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Publishing in Peer-Reviewed Journals

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Publishing in Peer-Reviewed Journals

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Welcome to our workshop, here at the Bishop's Mansion in Lund, on this lovely August day 2010. I have been asked by the Dean of Research in the Humanities and Theology, Marianne Thormählen to give you an idea of what peer-reviewers and editors want to see in a contribution to an international, scholarly journal. She has suggested I talk about typical “traps” that you don't want to fall into, how you go about deciding where to submit, etc. Before I attempt to do that, I want to say two things briefly: the first is that this is not a science, but rather an art—a black art, if you wish. To generalize and speak as if the rules outnumber the exceptions would be to mislead. Take, in other words, what I say with a grain of salt. Secondly, I can only speak from my experience of a limited number of academic fields and sub-fields, e.g. “Chinese studies,” 20th century social and political history, and politics and propaganda/media studies. It is with these subject areas that I am familiar. There are likely to be variations on what applies elsewhere (in comparative literature, etc.) with which I am entirely unfamiliar.

To submit or not submit—is that the question?

The first thing you should ask yourself is this: do I—as a university teacher, graduate student, recent PhD, professor even—really have anything important to say? Do I really have anything new or significant to contribute to knowledge? If you cannot honestly answer that question with an unqualified yes, then please—save us all a lot of work and time. Go do something else. Save a tree or two. Read. Journal editors are today so overburdened with submissions, they have been forced to put out what we may smile at as “blanket refusal slips” on the web, targeting the intellectual spam that is threatening to make their work all but impossible. Witness the following advice put up on the “writers' guidelines” webpage of *Foreign Policy*, under the rubric “Before you pitch us an idea, keep a few things in mind”:

- Avoid the obvious. We receive dozens of pieces with titles such as “NATO at the Crossroads” and “The Future of Trans-Atlantic Relations.” We publish almost none of them.
- Don't send us any article or proposal that begins with “Since the end of the Cold War...” or “In the wake of September 11...” Really. Please don't.

However, if you have decided that yes, I do have something I must contribute, I will not be deterred by any of that... then please do also perform the following exercise in role play and intellectual cross-dressing: ask yourself, if you were the editor of an academic journal, a peer-reviewer, or simply a reader of the journal you have in mind, would what's on your mind (and by extension in your scholarly submission) interest you *if it came from* someone just *like yourself*? Really! In the real world, it very much matters *who* says something, and not just *what* is being said. That is the way things are, a fact of life. Clifford Geertz deals with this in *Works and Lives—The Anthropologist as Author*; and Robin Tolmach Lakoff does it with great humour and insight in *Talking Power: the Politics of Language*. “With tenure,” Lakoff writes in “How to Write Like a Professor,” comes the freedom where “the sky's the limit—on length, breadth, and self-assurance, as well as interpersonal obnoxiousness.” But ruling this discursive universe that includes the peer-reviewed journals we talk about today is “the language of a society with complex power and territorial assumptions, often in conflict with its express mission.” Why does *Foreign Affairs* publish, it seems, just about anything that war criminal and Nobel Peace Prize winner Henry Kissinger cares to send their way? Well, it's certainly not because of what he says, but... because of who he was/is!

Yet curiously enough—and I speak from personal experience here—it is not always the young and relatively inexperienced scholars who themselves are the best judges of their own merit or the “publishability” of their own academic work. Well before I was done with my PhD thesis, back in the mid-1980s, I had submitted and published (I was very excited about this) a piece to a Swedish popular magazine *Internationella Studier* on the internal operation of the Chinese Communist Party's propaganda system. The fact that a limited domestic readership was judged by the editor in Stockholm to be potentially interested in what I had to say was... well, quite enough to satisfy me. I aspired no further. But, I had misjudged the relative merits of my own work, it turned out. A few months later, I visited London and the School of Oriental and African Studies where, at a public seminar, I met a lecturer in Chinese anthropology who definitely did not know a word of Swedish but who, as did one or two other individuals present, insisted he not only knew who this 31-year old punk from Sweden was but also had read a piece of mine. I soon discovered that the CIA in its *FBIS China Report: Political, Sociological and Military Affairs* (to which the SOAS library subscribed) had read my article, found it interesting enough to translate, and without my knowledge or the knowledge of *Internationella Studier* had published a full translation of it in English as “Swedish Expert Examines Country's International Propaganda.” The moral of this story? *You* yourself are on the whole the best judge of the value of what you have to say... *but* there are exceptions. Sometimes, without you knowing it, there may be readers unknown to you who want

nothing more than to know what you know. So be realistic, but expect now and then to be pleasantly surprised.

