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# Conservation Science and Discursive Violence: A Response to Two Rejoinders

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## ABSTRACT

We respond to two rejoinders to our review article “Science for Success,” which proposed fuller contextualization of epistemological approach, researcher position and interests in conservation research. This way readers—including reviewers and journal editors—can better understand and interpret findings. We suggest this contextualization is particularly important when conservation and development professionals undertake research about programs they are involved in, as this can potentially create a conflict of interest. Both rejoinders follow an extended process of complaint about our article that included academic and legal threats, and *ad hominem* attacks, with little engagement with the points made about researcher positionality. We consider this to be a form of “discursive violence” deployed to silence unfavorable perspectives, confirming our argument that positionality in conservation (research) begs self-reflection and transparent disclosure.

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## KEYWORDS

CBNRM; conflict of interest; conservation science; discursive violence; Namibia; reflexivity; researcher position; tourism; trophy hunting

## Introduction

In our review paper *Science for Success* (Koot, Hebinck, and Sullivan 2020), which went through three rounds of peer review with three anonymous reviewers, we emphasize the importance of researcher position and self-reflexivity for conservation and development professionals who do research about the programs they are involved in, as this can potentially create a conflict of interest. Following a social constructivist approach acknowledging that knowledge constructions are entangled with, rather than independent of, researchers and research contexts, we propose greater reflexivity and disclosure about the positionality of researchers in conservation research.

A number of recent papers regarding conservation research indicate that this concern is widely shared (Boyce, Bhattacharyya, and Linklater 2021; Brittain et al. 2020;

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Montana et al. 2020). It is especially relevant when researchers have a clear interest in the outcomes of their research. This situation may arise, for example, when organizations for whom research is conducted, or with whom research is affiliated, implement and fund the programs being researched. Publications resulting from such research may also percolate selectively through to the public sphere to become part of advocacy and policy. Our review of connected research and advocacy texts provides the context for raising the issue of researcher positionality and suggesting that conservation research is never fully neutral.

We highlight a gap between the findings of research carried out relatively independently of Namibian Community-based Natural Resource Management (CBNRM) institutional structures, and the findings of research carried out in significant institutional connection with organizations funding, facilitating, and implementing CBNRM initiatives. Our paper (Koot, Hebinck, and Sullivan 2020) provides illustrations of how this gap may be sustained, and makes a methodological proposal (including for ourselves as researchers) for more open reflection on possible sources of institutional bias, so that readers can better interpret findings. We maintain that this is a meaningful contribution to conservation research.

For clarity, we disclose that two of us share one previous co-authorship (Neimark et al. 2019) but the three of us have not all met in person. Our co-authored paper in *Society and Natural Resources* arose because in research stretching back over 30 years in different CBNRM-shaped contexts in Namibia we had all witnessed departures from promises and pronouncements of the success of ecotourism and trophy hunting, two pillars of the Namibian CBNRM programme. For example, as observed recently concerning CBNRM in the north-east of the country, only some 20% of the overall income generated from local resources and labor making ecotourism and trophy hunting businesses possible remains at local and household levels (Kavelage, Diez, and Bollig 2020). Such documented observations seem troubling for a programme that is now around 25-years old and that has been significantly subsidized through multiple international aid transfers.

In two separate rejoinders, Dickman et al. (2021) and Naidoo et al. (2021) filed different complaints to our paper. These rejoinders have a clear institutional connection, since Dickman et al.'s rejoinder is also authored by a conservation professional working closely with the authors of Naidoo et al.'s rejoinder. Together with this response, both rejoinders are the end result of a much longer process. Our paper was published online in May 2020. A week later we received a complaint from Dickman et al., demanding that the journal publish an erratum or retract the paper and threatening legal action for defamation. Several weeks later this was followed by a threat to "escalate" their complaint "both through academic and legal channels," and an assertion that "by sharing the article on social media and elsewhere" we would be increasing our "likely financial liability if it is found to be defamatory." In subsequent months we received a second complaint from undisclosed authors. Despite requesting for more than a year to learn the identity of these complainants, we were only informed on 21 October 2021 (less than two weeks ago at the time of writing) that they are Naidoo et al.<sup>1</sup> We are grateful

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<sup>1</sup>The time of writing here refers to November 2021. Importantly, after an earlier version of this response was published online on 15 December 2021, it was retracted by the publisher on 16 December 2021 without consulting us or the

to the journal editors and publisher for not immediately withdrawing our original paper despite these complaints and threats. Through a protracted process with the journal editors and the publisher we proposed that it would be of more value for conservation research generally for there to be a public exchange about the matters of concern here. We are grateful that this format was agreed to.

