (or at least only remotely) about anger and lamenting, but about curiosity (how can this be?) and reporting. I must admit, however, that the difficulties I encountered with the publication of interdisciplinary papers in general, and with one of my recent papers in particular, were the inspiration for writing about reviews at all.

I am aware of the fact that talking about reviewing is a communicative taboo area: those who are established typically don't (want to) care, and those of the rest typically can't afford to raise their voices. You will rarely hear something like "Wow, got some great reviews of my paper yesterday!", and neither will you hear lamentations like "Oh dear, my paper was rejected for the fifth time!",[1] for obvious reasons, not to mention details of some reviewing process. I will break this taboo, hence you will also find honest examples of reviewing cases in this text, with both positive and negative reviewer comments. Please forgive my trying to strike a balance between the different kinds of impressions this may evoke.

So, unlike Tom Petty, who in an interview about his rebelling against his record company said "Little did I know that this is something you do not do"[2], I do know. That's what I do, I write and I know things (as Tyrion would say). Even if it will appear to some as a Don Quixote-like tale of vain rebellion.

I am also fully aware of the fact that others probably have much more to tell about this scientific everyday phenomenon.[3] But then, why should I care?

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[1] Except for ironic reports of negative review(expression)s that can be found on https://twitter.com/YourPaperSucks (thanks to Elke Hentschel for pointing me to this thread).

[2] Tom Petty and the Heartbreakers, VH1 Behind The Music, 1999. I've been a fan for decades, and now he's gone. Some people are loved and even get famous for being rebels. RIP, Tom!

[3] ... and even write papers about it, for example, G. Cormode (2008), How not to review a paper: The tools and techniques of the adversarial reviewer. *SIGMOD Record*, 37(4):100-104; Mark Allman (2008), Thoughts on Reviewing. *ACM Computer Communication Review*, Editorial Contribution, 38(2), April 2008.

And finally, this text is at least *supposed* to be fun or entertaining (even if some reader will disagree on that point), and has a subjective, personal point of view. For some neutral/informative overview on "peer reviewing", you might simply look up the corresponding Wikipedia entries.

1 O journal, where art thou?

Everything we're going to talk about starts with the selection of a high-quality journal for publication. This is probably easy if the topic of your article perfectly fits some journal's aims and scope. It gets more difficult if you're spoilt for choice between more of them. It gets tricky if you don't find a journal (at once) and have to adapt, either in the presentation of your article to some journal's editors or even in your article's content, or by making concessions to the desired journal's quality or scope. And it gets nasty if you really don't fit, for example because you are working interdisciplinarily (partial fitting at best) or your stuff is new, not matching the basic assumptions of any journal at all. I am especially interested in these two latter options.

For decades, I have been working in the interdisciplinary field of cognitive science. To give an impression of what that is, see the text from a leaflet I designed some years ago (actually, as of the time of writing this, the text is more than 17 years old and still used –only slightly modified– for advertising the corresponding course of studies at the University of Osnabrück):

> **More than two millennia ago**, philosophers began to think about the properties of mind and matter.
> **A few centuries ago**, the idea to view thinking as mental calculation arose.
> **With the advent of computers in this century**, this idea could be realised.
> **In recent decades**, members of various disciplines have collaborated in studying mental processes and representations and in trying to build intelligent systems.
> **In the last few years**, it has become obvious that it is also necessary to study the brain and its neural architectures.
> **Now**, Cognitive Science has emerged as an 'inter-discipline' concerned with the scientific study of mind and brain, the explanation of human linguistic and non-linguistic behavior, and the building of artificial intelligent systems. It comprises central aspects of the following disciplines: anthropology, artificial intelligence, (neuro-)biology,

> computer science, (computational) linguistics, mathematics, (computational) neuroscience, philosophy, (cognitive) psychology.

Working in Cognitive Science means more than just working in one or two of the participating disciplines. Ideally, someone working in this field has basic, multidisciplinary knowledge about the fundamentals (problems, goal, methods) of *most* of them. It also means that one is investigating a topic crossing the disciplines whose problems can be better solved from an interdisciplinary perspective.

As to journals, they mostly reduce this complexity by setting a narrower disciplinary or thematic focus. You're fully right if you believe that this may lead to problems in publishing an article. Here's an example: I first submitted my paper about *Cognitivist ontologies* to the journal *Cognition* and was rejected because of the following reason given by the editor:

> Although the work does afford theoretical constraint, the work is not sufficiently broad to be accessible to the general readership - I believe that the audience at Cognition is not the right one for this work, and that this should be submitted to a journal that has closer links to information science, philosophy, or theoretical linguistics.

Then I submitted it to the quite specific journal *Applied ontology* (which, if you happen to know it, is in the intersection area of information science, philosophy, and theoretical linguistics) where it was rejected for many specific, but misleading, reasons. It was finally well accepted in *Cognitive Processing* (!).

2 Tales of the Unexpected: Breaking bad

I don't remember my first scientific review. I only remember my first *important* scientific review. It was definitely a good one, but it had bad consequences. That is, I tried to be objective (though not necessarily polite) by not only emphasizing the bad aspects of the reviewed paper, and elaborate by commenting all identified mistakes and not just bashing the paper on the basis of some. This turned out to be a mistake, and I would certainly write it differently today, given the experience I have now.

There's a background story that starts with a talk I gave at a European Artificial Intelligence conference where I presented a précis of my master's thesis. I met young colleagues who invited me to their institute's colloquium for an extended presentation. A seemingly nice guy, let's call him Walter Matthau, who had just gotten his informatics diploma, asked for a copy of my thesis, which I happily sent

to him. He was "raised" in a top-notch research group, so his interest rather flattered me. And as I said, he looked like a good guy.

At that time, I had become a member of the reviewing board of a thematically related conference. Guess what happened. A few months later I got a paper to review, written by this very guy. As can be expected, the structure and the style of the text was highly professional. The content, however, was an unbelievable impertinence. For the most part, he presented information from my thesis and cited the corresponding sources (mixed with others). Awkward passages, strange misuse of terms, mispresentations of content, and typos (e.g., of journal names) showed, however, that he had just copied without understanding, perhaps even without reading the cited papers (this being an instance of plagiarism). Furthermore, although the additional, "unique" aspect of his paper was an eye-catcher, his theoretical proposal was not more than a loudmouthed promise of things he wanted to develop.

There was an accordant rating scheme (structure, content), and I gave an A and an F, correspondingly. I should either have been dishonest (downgrading the structure part) or else explicitly disqualify the paper. Instead, with me the only expert for judging the faults and with the other reviewer(s) probably impressed, the paper was accepted. Sometimes it is indeed appropriate not to mention or highlight positive aspects of a paper.

There is another great disadvantage of such benevolent "good" reviewing, which I became aware of only afterwards. In listing all faults and (inferrable) corrections in my comments on the paper, I helped to iron out its weak parts and so improved its quality substantially. As a reviewer, you may not be thanked for that (I, for one, wasn't). And if you are a competitor, this may just be a silly thing to do.

This small mistake in reviewing had a tremendous effect on my career: Walter became the new expert of the field, and although I had moved to other research topics anyway, my just beginning "fame" in that field was swept away. He had, by the way, managed to only mention me in a footnote, only with regard to a minor aspect, and only derogatively. But it was somehow fun[4] to see (and remark) that he had cited a totally irrelevant paper (the author happened to have my surname). Here's the corresponding excerpt of my review (the full annotated text of the review is here):

- I don't know whether H. Carstensen with his paper on 'The Complexity of Testing the Equivalence of Transition Sequence' is relevant for this topic !?!
- I don't know the journal 'Environment and Behavioral' (->Kuipers)
- I don't know the journal 'Environmental and Behaviour' (->Leiser/Zilbershatz)

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[4]Isn't it ironic?

Suffice it to say, after some more loudmouthed papers, Walter grew quieter, eventually presented his dissertation thesis (scarcely anyone talks about), left the field and later got a professorship in an entirely different discipline.

My bad.[5]

3 How to get away with a million dollar (project-) baby murder

There was a time when one project I had worked in ended and the other (an expected follow-up) didn't come. We had connections to groups in other disciplines and eventually came up with the idea of applying for some *real* money, that is, to get a grant for a big interdisciplinary project (roughly, a million dollar amount). As I had not much to do, I was the one who gathered all information and wrote the proposal (and no, the picture of the bird wasn't my idea).

The topic was scientifically located in my old research field, so writing somehow felt like coming home. I managed to find a theme for the project that not only embraced the expertise and current work of the participating groups, but also presented a new and interesting research perspective. In a nutshell, the goal was to build a system that would produce multi-modal route descriptions of different granularity adapted to the requirements of a user in a certain situation (as opposed to rigid, context-free, off-the-shelf standard texts). Combined with our intention to emphasize the possible synergy of our collaborative work, this seemed to match the grandeur the application was expected to show.

On the other hand, it was somehow clear that this was more a let's-try-to-get-some-money patchwork thing than a product of a grown collaboration, and that this might shine through. With only scarce experience in writing project proposals (let alone of that size), I was unsure and quite pessimistic about whether I had found the right level and tone between textual bravado (see, one *has* to show off a bit to compete with fellow-applicants) and self-fulfilling prophecy of rejection. So when we finally found ourselves devoid of acceptance, disappointment should have been kept within a limit.

