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Letter to the Editor

Williams and Morrone misunderstand and inadvertently support my argument: Mexico's SNI systematically steers ecological research

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Abstract

My recent publication highlighting the unintended steering influences of publication incentives on the research agenda of Mexican ecological scientists elicited a vehement critique from two renowned Mexican ecologists, Williams and Morrone. The argument I advance is that Mexico's National System of Researchers (SNI) unintentionally steers ecological researchers away from the knowledge needs of local, national, and regional users of scientific knowledge and relinquishes research priority-setting to individuals and institutions disconnected from Mexico's knowledge needs. Rather than engaging with the substance of argument, these critics focus on what they believe to be factual errors. In their response, Williams and Morrone effectively but unintentionally confirm the premise, mechanism, and indeed the key factual claims in my article. I stand behind my analysis as published.

Key words: research evaluation; ecology; impact factor; Sistema Nacional de Investigadores; SNI; Mexico

My recent publication highlighting the unintended steering influences of publication incentives on the research agenda of Mexican ecological scientists (Neff 2017) elicited a vehement critique from two prominent and prolific Mexican ecologists, Williams and Morrone (2018). The argument I advance is that Mexico's National System of Researchers (SNI) unintentionally steers ecological researchers away from the knowledge needs of local, national, and regional users of scientific knowledge. These policies place substantial emphasis on—and financially incentivize publication in—Web of Science (WoS) Journal Citation Reports (JCR)-listed journals. WoS and JCR are commercial database products designed for their utility and salability to a particular community. The journals included in those databases themselves exercise substantial power in arbitrating scientific quality, but priorities of their editorial boards contribute few—if any—meaningful feedbacks to regional science users. SNI policies thus surrender substantial decision authority about science priorities to entities disconnected from Mexican knowledge needs and encourage Mexican ecologists to publish their work in a foreign language in journals to which many Mexican institutions lack subscriptions. My research is built upon 30 h-long interviews with Mexican ecologists, science managers, and natural resource managers. I analyze their experiences in the light of policy documents that define incentive structures and contextualize my analysis in light of relevant literatures in science policy and science studies. The ecologists with whom I spoke felt that the evaluation process actively steered them away from known knowledge needs and encouraged them to publish in journals to which they themselves have limited subscription access, an outcome logically consistent with the policies under which they work.

SNI, as I acknowledged in my original article, has been quite successful at increasing Mexican scientific contributions to the international scientific literature. Williams and Morrone are so committed to *that goal* that they misunderstand my point: *Mexican ecologists reported to me that they are spending their limited resources addressing international scientific priorities rather than contributing to local, state, national, and regional knowledge needs*. I wholeheartedly agree with my critics that Mexican science suffers from underfunding and a range of other substantial challenges. My interview participants reported that they were frustrated by spending their limited resources on topics disconnected from Mexico's needs.

Rather than providing evidence of my supposed sloppy research, Williams and Morrone effectively but unintentionally confirm the premise, mechanism, and indeed the key factual claims in my article. I stand behind my analysis as published.

Before I begin my response, I want to reiterate that I wrote my article to help give voice to the concerns of talented and dedicated scientists, science policy makers, and resource managers with whom I spoke. I am echoing what they told me and putting their experiences within the context of a systematic policy analysis and of established literatures on science policy and science studies. My goal is to help the scientists with whom I spoke challenge what they deem to be unintended impacts of a system established for very valid reasons.

Nothing that what I wrote should be seen as an attack on scientists in Mexico. Quite the opposite: I find my interview participants to be inspirational in their commitment to high-quality science in service of their communities, their country, and their region.

I published the article to encourage reflection by scientists and policy-makers worldwide, including those within SNI and CONACYT (Mexico's main national science funder), about how they evaluate and reward their talented scientists. Toward this end, it is beneficial to have the opportunity to respond to Williams and Morrone, as they clearly lay out the system I set out to analyze. By virtue of their academic prestige and ranking within the SNI system and its administration, these two authors have substantial power to influence its format and implementation. I hope that they read this response with minds open to my analysis and to the concerns of other scientists in their country.

Williams and Morrone begin their critique by describing what they believe are four factual errors in my argument that they assert undermine it, and they then question several aspects of the logic in my analysis. I will address each of those concerns in sequence to demonstrate how these authors inadvertently provide support for rather than undermine my claims. Their inability to see that they are defending my argument for me suggests the depth to which they have internalized the perceived hierarchies in academic publishing.

I concede one minor factual error, (within their second point) and acknowledge that part of my analysis was based on a document that has since been updated. I note these shortcomings below. They do not undermine my analysis, as they are tangential to it.

1. Claimed error 1

Williams and Morrone suggest that my 'most serious error' is in stating that journal impact factors 'are used by the evaluation committee as '"simplistic proxy measures"' for quality, that researchers modify their research topics in order to publish in high-IF [impact factor] or even top-tier journals as a means to ascend the SNI system' and that researchers avoid research topics that might not yield adequate SNI-recognized publications. *I never state that the committee uses journal IF as a 'simplistic proxy measure'*. I do use that phrase, but to describe the problematic but nearly ubiquitous use worldwide of publication and citation counts in researcher evaluation when the stated goal is societal benefit. Publication does not equate to impact, influence, or benefit; nor does a citation (Bozeman and Sarewitz 2005; Moustafa 2016). I make a point in my article of asserting that this misunderstanding is nearly universal in contemporary scientific culture. I encourage readers to see my original article for an explanation of these ideas. They are well-supported in scholarly literature of the science-society and science-policy interfaces.

Rather than undermining my argument, Williams and Morrone provide an extensive and detailed explanation of the centrality of WoS-related products to SNI evaluation criteria and processes. Their narrative can and should serve as the final word, given that they both have served on the relevant SNI area committee, and one currently serves on the appeals board. *These authorities on SNI directly confirm my point that Web of Science products play a central role in the evaluation of ecological scientists*: 'The requirements for [SNI] Level I involv[e] authorship of three papers in a Journal Citation reports (JCR)-indexed journal (IF > 0.5) in each three year period'. Elsewhere they state that 'journals with lower IF values are judged to be of poor quality'. *This is exactly my point.* The factors Clarivate Analytics (the current corporate owners of WoS and JCR) takes into account when it determines which journals to include in their commercial database products have no connection whatsoever to Mexican knowledge needs (see also Chavarro et al. 2017a). The committee's stated goal for research is that it gains recognition by international peers, and they operationalize that goal by counting publication in and citation from sets of journals curated by profit-oriented foreign corporations. SNI policy lets the owners of those database products determine what counts as worthwhile research for Mexican ecologists. Until very recently, that *systematically excluded nearly all Spanish-language journals* from being considered in the system of pay bonuses for Mexican scientists, regardless how well they were utilized and respected in the region. Regional and language biases in WoS databases make them inappropriate for use in researcher evaluation programs (Chavarro et al. 2017a)

Further supporting my argument even as they claim to undermine it, Williams and Morrone state: 'In addition, to attain Levels II or III, researchers have to demonstrate leadership and international presence in their discipline through higher rates of publication and citations'—again in those same foreign journals, curated by foreign corporate financial interests, and run by international editorial boards—and assert that 'publication of articles in mainstream journals within each discipline and books and chapters by international publishers (Elsevier, Springer, OUP, CRC, Taylor & Francis, etc.) is considered favorably for the upper levels'. Throughout the SNI criteria, the focus is on international publishers, international journals, and English language because *the stated goal is that Mexican science is recognized and utilized by the international scientific community*. