

## EDITORIAL MISCONDUCT

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This article interrogates the nature of editorial privilege and authorial integrity in peer-reviewed academic journals. Focusing on the authors' experience with publishing a letter critiquing high-profile authors in a high-profile journal, the article identifies key concerns with (i) the time it took to complete the peer-review process, (ii) the failure to provide the authors with the peer-review reports, and (iii) the decision to rewrite our text instead of allowing us to respond to the peer-review comments. Our experience suggests that despite the existence of editorial codes of conduct, encroachments on authorial integrity still occur, and the lines between helpful copyediting and unhelpful rewriting of an article are not always clear.

Much has been written about authorial misconduct with specific reference to plagiarism, fraud, and duplicate publication. Such misconduct on the part of some authors is arguably a threat to the authorial reputation and integrity of authors as a class. By comparison, there is scant literature on editorial misconduct. In part, this is because editorial misconduct is difficult to document and is easily reduced to an unresolvable "he said, she said" scenario. Moreover, in many ways, it is not always clear what constitutes editorial misconduct. Despite the existence of a range of editorial codes of conduct, the lines between helpful copyediting and unhelpful rewriting of an article are not always clear, nor are encroachments on authorial integrity. Indeed, there are significant questions about "where editorial judgement ends and misconduct begins."<sup>1</sup>

As an interdisciplinary and intergenerational group of scholars—one senior academic in philosophy and bioethics, and two junior scholars of political science—with more than thirty-five years of publishing experience between us, we are confident that, at the very least, we know the norms of academic publishing and know when problematic conduct is occurring. Like most scholars, we have stories from the trenches, of times when the peer review of our manuscripts has been sloppy, uninformed, or unnecessarily harsh, as well as stories of times when the peer-review process has been exceptionally careful and helpful. Like

others, we have stories both of wonderful and less than wonderful experiences with editors. But this story, of a recent experience publishing a commentary in *Nature Biotechnology*,<sup>2</sup> is different, and raises important concerns about the need for renewed attention to the purpose, scope, and quality of editorial oversight of the peer-review process and editorial decision making.

Our goal in sharing the details of this experience with the scholarly community is not to chastise the individuals involved in the editorial process or to criticize only one journal. Rather, we aim to shed light on a broader problem by openly asking questions about editorial privilege and authorial integrity using our case study. For myriad reasons, we did not issue a formal complaint to *Nature Biotechnology*, but we invite and welcome a public response.

#### EDITORIAL MISCONDUCT

As noted at the outset, scholarship critiquing the academic publication process, for the most part, has addressed impropriety on the part of authors, including plagiarism and authorial fraud<sup>3</sup> and the challenges of peer review, with relatively little attention paid to editorial practice(s).<sup>4</sup> One notable exception is the contribution of Richard Smith, former editor of the *BMJ*, who has chronicled both the problematic aspects of publishing in academic journals (identifying the ways in which editors have long been as “unaccountable as kings”) and the abuses that have occurred in recent years.<sup>5</sup>

In 2001, there were numerous efforts to codify the role of editors, particularly in medical and scientific journals. Whereas the International Committee of Medical Journal Editors has provided guidance on “editorial freedom and integrity” since 1988, it was only in 2001 that the Committee provided substantial guidance on how to manage conflicts of interest.<sup>6</sup> That same year, the World Association of Medical Editors issued a statement on the responsibilities of medical editors, and the Committee on Publication Ethics (COPE), which Smith helped to found, published its first Code of Conduct establishing minimum standards for editorial practices for academic journals.<sup>7</sup> These initiatives set voluntary guidelines for how editors should engage with readers, authors, reviewers, editorial board members, journal owners, and publishers. There were no real sanctions, however, for violating these parameters other than expulsion from the organization that established them.<sup>8</sup>