Beforehand we had been allowed to supply a list of possible reviewers. Our first reviewer (which I recognized easily) actually was one I had warned the others to

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[5]A colleague tells me that something even worse once happened to him: the paper he had submitted to a conference was rejected with strange justifications, yet to his astonishment and annoyance, he found a Walter-Matthau-style adaption of his text in the proceedings of the same (!) conference, authored by another person.

put on the list. He was highly competent and respectable, but as a psychologist, his level of interest in fine-grained testable assumptions was simply incompatible with our multidisciplinary research methodologies in this big project. And so it came. His review was decent, highly professional, objective (weighing pros and cons) and said what could be expected: that our proposal was very interesting, cutting-edge research, but a bit too ambitious and complex. As could have been expected, too much bravado for him.

But I definitely did not see the second reviewer coming. I am not referring to most of his little one-and-a-half-page text where he complained about being given too little information about details, which was named as one reason for rejection (you must know that the proposal had a maximal length of 20 pages that we fully exploited . . .), or to his non-interdisciplinary 'think small' viewpoint. I mean his text as being an instance of a more general review phenomenon.

What does a reviewer do if he has not much competence and/or not many good arguments for a rejection (which he has categorically chosen for some reason)? He tries to emphasize faults (although they are small and less relevant) in order to make the paper/proposal look bad! And that is what he did. The annoying part, however, is that it was unjustified to do so, hence, that he was wrong! The following is from a frustrated e-mail I wrote to my colleagues afterwards:

> one detail is especially important: we are accused of citing a source that allegedly gives incorrect information ('the quote on p. 4 from Höök is incorrect') and are given references we should have read. HOWEVER, THE REFERENCES ARE FROM 85,86, AND HÖÖK IS FROM 91 AND HAS ALL OF THE MENTIONED REFERENCES IN HER BIB-LIOGRAPHY!!!

Again, there is a fun part (see Fn. 1) of this story. We had to decide whether to retract the proposal or not within a week. Unfortunately, the results were put in a dead letter box (!), so we/I didn't get the reviews in time. We therefore rather retracted the proposal.

So we had planned a natural language (!) processing (!) system development (!) project and were rejected by a psychologist and a geoscientist (whatever exactly) who very likely had little experience with natural language processing system development projects. Although we presumably wouldn't have vetoed the second review anyway, this left a bitter aftertaste for me.

With respect to reviews in general, this points to the importance of the reviewers' competence in and attitude towards a certain research field. Every intelligent person is able to produce a reasonable opinion piece on request. But would you really ask a vegan for his opinion on the taste of a certain Wiener Schnitzel made of veal?

4 Parad(ise/igm) lost: The butterfly effect

Science is in various respects like religion. The god prayed to is analyticity, and the religions, which vary in space and time, are the scientific paradigms.

In a paradigm, some basic beliefs need not be questioned. On the one hand, this has many advantages, as intellectual effort can be freely and productively brought to bear within this range. On the other hand, however, these beliefs *must* not be questioned, and any violence of this rule corresponds to a heresy that is to be punished.

Like a religion (or even, a sect), a paradigm provides a home to its adherents, and they behave correspondingly: other members of the (peer) group are treated friendly as guests, strangers are treated with suspect or, if identifiable as outsiders, with intolerance, refusal or even hostility.

All this is as natural as the usual social behavior in the kindergarten. What is often overlooked, however, is that both science and religion undergo changes and developments, which are no less "natural" (a fact that adherents to some paradigm will probably be reluctant to admit). From this global point of view, it is quite obvious that a lot of injustice is involved here: without proponents of different ideas there will be no change, but those who propose a change are heretics in the current paradigm. They may be vindicated sometime later, but for most of them this never happens (or perhaps too late). In such a situation, one can only hope to be the butterfly heretic affecting the paradigm weather who becomes the one cited/famous for apparently having caused the storm/change.

As to reviews, which somehow exemplify this global scientific phenomenon, an author may be affected in this very respect. He/she may not share the basic beliefs, or may present results that contradict them, or he/she may only be suspected of either one of these conditions. Many papers are probably not rejected because of their objective (lack of) quality but merely for their inaptitude to the current paradigm.

It is certainly hard to tell ignorance from reluctance, but with some (bad) luck one can sometimes virtually smell the fear of editors and/or reviewers of losing their paradigm (their paradise) as it is. No further proof needed for why this in most cases ends badly for the author (although fortunately not at the stake anymore).

5 The silence of the lambs: Highlander

Let's talk about traumata. In real life, a politician might be bashed because he gave a frustrated, but honest interview while his lying, smiling opponent wins the day. A

singer might be booed because he/she didn't strike the right note in spite of having given a good concert overall. Somehow, life is not fair in general, and there are many reasons why someone dies of stomach cancer or becomes drug-addicted, a superhero or whatsoever.

In science, every unjustifiedly[6] negative review can be a traumatic experience. The best way to handle this (probably practiced by most) is the "whatever" method: simply ignore and go on.

I have opted for a slightly different resort, for the following reason. In case of acceptance, an author is asked to consider the comments of the reviewers, revise the article accordingly, and give a report of what was changed, e.g.:

> If you can submit a new manuscript to us, please provide us with a careful discussion of how you have addressed the comments [. . .]

No converse procedure (assessment of reviewers' comments) is envisaged in principle.[7] I simply use to ignore this fact and send back annotated comments, asking the editors to send them to the reviewers.

I know this is like asking for a doggy bag after a meal in a star-rated restaurant, and the editors use to sniff at that or complain, and of course (politely say that they) refrain from doing so. But it sends a clear signal about the editors' choice or the competence and thoroughness of the reviewers that might lead to an overall improvement. And it keeps me in control and sane, of course.

Once I was so bold to ask the editors to reconsider (I normally try to avoid this) because of the sheer mass of 'mis'es in the reviews (*mistake*, *misunderstanding*, *misinterpretation*). I got a polite reply on my re-comments that tried to give a justification of the reviewing process as it is, and in which it was stated that reconsideration was inconceivable. I found all that unconvincing. This is what I (imperfectly) wrote back:

> From an author's point of view, I would then kindly ask you to instruct your reviewers to not come up with statements (in the first place) whose necessary revision afterwards has no impact.

So, if I end up as a scientific lamb getting butchered, hear me scream, and watch out! This won't help at all, of course, but it's better to burn out than to fade away...

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[6] I am not talking about the trivial case of bad papers(' reviews) here, of course.

[7] with the exception of conferences who collect authors' ratings of reviewers for granting a *Best Reviewer Award*.

6 And now for something completely different

Is there a difference between reviewing *student* papers and reviewing *journal* articles? In my opinion, there isn't. It's all about competence and the quality of comments, everything else being entailed. In both cases, therefore, authors should receive high-quality comments on the paper covering all relevant aspects.

In reality, there is a big difference, however, as modern peer reviewing for journals unfortunately has some negative aspects. First, the reviewing process nowadays is mostly automatized via some Web-editorial manager. From the beginning, this puts up a barrier between author and editor and hinders loyal communication. Second, usual reviewing is double-blind. Originally, this was probably meant to serve objectivity. Effectively, however, it primarily leads to opaqueness: Editors cannot be called to account for their choice of reviewers, and reviewers do not need to pay much attention to the quality of their texts. This not infrequently results in textual berserk behavior, because nobody cares (except the author of the reviewed article). I simply hope that this does not happen in my/your communication with students.

Interestingly, some journals return to non-blind reviewing in these years. Correspondingly, author and reviewers might interact openly, the reviews might be posted on the web, and/or the reviewers might be named in the published article. As a result, only people feeling competent in a certain field should accept an invitation to review, and reviewers will certainly watch the quality and the style of their comments (more closely). Based on my experience, I can definitely recommend this procedure.

Obviously, I won't talk much about *conference* paper reviews here. It is well-known that every Tom, Dick, and Harry may get selected for this task, and I have evidence that this reduces review quality substantially. When I tried to present the central results of my dissertation thesis to some pertinent interdisciplinary conference, I was rejected, the reaction roughly being "what's that/new?", "don't understand", "not good enough" or the like. For the record, the same stuff has been published later in journals, even though only when the time was more ripe for it. And yes, that made me very angry.

By the way, I do not claim to have always given good conference paper reviews. They are sometimes a pain in the proverbial... But I have proof that I did so at least once or twice ;-):[8]

> Lieber Herr Carstensen.
> Vielen Dank für Ihre präzisen Gutachten der COLING-Proposals, sie

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[8] An email from 1998 roughly saying 'well done!'. You might be interested in the vita of Helmut Schnelle.

> sind sehr wichtig fuer die Entscheidung. Ich wuerde mich freuen, wenn
> die anderen Gutachter auch nur annähernd so prompt wären wie Sie.
> Mit besten Gruessen,
> Helmut Schnelle

While we're at it, there are also *book* reviews, which constitute an entirely different sort of text. I once reviewed a book (a festschrift, to be precise) about the perspectives of computational linguistics containing more than 50 small opinion pieces. Quite unreviewable, one could think. Here's the reaction of one of the authors of the book:[9]

> Lieber Herr Carstensen,
> ich hätte nie gedacht, dass man dieses Buch ernsthaft und konsistent rezensieren könnte. Das ist Ihnen in bewunderungswürdiger Weise gelungen: informationsreich, kohärent, kritisch, weiterführend und gut lesbar. [...]
> Mit freundlichen Grüßen
> Ulrich Schmitz

Interestingly, my last published text (at the time of writing this) is a 6000+ words (!) long review of an introductory textbook (preprint)(its English translation).

7 The good, the bad & the ugly (the Walking Dead)

I have found out that there are three kinds of reviews: the good ones which make you happy, the bad ones which make you sad, and the very bad ones which make you angry.

