Under repair: a publication ethics and research record in the making
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Editors act as gatekeepers for serial objects that, while no longer the exclusive or dominant media of science (Delfanti, 2016), are still its main authenticating site (Silbey and Ewick, 2003): scientific papers.[1] If the witness becomes less perceptible as facts become visible – 'modest witnessing' in Haraway's (1997) terms – then the labour of editors can be 'modest enough' to function as a key literary technology of objectivity in publications. But at the 'Forum' of the Committee on Publication Ethics, around a table or on webinar, journal editors and publishers alternate between types of witnessing. In some moments their witnessing can be made visible and more immodest: When editors share a personal anecdote, debate a particular chronology of events, or ask questions about the location, social conventions and hierarchical arrangements within which a particular research was undertaken, they pay attention to embodied and institutional practices. And yet immediately afterwards, editors are able to swiftly retrieve modesty and re-install distancing (Biagioli, 1993; Candea et al., 2015). In doing so, they show their reliance on the 'research record'. They demonstrate their commitment to repair and restore its integrity when they perceive it as potentially threatened by external human agents – humans, documents and 'partisan projects' (Haraway, 1997).

With this article I dissect a slice of academic life 'at home' (Lederman, 2006) by tapping into arguments that infuse the backstage of publications as publications. I introduce a number of patterns in publication ethics work performed by editors and publishers; these should be of interest to contemporary ethnographers of science but also to students of norms and regulation around knowledge formation and scholarly communication. Using ethnographic insights, I extend publication process research (Hirschauer, 2009: 72) beyond the ordinary life cycle of the scholarly communication process, by looking at controversies post-publication. In turn, I reframe, and denaturalize (Csiszar, 2017) publication from being a moment in time (when it's 'in press') to a series of events, interventions and objects (Delfanti, 2016).

Ethics has been the object of scholarly attention in recent years (Fassin 2014; Laidlaw, 2014; Jacob and Riles, 2007). In parallel, a growing body of work has ethnographically studied the daily life of 'doing good' through the use of ethics lexicons in settings such as research with human participants (Fassin, 2006; Hedgecoe, 2012, 2013; Lambek et al., 2015), corporate social responsibility (Rajak, 2011) and animal protection (Reed, 2017). As for publication ethics, its originators and practitioners define it as relating to the field of 'research integrity' (Jacob, 2013; Ho, 2010; Montgomery and Oliver, 2009) and including a range of problems such as disputes between authors and authorial behaviours such as plagiarism, data editing, image manipulation, and ghostwriting, amongst others (Biagioli, 2014; Frow, 2012; Sismondo, 2009).[2] In recent years the issues in publication ethics have diversified and journal editors thus face novel and increasingly creative forms of conducts that have been little addressed so far by policies and even less theorized by non-practitioners: instances of fake peer reviews and of inventing fake peer reviewers, so-called predatory journals, as well the impersonation of real individual reviewers (Biagioli, 2012; Xia et al., 2015).

Studies of research misconduct have made significant contributions to scholarship, including detailed studies on the history and sociology of deviance within science and on individual controversial cases, and types of misconduct. Our knowledge of the increasing scale and varieties of misconduct and of the number of policies aiming to foster research integrity has grown considerably as a result.[3] More recently, studies of research integrity have strived to document quantitatively the incidence and motivations of researchers' misbehaviors, delineated degrees of gravity in research misconduct, and made important contributions by fostering research integrity policies in institutions, (Fanelli, 2009; Godecharle et al., 2013; Mayer and Steneck, 2012; Redman, 2013). However, the implementation of legal and regulatory frameworks has so far remained under-theorized, and this has limited the possibility of grasping the distinctive interpretative challenges that engagement with research integrity problems poses to legal and other experts. Empirical work has evaluated normatively whether research integrity officers are well prepared to handle their activities (Bonito et al., 2012), but no studies have explored in depth how decision-makers reason about publication ethics and the 'backstage' processes (Hedgecoe, 2012: 7) through which decisions are arrived at.

These debates take place while 'publication ethics' itself has flourished through some of the conventional paths to professionalization. Self-trained experts have created and developed a new practical discipline to respond to institutions' concerns about misconduct, producing textbooks and course materials and training science students at avoiding and detecting misconduct. Start-ups, charities and international stakeholders networks[4] have organized themselves in voluntary partnerships at a global scale to develop best-practice toolkits, articulate definitions, create accreditation programmes and organize training workshops (Montgomery and Oliver, 2009). Self-help groups are organized where editors can turn to for advice when problematic submissions and allegations of misconduct land on their desks. So the committee whose work is at the centre of this article is part of a larger network of more or less formalized organisations that do publication ethics work. The 'Forum' is an example of publication ethics work; it takes place in a boardroom or webinar space where academic editors and publishers air and debate anonymized allegations of dubious research conduct. Publication ethics can be described as a 'contest', in that it is being constantly 'rewritten and recompiled according to diverse, partially shared, shifting, and incomplete objectives, not only by individuals but also by corporations, governments, and universities' (Kelty, 2005: 185). The flurry of objectives and activities that form this publication ethics contest means that it has become a field that stimulates relationships (cf. Andersson, 2014), to which practitioners, policy-makers but also academics have an interest in attaching their activities.

My material aims to show how its members tell us something about how creative and normative the work of publication ethics is. In this sense, taking on board Timmermans and Berg's insight that standardization is about 'creating new worlds' (2003: vii) I show that publication ethics and research records themselves get created via the language or repairing publications. Yet my material modulates Timmermans and Berg claim that standardization is not about uniformity: to my participants, harmonization is linked to the ability to invoke, apply and disseminate the created world of 'best practice' of publication ethics. Indeed, the idea that the same publication ethics problem could be found in different locations (see Busch, 2011: 98) is critical to the work at issue here. I thus hope that the article contributes to conceptual debates on ethics, best practices and standards in scholarly communication but also in other scientific fields.

In the words of one of its founders (Farthing, 2011), the Committee on Publication Ethics (COPE, hereafter the 'Committee') started in 1997 'as an experiment'. My goal was to try to elicit new analytical objects that re-describe publication ethics as a form of expertise, beyond (and despite) the rehearsed axioms of its now well-established professional field. This is a field made of adjudicatory experiences, deliberations and tensions. In the remaining parts of this article, I offer an analysis of the forms of doings that publication ethics in action can take during what is called the 'Forum'. The article does not reveal publication malpractices; rather, it examines recurring motifs within the review of publication practices whose ethics is called into question by my participants. These motifs include: the shaping of publication ethics as expertise that can be standardized across locations and disciplines, the separation of the research record from relations that produce it, and the divisibility of the scientific paper. Together they institute an ethics of repair at the centre of the curative enterprise of the Committee. As these forms of doings become categories in themselves they have the potential to generate new vocabularies to think about editorial labour and expertise within scholarly communication.

