**The Cultivation of Social Work Knowledge:
Towards a More Robust System of Peer Review**

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Abstract

In a recent issue of *Families in Society*, Caputo (2019) argues for the "centrality" of peer review in the cultivation of social work knowledge. Specifically, he favors the double-blind model of peer review. In this paper, I argue that social work should adopt a more dynamic set of reviewing practices. First, I define some terminology and discuss the limitations of the current double-blind model. Then, I describe recent trends in peer review, which I argue foster a more robust and open system. I frame this discussion within the context of the wider *open science* movement and urge social workers to engage with these scholarly practices. In line with these practices and values, a freely accessible preprint is available at: <https://osf.io/preprints/socarxiv/rvqze>

Peer review is one means by which the discipline of social work, and the social and biomedical sciences more broadly, governs the production and dissemination of knowledge. Editors and reviewers evaluate manuscripts submitted to their journal, commonly focusing on the quality of the methods used, strength of arguments presented, and overall utility of the findings/content, which results in a determination about acceptability for publication. In short, peer review can (at least in part) be thought of as a form of "quality control" for scholarly work (Funtowicz, 2001; see also Gambrill, 2018). When performing *optimally*, such governance promotes the type of scholarly work that advances our understanding of social problems and aids in decision-making—for example, by helping us choose which interventions to employ or social policies to adopt, in any given problem-situation. In other words, when peer review is functioning well, it is endorsing the type of scholarship which improves our ability to explain and predict, that explicates "fuzzy" or imprecise concepts, and contributes to the overall "evidence-base" or corpus of social work knowledge (e.g. Carnap, 1974; Gambrill, 1999). When performing *suboptimally*, our knowledge is varyingly stunted; with published papers resembling a form of academic "posturing" (see Tripodi, 1974, pp. 143-144), rather than genuine advances in knowledge. In this scenario, peer review is *more likely* to promote flawed scholarship. This often includes publishing studies that are not methodologically sound (e.g. Button et al., 2013; Dunleavy, 2020a), contain irreproducible results (e.g. Baker, 2016; Open Science Collaboration, 2015; van der Zee, Anaya, & Brown, 2017), or have suspect premises (e.g. Bem, 2011; Thyer & Pignotti, 2015), among other issues (Chambers, 2017; Heathers, 2020; Ioannidis, 2005). Consequently, a suboptimal system of peer review may result in the epistemic stagnation (or even regression) of the field—by interspersing what might be best described as forms of "avoidable ignorance" (Gambrill, 2014, 2018) among genuine advances in knowledge.

Caputo (2019), in a recent issue of *Families in Society*, passionately restates a familiar stance, arguing for the "centrality" of peer review in the cultivation of social work knowledge. Specifically, he favors the double-blind model of peer review (p. 6). This paper comments on Caputo's arguments (2019) and directly addresses the robustness of the current peer review system. I argue that social work and adjacent disciplines should adopt a more dynamic set of peer-reviewing practices; and further that doing so will push the field closer to the idealized form of epistemic governance described above. This argument parallels and complements other calls for reform within social work scholarship and publication (e.g. Holden et al., 2008; Jenson, 2005; Lindsey, 1999; Marsh, 2005; Pardeck & Meinert, 1999; Valentine, 2005). The stakes for reforming our practices is high, inasmuch as social work scholarship can be said to help understand and ameliorate social problems. Given public skepticism toward science (e.g. Lindee, 2019; Vazire, 2017), the increasing flood of (mis)information (e.g. Dunleavy & Hendricks, 2020; Hendricks & Vestergaard, 2018; O'Connor & Weatherall, 2019), and the role of social and biomedical research in resolving various global crises (Dunleavy, 2020b; Dunleavy, Tennant, & Francuzik, 2020), ensuring a robust and trustworthy system of peer review (and knowledge governance more generally) is seemingly more important than ever.

First, I define some terminology and discuss the limitations of the current double-blind model. Then, I describe recent trends in peer review, which I argue foster a more rigorous and open system; one that provides feedback *across* the publication life-cycle. This includes embracing open publication of peer review reports, engagement in post-publication reviewing, and the elicitation of pre-publication feedback (e.g. preprint review; use of Registered Reports). I briefly consider and respond to possible objections to these practices. Lastly, I frame this

discussion within the context of the wider *open science* movement and urge social workers to engage with these scholarly practices.