Of course, those leading to acceptance almost always belong to the good ones. Interestingly, negative reviews may sometimes be categorized as 'good', too. In that case, I am happy about the competent feedback I get, the effort that is spent or the empathy that can be felt (despite the article's shortcomings).[10]

There is not much to say about the bad ones. I get sad either because of me (unhappy) or because of the reviewer (dismayed)(inclusive *or*, of course). As to the latter, there's often these 'mis'es again (plus *mischief*, *misery*, and miscellaneous). As far as I am concerned, bad reviews are neither fish nor fowl, and the sadness appears to me as a rather boring feeling, corresponding to being in a limbo. Whatever that exactly is, I suppose it has the same 'you cannot do anything about it'-air.

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[9] In a nutshell: 'Wow, you did it, and quite good so!'

[10] The converse is true for positive, but incompetent reviews.

Reviews of the third category are definitely different. I love to hate them. Compared to the dull limbo, this is like being in hell, cracking the skull of every zombie that comes running to you (no, I'm not a 'Walking Dead' fan). Speaking of zombies, very bad reviews can be *that* ugly. They may either have deficiencies in what should be there (virtues) or show abundance in what should not be there (vices).

8 Seven samurai: Fair game of thrones

It is not so easy to get a grip on what the virtues are that a good reviewer should possess, apart from what one should always expect (intelligence, competence, skill). After looking for a while I found the seven virtues of the Bushido code, six of them being relevant here (with *honesty* being a too general virtue).

Rectitude/Integrity simply means 'always do the right thing'. For a reviewer, this amounts to giving an objective estimation of the author's article and a thorough and clear justification of that estimation, all based on competence and close reading.

Courage is needed when there is a conflict between the reviewer's knowledge and the article's results, with the latter being correct. The courage then consists in admitting this fact instead of obfuscating it by focusing on irrelevant aspects leading to rejection.

Benevolence can tip the scales if there is no conflict, but the author adheres to a different theory, paradigm or the like. Reviewers can make authors look bad, but should not do so.

Respect/politeness should be a prerequisite for all social interaction. It is easy to ignore this basic aspect in double-blind procedures, though.

Honor becomes relevant in the reviewing function per se. A reviewer has earned it and must (according to the code) enjoy it. A reviewer who is reluctant to fulfill the function wholeheartedly and earnestly does not show this virtue. In that case, this person might forget the important role she has in the scientific system and might not act adequately.

Loyalty is relevant in the sense of giving the author as a colleague an unconditional chance in the process. Here, it may lead to conflicts, though, as the reviewer must also be loyal to his journal. In that case, this may not be beneficial for the author: an article could be rejected not because of its quality but because the reviewer thinks (perhaps wrongly) that it does not fit the journal's aims, scope or addressed scientific group.

To be honest, these virtues seem a bit redundant to me. In our context, one simply expects reviewers to be *fair* in the game of ~~thrones~~ publications.

9 The unexpected ignorance of virtue

Unfortunately, reviewers often ignore the mentioned virtues and rather tend to the other end of the scale of measured morality. Correspondingly, one can approach the discussion of review quality from the question of which 'sin' is committed in each case. It is interesting that even in the 2900+ year old biblical sins below, there are already a few applicable ones.

> Proverbs 6:16-19 King James Version (KJV)
> 16 These six things doth the Lord hate: yea, seven are an abomination unto him:
> 17 A proud look, a lying tongue, and hands that shed innocent blood,
> 18 An heart that deviseth wicked imaginations, feet that be swift in running to mischief,
> 19 A false witness that speaketh lies, and he that soweth discord among brethren.

A *proud look* corresponds to the pride (or hubris) a reviewer may show if he (always) regards his competence as superior to the author's one. One has to keep in mind that the author can also be expected to be competent. Compare this situation to a speed car race. Here you do not get the pole position just by having won a race sometime before. You have to earn it anew each time! Accordingly, a reviewer should always check whether he really has this position or whether he is one of those behind. There is no maximum-competence guarantee just by being a reviewer. There is, however, some chance of becoming an arrogant prick.

Devising wicked imaginations is more or less a paraphrase of misreading or misinterpreting the article. The resulting allegations are disastrous, because once on the road of misunderstanding, the reviewer will quite probably never say something like 'in spite of that I recommend acceptance', and there is no means to object.

The fifth and sixth ones are the equivalence of judging/rejecting too quickly and/or without reason, i.e., despite author's correct results and argumentation. The final sin points to the inevitable social consequences bad reviews produce: in small-scale scope, this is the frustration of the author (remember, we are talking about reviews that make you angry), in large-scale scope, this may lead to 'paradigm wars' where articles of one paradigm are not published in journals of the other.

Among the *deadly sins* mentioned later in religion history are further vices: there is *Greed* if the reviewer's decision is influenced by thinking about his own advantage. *Sloth* can be found in the omission of looking for, respecting or presenting positive aspects of author's article. *Wrath* underlies the reviewer's flying into a frenzy because of minor faults (under the presumption of wrongness of the article,

although its main thrust is in fact correct). *Envy* may be the reason for not supporting an article that presents more elegant, practicable or general solutions than reviewer's own theory.

At the end of the day, it is probably a mixture of ignorance in detail (even despite competence in general) and slothfulness that is at the core of a bad review. Having some more of the others will make it an ugly one. Rest assured, if all this is paired with small-minded, arrogant nit-picking, it will drive you crazy.

Again, I don't know whether I myself always live up to the expectation of being virtuous, not vicious, in reviewing. See for yourself, here's a recent (anonymized) review of mine.

10 Pride and prejudice

Let me add another perspective to the aforementioned points. There is no doubt that *skepticism* is one of the basic principles of scientific procedure in general and of reviewing in particular. It is simply right to be careful in judging new proposals and hesitating in approving them. Unfortunately, there is reason to assume that this beneficial principle may take on its own life and morph into a loathsome trait of some reviewer/editor, for the following reasons.

First, everyone in any profession undergoes some character development: with your work done, your positions filled and your experience gained, there is a natural change in self-esteem. If positive, you will probably get proud of what you have done and become. Nothing to object to that, of course.

Second, while you may have become successful, the number of your tasks and functions will have increased. Accordingly, this reduces the available resources for every single part of your agenda.

Third, with increasing work load and time gone by, you may have missed some developments, as well as the fact that some of your favorite theories may already be outdated.

Now apply all of this to reviewing. I have observed so often that experienced people judge too quickly, because of self-confidence, lack of time, or half-lifed competence. As reviewers, such people may therefore be skeptical about a paper they did not understand, yet not for scientific, but personal reasons. Here we have the *proud look* from above, where pride turns skepticism into prejudice.

While the foregoing description evidently applies to the aged researcher, I just realized that a similar mental attitude may also be adopted by young researchers. They probably have dived deep into some topic, have written a complex dissertation

thesis, have been asked to give talks and write reviews and so on. Being proud of what they have achieved, however, they may not (yet) realize how much they do NOT know, and therefore act inappropriately. I just hope that I have not been one of that kind 20-30 years ago, although I fear I was. Unfortunately, while everything has its costs, arrogance comes for free. You just have to pay for it later.

11 The edge of tomorrow: The time machine

Did you ever ask yourself why doing a top-notch job in science is called *cutting edge research*? A simple answer could be: if you're doing it, you're on the verge of success, but you may also be cut off from opportunities, money, success etc.

It is interesting to observe that reviews often do not reflect this situation. Both editors' and reviewers' behaviors often suggest that the author is sitting comfortably in a streetcar named desire and that he may just take the next one after rejection, here is an example:

> I would like to thank you very much for forwarding your manuscript to us for consideration and wish you every success in finding an alternative place of publication.

This does not quite mirror the reality of the author having a hell of a ride on the road to success, with ample chances of missing the boat. After all, the whole writing-and-reviewing *is* being at the edge of tomorrow, as the author (unlike the time traveller in one of my favourite novels/films (guess which one)) hasn't got "all the time in the world". Reviewers may actually be the ones doing the cutting at the cutting-edge in research.

Another important aspect of the cutting-edge metaphor is: it may hurt.

12 The waiting . . . : the constant gardener

Wait, there is another aspect of time in reviewing: you never know how long it takes.[11] While the author is keen to get a quick response, the reviewer will probably put the paper on his/her stack.

However, there is something wrong with the use of the term *stack* here. In information technology terminology, a 'stack' is a last-in first-out storage device, like a pile of plates, where you can only access the topmost one at each point in time. If this sense were applicable here, the reviewer might actually grab the paper from the

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[11]and yes, the pun (in the title) is intended.

top of the stack, which corresponds to some chance of eventually getting a quick response.

This, regrettably, almost never happens, as far as I know. Usually, reviewers exploit the time of deliverance (around four months), until/unless all older tasks have been accomplished.

We therefore have to view the situation differently: rather than being put on a stack, papers are put on a pile instead, which functions as a *first*-in first out storage device (i.e., a 'queue' in IT terms). That is, papers to be reviewed sink down in the pile of to-dos of some reviewer like some rotten banana on a compost heap used in gardening. The question is: will the reviewer treat a paper as irrelevant waste, or as fertile humus for its scientific domain?

Back to the waiting: it can take a long time. And just like with restaurants: you are **not** allowed to send your order to two or more providers at the same time and take the first satisfying service, of course.

13 The judge

Now let's talk about the editors. They are responsible for the rough estimation of fitting ("screening"), the determination of the number of reviewers, their choice, and finally, for the ultimate decision based on the assessment of the reviews. I'd never had thought how much can go wrong with just that.