Entering a committee of publication ethics

At the time I began fieldwork, the Chair had called the Forum a 'self-help group for editors'. 'It's an open forum for the sharing of views; we don't decide or judge', she explained. The forum thus offers an interesting mix of features: transparency, a public and deliberative nature (cf. a tribunal), a case-by-case type of engagement with issues, and yet all this combined with an anti-adjudication tenet. 'We are not the police', was a point I often heard in the meetings over the years (see Frow, 2012: 375). These values have remained despite change in the operation of the Forum.

In its newsletter, the Committee likes to recollect how it was born, when 'an informal group of mostly male medical editors sat at a table together drinking tea … and discussing common ethical problems' (Wurz, 2012). Convened by Michael Farthing from Gut, Richard Smith from the British Medical Journal, and Richard Horton from The Lancet, the first meeting took place at the British Medical Association House in London: 'We discussed cases, and I think that we found it interesting and all learnt something. It was a very informal atmosphere, and we laughed and had fun', recalls Smith (Horton et al., 2012: 5). The group of about fifteen editors kept meeting

regularly to 'tell each other stories' (Horton et al., 2012: 5) but following legal advice they soon began to anonymize all the cases under discussion, and clarified their exact remit; they 'weren't making decisions … simply offering advice' (Horton et al., 2012: 5). The Committee became a membership organization for journals, and only fee-paying members could submit a case. From that point onward, a more formal procedure was established. Members could submit a 'query' by email or via a website. When received, the query became a 'case' and was processed: personal information remained confidential, as a paid administrator formatted the case into a standard template or solvable puzzle (Kuhn, 1962: 36): an anonymized summary followed by questions. Every four months the administrator compiled the cases into an agenda for a meeting, sent for approval to whoever is chairing the meeting. The agenda was posted on the website about a week before the meeting, and the officer emailed all members with information about how to register for the meeting and a link to access the cases in advance.

When I began fieldwork there were five or six cases per meeting, sometimes as many as nine. In the last year of fieldwork there could be only two or three. Proportionally speaking, there are fewer and fewer cases raised, as the Committee membership itself underwent a spectacular expansion in the early 2010s. Initially the 'cases' constituted the main course of the Forum: The meetings' agendas listed only actual cases as items for discussion. Most of the duration of the Forum (usually an hour and a half) was spent discussing the cases, following a ten-minute Chair's introduction on the procedures (e.g. keeping names out, taking turn) and update on the Committee's other initiatives. Today the Forum is only one of the many Committee's activities, and gradually less time gets devoted to the 'cases' and more to reporting to participants about the governance activities of the Committee as a charity, and about the dissemination of publications ethics via various outlets – the newsletter, training tools, the organization of seminars and the participation of governing members in international conferences. Combined with the production of guidelines and flowcharts, these activities further the standardizing work of the Committee (see Frow, 2012; Rappert, 2001). And yet, in the view of the Committee, the 'Forum cases' still remain its staple contribution to research community. This explains their central place in the present article; I discuss other publication ethics activities and their implications elsewhere (Jacob 2014; in progress).

Council members of the Committee (the Council includes journal editors, publishing consultants, academics and one lawyer, and in the past one medical writer), and anyone who has editorial functions for a member journal or who works for a member publisher, can come and contribute to exchanges in the Forum. So far, in the Forum's space (physical or virtual), suspicions of questionable conduct are discussed in real-time and participants share their experiences and offer suggestions of responses and advice as to what to do next. The Forum used to convene on a quarterly basis on the day of the administrative meetings of the Council. Since 2013, it has met mostly via webinar but kept one live Forum per year.

The research consisted of repeated ethnographic observations of publication ethics meetings, including the 'Forum' meetings, annual seminars, Council meetings and strategy days organized by the Committee. In addition to quarterly Forum meetings, seminars are annual or semi-annual, thus the research has been conducted over a long period of time, between December 2010 and May 2017. Overall, I have observed fifteen gatherings in London and High Wycombe, and also observed some of the Forum meetings via webinar from my office at my home university. Below I present a number of vignettes, in the form of numbered cases that evoke recurring patterns in the discussion. The 570 cases examined by the Committee to-date are all anonymized, catalogued and stored in the Committee's database for members to consult (see Committee on Publication Ethics (COPE), 2017). I selected a number of them not because they were representative, but rather because they meaningfully evoked patterns of knowing, deliberating and doing publication ethics. In this sense the vignettes are specific evocations of larger patterns, here encountered in specific forms (Strathern, 1992; Jacob 2012).

As I followed its actors, the milieu of publication ethics sorted out explicitly what was relevant and trivial about the object of my research. A key dividend of this approach is that my participants made publication ethics and misconduct tangible. In this sense, for the purpose of this study, publication ethics became something that exists because my participants talked about it.[5] This article thus deliberately adheres to the technical language of 'best practice' that the Committee members mobilized in order to provide an evocative image of how publication ethics and misconduct get made in globalized epistemic communities. Indeed, the members mastered a techne in the classical sense,

that is, the power and capacity 'to produce things whose eventual existence was contingent upon the exercise of that power, things whose existence was "caused" by the craftsman, rather than by the operation of necessity or nature' (Pottage, 2002). This is of course not to say that publication ethics' concrete existence is in doubt – the people I've been observing labour hard to solve problems that are very real – but rather it means that the origination of publication ethics is connected sine qua non to the 'ways of knowing' shared by the members. In this sense I agree with Sapp's claim that there are problems of research misconduct because there are instances of allegations of research misconduct, not the other way around (Sapp, 1990: 23; cf Latour, 1987).