Despite the publication of these documents, cases of alleged editorial misconduct have since occurred, some of which have involved a “failure to observe due process, undue delay in reaching decisions and communicating these to authors,” and “inappropriate review procedures” that impede timely scholarly publication.<sup>9</sup> Consider, for example, the experience of Donald Light and Rebecca Warburton. They wrote a commentary challenging an oft-cited claim about the extraordinarily high cost of research and development for pharmaceuticals that has

been used repeatedly to justify their high market price. This oft-cited claim was originally published in a 1991 article by Joseph DiMasi, Ronald Hansen, Henry Grabowski, and Louis Lasagna in the *Journal of Health Economics*.<sup>10</sup> Light and Warburton submitted their commentary on this article to the same journal. The journal editors requested “major modifications”<sup>11</sup> and would not proceed with publication unless all errors were addressed. According to Light and Warburton, however, the issues in question were not erroneous at all. DiMasi, Hansen, and Grabowski, authors of the original 1991 article,<sup>12</sup> were given an advance copy of the Light and Warburton article and permitted a lengthy reply, to which Light and Warburton drafted a short rejoinder. The rejoinder, like the original commentary, was modified substantially by the editors. The editors later decided to publish neither the commentary by Light and Warburton nor their rejoinder, with no reasons given. The saga continued through prospective litigation, further rounds of editorial oversight, and eventual publication, including a published rejoinder to the rejoinder.<sup>13</sup> In this way, according to Light and Warburton, the original authors were given “length and latitude for their wide-ranging, sharply-worded, and in our view, misleading ‘last word’ response.”<sup>14</sup>

Consider next a recent problem with Letters to the Editor at the *New England Journal of Medicine (NEJM)*.<sup>15</sup> In this case, *NEJM* published a letter signed by Benjamin Wilfond and forty-five scholars in bioethics and pediatrics criticizing the Office for Human Research Protections (OHRP) for “overreaching” in imposing a sanction on the lead institution of an extensive NIH-funded trial testing the impact of air pressure on lung disease in premature infants as well as oxygen uptake in the same population (Surfactant, Positive Pressure, and Oxygenation Randomized Trial; SUPPORT).<sup>16</sup> The authors argued that there was no evidence to support the OHRP’s claim that there were risks to participants above “routine clinical treatment,” and that the OHRP’s claim that adequate risk disclosure had not occurred in the parental consent process was unfounded. In response, Ruth Macklin and forty-four bioethicists and other scholars (including Françoise Baylis) sought to publish a letter to the editor of *NEJM* defending the actions of the OHRP. This effort met with considerable resistance.

The *NEJM* gives clear directions for authors of Letters to the Editor, indicating that letters that respond to an *NEJM* article should “not exceed 175 words (excluding references),” and letters that do not respond to an *NEJM* article “must not exceed 400 words (excluding references).” Further, letters “can be signed by no more than three authors.”<sup>17</sup> The letter by Wilfond and colleagues was not held to these rules, as the letter was 528 words long and signed by forty-six authors. Macklin expected that a similar “relaxation” of the rules would apply to her letter with respect to both the length and the number of authors. But this was not so. Initially, the editor, Jeffrey M. Drazen (having co-authored an editorial in *NEJM* defending the SUPPORT trial),<sup>18</sup> indicated that the Wilfond letter was considered a publication, so the word limit for correspondence did not apply. Drazen further indicated that the

175-word limit would apply to the letter by Macklin and colleagues. In response to Drazen's assertion that the Wilfond letter was not in fact a letter, Macklin wrote that "it was published under the heading 'Correspondence,' [and] was addressed 'To the Editor.'"<sup>19</sup> She also noted that there was no material difference between the two letters in terms of word count and the number of signatories. Indeed, the only material difference was that the letters represented differing opinions. After additional discussion, a letter to *NEJM* by Macklin and colleagues was published.<sup>20</sup>

Taken together, these experiences and interventions illustrate problematic editorial practices.

#### OUR EXPERIENCE

In the Spring of 2014, we made plans to write a paper that would provide a contemporary estimate of the number of human embryos in storage in Canada, as the most recent estimate (2003) was significantly out of date. The original idea was to work from a 2013 publication by Geoff Lomax and Alan Trounson, in which they estimated the number of human embryos in storage in the United States at 1.39 million.<sup>21</sup> This figure was itself an update to a 2003 study by David Hoffman and colleagues that pegged the US number at 400,000.<sup>22</sup> Our plan was to use the same methodology as Lomax and Trounson to update the estimated number of human embryos in storage in Canada. As the 2003 Canadian estimate used the methodology developed by Hoffman and colleagues, and the Lomax and Trounson estimate built on the estimate provided by Hoffman and colleagues, this seemed like a reasonable strategy.