First of all, the judgment of whether the article accords with the aims and scope of the journal. Here, editors may, especially in interdisciplinary contexts, simply not be competent for an adequate assessment of the submitted article. Above I mentioned my *Cognitivist ontologies* article that was rejected for *Cognition* by the editor but accepted for *Cognitive Processing* (trust me, there was no specific focus on processing in that paper, and I knew *Cognition* for 20 years). The editor was a psycholinguist (psycholinguistics typically has a quite narrow focus within cognitive science). I do not blame him for not knowing enough, I blame him for not *realizing* he didn't know enough and for not at least granting me further opinions.

Secondly, there is the number of reviewers. One might guess that for minimum regard to objectivity, at least two reviewers should be assigned. This would mean: a clear decision if both judge either good or bad, no immediate decision if there is a tie between a good and a bad review, and a less clear decision in most other cases (few of them positive). In the second case one would probably opt for a third opinion.

Reality does not seem to conform to this nice model, however. In two cases, I was granted only one reviewer, and both of them judged negatively. So when the articles were rejected, this happened on the basis of one opinion each. *Honi soit qui mal y pense*, you might say: the editor probably added his own, and rightly so. Unfortunately, though, I never saw a second review!

There was another case where I got *no* review (after submitting to *Journal of Semantics*). The editor, let's call him Calvin Klein, at least provided me with an elaborate explanation why he wouldn't consider granting me any.[12] Again, it is in his sovereign territory to do so, in principle. But what if I tell you that this was a resubmission (editors had changed in between), an option that I had been explicitly advised to consider by the previous editors (accordingly, the *Cognitivist gradation* piece got published later)? Here's what they had written:

> Rethink and rework the paper, and submit it again at a later stage. You should be aware, though, that if you choose to do so, it will be considered a completely new submission (and would likely be assigned a different handling editor who would probably assign it different reviewers). I would NOT recommend, however, submitting the paper again without taking into account the comments of the current reviewers.

And what if I tell you that the theory of Calvin was explicitly criticized in my article? *Honi soit qui ne mal y pense pas*.

Thirdly, the choice of reviewers. To be sure, this *is* a difficult task, and you probably don't want to be in some editor's shoes. Recently, I submitted an article to *Mind and Machines*, an obviously interdisciplinary Cognitive Science journal. I was happy to see it passing the editor's hurdle and waited curiously and patiently two further months. I had adapted to the journal's aims and scope and was quite optimistic that I had managed the balancing act between the theoretical/abstract and the practical/technical in my paper.

When the rejection came, I therefore was quite disappointed. The reviews were neither good nor ugly, but limbo-bad. There was nothing to learn from or to be angry about, the editor had simply chosen (the) wrong experts. More specifically, he had chosen monodisciplinary experts from the participating disciplines who showed a quite complete lack of understanding of the overall picture (see the appendix for more).

Finally, there is the decision of the editor. In most cases, the constraints given should make it easy. That is, all bad papers would be rejected, as would all unclear papers, because of the, say, 200 submissions, perhaps only 12 can "survive". The

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[12]A colleague tells me that things can be much worse: after eight months, her paper was rejected (without further reason or explanation) simply because the editor found no reviewers!!

real work probably lies in the choice from the good papers in most cases. As an author, I therefore could understand and accept the message "hey, your paper is good, but there are too many better (fitting)".[13]

Kenneth Church (in an opinion piece about reviewing in Computational Linguistics)[14] has quite nicely put to words that the decision should not simply be based on the impression of non-badness of the paper and the unanimous consent of the reviewers:

> Controversial papers are great; boring unobjectionable incremental papers are not. The only bad paper is a paper without an advocate. A paper with a single advocate should trump a paper with lots of seconds, but no advocates. Don't average votes. The key votes are the advocates. Negative votes matter only if they convince the advocates to change their votes.

It is the editor who is responsible for a corresponding procedure in reviewing. Yet there seem to be cases where he should (from an objective point of view) check some review and act correspondingly, but does not.

For example, the same article I just talked about was previously submitted to *Linguistics and Philosophy*. The editor, let's call him T.E.D., not only granted me just one reviewer, he furthermore chose a linguist with an extremely narrow focus. To understand what I mean recall that *Artificial Intelligence* (AI) is part of the interdisciplinary *Cognitive Science* (and my papers are typically written from that general point of view), and that it is an autonomous (sub-)discipline of informatics just like nuclear physics is for physics or dermatology is for medicine. Here's what the reviewer wrote about some part of my text:

> It is just standard AI–a combination of show-'n-tell, bravado, and caricature of everything in sight, opponents and natural language. [...] Nothing in AI is worth advertisement unless its improvements on its predecessors are cosmic

Do you get the craziness of this assertion? It is a public-opinion prejudice, it is an insult (both to me and to the discipline), it is wrong (I'm not Ray Kurzweil, am I?), it is thoroughly unscientific.[15] As an editor, I would be ashamed of having selected

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[13]I am aware of the fact that this conception probably does not match the usual reviewing procedure.

[14]Kenneth Church (2005), "Reviewing the reviewers" , *Computational Linguistics* 31 (4), 575-578.

[15]It is also in stark contrast to the detailed and differentiated picture on language-related AI I try to give in my German introduction to language technology.

But of course the reviewer didn't know that. So much for anonymous reviewing.

It also ignores the fact that I have worked on semantics all my life (although interdisciplinarily). That's why I just published a review of one of the world's best introductions to semantics: Carstensen, Kai-Uwe (2017), Rezension von "Löbner, Sebastian (2015): Semantik: Eine Ein-

him and would never base my judgment on his opinion. But the editor didn't check, didn't reconsider, not even answered my email. Although I promised him that I would go public with that. So here we are.

You wouldn't expect that this can be topped, do you? Well, when I politely asked the *Mind and Machines* editor to send my re-comments to the reviewers, I got the following answer:

> I would do you no favor, and indeed would do you harm, were I to honor your request to pass your comments to the reviewers. That I shall not do.

Let's put it that way: there is a whole scale of possible reactions here. At the upper end, something like 'Oh, seeing your comments, I realize that you must be unhappy about my decision. I am sorry. I will certainly do what you ask me to.' In the middle, simple ignorance. His answer, however, is –despite its appealing diction– definitely at the lower end. It perverts the situation of reviewing my paper: bad reviewer choice, bad reviews, bad decision, but the blame is on me if I object to all that. Here's my reply:

> thank you for your answer. But honestly, I do not understand the part 'would do you harm'. As the reviewers were quite polite, I stayed polite, too. And the harm is already done to me.

14 Close encounters with the third (kind) reviewer (a most wanted man): Pöppel's eleven

Sometimes you get more than you think is necessary: a third reviewer. Some situations I recall in which this happened to me turned out to be quite different. The first (conference!) paper was accepted although (as I found out when rummaging around in my files) it was probably a close colleague who gave the worst rating. I know exactly that this was not his field, but he definitely didn't want to put himself in the 'I know this guy and give an OK rating' corner. Phew! Thanks a lot, you (other) guys!

In the second case, the article was finally rejected. It was my only non-blind peer review, so I knew the colleagues and how they voted. Everything was nice despite the rejection (and in spite of my re-commenting their comments), so I did not only

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führung. 2., aktualisierte und stark erweiterte Auflage. Berlin/Boston: DeGruyter", *Linguistische Berichte* 250, pp. 247-262 (above mentioned preprint in English).

But of course the reviewer wouldn't have known that. So much more for anonymous reviewing.

tell them when the article was accepted in another journal, I also thanked them in the Acknowledgments. For the one who gave me a positive rating, this was probably some confirmation, too.

The third reviewer in the third case (the first gradation paper submission) belongs in the 'ugly' category. I must admit that the article may not yet have been in best shape at that time (and was correspondingly rejected), and it certainly benefitted (and its later publication certainly also resulted) from all the comments I got.

Anyway, how he/she phrased his/her comments (he/she must have been a big shot) while showing complete lack (of want) of understanding in detail still gets me groping for a club... You don't want a review starting as follows (as it did), do you?[16]

> Unfortunately, the paper cannot be published. The development of ideas is premature, and the formalism and discussion is full of errors. Also the structure and presentation are lacking, thus the goals the author wants to achieve are not clear.
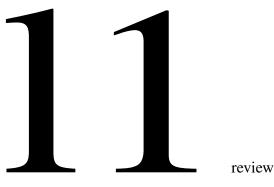
I should definitely publish this on https://twitter.com/YourPaperSucks. And by the way, I was happy and relieved to find there the letter of Hunter S. Thompson to Anthony Burgess concerning the latter's submitted text. It is as harsh in judgment, but even worse in wording. Google it, I won't cite it here.

Interestingly, I was once a third reviewer myself. Apparently, there had been a tie, and the journal needed a third opinion. I made very clear that for me, rejection was the only option. Later, however, I was informed about the decision: accepted with "Major revisions needed". And I saw that they had asked the opinion of a fourth reviewer, who was more generous than me. Hm. Who wants what/whom here most?

As to the quality of the two, three, four or even more reviews let me cite Ernst Pöppel, a renowned neuroscientist, from his web page (last visited 2019-04-19):

> One of the best paper[sic] I ever published together with some colleagues had 11 reviews before it was accepted for publication.

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[16]This is the case where I complained and asked the editors to reconsider, by the way.

11 reviews!!!