As a general observation reflecting possibly little more than my own prejudices, I believe that we Swedes, on the one hand, often lack confidence in the relative quality of our intellectual work by international (read Anglo-Saxon) standards. I also believe, on a different but related note that we, on the other hand, often believe that our academic *written* English is better than it actually is.

Choosing “your” journal—Things to bear in mind

In principle, you submit to the journal you, in consultation with your professor or a more experienced colleague, decide is the most likely to be interested in your work, and if that means more than one journal, the most prestigious one of the lot. But please remember, just because a journal has just published an article on topic X, this does not mean that “it is interested in topic X” and therefore will publish yours on the same topic a few months or years later.

If you don’t have reliable information that tells you otherwise, you should assume that English language journals in your field published in English speaking countries will be interested in submissions from Sweden that are up to their quality standards. You may assume that the competition to get your work published will be less fierce in an English language journal in your field, if one exists, published “closer to home” (in Scandinavia). An interesting phenomenon in this context is the publishing policy of some of the peer-reviewed English language journals published in countries that are yet to establish themselves as home to international centres of academic excellence: they can sometimes be attractive alternative submission outlets for your work. Why? To my pleasant surprise I discovered some years ago that a peer-reviewed journal published in English since 2001 by the Academy of East Asian Studies in Seoul, Korea, pays an impressive US\$ 1,500 to the author of every substantial research article it accepts. To the best of my knowledge, none of the fine and influential journals published by British or Swedish academic institutions offer the same kind of material rewards in recognition of fine scholarship.

Sometimes journals are contested fora, in which case submitting your work to one and not another becomes in itself a statement of sorts. Some editorial boards are tantamount to cliques, fiercely territorial, and very possibly engaged in intellectual turf wars with other cliques. In the China field, there is one excellent social science quarterly which most of the time (for many years) seemed to publish only the writings of one and the same group of scholars and their graduate students. Some journals are loved by some and others hated by other for this very fact. In the international peer-reviewed quarterly *Natural Language and Linguistic Theory*, volume 3, 1983, UC Santa Cruz professor of

linguistics Geoffrey K. Pullum wrote as follows about *Linguistic Inquiry*, a peer-reviewed journal in generative linguistics put out by MIT Press since 1970:

I regard *Linguistic Inquiry* as a miserable trash-stuffed rag of a journal through which the pathetic blitherings of an army of knuckle-dragging intellectual toadies are shepherded to prominence by the unprincipled back-room machinations of a pear-brained lackwit of an editor whose fawning subservience to the power clique that controls modern linguistic is matched only by his contempt for civilized standards when dealing with the work of those whose integrity prevents them from prostituting their scholarship by kowtowing to the self-ordained guardians of a baseless pseudo-theoretical hegemony.

OK, so Pullum probably wasn't 100% serious, but that is in any case what he wrote. It can be a tough world out there...

At the editor's end—How things really work!

Let me now proceed with some comments, admittedly superficial but perhaps just perhaps useful and of general interest. The peer review process is, in my view, a double edged sword that helps the practitioners in a “field” maintain “quality,” but it may also seriously impede progress! Why this should be so was beautifully explained almost exactly fifty years ago by Thomas S. Kuhn in his seminal *The Structure of Scientific Revolutions*. I am sure you have read it, and if you haven't you should. It is a morale booster for the rejected scholar!

What your peers will be asked to do...

Your reviewer/peer will not be paid, but will be thanked for agreeing to read and comment on your article manuscript. He will be given a deadline by which to do this by the editor, usually negotiable by a few weeks... He also has the option of saying “no I cannot do it”—in which case he usually tells the editor it is because of other commitments (sometimes this may actually be true). He may give the name of a colleague who could review your MS in his stead: editors appreciate help of this sort very much.

A peer-reviewer may initially have the following question put to her by the editor: “It may be that you have already reviewed this manuscript for another publication; if this is the case, please inform me as soon as possible.” This alerts us to the fact that some manuscripts have been around for awhile: more importantly, it reminds us of the problem of dual submissions in the humanities and social sciences. You must *never*—and this is very important—submit the same MS to two journals *at the same time*! If journal 1 takes too long, in your view, to get back to you, prompting you to consider a submission to journal 2 instead, you must communicate in this matter with the editor of journal 1. On this point, everything has to be above board. You don't flout the rules of the community with impunity...