Nonetheless, at the time of writing, the publisher has communicated that a “Correction” document will also be published alongside our original paper, which has been “written in consultation with the Complainants.”<sup>2</sup> This “Correction,” however, contains inaccuracies and lacks contextualization (as largely addressed here). In it we are even “corrected” for things we do not write in our original paper. Factual errors and misleading statements in Dickman et al. (2021) and Naidoo et al. (2021) that we have pointed out several times in emails to the publisher, as well as in this response, also remained uncorrected when we last viewed the “Correction” and the rejoinders. The reason for this remains unclear to us.

The two rejoinders we respond to here, and particularly the one by Naidoo et al., contain substantial detail which, to facilitate readability, we at times respond to and clarify in endnotes. We emphasize that the intention of our paper (Koot, Hebinck, and Sullivan 2020) was not at all to effect damage to anyone’s personal reputation, but to contribute to a conversation about the importance of disclosing and reflecting on institutional positionality in conservation research. As we write in the last sentence “[w]e are proposing [...] that a fuller contextualization of epistemological approach, researcher position and interests is crucial for other readers, including reviewers and journal editors, to be able to more accurately understand and interpret research findings” (14). It has been particularly surprising to us that especially the rejoinder by Naidoo et al. (2021) resorts to personal attacks directed at Koot and Sullivan, even after it was communicated to us by the publisher that it would be important for all authors to stay “focused on matters of academic discussion, rather than ‘ad hominem’ matters.” We do not consider such attacks to constitute a useful engagement with the content of our paper, but rather to constitute a familiar aggressive political tactic to silence, through attempted character assassination, observations and perspectives that depart from institutionally accepted CBNRM discourse. As our title here indicates, we consider this to be a form of “discursive violence,” a proposition we explore more fully below.

## Response to Dickman et al

Dickman et al. (2021) are mainly concerned about the first two paragraphs of our paper, in which we draw attention to the fact that a recently published exchange of “Letters” in *Science* prompted an alteration of the journal’s own policies regarding the

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journal editors. The reason remains unclear to us. We were first told that the retraction was based on “advice from a colleague in Legal,” but a subsequent Publishers Note suggested the retraction was “due to a production error.” We agreed at the time to this Publishers Note because our priority was to re-publish our response as soon as possible. In particular, we wished our reaction to the *ad hominem* attacks and factual errors in Naidoo et al. (2021) and Dickman et al. (2021)—published online on 3 December and 10 December 2021 respectively and not retracted—publicly available. An uncomfortable situation had arisen in which Koot and Sullivan were personally attacked, while our reaction to these attacks was denied public accessibility by the publisher.

<sup>2</sup>Which is, again, November 2021. As of April 2022, it remains unclear to us if the publisher will publish the “Correction” to our original article, and if so which corrections this will include and on what basis.

disclosure of interests. As we state (2), “*Science* has now reconsidered its policy ‘to ensure that authors of Letters also make readers aware of financial and advisory competing interests’ (Berg 2019).” Our intention here was to emphasize that the disclosure of interests is a very lively and timely concern in conservation research. This choice was also instigated by the reviewers’ and editor’s request regarding the first submission of our paper to show the relevance of our argument “beyond Namibia,” a suggestion we appreciated and thus elaborated in subsequent revisions. A recent policy shift in a high-impact journal such as *Science* that is in line with our broader argument was a good way to highlight this significance and its relevance to conservation research.