Key Terms in Peer Review

I begin by defining some key terms in peer review. This will help clarify core issues and assumptions, while setting the stage for later discussion. With that said, it should be stressed at the outset that this list is neither comprehensive, nor definitive; as some terms have taken on multiple meanings, depending on the discipline in which it is discussed, and/or have evolving meanings. I have attempted to keep all terms in line with standard usage and have tried to make any ambiguities as clear as possible.

Anonymized Reviewing

There are three common forms of anonymized peer review: *single-*, *double-*, and *triple-blinded* reviews. Typically, in the single-blind condition, the author's identity is known to the reviewer, but the reviewer's identity is not provided to the author. It is possible that these blinding roles may be switched, where the author knows the identity of the reviewer, but not vice-versa; though this is less common and only typically happens when the reviewer is required (or chooses) to identify themselves (i.e. in the case of signed reviews, discussed below). Under double-blind conditions, neither the author nor the reviewer knows the other's identity. Triple-blind review is arguably the most rigorous form of the three, but also the least common (Caputo, 2019, p. 9). In this condition neither the author nor reviewer knows the other's identity, nor does the editor know the author's identity (at least until after a decision about publication is made). These forms of peer review can be contrasted with *unblinded review* where "authors and reviewers are aware of each other's identities" (Ross-Hellauer, 2017, p. 9).

Reviewing Across the Publication Life-Cycle

Still further, we can distinguish peer review based on where it occurs across the life-cycle of a publication. *Pre-publication review* can occur any time prior to the publication of a manuscript. Typically, this refers to review that occurs during submission to a scholarly journal. However, pre-publication peer review may occur outside the bounds of a formal journal submission, such as when reviews and feedback are solicited from online drafts or use of *preprint* servers (defined and discussed below). *Post-publication review* refers to formal and informal feedback, generated through comments, letters, reviewer reports, and other forms of discussion (e.g. Berg, 2020; Teixeira da Silva & Dobránszki, 2015); which may or may not occur at the place in which the manuscript was published (e.g. [PubPeer](#); see also Committee on Publication Ethics [COPE], n.d.).

Open and Closed Peer Review

Lastly, peer review can be distinguished in terms of its accessibility. *Closed review* refers to reviewer reports and editorial decisions/correspondence that are kept confidential. In other words, only the author(s) who submitted the manuscript, peer reviewers, and editors have access to referee reports and decision letter. *Open review* (sometimes conflated with unblinded review) refers to reviewer reports and editorial decisions and correspondence that is made publicly available.

Limitations of Traditional Peer Review

The limitations of traditional peer review (i.e. oft closed, blinded, pre-publication review) have been known for decades (see generally Crane, 1967; Cole, Cole, & Simon, 1981; Horrobin, 1974, 1990; Rennie, 1986; Smith, 1997, 2006; Wager & Jefferson, 2001). In his commentary

Caputo (2019) identifies and describes a few of these well-known problems, including: *publication* bias (i.e. publication based on "favorable results"), *prestige* bias (i.e. publication based on institutional status), *gender* bias (i.e. favoring male over female authors), and *ingroup* bias; all of which can have a negative impact on the advancement of science. Given space constraints, let's set these problems aside for now; though we will not minimize their importance. The interested reader may consult recent discussions in the literature for further information (Heesen & Bright, 2020; Helmer, Schottdorf, Neef, & Battaglia, 2017; Lee, Sugimoto, Zhang, & Cronin, 2013; Smith, 2006;). What I would like to do here, instead, is briefly describe three other limitations that are perhaps equally problematic (though not necessarily mutually exclusive); that peer review is: 1) *unreliable*, 2) that it does not prevent *bad science* from being published, and perhaps more speculatively, the claim that it 3) promotes *groupthink* and *conservatism*.