Never lie about the publication status of other examples of your own work, any more than you would lie on your CV. The “community” is smaller than you think, even internationally, and your peer-reviewer may actually know far more than you realize about your ongoing work. Here is a comment of mine on a MS sent to me by one journal:

The author should be advised against making false claims about his own publishing record: in the final sentence of note 78 he claims that his piece on Mao Zedong’s personality and leadership style is “forthcoming” in the *Journal of Cold War Studies*. That MS is in fact still being peer-reviewed and it is by no means a givens that it will be accepted for publication.

I had, you will realize, been a peer-reviewer of the author’s other submission, to the *Journal of Cold War Studies*, and found it to be a very weak piece, arguing against its publication. If a MS of yours is “under review” then that is what it is: do not characterize or think of it as “forthcoming” unless you know for certain that it is.

The criteria by which you will be judged

Now let us go through the important things one by one. (My thanks to the superbly professional editorial staff of *The China Quarterly* for sharing with me the list of publication/rejection criteria on which the rest of this talk is based.) In *anonymously* reviewing your work, your peers are typically asked to pay attention to the following items.

CRITERION 1: *The importance of the topic*. A very slippery concept, this one—“importance”! To whom? For what reason? Here the editor’s personality and personal views need to be factored in, in addition to what may be divined from a journal’s webpage and its editorial direction as published somewhere: the journal XX is a forum for... and publishes... etc.

Of course, you can very well and often will be rejected even if and when you have picked what your reviewer admits is an important topic. This is but the very first proverbial hurdle. Witness the wording of this rejection:

This is a fine paper on an important topic, of general interest to a broad readership concerned with intellectuals and their fate in modern China. In its present form, however, I think it suffers from being vague in what it really wants to achieve. It would benefit, I believe, from a tightening up of the narrative and a more explicit focus on what appears to be its most controversial and therefore attractive contribution to scholarship... This is currently merely an interesting paper, but it could easily be turned into an interesting & provocative/controversial paper. I think it has the potential; all the author has to do is focus it more clearly...

You can see what this reviewer does with his cryptic references to “general interest” (not good: we’re a specialist journal), a “broad readership” (not good: it has to be of interest to a specialist readership, which is almost by definition never broad), the need to

“tightening up the narrative” (ouch! how does one do that, exactly?), and “what appears to be its contribution to scholarship” (meaning the reviewer cannot quite figure out what the author wants to say!). Obviously not a very good paper, albeit on a topic seen as important.

CRITERION 2: *The contribution of the article to the relevant “field” (and/or “subfield”) of study and research.* This is of course what you considered carefully before you submitted your MS. You will have a good answer to this question, you think; but the question is, do your “peers” agree? If you’re lucky, you will receive a comment like this one:

This is a remarkable and in many ways brilliant piece of scholarship. Meticulously researched, carefully argued, and clearly written, it tells a riveting and in many ways horrifying story about the subject. The author's mastery of sources is unmatched, and his understanding of the vagaries of state power and of policy making and policy implementation during this period are remarkably thorough and very sound. In my mind this is one of the best pieces of work on the subject to appear in the past decade, and it sets a standard few researchers will be able to match. This paper is likely to be widely cited in the years to come. I recommend publication with only minor editorial work.

But, such is the anonymous peer-review process, that the author who actually received the above comment on one paper can never be sure of what will happen to a different paper. We all have bad days or years. The same author once submitted a paper that was rejected by a peer-reviewer who prefaced his comments with a suggestion urging the author most strongly to study what were in fact his own earlier publications on the topic in question: they would be able to teach him a lot, and help improve his weak paper significantly... or so the reviewer thought!

Because of the way a reviewer may chose to express it, his *rejection* of your MS may sometimes indicates to you—the author—that you are really on to something new and upsetting to “the field.” That, at least, is one way of reading the following concluding words on a very different manuscript that had been given a positive reading by three peer-reviewers for one of the oldest journals in the Asian studies field, but was rejected by the fourth one, who said this to the editor: “Because I see this way of trying to publish research results as extremely dangerous, I think this article should not be accepted for publication and explicitly exclude the option of trying to convince the author of altering the manuscript so as to make it more convincing.” The editor, who could not afford to alienate this particular peer-reviewer, was extremely apologetic about it, but had to, he explained, reject the manuscript.

CRITERION 3: In the case of an area studies or trans-disciplinary journal, the following aspect is one that responsible reviewers will be asked to consider: *The importance of the*

article and the topic to its relevant discipline (history, geography, political science, economics) and its potential contribution to that discipline.