I made the ethnographic choice of deliberately relying on my participants' self-representation and their carefully tailored and protected terminology to describe what is at once their concern and my object of study. This choice followed the consideration of work by others on the dividends and limits of using authorized self-representation as the starting points of ethnographic analysis (Mosse, 2006; Riles, 2006). My effort to retain an empathic outlook on publication ethics practices, and to privilege neither denunciation nor acclaim, takes its cue from the work of Reed (2015) with libertarian activists and Hedgecoe (2012) with research ethics committees. Here it has translated into a distinctly collegial ethnographic relationship with my participants (see Riles, 2006). It meant that my work has been appreciated by the charity's leaders as 'interesting', I have been invited to speak at research and policy events organized by my participants or their colleagues, and excerpts of my research viewed as positive about the work done have been extracted and mobilized to showcase the charity's activities to its own members as well as to other organizations. My participants commissioned me to write a short piece for their newsletter, something I gladly did, both to express my gratitude for their generosity and to seize a rare opportunity to engage a community of research integrity practitioners. I can say that I have been enlisted by my subjects, and in more gradual and much less formal ways I have enlisted them while going through ethics review and preliminary research funding applications. My entry into the field was relatively smooth, infused with the positive programmatic terminology of research integrity and the highly topical concerns of open access and transparency. As one could expect, a committee branding itself as a global champion of publication ethics seemed well disposed from the

beginning to encourage research and dissemination about itself, and to welcome an ethnographer to observe its debates.

Standardizing publication ethics

Editors work under constraints – the informal canons of academic disciplines, sponsors and formal peer reviewers' reports (Hirschauer, 2009) – and also retain a sphere of discretion, within which they make 'judgment calls' in selecting papers for publication. When suspicions of misconduct by authors or peer reviewers arise, editors also have to make judgement calls as to what course of action to follow. Whilst they do their volunteer work at the Forum, editors (and also publishers and publishing consultants) advise other editors in their response to what are perceived as 'tricky' cases. For instance, experienced participants encourage other editors to abide by one of the Committee's published flowcharts or to find a solution by following their 'gut' feelings. In turn, the Forum produces evaluations of publication practices and editorial conduct itself, rather than pronouncements on the conduct of authors. While participants raise questions about authors' behaviour, its remit is to support its members by offering them advices.

The collective probing work of the Forum could seem resonant with the work of research ethics committees (Hedgecoe, 2012, 2013). Their members are volunteers, and both sites deal with potential or actual deviance. However, the remit of the Forum differs from that of these committees in that it deals with the portion of the research lifecycle during and following the publication process. Moreover, research ethics committee members need to scrutinize documents handed over by researchers themselves, and then detect and make explicit ethical issues and problematic behaviours that could arise in the future. They regulate by anticipation (Hedgecoe, 2012; Murphy and Dingwall, 2007), whereas Forum members are given an already delineated puzzle to solve.

What members do in Forum meetings resembles more a routine task in academic work – peer evaluation or peer review – than an adjudication of conduct by a disciplinary panel or research ethics committee. Peer-review and editorial decisions are distinct, but both offer helpful analogies to approach laterally the practices of the Forum members as a form of collective and 'live' peer evaluation (e.g. Cole et al., 1981; Hirschauer, 2009; Zuckerman and Merton, 1971). But although peer review is

often framed as a solitary act done in writing, here we face reading and evaluative events (Reed, 2011) that are collective and almost entirely framed by documents.[6] Ethnographic studies of scholarly 'reading events' are scarce. Hirschauer has shown that from an editorial perspective, 'review' is not an examination when readers look asymmetrically at a text but a reciprocal process where 'observations of judgments' complement and compete one another (2009: 74; see also Harnad, 1982). Other studies of academic review have centred not on dubious practice but on excellence. 'Intersubjective generalization' and mutual translation are thought to best ensure the fairness of evaluations in research funding panels (Brenneis, 1994; see also Callon 1994). To Brenneis, for instance, fairness is translated into normalization and generalization (1994), and evaluators can achieve fairness when they use a 'special ability to take on the style of the 'other,' to alternate between different social worlds and translate between them' (1994: 31). Some scholars (e.g. Mallard et al., 2009), however, point out that when trying to produce fair evaluations, panellists have tended to privilege contextualization, not generalization. Mallard et al. (2009) found that in research funding bodies' panels, peer evaluators adhere to 'cognitive contextualization', that is, they use specific – rather than universalistic – standards, that are the most appropriate to the respective object of their evaluation. Depending on the 'intellectual conventions and epistemological styles' that apply to that particular field, panellists will use specific criteria or lenses to evaluate fairly. In multidisciplinary panels, cognitive contextualization appears to privilege respect for differences and 'contextual fitness' over disciplinary or methodological hegemony (2009: 5-6). This rendition of collective live evaluation performed by funding bodies' panels has some resonance with what I observed during Forum deliberations. However, unlike funding panels, the Forum does not deliver a grade or result but rather advice, in the form of a published case, which editors can then rely upon while solving a problem in their respective journal.

The Forum thus carries unique properties that have no parallel in editorial boards or panels where funding decisions are made. In addition, while taken by the task of evaluating conduct, Forum participants were uniquely engaged in crafting their object of reference, here the research record. Deliberations over publication ethics draw on elements from both generalization and contextualization in the review of conducts, but the result is yet a distinct mode of evaluating, which emphasizes the nature of

publication ethics: one that can be made universal and thus standardizable.

Different strategies were employed to assess allegations of misconduct appropriately within the Committee's Forum. The language of fairness is not explicitly articulated, but indeed both generalization and contextualization appear to take place so that members can understand one another. Mutual respect and deference occur between members of the Forum, especially between 'repeat players'; that is, the Committee's leaders who also participate regularly and actively (cf. Sacks, 1987, as cited in Hirschauer, 2009) by asking follow up questions, offering advice, or making analogies with cases discussed in a previous Forum.

Following criticisms by some members that the Committee was still 'very based on medical sciences', there has been an effort 'work on' and 'battle with' this by including subjects outside of science, technology, engineering and medicine in the Forum, including calls for Council membership that specifically invite applications from editors of social science and humanities journals. At times, the medical and natural scientists underscore their disciplinary background, emphasizing as much 'what they do not know' as what they do know (Jacob, 2011) as a way to carve out their particular, context-specific domain of expertise in evaluating publication ethics and misconduct. Yet, most of the time far-reaching understandings of research integrity principles were thought to ensure not necessarily more fairness between constituents, but more 'public good', thus suddenly enlivening a common public for publication ethics.[7] Specific forms of misconduct were also perceived as having a universal element, and members would mention that certain norms should apply regardless of the disciplinary background, ethnic origin, career stage or experience of the author. The 'theft of someone else's ideas', for instance, seemed so universally and inherently wrong to a senior member that 'you should not have to teach this'. Another governing member, trained in the humanities, articulated a further sense of generalization by emphasizing the universal and trans-disciplinary character of publication ethics:

> It's more about people in humanities and social science feel that they
> have been addressed, rather than changing anything. … I often speak
> with people from humanities and they claim it's distinct but actually the
> issues are the same across fields

In the Committee, the term 'culture' was used on several occasions as shorthand to descriptively elucidate – not normatively justify – certain conducts. 'Culture' thus erected a 'us' versus 'them' discourse in talking about scientific publications. Terms or phrases such as 'Asian' and 'Saudi Arabia and the like' were used on some occasions as shorthand to explain and make sense of practices that deviated from the norm. Such malfeasance (often relating to plagiarism, but also more recently with impersonation) was rarely appreciated on its own terms or evaluated on merit, but rather explained away with the proviso of 'culture'. Active members are part of a globalized scientific community that cherishes diversity but within universality.