Taking a closer look at the Lomax and Trounson text, however, we found a number of methodological and technical errors. We were surprised by this: Lomax and Trounson are well-established professionals,<sup>23</sup> and *Nature Biotechnology* is a high-impact, well-regarded peer-reviewed journal.<sup>24</sup> We shelved our plans to use their methodology as a template to update the estimated number of human embryos in storage in Canada. Instead, we decided to write a brief commentary outlining the problems with their estimate, and calling for careful empirical research on the number of human embryos in storage and available for embryo research in the United States. This seemed to us an important and timely initiative, as the Lomax and Trounson publication was beginning to garner citations.<sup>25</sup> On October 23, 2014, we submitted a short manuscript to *Nature Biotechnology* for publication as a "correspondence."

On February 25, 2015, four months after the original submission date, we wrote to *Nature Biotechnology* to ask about the status of our submission (having had no communication from the journal other than a note confirming that the manuscript had been received). There was no response to this communication. On March 17, 2015, we sent a second query to members of the editorial board. Two days later, we received a response from an editor thanking us for our inquiry and

stating that we would be contacted upon completion of the peer-review process. On May 8, 2015, having had no further communication from the journal, we again wrote to ask about the status of our submission. A week later, we received another response from the editor stating that they had received two reviews and were awaiting a third. After another two months of silence, we again enquired about the status of our submission, and on July 22, 2015 (now nine months after the original submission date), we received notice that the journal would accept "a shortened revised version" of our correspondence.

On July 24, 2015, we received a revised version of our manuscript, with no reviewer reports attached and no reference to such reports. The manuscript had been reduced from 1,645 words (excluding tables and references) to 586 words. This cut was achieved, in large part, by removing supporting data, explanations, argument, and some references. In addition, some of our original criticisms were tempered (e.g., the term "errors" was replaced with "methodological difficulties"), other criticisms were deleted, and at least one error was introduced. The justification offered for the sweeping changes was the need to omit criticisms discussed by Lomax and Trounson themselves. To this end, the editors also added a sentence to our correspondence saying that "Lomax and Trounson discuss some but not all of these difficulties." In our estimation, this statement was inaccurate because in their original correspondence, Lomax and Trounson only acknowledged one of the seven problems we identified, and did not substantively discuss this (or any other) problem.<sup>26</sup>

We replied with a request to see the reviewers' comments, and with an inquiry about potential flexibility regarding the length. We were aware of the fact that *Nature Biotechnology* instructions to authors specify that "correspondence should never be more than one printed page." However, as the Lomax and Trounson correspondence was nearly two pages and as our original submission was shorter than their correspondence, we hoped for some flexibility on this point.

Within a few days, the editor responded stating that there would be little flexibility regarding the word count. At this same time, we were provided with two reviewers' reports. Surprisingly to us, there was no mention of the third review, which, according to previous correspondence with the editor, was responsible for the delay in decision making between May 8 and July 22, 2015. Collectively these two reviews suggested that we alter some language slightly and that we provide an alternative to Lomax and Trounson's methodology. One of the two reviewers claimed our submission was "too lengthy," but nothing in either of these peer reviews called for the scope of editorial changes made to our original submission.

We were not given a chance to respond to the reviewers' comments. Instead, we were asked to accept the revised text. We pushed back on a few things, including the error that had been introduced, changes to the title, the removal of certain references, and a few semantic points. Most importantly, we asked for the re-insertion of a sentence detailing one of Lomax and Trounson's methodological problems

that had been deleted in the editorial rewrite: namely, that their baseline estimate of 400,000 embryos incorrectly assumed that this number had remained static for over a decade.<sup>27</sup> Ultimately, the editor only accepted changes on minor semantic points, and rejected the re-inclusion of the sentence detailing the methodological baseline error—citing space issues as the reason. On September 9, 2015, our correspondence was published in *Nature Biotechnology*. Our correspondence took up two-thirds of a page, and was followed by a 180-word response by Lomax and Trounson.

#### PROBLEMS WITH THE EXPERIENCE

From our perspective, the peer-review and editorial processes were highly fraught. On close examination they reveal key concerns with (i) the time it took to complete the peer-review process, (ii) the failure to provide the authors with the peer-review reports, and (iii) the decision to rewrite our text instead of allowing us to respond to the peer-review comments. These concerns raise questions about academic freedom and authorial integrity.