If you're out of luck, the originality and quality of your research may be judged by someone who sees the academic universe in a way that is quite different from your own. In what follows, the peer-reviewer is obviously a hard-core empiricist historian with little or no respect for that which in political science goes by the name of "theory" or paradigms/models:

I am sceptical of modelling of the kind which the author engages in and I simply do not find his/her account, explanation, and ascription of historical meaning convincing, or, for that matter, exciting and thought-provoking. Reductionist essentializing is something that works fine—this much I am prepared to admit—in the context of a 12 second slot on CNN or even a brief "think piece" in a popular newsletter of some sort. But I would very much hesitate to endorse the publication of this article in a serious scholarly journal or forum for peer-reviewed research.

And yet, should this have been your submission, all is not lost just because one of your peers happens to think your dubious "scholarship" is tantamount to cable television talking heads blurting out a load of rubbish. This particular article was a talk given by its author during a visit to the university where the peer-reviewed journal in question is based. The chief editor of the journal, on the staff of the university and the person who had arranged the visit and covered the cost of it, was very eager to see the article appear in his "own" publication—because its author was seen as senior and to be able to publish him was regarded as something akin to a privilege. And so his intellectually shallow think piece appeared in print in spite of everything, in the journal where it had been given the far from favourable peer review just cited.

CRITERION 4: *Are the main points of the argument supported by the proper citation of credible sources and primary data?* Beware, some of your peers actually *do* read your footnotes with a magnifying glass, and if they are "in the field" they can see right away if and where there's a problem. Compare the following peer-reviewer's comment on a MS dealing with a sequence of events in China in the 1950s, submitted to the area studies journal rated No. 1 in the field:

A careful reading of the MS reveals that of the author's 78 notes (i.e. source notes), a full 68 are to printed books published by a single agency, the CCP Central Committee's Historical Documentation Research Office. In addition, the author in two very unclear notes appears to refer to staff members with that same agency when he speaks of having managed to "*obtain some documents* from the closed central archives of the CCP *via interviewing* some Chinese *fellow historians*." I do not understand what "documents-via-interviews" are, but assume they coincide with what most of us refer to in plain English as "I was told by a colleague that..."

If you seek to impress your reviewer with your sources, do not—whatever you do—attempt to cheat by using a secondary source that makes reference to a primary source, and then pretend you yourself have actually seen and consulted the primary source. A Harvard colleague once referred to a fairly obscure source which she had undoubtedly seen, but got the exact page reference wrong; a different writer later claimed to have used the same source, but obviously hadn't. How do we know? His page reference was also wrong, in the same way as the Harvard author whose work was, undoubtedly, the (secondary) source he in actuality had consulted. Do not cheat! You *will* be found out!!

“To ‘make your mistakes quickly,’” Harvard biologist Edward O. Wilson wrote 1998, “is a rule in the practice of science.” Even in academic fields moving by comparison slowly, such as history, you want to get your work out before it, in itself, is “history.” By this, I simply want to stress that what at one point would indeed have been an impressive array of “credible sources and primary data” may after another few years have become an out-of-date body of citations. Witness the following reviewer’s comment on what had started out as a fine piece that, by the time it was submitted for consideration, needed to be reworked and incorporate a lot of new data that had appeared in the interregnum:

I am very ambivalent about the ms given that, on the one hand, it represents a solid piece of scholarship that certainly deserves to be published, while on the other hand it clearly no longer represents the “state of our knowledge.” By this I mean to say that virtually none of the numerous important new sources of information on the Great Leap Forward to have been published in China since the end of the 1980s (or otherwise have become available outside China in recent years) have been consulted by the author. Had this ms been submitted in the late 80s, I would not have hesitated to warmly recommend its publication. Today, I find myself thinking, again and again, “Why, this is a point on which he should have consulted X!” and “Awgh, why does he not look through Y in search for the answer? I know it’s there!” My proposal is that the author “update” his ms by rewriting parts of it on the basis, first of all, of the works I list in my comments. If he does this and does it well, then there is no problem and he should resubmit without hesitation.

CRITERION 5: *The logical coherence of the author(s) argument.* You’d think this was pretty straight forward, would you not? Alas, in the world of cutting edge science and publishing, one man or woman’s utter incoherence or incomprehensibility may be another man or woman’s proof of true greatness. So do not fret too much about this, except seriously. Once you’re a name in your chosen peer-reviewed field, you can get away with the most remarkable things. A Guggenheim Fellowship-winning professor of rhetoric and comparative literature at the University of California at Berkeley published

the following sentence in “Further Reflections on the Conversations of Our Time” in the scholarly journal *Diacritics* (1997):

The move from a structuralist account in which capital is understood to structure social relations in relatively homologous ways to a view of hegemony in which power relations are subject to repetition, convergence, and rearticulation brought the question of temporality into the thinking of structure, and marked a shift from a form of Althusserian theory that takes structural totalities as theoretical objects to one in which the insights into the contingent possibility of structure inaugurate a renewed conception of hegemony as bound up with the contingent sites and strategies of the rearticulation of power.

Perhaps you would like me to read it again, slowly this time around? Obviously, you should aspire to coherence and logic. But the truth of the matter is that once you are established—and timid, irresponsible editors begin to publish just any rubbish you may chose to send their way, instead of asking you what on earth you’re trying (but so miserably failing) to say—there are actually prizes to be won for writing badly!

I urge all of you, if you worry about your own clarity of exposition in the English language, to look up the scholarly journal *Philosophy and Literature* and its internet discussion group PHIL-LIT (http://denisdutton.com/bad_writing.htm), which between 1994 and 1998 sponsored something called the Bad Writing Contest. The contest attempted to “locate the ugliest, most stylistically awful passage found in a scholarly book or article published in the last few years.” Entries, it was explicitly stated, *had to be* “non-ironic, from actual serious academic journals or books. In a field where unintended self-parody is so widespread, deliberate send-ups are hardly necessary.”

You may only know Homi K. Bhabba, Director of the Humanities Center at Harvard University, as the author of works like *Nation and Narration* (1990) and *The Location of Culture* (1994). Well, if you admire his work and dream of one day writing something equally influential and paradigm-shifting, take note of the fact that Bhabba was also the runner-up in the 1998 Bad Writing Contest, winning the much-coveted second prize with the following sentence:

If, for a while, the ruse of desire is calculable for the uses of discipline soon the repetition of guilt, justification, pseudo-scientific theories, superstition, spurious authorities, and classifications can be seen as the desperate effort to “normalize” formally the disturbance of a discourse of splitting that violates the rational, enlightened claims of its enunciatory modality.

On the whole, you should seek to express yourself clearly and try to “make sense” even to the odd reader of your work who only knows plain English. What F. R. Ankersmit, professor at Groningen, Holland, said about historians holds equally true for all of us, regardless of which sub-field of the humanities we are in: our “capacity to develop

(metaphorical) narrative scope is the most formidable asset in [our] intellectual arsenal.” Humour, even, should not be avoided in serious writing, nor should making something genuinely interesting or enjoyable. The following rejection letter from a leading academic publisher (shared with me by a colleague in Bangkok) is, of course, a spoof:

However, I regret to inform you that the consensus judgment of the reviewers is that the proposal should be rejected for publication by our prestigious academic press. The reason for this recommendation lies in the simple fact that the proposed book is too interesting to be published as an academic source. I'm sure that you can appreciate that to publish an interesting academic book has the potential for scandal that would greatly tarnish our reputation. We have actively avoided the publication of anything interesting, and while the material covered by your proposed book has scholarly merit, its level of interest has the potential to lower our reputation in the academic world. Imagine the embarrassment of some faculty if they were to find out that their students actually enjoyed reading any of their assigned materials. It should never be the case.

CRITERION 6: *To what extent does the author engage issues and arguments made by others in the field (should the author be aware of other specific works?).* One brutally forthright ethnographer once wrote to another, saying “If you have to read about it in journals you are out of the loop of research.” Well, she was speaking of high energy physics communities, so maybe her comment does not quite describe what goes on in the humanities. But this is a very difficult question, perhaps even more so for us in the (by comparison) more out-of-the-way place in which we work, than say for our colleagues at Cambridge, Oxford, or Berkeley.

Some authors just don't seem to realize beforehand how likely they are to seriously fail the peer-review test if they go wrong here—by asserting with a disparaging twist that something negative about the field just “is the case” without having serious source-notes to back it up. Some authors maybe genuinely ignorant when voicing their criticism of this or that “evident flaw,” but others seem to me to be merely bluffing or intellectually lazy. One reviewer once refereed an article whose author was dead set on leaving his mark by overturning all the received knowledge about the operation of China's legal system under Mao, and somewhere at the beginning of his MS he claimed the following:

However cogent and effective the recent revisionist arguments are, *there has not yet been any archival research that could challenge* the dominant view of the lawlessness of the Cultural Revolution, one of the crucial factors underlying the Chinese government's total negation of this period.