In turn, a certain use of diversity can itself support the global, harmonizing, making-uniform nature of publication ethics. Ciotti (2014: 64) questions whether 'searching for difference' is still a productive line of inquiry in the study of global knowledge production, as other lines might be more productive in capturing global forms and what sustain them. In this respect, the recruitment in 2012-3 of Council members from Brazil, China and Iran did not serve to enhance the diversity of views on publication ethics; instead, it strengthened and better demonstrated, through the mobilization of regional champions, the culture-free universalism of the principles of publication ethics. Here, 'difference' can take the form of international membership, inclusion of Iranian or Chinese scientists, or referring to insights from scholars in humanities. This 'difference' accumulates cultural capital for publication ethics as a uniform good and helps with its global circulation. Difference is rather about the combination of practices that produce, stage and circulate it.

Further, diversity in the sense of its 'inclusion into the terms' of the discourse and self-fashioning of an organization (cf. Ahmed, 2012) can give harmonization a new style. During a strategy day, Ahanti, a market research survey specialist, described the results of the members' survey her firm conducted on behalf of the Committee. The survey was meant to discover members' view of the organization itself, yet, as Ahanti explained, one of the findings was that a number of members have concerns about other members themselves and their practices: specifically, their 'cultural and regional differences', and 'them not appropriating Western/international publication standards'. 'Gifting, when an honour is bestowed upon a person, an author who didn't contribute, is seen as a normal way to work in some "culture"', noted Ahanti, who put quotation marks around the term 'culture' on her PowerPoint slides. Her studious use

of quotation marks, here and there, emphasized that the terms used were not hers but rather those of the members. This usage reiterated to the governing members a clear message – this time data-driven, and packaged in expertise, that culture was 'a problem' separated from, rather than enmeshed with, the working out of a uniform publication ethics. As much as members cherish the idea of respecting differences, the discussion often embraces a 'pluralist vision of the world [understood] as an expanse of private exclusive clubs, interacting with as much civility as they could, but each defined, animated, and sustained by a vivid sense of the difference between 'we' and 'they' (Hollinger 1995: 67, cited in Mallard et al. 2012: 6). As it often happens with epistemic communities (Haas 1992), Committee members are part of a cosmopolitan elite, which cherishes diversity within universality.

Differentiation is not exclusively within the remit of 'culture.' Lewis, an editor and one of the Committee's governors until 2013, once raised the issue of his age or generation as an explanatory device of why they saw a certain conduct as misconduct, and often mentioned his home discipline as a background for his views. In case 11-03, the editor invited to present a problem to the Forum reported that a researcher contacted them when she realized she had been plagiarized when she was peer reviewing a draft for their journal. The editor reported that the senior author apologized, the junior author was contrite, and that the incident had been resolved amicably through an agreement between the senior author of the paper and the plagiarized researcher, who accepted being added as co-author on the paper. After summarizing the case, the editor asked the Forum 'so did we do the right thing, what else should we do, or should we have done?'

> Chair: Any thoughts, reactions on this tricky case?
>
> Isabelle: A good resolution, it was caught, and everyone is happy.
>
> Chair: One reaction is: Happy with it (and several people nod around the table) .... One point of suspicion is: Are senior authors hiding behind the junior author? How did the junior author know how to look for this dissertation, it was an unpublished thesis. Question is whether the senior authors should have remembered this PhD dissertation. It sounds like a good outcome, they might be a bit shaken, which is good.

Editor presenting the case: They felt bad, they apologized.

Patrick: There's awareness within the department that work that's being done has changed. These days work is constantly being presented, it's much more public, it is difficult to remember all the work that is done in one's department.

Lewis: We forget the point: that the first author is guilty of theft. The first author, who stole, is getting away with his bad behaviour. I don't think it's in line with editors and COPE's practices. That they are more contrite is noted, and we're going from there. The first author should be more than chastised.

Isabelle: Some young scholars are unaware of the rules, they are not well mentored.

Lewis: The author did not own the science. It's a theft in the world. I don't believe that one needs to be instructed not to do this. ... I don't know, maybe I'm just too old.

Vivian: We don't know what instructions the junior, first author received. It's hard to judge.

Isabelle [nodding]: It's hard to judge.

Chair: I'm looking at it from the point of view of the advisors of the researchers ….

Lewis: I'd be concerned about my students whose work is being stolen, and about the integrity of the person who stole. I don't think you need to teach this. It's theft.

Chair [to the Editor]: have you thought of informing the institution? Some may say it's an overreaction, but somebody attempted to plagiarize.

Vivian: In a letter to authors, you could say you take plagiarism seriously, but say here in this case it is a junior author, here it's

someone who has not been well mentored, and ask what they plan to do
in the future.

Marc: The laboratory may feel ownership of the work done in their
labs. They feel it's theirs, that they have ownership of the work.

During the exchange, Lewis was shaking his head, making a value judgment about the contextualizing efforts of his editor colleagues and expressing his nostalgia towards what he perceived as the more generalizable, ethical and substantively better authorial norms of the past. Up until Marc's interjection, to which I will return in a moment, everyone around the table apart from Lewis seemed content with an amicable hence 'good' resolution. Everyone feeling 'happy' speaks to the civility and professionalism with which the members conduct them selves and wish to be seen.[8] It also speaks to the trust members feel towards one another, and to how a collective feeling of being satisfied, shared across the table, provides a signal of the common sense and harmonized nature of publication ethics. Even Lewis, who disagreed with the substance, relied on the common sense generalizability of the idea of publication ethics, arguing you shouldn't even have to even teach it. The contriteness of the perpetrator is also part of a good resolution here: 'feeling bad' about a transgression could vouch for internalizing, but also harmonizing publication ethics, because it meant that the pedagogy of the Committee works by reaching, educating and impacting its publics in some form.