15 A Rogue gOne/Ant Man: Apocalypse now

Ok, here's another case with three reviews: one of my papers had a very good one, a good one, and a "revise and resubmit" one. The last reviewer honestly said: "the paper cannot be published in a reputable journal without substantial reworking". This is evidently someone who needs to emphasize the quality of the journal he/she is a reviewer of. How arrog Ant.

What if I tell you that 90% (really!) of his/her main arguments were easily refuted in my re-comments? Apart from apparent misunderstandings it turned out that he/she had a quite narrow conception of science, of the journal's paper structure, and of the corresponding presentation of content, including some prescription of what I should write about. Interestingly, the editors adopted the view and put me on "major revisions". This is where you know that you met one of the big shots of the journal's community.

And that, in turn, is where I go rogue. Pointing to the other reviews and the refuted arguments, I insisted on "minor revisions" status (or else, I would call it a day). Hm, did you ever see how an X-wing fighter is rejected when smashing on an Imperial Star Destroyer? No harm done if you Ant icipate that, though.

This is clearly another case of the presumptuousness of thinking that one's beliefs are always correct, and whether this is or should be regarded as legitimate. Now, imagine someone on the streets announcing the apocalypse ("the end is near"). This may have been regarded as normal or even respectable in medieval times. Disregarding climate change, people nowadays would probably rather react with a sarcastic "The horror! The horror!". And that's exactly my stance toward this Taalen-Tongval reviewing.

16 How it should always have ended

So far, I have elaborated on *negative* aspects of reviewing for the most part. This might induce a wrong impression of my experiences, and of reviewing in general. Let me therefore make a break here and give you an example of a '(very) good' reviewing case. It's about the harshly criticized gradation paper I mentioned.

Starting with the editor, Klaus von Heusinger, we had a nice cooperative e-mail (!) interaction during the reviewing process. His choice of reviewers was excellent, not just because of their positive ratings but also because of their competence and their constructive critique. They were the first to acknowledge the positive, new, and interesting aspects of my paper:

> I think the most fascinating data discussed here are those in (13), which as the author notes have not been give much attention before

> I feel that the major (and exciting) contribution of the paper is an entirely new perspective on the data

These are comments which you do not find in bad, let alone ugly, reviews.

Both reviews were thoughtful and the comments were well-considered throughout. This does not mean that I agreed with each point made, but it was a healthy mixture of assent and non-assent at any rate.

As I had to respect the reviewers' elaborate comments and to show how I had taken them into consideration, I sent a re-commented file back. By color-coding agreement/change, non-agreement/non-change and neutral discussion, I could easily show how the discussion of the comments led to an improved paper. Note that red coding for disagreement is rare here, but typically dominates in commenting bad reviews (as in the *Mind and Machines* case). I furthermore admit that my style of re-commenting adapts to the review category, and I take full responsibility for that.

17 Pathfinder: A beautiful mind

Even if it may only be true for the non-technical disciplines I am concerned with,[17] scientific reviewing seems to be different from what I always thought it was and what I still think it has to be: an objectivity-oriented rather than a subjectivity-dependent procedure of securing scientific progress. Overall, it turns out to be evo-

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[17]in which the quality of a theory/model/approach cannot be measured so easily

lutionary rather than fully determined by rationality. The papers that survive are not necessarily the best, only the best adapted.

The problem might lie in the locality of reviewing. Scientific progress is determined by local decisions (on the basis of certain disciplines, current paradigms, recent theories, given peer groups, thematically restricted journals, factual editors' choices and actual reviewers' knowledge and behavior). Therefore, reviewing as it currently is may optimize paper selection. But if this kind of search of the best path for scientific advancement were seen as a search problem of AI, then it would have to be categorized as one of the simple strategies (so-called 'hill-climbing') that run into the well-known problem of local maxima. That is, scientific progress in some field might simply come to a halt, after having been on the wrong track for a while.

This is not desirable, and one should find ways to avoid it. Intuitively, this would require more open-mindedness of some sense in scientific discussions (including reviews). In an <u>introduction</u> to papers of the <u>OPEN MIND</u> project, <u>Thomas Metzinger</u> and his co-author elaborate on the aspects of such a methodological stance they propose (my emphasis):

> This variant of open mindedness is characterized by epistemic humility, intellectual honesty, and a new culture of charity. It also has a pragmatic dimension: open mindedness of this kind is research generating and fosters an environment of sincere and constructive interdisciplinary collaboration. And it is profoundly inspired by the classical ideals of philosophy as a pursuit of genuine insight and rational inquiry, the importance of a critical and in a certain sense non-judgmental attitude, and the deep relationship between wisdom and skepticism as an epistemic practice.
>
> *[... This involves] reading others' statements according to the best, strongest possible interpretation–that is, to never attribute irrationality, falsehoods, or fallacies to another if alternative and more charitable readings exist.*

18 Conspiracy theory: Proof

I have not yet met that much open-mindedness in reviewing and reviewers. Sometimes, one rather meets strange verbal behavior, as if produced by exponents of conspiracy theories or by UFO believers. The common characteristic of those is that they *believe* in something and try to *prove* their beliefs/theories.

There are reviewers who do just that: they believe that a paper is insufficient/bad and come up with justifications for this belief. They might, for example, tell a long story about how some aspect is treated (differently) in the literature, what they think the paper can or cannot explain, or how they don't like or understand it.

Unfortunately, such behavior is unscientific. First, it doesn't comply with what is called "Occam's razor": the principle of parsimony used in science for the construction of theories. According to that principle, the default reviewing theory should be based on the core assumption "author is probably competent". This would not exclude positive judgment as long as there is no fact to the contrary. The theory "author is probably competent but I think he is an idiot" is clearly less parsimonious, and the added assumption would not allow acceptance of the article anymore.

Second, it is one of the current axioms of science that theories cannot be proven (right), but only be disproven (proven wrong). Instead of just *stating* some negative aspect (allegedly proving incompetence of the author), reviewers should *show* that at some point, the argumentation of the author is wrong for this or that reason.[18]

Accordingly, if the paper's theory cannot be disproven by the reviewer, the paper should better be treated as if it was right. Such a principle, corresponding to the *presumption of innocence* in law, could be viewed as a formal implementation of open-mindedness in reviewing: *in dubio contra rejectionem*.

19 F for Fake: Collateral damage of idiocracy

There's a lot about fake news in the news today. One should think that fake news and reviews are totally different sorts of texts: while the former are (intended to be) distributed widely, the latter are not. There may be some similarities, though.

Consider ugly reviews. For the author, the assertions made by the reviewer may not be different from the assertion that information about climate change is a hoax and that one should better burn more coal to shield earth from the hot sun. Both with fake news and ugly reviews, it is hard to understand why—despite irrational content and bad style—they still find approval by some people. Both may have more or less direct impact on non-believers of the texts.

In general, one must be careful to prevent a change of democracies into political systems led or supported by idiots or autocrats. Correspondingly, one should

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[18]Aside from that, reviewers should distinguish the main and important aspects of the paper (core of the proposal, argumentation) from the less important ones. Of course, they should put a focus on the former. In any case, it doesn't help much to only show flaws regarding minor aspects.

somehow try to prevent intellectually non-satisfying reviews/reviewing. Else, there might also be some considerable collateral damage done to the systems.

Funnily enough, I just stumbled across an automatic generator of fake reviews: In order to caricature those reviews that only react on superficial formal or syntactic aspects of a paper, Andreas Zeller programmed—as an April Fools' Day joke—an automatic review generator (cf. https://autoreject.org). Selecting some of the options results in a realistic typical negative review text resembling those bad reviews out there (actually, the program is based on real text snippets).

On his Github page (https://github.com/uds-se/autoreject, last visited 2019-04-19), he comments the potential question whether such reviews really exist:

> Of course! As a reviewer, you can always make your life easy by pointing at one of the "issues" listed in the autoreject reviews. Ignore the potential of the work, ignore whether it may make some difference or

not. Avoid taking a difficult decision and simply go for the first flaw you can find.

20 Hot shots!: Aftermath

Sometimes, life itself is funnier than any comedy/parody. At least if you ignore the fact that what happens is real and maybe not funny at all.

A paper of mine had been treated—let's say—*strangely* by the assistant editor of some journal. After a first round of reviewing, I was allowed a *second resubmission* (!!), all on the basis of one reviewer's feeling that he had a hard time of reading it (he had indeed needed four months for each of the two versions) and that the paper therefore should have to be written differently.[19]

All this had taken almost a year, and I couldn't imagine myself going down that road of improving, waiting, and still not getting accepted again in the end. So I wrote a letter to the editors in which I detailed my concerns, mostly about the reviewer whom I knew to be 20 years younger than me (because he had asked me to cite him <ahem />) and who had shown some more hints of lack of professionality. I pointed to the fact that I had counted and analyzed his comments and found 40(!) of 59 really bad. And that I found his ideas of "rewriting" the paper quite inconvincing. Finally, I simply asked for a clear status other than "try again" and for some specific constraints (e.g., maximal paper length).

In an attempt to be polite and save the face of the assistant editor, who had a leading part in this charade[20], I addressed all editors by sending her the following mail (with the letter and, as usual, the commented comments; bold face is added here):

> **would you please be so kind** to forward this Email with attached two files **to the editors** of [...]?

Six months full of no answers later, after having sent her a reminder, she wrote

> I found the letter you **instructed me** to send **to the reviewer** unnecessarily antagonistic

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[19]In fact, he wrote "in spite of my sympathy, and because of my problems understanding it, I think the paper should not be accepted". However, the other reviewer had no more comments to offer after the first *resubmission* (!). Because of that, I was given another chance. Lucky me.