Poor Reliability

As in other facets of inquiry (e.g. risk assessment in child welfare, diagnosis in medicine), reliability is a necessary condition for generating valid knowledge in peer review. A highly reliable system *may* help identify genuine contributions to the literature, while an unreliable system frequently will not. High variability in reviewer reports make it difficult to arrive at a correct decision about whether to accept or reject a submission, leaving editorial decisions plagued by false positive and false negative errors. The greater the variability, (likely) the greater rate of error. This may be grounds for questioning the value of traditional peer review altogether (see Heesen & Bright, 2020 for an exploration of this question).

The overall reliability of peer review is unclear, whether within social work or more broadly. In part, this is due to the closed nature of peer review (Hunter, 2012; Tennant, 2018;

Tennant & Ross-Hellauer, 2020). Readers cannot readily see whether reviewer reports agree or disagree and to what extent; making systematic evaluation of peer review near impossible.

Additionally, when there is evidence of unreliability, it is not always clear *why* this is the case.

Are any discrepancies driven by differences in (actual) appraisal of a manuscript's content or are they caused by underlying biases (e.g. as outlined above)?

There are however a few proxies for the reliability of peer review. Peters & Ceci (1982) resubmitted 12 published articles, one from each of 12 top psychology journals. The authors' names and institutions were replaced with fictitious names. Only 3 of 38 editors and reviewers identified the articles as resubmissions. More notably, 16 of 18 referees recommended *against* publication (p. 187, see Table 1 for a breakdown). Though the study was small and its results not without alternative explanations (e.g. see related commentary pp. 196-255 and Cicchetti, 1991 for discussion), it should give us pause when considering how many published social work manuscripts would reliably be *republished* if submitted again (even if only hypothetically).

Bornmann, Mutz, & Daniel (2010) provide a more up-to-date and rigorous assessment of the problem. They published the first meta-analysis on the inter-rater reliability (IRR) of peer reviews. Both intra-class correlation (ICC) and Cohen's Kappa coefficients were low (.34, 95% CI [.29,.39] and .17, 95% CI [.13,.21] respectively). The former suggests that only 34% of reviewer variability could be explained by agreement (with 66% being explained by disagreement). The latter finding suggests that "...reviewers agreed in their evaluations for 17% more of the manuscripts than would have been predicted on the basis of chance alone." (p. 4). These findings, along with the low degree of transparency around peer review, should make us wary of the assumption that peer review is a reliable process.

Of course, reliability, in and of itself is not sufficient for ensuring a robust corpus of knowledge. After all, peer reviewers who agree in rejecting a valid contribution to a field are not advancing the knowledge base at all (and vice versa). Peer review should, ideally, be a reliable means for endorsing genuine contributions to a field, and rejecting false ones. This brings us to our second limitation: Peer review does not keep "bad science" out of the published literature (Smaldino & McElreath, 2016; Tennant et al., 2019; Vazire, 2020).

Bad Science

In recent years, the veracity of many scientific findings has come under intense scrutiny in what has been called the "replication crisis" (sometimes called the "reproducibility crisis" or "crisis of confidence"). Scores of published, peer-reviewed articles (and even entire fields of inquiry) have been criticized for being based on flawed designs and methodologies (e.g. Button et al., 2013; Chambers, 2017; Dunleavy, 2020a), for being unreplicable (e.g. Open Science Collaboration, 2015; Yaffe, 2019), or being altogether false (e.g. Herrera-Perez et al., 2019; Ioannidis, 2005). The point here is not to defend this *strong* claim (though see Dunleavy 2020a for a defense regarding social work research), but rather to argue a *weaker*, perhaps more tenable position, that "bad science", science that relies on poor methodology or weak premises *does* get published in peer-reviewed journals; even "top" ones. If true, the stance that traditional peer review serves a "vital gatekeeping function" (Caputo, 2019) or as a form of "quality control" (Functowitz, 2001) is diminished (Smith, 2006). A few examples will serve to illustrate this point (though the reader is redirected back to citations above for further argument and defense, as well as to [Retraction Watch](#)).