But if we return to the excerpt above we see that it was at the end of the debate, after the Chair had synthesized the views about the good resolution (leaving the dissent of the 'too old' member aside) that the only genuine challenge to the category of authorship surfaced: Forum member Marc suggested that perhaps there had been no plagiarism, after all, that the research belonged to the lab and the author had acted reasonably in using it. This view starkly contrasted with everything that had been uttered during the debate. It suddenly made those who had disagreed look as if they had been in agreement. The last minute intervention posed a radical challenge to the very terms of the shared understanding of publication ethics. Yet it seemed to hang there. Marc's point was neither foreclosed nor dismissed by others around the table. It triggered no response; there was a pause of a few seconds, during which the debate

was suspended. And the Chair then thanked the group for the helpful debate, and the Forum moved to the next case. Here, a member's effort at 'cognitive contextualization' – that is, at thinking from another's perspective, and at imagining how else publication ethics could be made – failed in gathering immediate interest around the table. This member's voice was not silenced as such during the event (cf. Ryan-Flood and Gill, 2010), but it was silenced in that it didn't make it to the record, that is, to the case summary published after the meeting.

The standardizing work also occurred in a case that particularly epitomizes the amplification of the expertise of publication ethics. At the beginning of a Forum, before turning to cases the members had a general discussion on a theme raised by a representative of a small publisher, about the publication of offensive material. The publisher was worrying about the use of certain terms used in the proofs of an accepted article from the field of alcohol studies:

> We don't want to censor, but when does material become
> unacceptable? It's research on binge drinking, sexual violence … you
> see the kind of material ... I find it unacceptable … could it be
> removed without detracting from the main argument … If the method
> is rigorous, should we leave it? The researcher had to get intoxicated
> too in order to get trust [mocking smile] … and they had to debrief
> when were sober …

The Chair asked for feedback around the table. Raising her hand first, Christine, a nursing editor and very active Forum member, immediately kicked off a lively exchange:

> Christine: But what is the research? What is the hypothesis of the
> research? That's the first thing to …
>
> Publisher: I think, not sure, I guess to explore further why people
> behave in this way?
>
> Christine: There are two issues, one is the ethical conduct, consent of
> participants and debriefing. And the second issue is the content of the
> research, it's probably not offensive to the readers of this journal
> although it might make people blush …

Publisher: Perhaps I'm a bit prudish … we wondered if it would be ok to put asterisks instead of certain words?

Forum participant: This is valid scientific research to care for people in this community. I'm a social scientist. Everybody who does qualitative research need to bring it back to the literature, if the paper is well written, pulls everything together and advances the literature, put the quote in context.

Chair: I agree.

Lajos: I work on literary magazines, on history of literature, think of William Burroughs, Bukowski, they would be out, there is no way! As long as the method is sound, have the quote, however obscene.

Publisher: You wouldn't put an asterisk?

Lajos: I wouldn't dream of it.

Chair: It would be bad practice?

Lajos: Certainly!

Publisher: but what about expletives? I don't want to be prudish …

Other forum participant: It has been accepted by peer-reviewers. Actually, for non-English speakers, the asterisks would just be confusing …

Publisher: I don't want to be a feminist, but some words are offensive to women, it's unpleasant …

Chair [firmly]: But this is precisely the point. I work in a medical journal and in medical journals you show pictures of injuries …

Other forum member: If there is an ethical issue that's another matter. The data is critical to report the case studies.

The Chair quickly wrapped up the discussion on this particular case, mentioning that people agreed that the quotes have to stay in, adding that the discussion had been good.

Here humanities and social sciences took the lead in revealing an obvious consensus in publication ethics. Yet the Chair, an editor from medical sciences, restated the norm using an example from her field. Indeed, participants to the discussion each introduced their comments as coming from a certain discipline, yet each also tried to use very strong language to make clear the generalizability of their argument. Most interestingly, during the discussion what was firmly asserted was the universalism of publication ethics principles, not the fact that this question might have been beyond the remit of or irrelevant to the Forum on publication ethics. The discussion thus also revealed the creeping remit of publication ethics issues.

Separating papers from relations

Aside from standardizing and harmonizing the expert field of publication ethics, the Forum debates focused on how to characterize, speak about, and repair the specific objects at stake. This brings about another component in the Forum's evaluative events. The concern with the restoration of 'the record' for the benefit of the users of scientific journals – a 'public', as we see below – gets modulated by the editors' felt need to get personal disputes between individual authors or between authors and editors resolved. A Forum participant used the phrase 'punishing the authors or correcting the literature' (15-14) to capture this recurring separation drawn between papers that form the research record and the relations surrounding them.

In case 10-35, one editor told the Forum that late in the production stage of an article one of the co-authors had refused to sign the journal's copyright form because she disagreed with the order of names in the author line and claimed she should be the first author. A group of Forum members maintained that disputes between authors affected the integrity of the concept of authorship and revealed moral failures of a sort. According to this group, editors could not and should not adjudicate personal disputes between authors, but if the co-authors could not agree, the process of research and its dissemination would simply get halted. Here, new scientific research appeared to be conditional on the ethics of its authors.

Yet another group of members focused on the research record itself, and on the importance of getting knowledge 'out there', or of fixing knowledge already out there when it is wrong and misleading for readers. To the supporters of this approach, maintaining the integrity of the record of knowledge ought to be given priority irrespective of personal disputes or authors' behaviour. In other words, from this perspective the research publication has a career and agency of its own. Even if an author behaves inappropriately, their research would still have to be published – if it's important and rigorous research, to ensure the integrity of the record. The scientific record 'needs to be corrected' (11-01) if wronged. This view, according to which publication (or retraction of a publication) should take its course for the integrity of the record and the benefits of users, both sanctifies and further reifies the 'research record'. It also has strong utilitarian underpinnings, although the members would not use this term explicitly, or discuss how their position might be grounded in any ethical theory for that matter. The tone was usually frank and good-humoured, as in 10-35:

> Lewis: Journal editors should not adjudicate amongst authors … if they don't agree, then no paper gets published, the paper disappears …
>
> Eric: Some research is important and should go ahead and be published even when authors are not reasonable …
>
> Lewis: Well, some research is not that important!
>
> (Laughter around the table)
>
> Eric: Then we try not to publish it!
>
> (Laughter around the table)
>
> Ray: Seriously, once a co-author disappeared while doing research in Asia, even her parents don't know what happened to her … (Laughter), and we needed to go on with the publication ….