OK, great stuff—if it were true! His submission amounted, of course, he genuinely seemed to believe, to that first supposedly pathbreaking, archive-based serious probe

that would overturn the “dominant view” and make him an instant authority. The problem was of course that there had been plenty of archival research and that in this case, the author’s peer was among those who had been conducting it for quite some time. Here the attempt to “engage” with (read “against”) “arguments” in the field went badly wrong... and so, being after all human, the peer could not resist the temptation to write the following:

Comment: Archival research on the CR is being carried out on a fairly wide scale, at least since the second half of the 1990s. The author’s assertion here is mistaken. The reason why some of the archival research does not challenge the “dominant view of the lawlessness” is because lawlessness, contrary to what the author asserts, was quite prevalent.

First lesson for those of you who do, as you should, attempt to engage: do it with modesty, and make damned sure you can back up your claims with good data. Even if you don’t quite believe it, there is nothing wrong with writing a sentence like this to preface your new finding that will overturn the received wisdom established, in this case, almost singlehandedly by Professor K of the United States:

When K wrote the book [title withheld], most of us would have agreed with his assumption that the CCP Central Committee is a signatory to each and every CD.

But the truth is not quite so simple, as our extended data base now shows...

Second lesson: do not “engage” with the field by writing a piece that serves no other purpose than to show that Professor L was wrong in his book from 1999, if and when the reason you are now able to show this is because you have certain new data that was unavailable to Professor L back then. Exciting new data is a good entry ticket, but it is not enough. Exciting new conclusions, convincingly presented, based on new data, are what truly “engaging with issues and arguments” should be about. It is what stands you a far bigger chance of getting onto the pages of a peer-reviewed journal.

CRITERION 7: *The originality and quality of the research.* This is where things get to be really interesting. What you want to aim for is a brief comment like the following, and little else:

It is exceptionally well researched, covering a wide range of Chinese sources, many of which are not easily accessible. The analytical approach is mature and nuanced. The text is well written, with a delightful passion. It is really one of the best pieces of scholarship on the [...] that I have seen in a long, long time.

This, of course, is the criterion that some peer-reviewers may actually have a bit of a problem dealing with. One reviewer’s commendable originality is another reviewer’s rubbish. One reviewer whose expertise in your narrow area of work is limited may regard what you have written as being of good quality, while another genuinely expert reviewer may hold a very different opinion. Reviewer B recognizes it as largely cribbing the work

of an underappreciated colleague who only publishes in a language not known to reviewer A. Reviewer X confuses “originality” with beauty or coherence of exposition, while reviewer Y is frustrated by the slick penmanship of a widely published colleague—an arrogant tenured ignoramus she rightly views as an Ivy League apologist for the dreadful academic status quo in the “field.” (She knows the author’s identity from the giveaway 1st footnote of the MS that reads “For more on this, see my work [title]”)

CRITERION 8: *The technical nature of the manuscript, e.g. how much copy-editing does the prose require, are tables and statistical data in order, etc.* I will not address this question here today, since it is my understanding that others will. All I want to say is that some editors are known to have gone to remarkable lengths in order to help the author of a poorly written but in the intellectual sense genuinely significant piece get it published. A young Swedish graduate student (still working on his PhD) who in the mid-1980s submitted a short piece that was obviously “one big mess,” in terms of how it presented its findings, to an American journal, was able to publish it all the same after the editors—without consulting him—had almost completely restructured it, and then written him and said something euphemistic to the effect of “we did some minor syntactical and other changes that we hope you will not disapprove of...” So do not despair, if you have something important to share with your field but do not perhaps yet quite know *how* best to share it...

CRITERION 9: *Is the paper an appropriate length or could it be shortened? Please suggest possible areas to be cut.* As author, always aim for the length specified by the editors or publishers of your journal. But, in the first instance, pretend you did not notice that it said 8,000 words *including* references. Go for 8,000 words of text plus another thousand or so of notes and references. Assuming your piece satisfies criteria 1 to 8 above, getting some extra space will not be difficult. There is a reason why this is typically the last criterion on the shortlist of “things to bear in mind” given peer-reviewers by editors.

I will stop here and take your questions. Thank you for listening!

Peer comments and criticism may be sent to:

michael.schoenhals@ostas.lu.se