Consider the case of Study 329, an industry-sponsored clinical trial, testing the efficacy and safety of paroxetine (Paxil) (and imipramine) vs. placebo in adolescents with depression. Funded by SmithKline Beecham (what is now GlaxoSmithKline), the study reported that paroxetine was a “generally well tolerated and effective for major depression in adolescents” (Keller et al., 2001, p. 762). Other concerns aside (e.g. ghostwriting, conflicts of interest; Lacasse & Leo, 2010), the study is marked by serious charges of selective reporting. Jureidini, McHenry, and Mansfield (2008) argue that the study authors minimized two of the prespecified primary outcomes, which did not favor paroxetine, and instead promoted secondary outcomes which did (p. 76). Still further, a re-analysis of the trial data by Le Noury et al. (2015), contra Keller et al. (2001), found neither drug were effective treatments for depression, and both had significant increases in harms (i.e. suicidal ideation). To date, Study 329 has not been retracted, has impacted treatment standards, and been cited nearly 800 times (according to [Google Scholar](#)).

A more recent example is provided by Heathers (2020). In the first half of 2020, *The Lancet*, one of the world's most revered medical journals, published a study on hydroxychloroquine (HCQ), a potential therapeutic for COVID-19 (Mehra, Desai, Ruschitzka, & Patel, 2020). The authors performed a multinational registry analysis of both hydroxychloroquine and chloroquine (with or without macrolide) used in patients hospitalized between December 2019 and April 2020, with a confirmed laboratory result for SARS-CoV-2. The study suggested that HCQ (and the other three treatment regimens) was associated with higher risk of irregular heartbeat and death. The paper was retracted within weeks of its publication, by three of its four authors, who were unable to verify the accuracy of the data findings. Despite being peer reviewed and accepted for publication, the submission raised a number of concerns around methodology/analysis, and data integrity (see comments [here](#)).

Both of these cases show us that poorly done research can make it through peer review, even in top medical journals, even when ghostwritten, fraudulently conducted, or poorly designed (see also Marcus & Oransky, 2018). This suggests that traditional peer review is neither an optimal system of knowledge governance, nor facilitates a system of scientific practice that is self-correcting (see generally Ioannidis, 2012).

Groupthink and Conservatism

Lastly, and perhaps more speculatively, we might consider that the traditional peer review process suppresses or marginalizes unpopular, unconventional, controversial, or competing viewpoints. Put differently, one could say that, "[D]ecisions of 'peer review' pre-empt the outcome of open scientific debate and determine the exposure of new ideas..." (Atkinson, 1994, p. 147. Now, that is not to say that reputable journals should publish any and all ideas, no matter how improbable, or deviant (e.g. Bem, 2014; Thyer & Pignotti, 2015). Rather, framed more charitably, it is to suggest that theories, ideas, and studies that are "out of step" with a field's current theoretical and methodological paradigms are more likely to be reviewed negatively, rejected, and/or all together "filtered" out of scientific and professional discourse (e.g. Charlton, 2010 discusses the costs, benefits, and factors surrounding the publication of scholarship critical of mainstream theories of HIV/AIDS; see also Goldacre, 2009). Note, this of course neglects consideration of any interplay of commercial and legal factors that may impact decision-making (e.g. Healy, 2008; Kahr, McHenry, & Hollingsworth, 2019; McHenry, 2018).

When groupthink and conservatism are prevalent a field becomes less epistemically diverse and innovation is potentially stunted (e.g. Horrobin, 1974, 1990). Fundamentally, this is bad for science and the search for truth more generally, as critical discourse, potential falsifiers,

and/or alternative explanations become scarce (Gomory, 1998, especially pp. 210-212).

Unfortunately, quantifying the prevalence of groupthink or conservatism (and its effects) within a closed and opaque peer review system (Hunter, 2012) is difficult and thus remains speculative.