[20]For example, my first submission was a perfectly readable LateXed pdf. For my first *resubmission*, I was asked to provide a(n ugly) Word file, which cost me a lot of effort and time to do. So I was all the more astonished when this version was not accepted later.

Note especially the telling differences in the highlighted parts. So she didn't have a clue what this was all about and had not forwarded it to her boss. I immediately informed her editor-in-chief about this and our following short and impolite email interaction, and protested. NO RESPONSE.

How crazy is that? Picture yourself in a restaurant. Your chicken breast has an icy, red raw core, and you complain to the waiter. Wrinkling his nose at your evident impoliteness, he says you should try again next time. You ask for the chef. He doesn't turn up (maybe because the waiter didn't tell him). It's *that* crazy.

Let me summarize: while journals have some important function in science, and editors/reviewers have some important function in journals, authors have even more: the aftermath of both what happens and not happens to them, aside from the work before, the potential trouble in between, and all the lost time after all.

21 Response and responsibility: Absolute power

Now recall all the cases where I got no response, or strange ones which make no sense. Generalizing a bit, this points to the responsibility of reviewers and editors to fulfill their function adequately (somehow, the above 'honor' aspect).

It also points to an even more general, hidden aspect: the whole intransparent system of (anonymous) reviewing leads to people having absolute power over the author (at least as far as the submitted paper is concerned), who often doesn't even get to know them. While this is not necessarily bad *per se*, "misuse of power" is just a paraphrase of what I am writing about in this text.[21]

22 Rejection: (Chain) reaction

There are funny attempts to show how one can keep control and regain, at least virtually, power. A colleague pointed me to the following[22]:

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[21] I just learned that the topic *Knowledge and Power in Science* is vigorously debated these days, and that this might ultimately lead to an overall change of the scientific system.

[22] Shit Academics Say @AcademicsSay: How to reject rejection letters: A useful template | @bmj_latest | http://www.bmj.com/content/351/bmj.h6326. 15. Dez. 2015.

Rejection of rejection letter

[*insert university emblem here*]

Dear Professor [*insert name of editor*]

[*Re: MS 2015_XXXX Insert title of ground-breaking study here*]

Thank you for your rejection of the above manuscript.

Unfortunately we are not able to accept it at this time. As you are probably aware we receive many rejections each year and are simply not able to accept them all. In fact, with increasing pressure on citation rates and fiercely competitive funding structures we typically accept fewer than 30% of the rejections we receive. Please don't take this as a reflection of your work. The standard of some of the rejections we receive is very high.

In terms of the specific factors influencing our decision the failure by Assessor 1 to realise the brilliance of the study was certainly one of them. Simply stating "this study is neither novel nor interesting and does not extend knowledge in this area" is not reason enough. This, coupled with the use of Latin quotes by Assessor 2, rendered an acceptance of your rejection extremely unlikely.

We do wish you and your editorial team every success with your rejections in the future and hope they find safe harbour elsewhere. To this end, may we suggest you send one to [*insert name of rival research group*] for consideration. They accept rejections from some very influential journals.

Please understand that our decision regarding your rejection is final. We have uploaded the final manuscript in its original form, along with the signed copyright transfer form.

We look forward to receiving the proofs and to working with you in the future.

Yours sincerely

Dr [*insert name here*]

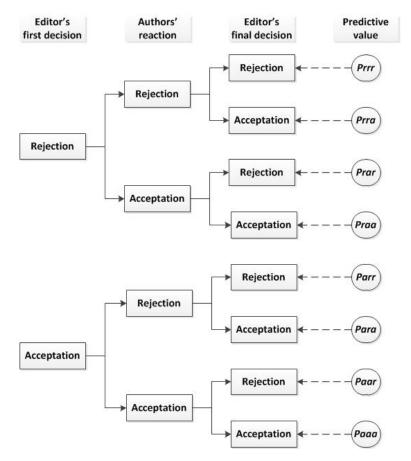[*Insert research group acronym here*]

[*Insert university here*]

[*Insert country here—that is, Australia/New Zealand/small European Country/Canada*]

There are even <irony>serious scientific attempts</irony> to generalize such an approach as one instance of a set of possible chain reactions, depicted in the following graphics.[23]

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[23]found on http://www.bmj.com/content/351/bmj.h6326/rapid-responses.

Don't forget to stop laughing. ;-)

23 Reality bites: Blacklisted, The Scarlet Letter

So now that you had some fun, back to reality: If you as an author object or reject, or just write ((too) many) emails to the editors, warning lights suddenly start to blink on their desktops. Even if you've been the 'Jurgen Klopp'-normal one all your life, you may be categorized as a strange, huffy one in an instant.

I must admit that this is a quite natural reaction, probably justifiable by the number of strange people out there. However, I am told that there are even systematic procedures for handling such situations. Unbeknownst to you, you can be put on a blacklist to be treated accordingly. In that case, you got your mark. Any similarity to treatment of people in certain socio-political totalitarian systems is by no means accidental.

24 Trust and Shame

Ultimately, all discussion of reviewing and its problems boils down to the important role of a single bidirectional aspect: *trust*. In an ideal world, reviewers and reviewees can trust each other to be competent participants in the process, which is established/guaranteed by the editor.

Needless to say, however, that this is not the case in our non-ideal world (given the actual experience). Yet while this seems to mark a general problem of reviewing, there does not seem to be a corresponding general solution in the scientific community.

But then, what if reviewing would be regarded and performed as a scientific discourse moderated by the editor (even double-blind with the help of capable reviewing systems) in which misunderstandings can be clarified to establish trust?

Unfortunately, such improved reviewing would probably slow down the whole process (is that so?), and therefore be in conflict with the predominant 'quantity-instead-of-quality'/'publish or perish' view of science. It's a shame.

25 Blind date

There is something else very wrong with anonymous and double-blind reviewing.

Imagine you're looking for a (new) partner and decide to use a dating portal which boasts with the advantages of information-restricted ('blind') online dating. These are the rules: your counterpart is not allowed to talk in a way that reveals his/her social, economic/financial, or health status. He/she is not allowed to tell, and you're not allowed to ask.

That is, you won't know much about the person you date, and you literally decide at the face value of what you see. Now everything can happen: you may be satisfied, or you may be dissatisfied, or you may not know whether to be the one or the other (although you will have to make a decision).

Unfortunately, the information you get (view and conversation) will probably not give you the information to decide adequately. Therefore, the candidates you dismiss could be adorable, potent, healthy millionaires, and the candidates you prefer could, although nice and good looking, turn out to be poor bipolar schizophrenics.

So you wouldn't date like that, do you? But then, assuming that double-blind reviewing ultimately works just like that: Why hasn't anyone stopped reviewing(and therefore science)'s being based on such an idiotic blind dating scheme?

26 Masked and anonymous: The joker

Practically, double-blind reviewing may lead to severe misunderstandings. For example, as an author, you have to remove every identifying features in your manuscript, including specifics of your own publications in the references section (name, title, journal name etc.).

Recently, I was so intelligent or stupid (I don't know which) to not just *delete* this information in my paper as seemed to be required in the journal's guideline (boldface by me):

> All in-text and final references should be replaced **only** with "Author 1 (2003)", "Author et al (2006)", "Authors (2017)", etc. Thus, the reference section must not reveal not only (co-)author names but also the titles of their articles or books, or any other publishing information, such as journal names, titles of edited volumes, editors or publishers.

For me, it doesn't make sense to only specify "Author (2000)" in the references: I want the reviewer to know that I wrote about some topic X in a paper, published as a journal article (all still fully anonymous). Therefore, I rather masked the references, providing corresponding fake titles and the publication type, literally like "Author (2000). On X. Journal article". Here's what a reviewer remarked:

> As regards the articles written by "Author", one would expect of an anonymous manuscript that the titles of the articles should not be written out. However, none of the titles can be found on Google Scholar, so I believe the author is playing a hoax on the reviewer.

What the ... (compare the OPEN MIND maxim above)?!? Why the reviewer mistook "On X" for a title, but "Journal article" not for a journal name, escapes me.

In spite of the "only" in the guidelines, I am still convinced that my interpretation is at least acceptable if not correct, so I crystal-clearly wrote back:

> No. What an irritatingly quick jump to some conclusion. Such an absurd hoax-theory would be cut off by Occam's razor. The more parsimonious, correct explanation is that I deleted the real titles, but specified a raw informative content description each, to at least indicate the domain/topic of the publications.

Anyway, even the journal insisted on reduced references before resubmission, so I gave in.

What a joke.

27 (Not) The Usual Suspects

Admittedly, there is clear evidence that double-blind reviewing has some benefits. In a study on the effectiveness of double-blind reviewing[24], it could be shown that knowledge of author identity had an impact on review scores: male-first authored papers were found to be scored higher (as opposed to female-first authored papers), and same-country papers sometimes were more likely to be accepted.

The authors note that "these biases can affect anyone, regardless of the evaluator's race and gender", and correspondingly recommend continued use of double-blind reviewing.

While such studies do make a point (especially for conference paper reviewing), it is less clear how these results compare to the hard-to-measure negative aspects of double-blind reviewing discussed here, or to the positive aspects of some ideal case of non-blind reviewing.

28 Anonymous: Perspectives

To be honest, I do not think that there is an easy answer to the question of anonymity in reviewing. There were certainly good reasons to introduce (double-)blind reviewing: it was supposed to foster and secure objectivity in the process and to reduce the usual socio-emotional consequences observable in every type of critical writing. As I have tried to show, however, objectivity is neither reached nor secured, and the socio-emotional consequences have been imposed quite unilaterally on the author.