Furthermore, trophy hunting—as focused on in the referenced Letters exchange in *Science*—is a core component of CBNRM in Namibia, illustrating the wider relevance of this Letters exchange for material we review in our paper. The information we used was drawn from an Editorial Addendum published by *Science* (Berg 2019). We were thus working with published material concerning a lively issue in conservation research regarding the importance of the disclosure of competing interests by researchers. The suggestion by Dickman et al. (2021) to also include potential conflicts of interest by other contributors’ to the *Science* Letters exchange would have gone beyond our intention in the opening of our paper, which was to introduce the topic of positionality and potentially competing interests in conservation research. In acknowledging (2) that *Science* had subsequently reconsidered its policy “to ensure that authors of Letters also make readers aware of financial and advisory competing interests” (Berg 2019, 874), however, we also indicate our awareness that this journal policy shift obviously applies to *all* “authors of Letters.” Our primary intention was to draw on this policy change to demonstrate a newly energized commitment toward considering the relevance of researcher position and the importance of full disclosure of potential material interests in conservation research.<sup>3</sup>

It is striking to see how much Dickman et al. (2021) appear to have misunderstood and even misrepresent our paper, and how many factual errors they make: they attribute an addendum to us which we have not written (“their addendum,” 3);<sup>4</sup> they accuse us of “considerable inaccuracies” (1) without specifying what these are; they frame us as incorrectly attributing bias to them where we in fact clearly speak about the change in editorial policy in *Science* (1); they make up that our article draws “attention to the malign influence of COI [conflict of interest] in conservation” (2), when instead it is clearly concerned with transparency about researcher positionality in conservation research; at one point they incorrectly state that we “cite only [our] lead author” (2) where in fact we cite *three* publications (Batavia et al. 2019; Hannis 2016; Koot 2019), using their inaccurate assertion to attribute bias to us in our view that trophy hunting is receiving public criticism when such public criticism is widely prevalent and available, albeit contentious; and finally, while we clearly disclose our own positionality, they

<sup>3</sup>Regarding the exchange of letters in *Science* initiated by Dickman et al. (2019), we learned that a complaint similar to that received regarding our paper was made by Dickman et al. to *The Times* newspaper in the UK, detailed at <https://www.ipso.co.uk/rulings-and-resolution-statements/ruling/?id=08417-19>. In this case 5 of the 6 specific complaints were *not* upheld in formal review by the Independent Press Standards Organisation—as explained in clauses 16–21 in the shared link. This is not to say that the one complaint upheld in this case (clause 16) was insignificant, but that the overall consideration and ruling here provides relevant background to Dickman et al. (2021).

<sup>4</sup>“Addendum” here refers to the “Correction” mentioned above, but the title of the rejoinder by Dickman et al. (2021) still refers to a Correction: “Comment on Koot, Hebinck, and Sullivan (2020) and correction.”

make unsubstantiated insinuations about “whether conflicts of interest existed for [our] own work” (2), again without any further specifications.

The remainder of our paper does not relate to any details of the Letters exchange in *Science* initiated by Dickman et al. (2019). Their concerns seem much more an issue between them and the editorial board of *Science* than with us. That said, Dickman et al.’s (2021) elaboration of the definition and context of what exactly constitutes a conflict of interest is important and enhances points we make in our paper. For example, we may indeed wonder why conflict of interest is debated in medical and pharmaceutical research (Brody 2011), but only more recently so in conservation science.

## Response to Naidoo et al

### Our Usage of Public Sources

We agree with Naidoo et al. (2021) that we draw on a range of sources. We are completely open about that. This material forms part of our methodology (Critical Discourse Analysis) in illustrating how scientific findings and peer reviewed papers may be used selectively in the public domain (for example, by advocacy organizations) to present often simplified versions of research. In the section, “CBNRM Success and Public Institutions” (8–9) we separately present examples of the public discourse and the ways it draws selectively on peer reviewed research. It is unclear to us why the use of a range of connected sources is considered problematic and we disagree that we create “deliberate conflation of results of scientific papers with unpublished research, or with statements by advocacy organizations” (Naidoo et al. 2021, 1). We also clarify explicitly in our paper that one publication is based on a conference presentation.<sup>5</sup>