The *Nature Biotechnology* guide for authors clearly states, with respect to correspondence, that “refutations are always peer reviewed.”<sup>28</sup> Moreover, the *Nature* policy for peer review states that “correspondence and all forms of published correction may also be peer-reviewed at the discretion of the editors.” As such, we expected our correspondence to be peer-reviewed. What we did not expect is that this would take nine months. In our view, the time it took to complete the peer-review process was problematic especially in light of the fact that “*Nature* journals are committed to rapid editorial decisions and publication.”<sup>29</sup> Long publication delays at *Nature Biotechnology* are not unheard of,<sup>30</sup> but given our previous experiences with publication, the short length of our correspondence (1,645 words), and the straightforward nature of our concerns about Lomax and Trounson’s claims, we did not expect it would take nine months. We do not (and cannot) know all the factors that resulted in this delay, but the number of peer reviews solicited may have been a contributing factor.

When we were notified of the decision to publish our correspondence, we were not provided with copies of the peer reviews. When we requested these to try to understand what might have prompted the journal to rewrite our correspondence, we received copies of only two of the three reviews, both of which were positive. No explanation was given for the missing third review. If a third review was received and suppressed, this runs counter to *Nature*’s “Peer-Review Policy,” which states: “We do not suppress reviewers’ reports; any comments that were intended for the authors are transmitted.”<sup>31</sup> If a third review was not received or did not provide specific or substantive feedback, then presumably the editors could have been transparent about this. Of the two reviews that were forwarded to us, one suggested specific revisions to improve the text. We were not given

the opportunity to act on these suggestions, however, and we were only provided with a substantially revised text for approval.

This second concern—the initial failure to provide us with the peer reviews, and the decision not to allow us to revise our text in response to reviewers’ comments—represents an important digression from the peer-review process as we know it. Authors should be entitled to see comments provided by peer reviewers (apart from confidential notes from reviewers to the editor) without having to ask for them. As well, they should have a chance to respond to those comments if their manuscript is being considered for publication. These are critical elements of the peer-review process that *Nature Biotechnology* acknowledges in its editorial policies, which state that peer review is integral to the publication process, not only insofar as it aids editors in decision making, but also because “feedback from referees conveyed to authors with *the editors’ advice* frequently results in manuscripts being refined so that their structure and logic is more readily apparent to readers.”<sup>32</sup>

This raises the third issue: rewriting the correspondence. It is a disservice to readers, authors, and reviewers to deny authors the opportunity to respond to peer reviewers, and instead to simply rewrite the authors’ text. Hyphenation and rephrasing are one thing, but in our case, very few of our sentences or clauses remained intact, while certain substantive methodological points were eliminated. Our text was not simply “refined”; essentially we were presented with a substantially rewritten manuscript, at one-third the length of the original manuscript that we had submitted.<sup>33</sup> The changes made to our manuscript were well beyond what might reasonably be described as copyediting. Further, when we attempted to make changes to the revisions provided by the editors, we were told that there was no space for any of our suggested revisions.

On this point, we draw attention to Cooke and Lapointe, who reference a number of journals—namely, *Science*, *Nature*, *Tree*, and *BioScience*—that employ “managing editors to essentially re-write papers after they are accepted.” Cooke and Lapointe note that while this practice is not generally acceptable, it is tolerated given the high impact factors and prestige associated with these journals.<sup>34</sup> This is an important area of publication ethics to interrogate, as prestige and high-impact factors should not legitimate the appropriation of authors’ content or the rewriting of their text to fit editors’ preferences.

#### THE DECISION TO PUBLISH WITH *NATURE BIOTECHNOLOGY*

Following our failed attempts to negotiate changes to the revised text provided by the *Nature Biotechnology* editors, we were left with a difficult decision that, in our view, authors should not have to face. Our choice was either to accept the publication of the significantly revised correspondence and risk compromising our authorial integrity, or withdraw our manuscript and send it for peer review elsewhere. Ultimately, we decided to publish the manuscript in *Nature Biotechnology*, the journal

for which it was intended. This was not a decision we took lightly. We had several reasons for proceeding in this way.

First, we had originally chosen *Nature Biotechnology* as our publication venue in the belief that it was important to publish our work in the same journal and category as the original publication to which we were responding. This, we believed, would be the most effective way to reach the target audience—that is, those who would be reading (and quoting) the incorrect information in the Lomax and Trounson correspondence.

Second, we thought that publication in the same journal as the correspondence we were criticizing might occasion a corrigendum on the part of the editors (or an invitation to the authors for them to issue an erratum), thereby increasing the likelihood that the erroneous claim about 1.39 million embryos in storage in the United States would not be cited uncritically.