Objectively, then, there seems to be a tie in the pros and cons of anonymous reviewing. Accordingly, taking up a stance here is more a matter of practical considerations and of perspective. Disregarding the practical aspects (although they will probably tip the scales in this discussion), the main different perspectives on reviewing are either *expecting the worst*, or *hoping for the best*. Obviously, anonymous reviewing is an instance of the former.

Personally, I would generally opt for non-blind reviewing, i.e., the latter perspective. This is not easy, and there is the danger of non-perfect comments on both sides. As to tolerating them, one of my favorite essayists proposed the following rule these days[25]:

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[24]C. Le Goues, Y. Brun, S. Apel, E. Berger, S. Khurshid, Y. Smaragdakis: Effectiveness of Anonymization in Double-Blind Review. *Communications of the ACM*, June 2018, Vol. 61 No. 6, Pages 30-33. DOI: 10.1145/3208157.

[25]Harald Martenstein (2017), "Über Duldsamkeit, Kritik und Spott [About forbearance, criticism, and mockery]", *Zeit Magazin* 24, p.10. My translation.

> Insults and threats are always to be rejected, criticism and mockery must be tolerated by everyone.

I like this, but I admit that my tolerance degrades with the *stupidity* of criticism and mockery: nothing is worse than an idiot trying to be ironic/sarcastic.

Furthermore, as I already said, there is an intermediate solution which I would ultimately prefer (considering practical aspects, and with a realistic view on open reviewing). The current situation could be much improved if reviewers/editors and authors would be allowed and even requested to communicate with each other to clarify misunderstandings and to answer open questions. In our modern times, this could and should be implemented in an easy-to-use way (ideally, allowing Whatsapp-like textual interaction), like for example:

> **Rev1:** p. 31: are you sure wrt. the maximal axis and the example?
> **Author:** Yes, there's a whole theory behind it, spelled out in Anonymous (1991)

I KNOW this would help a lot. Actually, such interactive facilities in reviewing systems are on the road (although still imperfect, as I recently had to realize).

29 Cinderella, Judge Dredd, Inception

Apart from all the problems we have seen in reviewing: do we know what counts as a "good paper" at all and whether reviewers can identify them? This should be a prerequisite of what many seem to regard as the essential task of the reviewers and editors: to pick the good lentils out of the ashes.

But wait, this is a horrible misunderstanding. It would mean that in each case, a handful of people, at most, is assumed to know (and empowered to decide on) the global quality of some article, determining its further path, its impact on the scientific discourse, and the development/career of its author. Hey, we are not at the law court, and even there the matter is processed openly, arguments being presented and discussed meticulously before judgment. In reviewing, the whole procedure is somehow hidden, much more superficial, and the defense part is completely missing (!), somehow a Judge Dredd comic-book version of law (and judgment).

Actually, the task of the reviewers and editors is (or should be) much more restricted: to filter out the bad papers, not to judge whether some paper is good (in the 'objective' sense). To identify and name good and bad aspects, weigh them up and give an estimation of overall quality.

Within some range of non-badness, it should be the scientific community that determines the effective quality of some paper, and authors should be given some

leeway to plant new ideas into the readers' minds. Only then innovative papers have a chance of being read, although they might not fit the expectations of the reviewers, or might not be fully understood by them. In the worst case, rather than being the gentle doves, reviewers otherwise might act like the nasty stepsisters preventing Aschenputtel from marrying the prince.

30 Great expectations: Last resort

It should be clear that—ideally—there are great expectations on reviewers (see the above virtues they should show). In reality, many of them do not come up to these expectations, and sometimes they even know or realize that.

I found a poem of the German lyricist (and humorist) Eugen Roth (1895-1976) that puts a fine point on this situation and especially its consequence: a cheap escape for the reviewer that may lead to the negative aspects of reviews and reviewing I have discussed. My translation of the poem is quite free (even with a different rhyme), but still synonymous.

Ein Ausweg
Ein Mensch, der spürt, wenn auch verschwommen,
Er müsste sich, genau genommen,
Im Grunde seines Herzens schämen,
Zieht vor, es nicht genau zu nehmen.

A resort
A man who senses, albeit faintly,
that, to be quite accurate,
so much ashamed he'd have to be,
prefers to be not that exact.

31 The Remains of the Day: Rain/Hit Man

Recall that we don't talk about all the reviewing cases (that hopefully constitute the majority) where papers are rightly accepted or rejected.[26] What remains are the

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[26]to be precise and complete, the *wrongly accepted* are also excluded here.

cases in which an author is the proverbial one left out in the rain (which is the only handicap he/she probably has at that time).

Now consider this: if someone hits you in the face—a matter of (milli)seconds—, you can strike back or sue him. If, on the other hand, your paper is rejected—after having worked years/decades in the field and having spent weeks/months on writing the paper and on waiting for the results—, you neither have a direct nor an indirect possibility of reaction. I sense a slight distortion of the moral universe here.

The lesson to be learnt is clear: don't get rejected, there's no refund of expenses! So unless you like gambling, stick to every available habit and smoothen your paper to prevent being attacked in the first place!

If you ever wondered why most papers in science are only minor, often boring variations of others in the corresponding paradigm, here you might have your answer.

Afterword: End of watch

Those who don't know me may ask: who is this guy lecturing us about reviews and reviewing? Let me tell you who I am.

In my fourth semester (in 1985) of my computational linguistics studies I decided to build a natural language generation system, more specifically, a system that automatically produces route descriptions (which nobody asked or urged me to do). If anyone had known, he would have thought I was crazy. Building such a system was deemed a many-man-year venture at that time, and even then some failed. Most people I know are hardly able to look beyond their own nose, this was glancing beyond the horizon. The route description generation system I successfully developed was later integrated in the big LILOG[27] project and ultimately ran in an IBM natural language system prototype presented at CEBIT, Hannover.

I then implemented the theory of a linguist who later became the director of one of the biggest linguistic institutes in Germany (the ZAS Berlin), co-wrote a paper, presented it on the German conference of Artificial Intelligence, and co-wrote a book on that topic, all that still being a student.

My studies and especially my Master's thesis already had a general, interdisciplinary Cognitive Science perspective on natural language (processing). Working in a Computational Linguistics and Artificial Intelligence department, my first dis-

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[27] in a nutshell: Linguistics and Logic; full meaning: *LInguistische und LOGische Methoden zum maschinellen Verstehen des Deutschen* (linguistic and logical methods for the machine understanding of German)

sertation project then was about concept theories and their relevance for knowledge representation and learning.

I had asked myself, if concepts are abstractions (the prevailing model at that time) and abstraction means loosing allegedly unimportant (e.g., modality-specific) details, what determines pruning the details and how can there be such (context/ situation) specific concepts as have been observed? I gave a presentation of this research question (plus an overview of loads of literature I had found).

Nobody understood what I wanted, or wanted what I had understood (and maybe I was was too provocative again), I therefore let go of that idea. Meanwhile, however, research in aspects of situated/grounded cognition (an extreme version of that point of view) has become famous, and corresponding (e.g., instance-based) models of concepts and concept learning have been developed in psychology and AI (the applied versions of which are collectively known as *deep learning* today). In the year 2018 (more than 25 (!) years later), the University of Osnabrück offers the CARLA Summer School and Workshop "Concepts in Action: Representation, Learning, and Application" with the following announcement:

> Both events will focus on (but not be restricted to) three core questions: How can we formally describe and model concepts? Where do concepts come from, and how are they acquired? How are concepts used in cognitive tasks?

My next (and ultimate) dissertation project was about the role of attention in spatial representation and semantics. While this was a turn back to spatial linguistics stuff I had worked on before, I had found interesting and highly relevant literature on selective attention when scanning recent journal articles for my previous project. Unfortunately, riding such a recent scientific wave, I was probably the only one in Germany, and perhaps one of only few in the world, who saw a connection here. Accordingly, the reception of my work was disappointing. The situation has dramatically changed since, but that comes too late for me, and the majority of the cognitive scientists still has not yet acknowledged the importance of the role selective attention plays in the cognitive system. As to German Cognitive Science, what can you expect of 'experts' who in a bulletin of their society admitted that they lagged behind the American state of research as much as 15 (!) years (at least when I cited that in my above-mentioned book review)?

When in the mid-1990ies there was a structural change to come at the university of Osnabrück, where I worked, and everyone complained about the restructuring and financial cutting to be expected, I was the only one who stood up in a meeting and pointed to the positive perspectives of that situation. When sometime afterwards my boss gave me a brochure of the DAAD about funding new international courses

of studies in Germany ("take a look at that"), it was about five minutes later that I ran back to him saying that we definitely should apply. Now, Osnabrück has *Cognitive Science* as an international interdisciplinary course of studies, and a corresponding flagship institute with a 50+ people staff. What do you know!

Perhaps you can see the pattern here: scientifically, I am always a very early bird (this may be different in real life), so the worms may not have come out yet. This lack of timing leaves me starving (also because I am much too stubborn to adapt). Seriously, after years of experience in some domain, I do apparently develop some sense of research direction. I wonder why this should be different with the paper that inspired me to write this piece (see the following case report).

I'd like to end this with a striking incident. Recently, I submitted a paper that refers to Bob Dylan's work (more exactly, to *All along the watchtower*) and whose main title is *From motion perception to Bob Dylan* (it's about the semantics of directionals). Something like that is rare in science, isn't it? A few weeks (!) later it was announced that he won the Nobel prize for literature. Pure coincidence, I have to admit.