Naidoo et al. (2021) also express concern about self-citations in our paper. For clarification, we cite nine papers of which one of us is an author or co-author, meaning that with three authors on average we “self-cite” 3 papers. Importantly, however, the cited papers are not co-authored between us. The point we are making in our paper is that a cluster of connected papers with similar findings and interpretations was written by an overlapping group of authors with institutional interests in their research. In keeping with Critical Discourse Analysis, what is more relevant for our paper is not *the number* of citations but (i) the connectedness in terms of citations between a small number of

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<sup>5</sup>With regard to Naidoo et al.’s accusation that we misrepresent a conference paper (Weaver and Skyer 2003) by conflating it with peer reviewed journal articles, on 7–8 (emphasis added) we state explicitly that “In 2003, at the fifth World Parks Congress in South Africa, scholars working at WWF Namibia and NACSO presented their work,” something that is also clarified in the reference list. Again, we do not understand the problem of using such “unpublished” grey literature material which is publicly available at the website of a crucial donor for CBNRM in Namibia at the time, see [https://pdf.usaid.gov/pdf\\_docs/PNACX280.pdf](https://pdf.usaid.gov/pdf_docs/PNACX280.pdf): this too is authored scientific research output. We also note that Naidoo et al. (2021) use this one unpublished conference presentation in their list to show that they might *not* frequently refer to each other, such that in their own terms they are in fact “conflating” different types of sources here. Be that as it may, it appears that our use of the term “frequently” is the problem here. More importantly for our paper, though, is the date of publication, the pattern of citation in corroborating findings and interpretations, and the fact that the institutional positionality of the authors is little discussed in connection with these findings. Disregarding the earlier conference presentation, the pattern is that Naidoo et al. (2016) refer to all other papers in our list that had been published at the time. The same counts for Angula et al. (2018). Störmer et al. (2019) cite two of the other four papers, as well as the conference proceeding. Whether this can be considered “frequently” or not we accept is a matter of interpretation.

outputs by overlapping institutional authors that articulate similar findings and interpretations, and (ii) how this material is taken up in the public domain.

### ***Our Engagement with Data and Literature***

One thing we illustrate in our paper is a pattern in discourse regarding the contributions of tourism and trophy hunting to CBNRM in Namibia by collectives of researchers connected with the programme's facilitating organizations and donors. We considered two papers in particular, Naidoo et al. (2016) and Angula et al. (2018), which focused on these elements of the programme. Regarding Naidoo et al. (2016), we engaged extensively with this paper, particularly on pp. 8–12, as an example of a scientific paper favorable to trophy hunting that is used by an influential trophy hunting body (the Namibia Professional Hunting Association, NAPHA) to support trophy hunting business, showing how science is selectively used in the public domain to strengthen particular stakeholders. On p. 12 we suggest possible issues with stated economic benefits of meat distribution through hunting. We indicate that a specific area of discussion could be the assumptions on which calculations for the monetary value of distributed hunted meat are based, given known problems of “willingness to pay” suppositions in environmental economics that translate “value in use” to putative “exchange values” through shadow pricing and benefit transfer techniques (e.g. Sagoff 2004; Plummer 2009). We maintain that such concerns remain worthy of discussion because of the strong reliance of overall income calculations in CBNRM on figures derived through these methods. In addition, we disclose that by invitation from the journal *Conservation Biology* one of us had in the past contributed a detailed double-blind review of this paper (see also endnote 1, p. 14 of our paper). In doing so s/he had engaged in depth with the data and analyses presented in this paper, which also show—unsurprisingly, given different resource rent possibilities across Namibia's remaining communal areas—that trophy hunting income is highly differentiated across conservancies (confirmed in Natrass 2021). Regarding Angula et al. (2018), we refer readers specifically to pp. 12–13 of our paper, endnote 6 and the clarifications below (“Ethical Concerns”).<sup>6</sup> We thus have not failed to engage with the data and analyses in the papers we review, and we stand by our suggestion that these important analyses are worthy of broader discussion.