To the last participant in this discussion, a situation when a person who is an author has gone missing without having previously signed a copyright form (a co-author in absentia) served as a helpful analogy to a situation when a co-author objects to a certain authorship arrangement and refuses to sign a copyright form. If we

continue this intriguing line of thought, it becomes unclear whether and under what terms a decision to carry on with a publication when a co-author has gone missing would become a matter of 'publication ethics' – a question related to the amplification of the ambit of research integrity and publication ethics.

In case 12-08, an editor asked what to do about an already published paper that contained information that had allegedly been given in confidence, in a seminar, by a third party (another researcher) to the author of the paper. The question for the editor was whether the information had been given in confidence as a matter of fact, and whether the author had breached confidentiality. When presenting the case, the editor also mentioned that, crucially, the paper in question was a 'theory paper' and therefore that the contentious data contained in it was not necessary for the paper to stand and be published. The Chair first confirmed with the editor that the paper was otherwise solid, and then asked members whether this matter called for a correction or a retraction, or whether they had any comments. Much of the discussion reiterated that the data allegedly obtained in breach of confidentiality was not needed anyway for the paper to be sound. One new member of the Forum told a personal anecdote about one of his students being used as a star case in a psychiatry paper, and of the unfortunate consequence this had on the student's research career. He also mentioned that the misuse of confidential information in clinical trials was problematic, and not unusual. Other members retorted that the case 'was not about ethics of [patient] confidentiality', implying that the new member had misunderstood what was at stake. 'Could the paper have been written without access to this information is what counts', an experienced member declared. The deliberations on that case showed how important it was for members to convey to the editor that whether the meeting (in which the information was allegedly given to the author in confidence) was confidential or not was 'not the editor's problem'. His 'problem' was rather to keep knowledge intact. The admonition 'it is important not to retract a solid paper', highlights the editor's strongly felt duty towards maintaining the integrity of the research record. Under this view of the research record as an incremental, coherent, diverse, aggregate body of individual outputs, removing a valid paper would artificially create a hole and thus erode both the research record and the process of shaping it. In turn, removing a paper that is not valid would not make a hole; instead it

would restore the integrity of that aggregate. The advice in this case was to not retract but to offer the researcher to write a letter to the editor, and give a chance to the accused author to reply.[9]

In a similar vein, in a case (10-32) where a conflict erupted in a department following allegations of misconduct and a call for retraction of a paper, some members questioned the motives of the person calling for a retraction (here, the rector at the author's institution), raising concerns about 'conflict of interests' in the dispute, or asking whether the call for retraction is like a 'vendetta'. Others dismissed such conflicts of interests amongst actors as 'irrelevant' and tried very hard to re-focus the debate around the heart of the matter: 'what counts is the paper'. Here it was thought 'important to retract, otherwise you mislead the readers'. The debate shifted to a question of fact, when one member asked about what kinds of paper would call for a retraction – implying that minor mistakes on a paper would not unduly affect the 'research record':

> - There are many bad papers published, it worries me that,
> taking aside the Rector, the paper went through but later you
> find out it's crap, as an editor, I don't want, or should I, could I,
> retract every bad paper I published?

> - Here it's not a bad paper, it looks like falsification … we don't
> publish a retraction for a paper with one wrong equation.

Under the language of correcting the literature and keeping knowledge intact, the members are in fact working out, along with authors, what the research record should be and, inevitably, what it is. In pursuing this logic they risk implying that the published record represents a full picture of all the scientific work that is done -- even though it is now common knowledge that scientific articles represent only very carefully selected and curated parts of scientific research work (e.g. negative results rarely making it to formal publications). Despite an explicit intent to only recover and restore an object that exists a priori, Committee members construct a particular version of their object of reference At once they repair and make a highly specific kind of research record.

Sennett suggests that restoration is 'self-effacing work', however 'the restorer is hardly mindless; rather he or she is crafting an illusion, and that craft is demanding' (2012: 213; see also Graham and Thrift, 2007). Restoration is thus the source of a double illusion: the illusion of the 'original' produced by the restored object, but also that elicited by the self-effacement of the restorer. As Sennett continues: 'the act of reconstruction requires a certain modesty in the craftsman: intruding his or her presence is not the point of the work; the restorer thinks of him- or herself as the past's instrument' (2012: 213).

At the Forum, the research endeavour under scrutiny – a chain of acts involving persons with various roles – often gets 'reduced to the contour of the research record' and thus takes the form of a thing, an object. This involves the eclipse not only of human agency, but also of the translation work, as members try to backtrack, to retract the chronology and unwind the past career of the paper in question. The 'research record' becomes this potentially damaged object whose purity can be recovered. It is an artefact, literally.

Through correction or retraction, for instance, editors intervene with what is and what is not in the research record. Of course, this remedial intervention comes at the risk of creating elements of falsity, by supplanting the vulnerable, stained and untrustworthy research record with something more correct and corrected. This means the editors' intervention has to be made visible only as remedial or corrective, since otherwise it risks becoming itself a misrepresentation of sort. The work foregrounds the repairer, and yet as for restoration, the repair still has to be somewhat self-effacing, to be valid and to not risk being interpreted as yet another attempt to falsify.

One way to confine the risk of further falsifying is through distancing. Forum members distance themselves from any assessment of truth or falsity. Instead, they assess what constitutes correct conduct and what can be publishable. The making of that object, the 'research record', is key to this distancing. It constitutes a buffer of sort that creates a distance. Distance, instead of being an obstacle, empowers certain individuals and groups to make truth claims (Biagioli, 1993). When debating and assessing an enigmatic case, the Forum members have to make sure they keep a distance from it while having authority on the 'case'. The acts of distancing unfold through the anonymizing of cases – again, a separation of certain text and removal of

certain relations from view – by the Committee's office staff, and by the publication of objective, neutral advice about facts, never accusations. Of course, the objectifying of the research record allows for the development of authority while avoiding liability, which has been concerning the Committee since its early days. The need to restrict their remit through distancing (and the use of particular language around advising) has a policy rationale, though this rationale did not evolve smoothly for the Committee founders. It has been described as the result of a prompt, anxiety-laden consultation with an eminent academic medical lawyer in response to early encounters (and fears thereof) with authors and publishers, and to worries of being perceived as a 'kangaroo court' (Horton et al., 2012). In other words, the distancing of the Committee is packed with affect (Candea et al., 2015). Part of it takes place through a repeated emphasis on the independent existence of the entities of the 'paper' – the piece of research itself – as well as on the 'research record' that encompasses and frames the sum of these 'papers'. The papers are conceived as entities with their own trajectory, and needing protection. It is also seen as important to maintain the integrity of the research record, to restore it when it is broken, to detach it and cleanse it from what can be unsound, especially problematic relationships. And yet Sennett (2012: 218) reminds us that curators have always emphasized that close connection between making and repairing. Throughout the process of repairing papers, the Committee members also make and reify papers, not unlike how the current publication-based regime of evaluations reifies authors (Biagioli and Lippman, forthcoming). But again, more than only reifying the research record, they construct a particular kind of research record.