Although *All along the watchtower* is a good source[28] for closing words of this piece (as is Tom Petty's *I won't break down*), I'd like to end this essay with some lyrics of the German, Hamburgian band *Kettcar*, from their song *Deiche (dikes)* (my translation)(youtube video):

> *Just because one has become used to it*
> *it's not normal.*
> *Just because one doesn't know it better*
> *it is not the case,*
> *not even close to that,*
> *that I don't care.*

Cold case: A History of Violence

If you're interested, let me talk about my recent paper (at the time of starting to write this text) again, as it gives rise to all (negative) flavors of reviewing. Imagine

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[28]Some say that the song is about revolution, of lower and middle class (thief, joker) against upper class (the princes in the watchtower). In our context, this would correspond to authors and reviewers plotting against the current reviewing system. I like that interpretation.

you have an idea that is based on long-term interdisciplinary experience and some time of research, but that is in conflict with most, if not all, disciplinary paradigms. You want to elaborate on that idea, so you need scientific feedback and funding. The question is, however, where to start.

Since I am not a conference attendee anymore (and since I had no interest in throwing myself in some disciplinary lion's den), I decided to first try some opinion piece to get feedback[29], and if positive, then to apply for funding. Since Computational Linguistics is my nominal home discipline (aside from having a corresponding degree and some practical experience, I am co-editor of the German introduction to this discipline and have written an introduction to language technology), I chose *Computational Linguistics* and its *Last Words* section. As it turned out later, the choices I made were all wrong.

The idea I have is a paradigm shifter. I argue that the most well known and widely used logic, *Predicate Logic* (PL), is defective and should be revised according to modern insights. Doing so, I am clear, simple, but specific about the single basic aspect that has to be changed according to my proposal. I was fully aware of the fact that this would question the whole paradigm, but I thought that either my argumentation would be right and convincing, or that I would be proven wrong by good counterarguments of competent reviewers.

Last Words section pieces are usually short and should offer "a personal opinion or provocative perspective on some aspect of the field". I had an important opinion to tell ("PL is defective and needs to be revised, which is relevant for the discipline. This is quite obvious, but nobody acts accordingly!")[30] and did so provocatively: here's the intro and the last paragraph of my first submission:

> Quite recently, I had a discussion with Joseph Weizenbaum, Terry Winograd, Bill Woods, and Wolfgang Wahlster. We talked about past ambitions in the field of *Computational Linguistics and Natural Language Processing*, and compared them to the current state of the art. We acknowledged the practical achievements made in recent years (see Carstensen [2013b], Carstensen et al. [2010]), but we all agreed that there is virtually a stagnation in (computational) semantics, leading to an aggravating slowdown in the progress of the whole field, and to a limitation to pattern- or statistics-based technologies.

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[29]I had asked colleagues beforehand, but the replies were less helpful than I had expected. I could already sense some disbelief, though, and some even seemed to think that I had gotten crazy.

[30] You must know that "deep AI", based on logic, had dramatically failed in the 1980s, but only the scientists *using* logic –and the unfit systems– had been blamed, not logic itself.

I told them that, with all due respect to Gottlob Frege, this can be traced back to his conception of what is now known as *Predicate Logic* (aka *First-Order Predicate Logic (FOPL)*). I explained to them why predicate logic in its current form should be regarded as ill-suited for the tasks at hand and pointed out that without modern semantic fitting, *Computational Linguistics* itself might some day only be remembered as the discipline of Chatterbots and sophisticated text/speech crunching. They were thrilled about this provocative perspective and asked me to write my arguments down as a *Last Words* of this journal.

I may have dreamt this, but anyway, the following is a serious attempt to motivate disposing of predicate logic in its current form.

[...]

Somehow, the reverence of the scientific community for FOPL despite its apparent deficits is reminiscent of Andersen's fairy tale *The Emperor's New Clothes* where no one dares to speak out the obvious truth. Yet what *Computational Linguistics* (among others) needs is a logic that is fit for the tasks at hand and for those to come in the next decades. So get dressed, FOPL!

As I found it unsatisfactory to only criticize, I also presented aspects of and some motivation for an alternative (which I regarded as an asset of that opinion piece). Doing so, however, this piece seemed to exceed its expected length and rather looked like a "normal" paper. Correspondingly, the editor (after discussion with a co-editor) wanted to recategorize it as a "squib". Yet this was not my intention, so I explained my point of view, insisting on a *Last Words* text status.

As a matter of fact, the editor gave in and I was granted one (!) reviewer for the adapted text which I had cut down to 8 pages and which now had already lost a bit of its original vigor. The review, however, was everything but a helpful feedback on my idea. It rather put me down as a braggart, close to scientific cheating:

However, the 'Last Words' column in CL is not intended for polemics saying that what the world needs is one's research program, of which preliminary sketches have been published here and there, but none of which has as yet resulted in anything at all new.

Hm, he missed the central point of my paper saying that what the world needs is a *different* research program. He also gets driven away by the anger about the fact that someone dares to question the holy axioms of his research paradigm, and all that polemically. It is a classic example of hubris (the irony being that it suggests hubris on my part) as the reviewer simply doesn't get the point (even in the parts where he should have been able to get it) but tries to teach me. I realized that this was leading

nowhere (and that it was all my fault), sent back some quick re-comments (you can find them here, together with the pathetic little review) but accepted the rejection.[31]

Afterwards, I reworked the paper, extended it substantially (~40 referee-formatted pages), and submitted it to *Linguistics and Philosophy* which covers the theoretical domains of my work (semantics and logic). Suffice it to say that I attached a cover letter in which the background of the submission and a description of my expertise[32] was detailed. Yet we already know what happened: again I only got one (!) review, in which I was categorized as one of <sarcasm>those well-known AI braggarts</sarcasm>.[33]

This review is a prime example of what can and must not be done wrong in review writing. As a matter of fact, it served in some parts as a model for my satirical reviewing rules. It is perhaps even more arrogant, superficial and inattentive to my argumentation than the one before, and full of "wicked imaginations" . I therefore did not write down re-comments carefully, but annotated the pdf in the same way I treat errors in my students' papers. I sent the annotated pdf to T.E.D., complained a bit, and never got a reply. It contains my assessment of the review (my personal rejection rejection, so to speak) which was a delight to write:

> Hence, no sufficient ground for rejection. Instead, the review has a strange start, a superfluous middle and a not so consequential end. There is too little understanding and there are too many (wrong) assumptions. Besides that, there is no word of appreciation for the ideas presented which could be the start of a discussion. Instead, the reviewer simply felt stepped on his toes and reacted unprofessionally.
>
> Sorry, review rejected.

To complete this history of violence to my idea, I then presented the full discussion of theoretical and practical aspects in my submission to *Mind and Machines*. Again, the outcome is already known: while the two monodisciplinary reviewers at least stayed polite and acted professionally, none of them got the whole interdisciplinary picture. Accordingly, almost all of my re-comments are red. To give an example,

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[31]For the uninformed reader: I am the expert here, not some douchebag trying to place a paper unwarrentedly. So don't get me wrong: I still think that the right reaction would have been something like 'Wow, this is interesting, let's give this some people to read and see how we can do this.' It's just that I realized that this was not going to happen.

[32]For example, that I have taught courses/introductions to each of the following fields/ topics/ disciplines: Artificial Intelligence (practice level), Computational Linguistics, Cognitive Science, Knowledge representation, Language technology, Linguistics (especially semantics), and Logic. I'd love to see whether anyone of my reviewers even only comes close to this range of expertise.

[33]On the basis of the two reviews, statistical methods would already have projected a clear tendency here... But see Fn. 32.

here's the critical point made about the part where I motivate the relevance of my research for both theory and application:

> The author makes a distinction between theoretical and practical considerations for changing the logic. It's not obvious what the distinction is and it might even be controversial that there is such a distinction. Does the author need to get involved in this dispute?!

In my comments, I wrote "Wow, this is *Mind and Machines* [...] And it's just the motivation part" ...

The other reviewer also had his coming-out moment. In my article, I criticize a theory as being too complex and point to the aspect of learnability (by 2-3-year olds). His comment goes like this (my emphasis):

> Children of 2 - 3 years old, in learning to use quantifiers of various sorts, don't need to learn the semantic clauses for these expressions. Rather, they need to learn patterns of **behavior** that are in accordance with the clauses.

Note first that it is interesting to be lectured like a school kid just because of blind reviewing. Then you must know that at least classical Cognitive Science is somehow defined by *not* theorizing on the level of "behavior", best exemplified by Noam Chomsky's famous critique (review!) of Skinner's *Verbal Behavior*. The reviewer simply disqualifies himself with his comment.

But note that one reviewer at least finds some positive words:

> The author's novel idea concerning the treatment of quantification seems interesting and worth pursuing.

"I'm the luckiest son-of-a-bitch alive".

That's what I thought, and pursued: stopped working on this misrouted paper, started a solid investigation, published a paper in **Frontiers in Artificial Intelligence: Position Papers in Language and Computation** (open access online version, formatted Pdf version) on "Quantification: the view from natural language generation". The penultimate sentence is

> Such a view of quantification indicates a need to rethink basic aspects of quantifier logics and semantics in the 21st century, and to redesign them accordingly.

Not the last words, but all I wanted to say.

This is the end: Arrival

Remember the paper that was rejected by Applied Ontology (first section, "O journal...")? Eight years after its publication in Cognitive Processing, I got an email by Barry Smith, the leading expert in the field of applied ontology, asking for a copy. The next day, I got another email starting with

> This is an excellent piece [...]