Naidoo et al. (2021) also focus on elements that were *not* the focus of our analysis to suggest that we exclude other literature on CBNRM. As clearly stated on p. 6 of our paper, “[t]he most important variables we used for this sample were first, that they address Namibian CBNRM for which the crucial elements of trophy hunting and eco-tourism emerge as critical, and second, that they have been written by professionals in

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<sup>6</sup>We agree with Naidoo et al. (2021) that they are transparent about the fact that the survey in Angula et al. (2018) was opportunistic and could potentially be biased. In fact, we write positively about this transparency saying “[t]his acknowledgement of potential bias is important” (13). However, they *misquote* us in their rejoinder when they write that we state they “incorrectly generalize our results to an incorrectly homogenized community” (Naidoo et al. 2021, 3). What we in fact write is this: “The authors [i.e. Angula et al.] suggest that a conclusion is being drawn in which a potentially incorrect generalization is made to a similarly incorrectly homogenized community, namely a small selection of trophy-hunting-supporting elites in each conservancy” (13), i.e. this is *their finding* that we are reporting in our article. Moreover, it is incorrect that we say in our paper that Angula et al. (2018) exclude non-literate or non-English speaking people. Instead, and in line with the focus of our paper, we raise a point of concern regarding the position of researchers doing surveys with non-literate and non-English speaking people. This is a different issue altogether.



organizations whose work is to promote and implement exactly this CBNRM model.” For this reason, we do not understand why Naidoo et al. (2021) suggest we engage with “three very pertinent articles” that fall *outside* the scope of our paper,<sup>7</sup> although we accept that as in any research selection choices may introduce bias.

Finally here we point out that we have disregarded additional statements from the reviewed papers that iteratively emphasize specific positive outcomes of CBNRM in their conclusions.<sup>8</sup> Again though, these interpretations and conclusions are not in themselves of concern. What we argue instead is that researchers publishing such results have competing interests because their research is structured for the organizations funding this research who are also financing and implementing the CBNRM program. We maintain that this positionality needs more foregrounding and attention within conservation research to assist with the interpretation of findings.<sup>9</sup>

### **Ethical Concerns**

Regarding Naidoo et al.’s (2021) concern with our observations about the issue of ethics observed in Angula et al.’s (2018) paper, we note that a short question about why trophy hunting is good or bad (as in the survey included in their paper) is only a small element of the much larger moral philosophical consideration that defines the field of ethics. Based on this question, Angula et al. (2018, 27), claim that “the ethical concerns of hunting individual animals for sport that are mentioned by wealthy people living in developed western countries as justification to stop trophy hunting were not mentioned at all by local community members living with wildlife.” We suggest that this observation is weak, however, because the published paper is unable to show that these kinds of “ethical concerns” by people from western countries were fully asked about. The authors do not specify, either in their paper or in their survey, what these concerns are, leaving the reader in limbo about what exactly is under consideration here. Complex patterns of local support for trophy hunting exist in CBNRM contexts (Mbaiwa and Hambira 2021; Hewitson and Sullivan 2021) and we are not ourselves necessarily opposed to hunting practices. What we are discussing is how so-called “Western

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<sup>7</sup>Naidoo and Johnson (2013) does not match the first variable (is not about trophy hunting or ecotourism) nor the second; Riehl, Zerriffi, and Naidoo (2015) does not match the second variable; Naidoo et al. (2018) largely matches the second variable but is about a different topic (namely wildlife corridors).

<sup>8</sup>For example, “[w]e have shown here that Namibia’s community-based natural resources programme has led to increasing economic benefits for 230 000 people,” which “has coincided with the improved management and recovery of populations of large wildlife throughout the affected communal areas” (Naidoo et al. 2011, 452); and “[t]he conservation of biodiversity in Namibia is not only of aesthetic or ethical significance, but yields tangible economic benefits that enhance the well-being of rural communities in one of the world’s poorest regions” (Naidoo et al. 2011, 315).