Recent Trends in Peer Review

By all accounts, traditional peer review is an imperfect system. Nearly two decades ago Richard Horton (2000), Editor-In-Chief of *The Lancet* wrote:

"We portray peer review to the public as a quasi-sacred process that helps to make science our most objective truth teller. But we know that the system of peer review is biased, unjust, unaccountable, incomplete, easily fixed, often insulting, usually ignorant, occasionally foolish, and frequently wrong." (p. 148)

Despite my own concerns about peer review, I do not share Horton's seeming pessimism. Peer review has served an important purpose historically and can (contra Eisenhart, 2002) be made more rigorous. I argue that this can be done by shifting from a closed, uniform system (i.e. one that almost exclusively employs double-blind, pre-publication peer review), towards one that integrates pre- *and* post-publication review, makes the review process more transparent, and takes advantage of the larger pool of intellectual capital. These changes, as will be shown below occur across the scholarly ecosystem; involving authors, reviewers, and journals.

Preprints

To achieve the first aim, a variety of strategies may be employed. Social workers may consider submitting their manuscripts to one of many preprint servers. Preprint servers are platforms where researchers can freely share papers, drafts, and other unpublished materials

(including pre-publication versions of manuscripts). Examples include, [PsyArXiv](#), [SociArXiv](#), and [medRxiv](#), which publish preprints from psychology, the social sciences, and medicine, respectively (other examples include [bioRxiv](#) and [PhilSci-Archive](#)). Each platform has its own set of policies and practices regarding submissions and screening.

Not only are preprint servers a common tool for freely distributing one's work (e.g. by making materials "open access"), they have become a key method for soliciting formal and informal pre-publication feedback/review from one's peers; especially during the current pandemic (Kupferschmidt, 2020; see also Conroy, 2020). For instance, a researcher may upload their manuscript (which receives Digital Object Identifier or DOI) and ask for feedback from colleagues and peers; whether known or unknown to them. Some journals are even beginning to integrate preprint review into their publication workflow. For example, authors who have submitted work to *bioRxiv*, may request to have their work submitted for review at *eLife* (Eisen, Akhmanova, Behrens, & Weigel, 2020); a leading journal in the biomedical and life sciences. If acceptable, the work may be invited for publication. Regardless of the decision (accept, reject, revise, etc.) *eLife* provides a reviewer report, which is able to be submitted alongside the preprint (at *bioRxiv*), and can be transferred along with the manuscript when submitting to another venue.

In whatever manner they are utilized, and whatever their current limitations (Dunleavy & Hendricks, 2020; Kupferschmidt, 2020), preprints allow a researcher or scholar to access and solicit feedback from the larger pool of intellectual capital available (within and outside of social work), rather than the two or three persons that would traditionally review a manuscript. Consequently, there are more opportunities to provide feedback and correct potential errors. Arvan, Bright, and Heesen (2019) argue that "crowd-sourced" approach, like these, may be a more reliable means for identifying high-quality work (p. 2).

Post-Publication Peer Review

Post-publication peer review (PPPR) refers to formal and informal feedback, generated through a variety of means (e.g. comments, letters, reviewer reports, etc.), that occur after publication (e.g. Berg, 2020; Teixeira da Silva & Dobránszki, 2015). PPPR, whatever its form, is an *essential* part of scholarly publishing, helps identify errors, and facilitates the revision and/or retraction of a published manuscript (COPE, n.d.; see also Marcus & Oransky, 2018). Recently, formal systems such as Pubpeer and [Hypothesis](#) have been developed for, among other things, eliciting explicit PPPR (Staines, 2017). These tools can be formally incorporated into journal platforms (without needing additional resources) (e.g. Angell, 2018) or journals may seek to foster their own systems of PPPR. Ultimately, post-publication peer review may make scholarship more reliable, help in the identification and correction of published errors, and support the self-correcting processes in science and scholarship (Bordignon, 2020; Knoepfler, 2015; Teixeira da Silva, 2015).

Opening Peer Reviews

Journals are increasingly making reviewer reports and editorial decisions/correspondence publicly available, alongside published manuscripts (e.g. The PLOS Biology Staff Editors, 2019). Social work journals may consider publishing pre- and/or post-publication peer review reports alongside articles (or as supplemental materials). This helps make decisions more transparent and accountable, and may help reviewers receive more meaningful credit for their work (Godlee, 2002); for example reviews could receive a permanent DOI, Moher et al., 2020). It may also increase the quality and depth of reviewer reports (Bornmann, Wolf, & Daniel, 2012; Polka, Kiley, Konfortia, Stern, & Vale, 2018). Open peer review ensures that journals are

engaging in adequate peer review practices, allows readers to see explicit feedback on those papers, and helps the scientific community see how the authors may have responded (or not) to critical feedback. Individual researchers may also help increase the transparency and openness of peer reviews by signing their names to reviewer reports (Aleksic et al., 2015). Of course, this may be done on a case-by-case basis; since adopting open reviews does not entail de-anonymizing reviewers (Polka et al., 2018). In all, making peer review more open can help create a more transparent and comprehensive view of scholarship.