Third, we were concerned by the fact that the original publication by Lomax and Trounson was garnering citations,<sup>35</sup> and we were worried about incorrect data being allowed to stand and take hold. Once information is cited multiple times, the very act of repetition can result in the information acquiring an authoritative status that can be difficult to dislodge.<sup>36</sup> Submission to another journal would have resulted in further delays and the continued citation of incorrect information. Given the pace of scientific advancements and political changes related to human embryo research, we deemed it important to publish our critique—already delayed by a year—in a timely manner so that flawed data would not become entrenched. Since the publication of our correspondence, citations of the Lomax and Trounson's data are qualified by reference to our work. For example, our correspondence is cited in the National Academies of Sciences, Engineering, and Medicine's 2016 study on mitochondrial replacement techniques as a qualification on the Lomax and Trounson data.<sup>37</sup> Similar qualifications are found in academic publications.<sup>38</sup> From our perspective, these citations vindicate our decision to publish our manuscript in *Nature Biotechnology*, even in a highly edited form, to prevent the ongoing use of misinformation.

Given that our original reasons for choosing *Nature Biotechnology* as the publication venue were still valid, and given our concerns about possible further delays in publication, we elected to accept the terms of publication stipulated by *Nature Biotechnology* and committed ourselves to write a paper on the challenges we had faced with regard to the appropriate balance of editorial oversight and authorial integrity.<sup>39</sup> In this way, we hoped to achieve the initial publication goal (i.e., to correct erroneous data in the public domain) and to initiate a discussion of editorial practices by documenting our recent experience.

In making this decision, we were mindful of the fact that because editors control publication venues, there are many incentives for authors not to speak out against biased or problematic editorial practices. For example, a concern for both emerging and established scholars is that any commentary on editorial misconduct may

adversely affect future attempts at publication. For our group of authors—one senior scholar, one new assistant professor, and one postdoctoral fellow—the stakes were varied, as the protections offered by tenure and a long career are very different from those facing a new faculty member at the beginning of his career, and still different from those of a postdoctoral fellow still on the job market.

## CONCLUSION

Editors (and editorial boards) wield tremendous power insofar as they decide what is triaged, what is reviewed, what is published, what is corrected, and what is retracted. As David Resnik and Susan Elmore note, “editors are the linchpin of peer review, since they make decisions that have a significant impact on the [peer-review] process and its outcome.”<sup>40</sup> The current COPE Code of Conduct and Best Practice Guidelines for Journal Editors emphasizes the importance of “ensuring the integrity of the academic record” and stipulates that “errors, inaccurate or misleading statements must be corrected promptly and with due prominence.”<sup>41</sup> This was not our experience, and we suspect that it may not be the experience of others. The Code also comments on the value of encouraging debate: “Editors should encourage and be willing to consider cogent criticisms of work published in their journal.”<sup>42</sup>

Our original correspondence criticized high-profile authors in a high-profile journal. The criticisms were many and, from our perspective, significant. The original published correspondence by Lomax and Trounson generated an erroneous conclusion that needed to be corrected. Instead of being assisted in this task, we were hindered in many ways, including the elimination of supporting data, explanations, and argument, together with significantly delayed publication. This seems antithetical to the purpose of publication: the effective and timely dissemination of new knowledge.

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## NOTES

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1. Smith, *Trouble with Medical Journals*, 144.
2. Snow, Cattapan, and Baylis, “Contesting Estimates.”
3. Martinson, Anderson, and de Vries, “Scientists Behaving Badly.”