<sup>9</sup>Naidoo et al. (2021) are additionally concerned that we misrepresent Störmer’s and Naidoo’s positions in relation to CBNRM in Namibia. To be clear, we do *not* state anywhere in our article that Störmer was a WWF-employee or that Naidoo worked for WWF-Namibia. That said, for the paper we cite by Störmer et al. (2019) four of the five authors are directly connected to a CBNRM facilitating or financing organization, with Störmer’s contribution linked with KfW financing support for CBNRM activities in Namibia (see <http://programme.exordo.com/pathways2018/delegates/presentation/34/>). It is unclear to us why it needs to be pointed out that Störmer has “not play[ed] a role in implementing CBNRM in Namibia” (Naidoo et al. 2021, 1) since we do not state this in our paper. Regarding both the positions of Störmer and Naidoo our elaboration on p. 7 remains correct: we explain that the papers “have been written by professionals working for organizations that have collaboratively instigated *and supported* CBNRM initiatives throughout the country, through work financed by *and linked with* especially WWF Namibia, WWF US, the Ministry of Environment and Tourism (MET) and the Namibian Association of CBNRM Support Organizations (NACSO)” (emphasis added).



opposition” to trophy hunting is “neutralized” in research in the course of exploring possible mechanisms of confirmation bias. When studying Table 1 (in Angula et al. 2018, 28), to which Naidoo et al. (2021) refer, for example, it reveals relatively simple answers, focused predominantly on the *benefits* of trophy hunting. The questions and answers do not encompass the full range of possible ethical or justice concerns (see, e.g. Martin, McGuire, and Sullivan 2013). Literatures exploring “non-Western” concerns about structures of trophy hunting are also not mentioned (see e.g. Mkono 2019; Koot 2019; Schnegg and Kiaka 2018; Yasuda 2011).

### Ad Hominem Attacks

Using strong language, Naidoo et al. (2021) show a serious interest in Koot’s and Sullivan’s histories in Namibia. We address these personal attacks in turn. Koot worked for several years (2002–2007) at a community-based campsite (*not* a lodge, as they state, which makes for a crucial difference in this context) supporting a very marginalized community. He indeed wrote about this experience but the problem with doing so remains unclear in Naidoo et al. (2021). The methodological point we are making in our paper (Koot, Hebinck, and Sullivan 2020) is that it is important to be transparent and reflexive regarding one’s institutional positionality, *not* that one should not write about one’s practical work experience. In the chapter they mention, Koot (2012, 155) *is* reflexive about his own position, stating:

I was probably—and in some ways still am—more closely attached to this project than the average researcher. The methodological consequences of this are that I know the project very well, in detail, but I am probably less objective than an outside observer.

In a separate autoethnographic paper Koot (2016b) also discloses the difficulties that arose when working there, especially concerning power relations and inequality. Naidoo et al.’s (2021) reference to the discussion between Tomaselli (2017) and Koot (2016a, 2017) raises the question of why they leave out Koot’s response to Tomaselli (Koot 2017). About half of this response focused on researcher positionality and is more relevant than the derogatory (and inaccurate) quote they use by Tomaselli about Koot. The selective choices by Naidoo et al. (2021) here seem to us to reveal bias with an agenda to conduct character assassination. In addition, the context or relevance of a small amount of funding from WWF Namibia received by Koot for his PhD fieldwork in 2010, as mentioned by Naidoo et al., remains unclear.<sup>10</sup>

Sullivan is surprised to read Naidoo et al.’s (2021, 4) statement that “Sullivan does not reveal she has had conflicts with CBNRM practitioners during her research in Namibia.” She presumes this statement refers to her attempts to engage “CBNRM practitioners” over a period of 4 years from 1995–1998 regarding observations from her PhD field research on natural resource use suggesting that outcomes “on the ground” appeared to be departing from CBNRM intentions, as she and others locally understood them at that time. She was seeking to support the manifestation of the CBNRM programme in the area of her field research. She did not receive any response to her communications until a draft and

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<sup>10</sup>Koot worked for the community-based tourism project Treesleeper Camp at the resettlement farm Tsintsabis from 2002 to 2007. In 2010 he received US\$10,000 from WWF Namibia, in addition to several smaller grants from other donors, for fieldwork costs in Namibia which resulted in a PhD dissertation (Koot 2013).