Taking papers apart

The latter cases also complicate the issue of the 'integrity' of papers, by cracking open their taken-for-granted internal coherence. The paper in (and with) its integrity could ordinarily be conceived as a static and whole entity. However, a key tension underlying the Committee's publication ethics endeavours is the contrast between editors' decision-making and hesitation, and the apparent fixity and programmatic integrity of the paper (Latour, 2002). Further, as an event the publication does not necessarily solidify the paper as a whole. What we see appear are papers that are

divisible into parts (e.g. an equation, an author line). If Strathern's whole-parts relationships come to mind (1992), Goody's list even more so. To Goody (1977), the list has a clear-cut beginning, and a precise end: boundaries. It is made as whole, but its parts always remain detachable. The capacity to have its parts isolated and detached from its whole, whilst remaining true to itself, is key to this predominant bureaucratic but also scientific artefact (Belknap, 2004). In the list, as in 'the paper', 'separate units cohere to form some function as a combined whole' while 'the individuality of each unit is maintained as a particular instance, a particular attribute, a particular person or object' (Belknap, 2004: 15). We can analogize the 'particulars' of the list with those of the paper as conceived by COPE members. The methods, dataset, discussion, one or more of its authors, and more recently the reviews (Groves and Loder, 2014): All are conceptually and practically isolatable from the paper as whole. With the support of digital publishing, datasets and methodological tools or apps can be effectively detached from a paper and travel on their own (Strasser and Kratz, 2014). We can thus return to Sennett's analogies with remediation and reconfiguration, which explicitly take account of the divisibility of objects to be corrected.

Further, the Forum is an occasion for suggesting emergent add-on objects that supplement the research record by attaching themselves to articles. Impelled by transparency and open-access agendas, these objects can relate to different temporalities in the career of an article: peer-review reports take part in the 'prepublication histories' of articles (Groves and Loder, 2014), whereas a correction, a note by the editor, a post-publication review or response on the blog of a journal relate to the moment of or after publication. In a discussion over a case of duplicate publication, for instance (15-14), members debated how exactly to complement a paper with an add-on object to signal misbehaviour to readers, when 'there is nothing wrong with the validity of the paper'.

> Carl: It's important to have a formal note, an expression of
> concern attached to the article, to the paper in Journal A, in
> order to have a permanent note of this.

> Editor: We want to be transparent, to have a correction … but
> what do we correct? We think a retraction is too harsh, and an

expression of concern is usually when there is a problem with the data, and here it's not the case

Chair: How about an editor's note? An addendum, not a correction to the article …

…

Chair (concluding): There is a need for something permanently associated with the paper.

By breaking the article into fragments and complementing it with a note, editors make explicit how the article is no longer an invariable entity that either stands on its own if unread or affects other things if widely disseminated. These days, editors have access to a number of tools to make text more 'fluid', such as notes and back matter (Bryant, 2002). Bryant proposes that editors use these tools to keep track of different versions of the research record and thus evidence the versions' historicity and reliability (2002: 130). The public of the research record is anticipated, examined and fed accordingly with articles, or parts of them or associated add-ons, under the curation work of editors. The 'expression of concern', the correction, the retraction, editorials with response by the author, and debates form part the text 'apparatus' (Bryant, 2002) that is used to 'correct the literature' in publication ethics. These add-ons are not interchangeable and rather stand on a scale of gradation. Corrections are seen as a fix to small mistakes, whereas retractions are conceived as an indication that the original article should not have been published and that it should no longer be used in research. These graded add-ons carry a strong normative component and moral load, to which I will return briefly below.

Montgomery and Oliver (2015), concerned with the public health effects of publication misconduct, agree that the self-contained unit imagery of the paper is mistaken, and instead suggest another metaphor: the paper as contagious agent. A study with bad data is 'a virus in the scientific body of knowledge that can spread and contaminate the knowledge system'. To Montgomery and Oliver (2015), such infective objects become uncontainable as soon as they are out in the public domain, as their modes of transmission can follow many pathways, including citations.

Informal discussions between colleagues and students, personal websites and blogs, seminars, funding proposals and so on are also vital, leading to the 'implantation' of papers within the body of knowledge. This metaphor attaches the paper to the research record in ways that cannot be easily disentangled. It leads Montgomery and Oliver (2015) to the conclusion that current mode of repairing the record – the publication of a retraction – is unhelpful. Once a version of the paper (and its apparatus) is out, it would allegedly acquire a movement of its own, as if it were unmediated and desocialized. Such viral metaphors connote an unavoidable and out-of-control movement, and create a moral alarm. Like the Committee, Montgomery and Oliver (2015) rely on heavily loaded metaphors about the nature of 'the paper' and acknowledge that the formal retraction cannot stop viral dissemination. The Committee discussed the limits of formal retraction as a kind of 'stamp' attached on something that is often already gone and transformed, because it has been printed, handed over, or talked about already in ways that cannot get captured by the 'stamp'.

It is not only the research record and papers that get made here. The public itself gets made again and again. The public can be made and addressed as highly subjective, interested and agile agent, who is asked not only to read but to navigate and interpret the research record as something in the making rather than a final product (see Warner, 2004; Jacob, 2007). With viral connotations, however, the 'public' is made differently. It is constructed as more passively reliant on and vulnerable to being misled by the research record.

Conclusion: Repairing and making ethics

I hope that the above has shown how the Committee's labour makes the work of standardizing, harmonizing, separating and repairing highly normative. Through the work of the Committee, publication ethics itself gets made rather than revealed by the cases. Most importantly, I have made explicit that within the Forum it is not only papers and the research record that get standardized, cleansed from relationships, and then repaired, but the institutions and practices of a research community. Consequently, publication ethics has epistemic effects. For the community of scientists it mobilizes and influences, it reasserts the increasingly contested precept that the publication in the form of a paper is at the core of the authenticating of science. The sum of these individuated papers build a research record available for

consumption; in turn, this research record is conceived of as an object taking form as publications get into print, as well as an object that can get repaired if found to be defective. I hope this article has highlighted how a particular research record is instead constantly in-the-making through the repair work of the Committee.