Registered Reports

Registered Reports (RR) are a relatively new form of article (and pre-publication review) (Chambers, 2013; Chambers, Dienes, McIntosh, Roshtein, Willmes, 2015; Simons, Holcombe, & Spellman, 2014). Journals that participate in the RR format require submission of a study's methods and analytic strategy prior to data collection. After undergoing (blinded or unblinded) peer review, the manuscript may gain "in principle acceptance" for publication at the journal where it is reviewed. Studies that are accepted as Registered Reports (e.g. Hall et al., 2018), are done so on the merits of methodological rigor, rather than its findings. A RR would undergo a second round of peer review after data analysis and reporting of results, to verify that all facets of the proposed analytic strategy are conducted and included in the manuscript. Through the creation of RRs, there is incentive for researchers to follow their pre-specified protocol (i.e. to maintain in principle acceptance) and disincentive to engage in questionable research practices (QRP), such as selective reporting (Chambers, 2017; Chambers et al. 2015). Registered Reports are worth highlighting here, because they are easily implemented in the current peer review system (see <https://cos.io/rr/>) and directly address broader concerns around publication bias and suboptimal research practices that often underlie "bad science".

Concluding Thoughts: Towards a More Robust System of Peer Review in Social Work

The last two decades in social work have been dramatically shaped by the advent of "evidence-based practice" (Gambrill, 1999; Okpych & Yu, 2014). Despite this advance, it remains plausible that many evidential claims made within social work research (and social work scholarship more broadly) are inaccurate, incomplete, or overstated — if not outright false (Dunleavy, 2020a; Gambrill, 2016; Ioannidis, 2005; Yaffe, 2019). *Even if* this assessment is overstated, the potential risk involved makes it prudent that the field take *precautionary* measures (Howard & Garland, 2015, p. 194) to improve scholastic and scientific rigor.

Reforming and advancing social work peer review is one means toward creating a more robust form of knowledge governance, and thus a more reliable knowledge base. The call articulated in this piece, should not be viewed as a romantic appeal for pluralism in peer review. Rather, it should be viewed as an opportunity for the growth and calibration of our scholastic mechanisms (Hardwicke et al., 2020); one that can be guided by the principles of EBP (Bailar & Patterson, 1985; Ioannidis, Berkwis, Flanagan, Godlee, & Bloom, 2019; Tite & Schroter, 2006). This opportunity holds, even if any or all of the limitations of peer review outlined above are overstated. It is also an opportunity and invitation for open dialogue among members of the social work community (including Caputo) — *regardless* of rank or background. Together, as vigilant, truth-seekers (Campbell, 1998; Merton, 1973), we can better our scholarship and the world around us.

Still further, this discussion parallels and complements conversations about “open science” happening in the biomedical and social sciences (see Spellman, Gilbert, & Corker, 2018; Wolfram, Wang, Hembree, & Park, 2020). Researchers, advocating for open science, are

calling for data and materials, methods, manuscripts, and other scientific output to be made more transparent and freely available. The suggestions made here (e.g. for open reviews, publication of PPPR, use of RR, etc.) are in line with these calls and broader open science values.

Some of these recommendations may cause unease or discomfort. Indeed, one may question how such changes will alter the peer review process or profession generally. I share these concerns, but am not worried. For these suggestions are not mandates, but an opportunity for testing, for trial and error. We owe it to our clients (and each other) to hold our research to the highest standards. Lindsey (1999) captures this sentiment well, noting that "...until we are resolute in upholding research standards, we will be failing our clients and in danger of failing as a profession as well." (p. 119).

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