4. Smith, "Ripping Yarn." The Committee on Publication Ethics (COPE) has listed nine cases of editorial misconduct on its website: [http://publicationethics.org/cases/?f%5b0%5d=im\\_field\\_classifications%3A790&f%5b1%5d=im\\_field\\_classifications%3A793](http://publicationethics.org/cases/?f%5b0%5d=im_field_classifications%3A790&f%5b1%5d=im_field_classifications%3A793). See also Godlee ("Institutional and Editorial Conduct").
5. Smith, *Trouble with Medical Journals*, 144.
6. Committee on Publication Ethics, "History of COPE."
7. Godlee, "Dealing With Editorial Misconduct."
8. Smith, *Trouble with Medical Journals*, 153.
9. Gollogly and Momen, "Ethical Dilemmas," 24.
10. DiMasi et al., "Cost of Innovation." This paper was updated in 2003 (DiMasi, Hansen, and Grabowski, "Price of Innovation"), and the updated information was considered in the analysis by Light and Warburton ("Extraordinary Claims").
11. Light and Warburton, "Ethical Standards," 62.
12. Louis Lasagna was deceased.
13. See Light and Warburton ("Extraordinary Claims"); DiMasi, Hansen, and Grabowski ("Reply: Extraordinary Claims"); Light and Warburton ("Setting the Record Straight"); DiMasi, Hansen and Grabowski ("Setting the Record Straight to Setting the Record Straight").
14. Light and Warburton, "Ethical Standards," 65.
15. Macklin, "Conflict of Interest."
16. Wilfond et al., "OHRP and SUPPORT."
17. *New England Journal of Medicine*, "Author Center."
18. Drazen, Solomon, and Greene, "Informed Consent." Drazen's involvement highlights an additional concern regarding the conflict of interest inherent in publishing in a journal that one simultaneously edits.
19. Macklin, "Working with Public Citizen."
20. Macklin et al., "OHRP and SUPPORT."
21. Lomax and Trounson, "Correcting Misperceptions."
22. Hoffman et al., "Cryopreserved Embryos."
23. Both Lomax and Trounson were employees of the California Institute of Regenerative Medicine (CIRM, California Stem Cell Agency). In 2013, at the time of the original publication in *Nature Biotechnology*, Lomax was the senior officer to the Standards Working Group at CIRM, and Trounson was the president.
24. *Nature Biotechnology* has an impact factor of 41.514. This is the second highest impact factor among the research journals published by the Nature Publishing Group (2/162).
25. For example: Ishii, Pera, and Greely, "Ethical and Legal Issues"; Côté et al., "Posthumous Reproduction."
26. To summarize, our correspondence identified seven problems with Lomax and Trounson's original correspondence: (1) missing data from 2004, 2011, and 2012; (2) the assumption that the 400,000 embryos in storage had remained static; (3) the assumption

that five embryos stored per live birth delivery, based on eight Northern California clinics, would hold across the country; (4) the use of inconsistent live-birth delivery numbers between 2005–2010; (5) a duplication error for the year 2006; (6) the use of live-birth deliveries rather than fresh non-donor and donor embryo transfers to determine the number of stored embryos; and (7) the lack of appreciation of how changes in ART practices since 2003 would affect embryo storage. Of these seven issues, the authors acknowledged that only 2004 data were missing but did not explain why.

27. The editor claimed that this sentence would simply reiterate difficulties already mentioned by Lomax and Trounson, and in defense of this claim, pointed to their sentence: "One must also consider the rate at which embryos are taken out of storage to be discarded, provided to another couple for fertility treatment or donated to research" (Lomax and Trounson, "Correcting Misperceptions," 288). However, Lomax and Trounson did not write this sentence in reference to their 400,000 embryo baseline, nor did they apply it to that baseline. As such, our criticism of their baseline was relevant and should have been published.

28. *Nature Biotechnology*, "Content Types."

29. Nature.com, "Authors and Referees."

30. Himmelstein, "History of Publishing Delays."

31. Nature.com, "Authors and Referees."

32. Nature.com, "Guide to Publication Policies"; emphasis added.

33. Snow, Cattapan, and Baylis, "Contesting Estimates."

34. Cooke and Lapointe, "Addressing Editor(ial) Malpractice."

35. For example: Ishii, Pera, and Greely, "Ethical and Legal Issues"; Côté et al., "Posthumous Reproduction."

36. Downie, "Debunking the Myth."

37. National Academies of Science, Engineering and Medicine, Committee on Ethical and Social Policy Considerations of Novel Techniques for Prevention of Maternal Transmission of Mitochondrial DNA, *Mitochondrial Replacement Techniques*, 99.

38. For example: Cohen and Adashi, "Embryo Disposition Disputes"; Baylis and Widdows, "Human Embryos and Eggs."

39. The original submission is on file with the authors and is available on request. One evident difference is that the original submission included supporting data, explanations, and arguments. The revised published correspondence is stripped of this information.

40. Resnik and Elmore, "Ensuring the Quality," 169.

41. Committee on Publication Ethics, "Code of Conduct," §12.1.

42. Committee on Publication Ethics, "Code of Conduct," §14.1.

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