To end, I underline a noteworthy proposal debated by the Committee leaders: the use of exemplar cases, through which I argue both the harmonizing character of publication ethics and intimate relation between repair and making get reaffirmed. The proposal of published cases ultimately did not materialize, but the Committee's evocative discussions around it remain relevant here. As members got increasingly concerned that they or their journal will be recognized during the Forum, they have become less eager to share details of their tricky cases. As a response, the governing members of the Committee have decided to edit the recording of its Forum discussions before making them available to members. Because it was increasingly difficult to edit Forum recordings and make them genuinely unrecognizable and unattached to anyone in particular before they are made available to all members, the committee has considered the production of podcasts that would feature either heavily edited real cases or invented generic cases that would collate issues arising from a couple of real ones. Committee-made scenarios could serve as exemplars (cf. Hojer and Bandak, 2015) in lieu of queries brought by members. The Committee's Forum had generated a distinctive set of problem types (see Hames, 2014) and according to the proposal, their homogeneity and recurrence meant that an example of a case could now be abstracted and fictionalized, as long as it gets shaped according to the proper types within the taxonomy. Down the line the case could be published, attributed a DOI, and be the intellectual property of the Committee. But the exemplar constructs the homogeneity it aims to only make intelligible: 'The example as exemplar, then, has the potential to conjure up (momentary) wholes and extend itself in series of concrete examples, and, in so doing, it reconfigures and moulds the world in its own image' (Agamben, 2009, cited in Hojer and Bandak, 2015: 7). By creating its own exemplary cases, the Committee would remain in better control of its own remit of making publication ethics.

The made-up quality of fictive cases does not evoke fabrication in the sense of fraud, but rather another 'fictive' property of the cases. First, the making of cases that collate and fold many real facts into a generic example denotes the powerful dexterity of

repair work, and highlights the link between repair and making. As in Navaro-Yashin's 'make-believe' mode (2012), the crafting of exemplar cases emanates from the very tangible stuff of the field of publication ethics. Like products of imagination that always have a concrete materiality, exemplar cases of publication ethics could work because they would be shaped on concrete cases.

The main expertise of the Committee members, publication ethics, takes a surprising turn here, and we might say that I have come to a full circle. According to the proposal, the members 'cases,' the Committee's staple achievement from its beginnings, would take on a new life, as they would become literally 'made' by the Committee's governing members. The made-up quality of pedagogical 'cases' is intriguing not because it denotes fabrication in the way that the Committee itself denounces it, but because its shows, perhaps more explicitly than ever before, that the members make the research record instead of being merely its guardians or custodians.

Yet, as we have seen, the Committee's ethics of repair encompasses much more than the correction of the research record. As I hope I have shown, the very process of publication and the conduct of editors are under scrutiny at the Forum. Editors, not only authors, in turn create an ethics of repair when they are examining their own conduct or asking other editors to do so, take a specific course of action towards authors etc. Publication ethics is not seen as necessarily broken or unfit for purposes when it is so debated in the Forum: it is seen as intact but weakened by scientists. Its normative force has to be reasserted as a gold standard, and helped to reverberate more effectively. And here, to describe what is done with publication ethics itself, we might need metaphor of repair that is a little more normatively loaded than restoration, such as redemption. To Lezaun (2012), this refers to the work of "watching for deviations or degradations, and, every so often … correcting the original assessment or produce a new, immaculate version of the standard" (35). My participants might not recognise themselves in the work of redemption of the research record: they do not create something anew. But they certainly redeem publication ethics as a persuasive uniform guide of conduct for its public.

I showed how ethical misbehaviour is ideally to be managed in the backstage of publications, and papers 'stamped' or retracted should circumstances absolutely necessitate (i.e. when misconduct affecting the results of a paper). This push to

manage ethical concerns behind the scene shows the labour required in order to sustain a particular kind of research record. This publication ethics labour offers the scientific public a particular account of what counts as 'good science,' and limits the criteria they might use in order to witness for themselves and come to their own conclusions. Less active a witness, the scientific community seems to turn into a more passive vessel for the receipt of scientific facts, posited at the receiving end of the pedagogical reach and publication ethics of the Committee.

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[1] 'Paper' and 'publication' are no longer the same thing, given that nowadays codes, datasets, films and images populate, along with papers, the space of scientific publications (Goble, 2014; Strasser and Kratz, 2014). While it remains at the heart of contemporary scholarly communication, in certain sub-fields the peer-reviewed journal 'paper' is gradually being complemented and even displaced by posted papers on digital preprint archives (Delfanti, 2016).

[3] See Ben Yehuda, 1985; Kevles, 1998; LaFollette, 1992; Montgomery and Oliver, 2009; Sapp, 1990; Sismondo, 2009.

[4] They include: the European Network of Research Integrity Offices (ENRIO), Council of Science Editors, the World Association of Medical Editors; International Committee of Medical Journal Editors; the Committee on Publication Ethics; and the European Association of Science Editors, and more recently, the Publication Integrity and Ethics (P.I.E.) company.

[5] This echoes Marie-Bénédicte Dembour's suggestion that the idea of human rights 'exists because it is talked about (Dembour, 1996:22).

[6] Elsewhere, I have drawn some parallels between the styles of collective evaluations performed in the Forum and Shapin and Schaffer's readings of the early meetings of the Royal Society in London (Jacob, 2014), but there seems to be very few contemporary illustrations of collective evaluation of research and publication conduct.

[7] This public is rarely described. It seems an undifferentiated, often non-contextualized aggregate of lay and expert 'users' who search and read intelligently but who ultimately trust printed and online published material and consume it on that basis. This public of users is conceived as being in need of protection.

[8] I discuss this theme and the institution-building effort of COPE elsewhere.

[9] In this case, the lack of causality between the contentious data and the paper justified a decision not to retract a sound paper. Is the duty to publish symmetrical to a duty not to retract? The 12-08 case and ensuing deliberations did not elaborate on the broader implications of the 'lack of causality' argument: if the case had been about an editor wondering about publishing a theory paper with contentious data in it, would members have emphasized the editor's duty to publish the paper? Forum members tended not to delve into these hypothetical questions.