incomplete conference paper circulated for Oxford University's 1999 conference on *Displacement, Forced Settlement and Conservation*, in which she voiced some concerns, apparently found its way to WWF Namibia. The first Sullivan knew about this situation was a series of threatening, abusive and widely distributed communications from various people involved with designing and implementing CBNRM in Namibia who repeatedly sought to prevent publication of her research. She also received a number of significantly supportive communications, evidencing a diversity of perspectives regarding the design and implementation of the programme in these years. These circumstances are documented ("revealed") in published work including detailed reflections on positionality (Sullivan 2003). In our paper (10) we make clear reference to this past situation from 20 years ago: "[i]n relation to Namibian CBNRM, Sullivan herself, for example, has been 'subjected to attempts to close down publication of disconfirming evidence, through personal and legal threats' (Sullivan 2018, 8)." We are unclear how this statement constitutes a "sanitized version of [our] record of engagement with Namibian CBNRM," as claimed by Naidoo et al. (2021, 4). We would like to also add that Sullivan continues to work closely with organizations and individuals involved with CBNRM in north-west Namibia, including the Sesfontein Conservancy, Save the Rhino Trust, the Nami-Daman Traditional Authority and the Ministry of Environment, Forestry and Tourism.

We agree that we do have a critique of market-based instruments and "neoliberal conservation." We assume this is what is being referred to by Naidoo et al. (2021, 4) as evidence that we "are part of a branch of social science that some consider to be anti-capitalist and/or anti-neoliberal" and thus are ideologically driven, as if "science" and its practical applications exist somehow outside of ideology. Our research on CBNRM in Namibia is public and visible (for example De Vette, Kashululu, and Hebinck 2012; Koot 2019; Lubilo and Hebinck 2019; Sullivan 2006, 2013) and our critique has arisen because of an observed deepening of structural inequality linked with market-based solutions connected with conservation, as they are currently designed. Additional concerns include a naturalizing of past displacements in contemporary CBNRM "territories" (Sullivan and Ganuses 2020), and the ways that local resource and labor realities become dependently determined by external investments and associated market demands, as opposed to supporting local markets (Hebinck, van der Ploeg and Schneider, 2015). This critique, however, is not the same as having institutional interests in the outcomes of research.

## Discursive Violence

When we proposed to the publisher that perhaps the complainants would make their concerns public with a chance for us to respond, we wrote that "we hope that such a public platform will encourage the complainants to refrain from *ad hominem* attacks and stay focused on the content" (letter to the publisher, 8 September 2020). Instead we find that Naidoo et al.'s (2021) rejoinder, written mostly by WWF employees, continues to use personal attacks to silence different perspectives. We consider the attacks and threats we have received to be a form of *discursive violence* deployed with the intention of extinguishing unwelcome findings and opinions.

The concept of "discursive violence" comes from feminist and critical race theory to foreground how the silencing or muting of perspectives may be structurally linked with

specific axes of difference (e.g. Douglas 1995; Karlberg 2005; Smalls 2018). Given that we are writing from relatively privileged positions ourselves, its explanatory value may be surprising here. We note, however, that to the extent that social sciences, humanities and practitioner engagements with conservation have observed and disclosed findings unpalatable in conservation research, a pattern of response has been widespread and systematic forms of silencing (Igoe and Sullivan 2009). In conservation research specifically, the concept of discursive violence has been described as a pillar of the “green violence” that may be deployed to effect nature protection (Büscher and Ramutsindela 2016). Recently, WWF has also been associated with such broader practices (FPP 2021; Warren and Baker 2019), but here we explicitly make the association with *discursive* violence.

Personal attacks and threats of defamation seem to us more an attempt to silence unwelcome perspectives than to engage with these perspectives. We maintain that they do not counter or build on our proposal for normalizing transparency and reflexivity regarding institutional positionality in conservation research, especially when authors and the organizations their research is linked with have interests in their interpretations and findings. We are not clear what the authors of either rejoinder are trying to achieve. If anything, their responses are further confirmation of the central argument of our paper: that positionality in conservation (research) begs self-reflection and transparent disclosure.

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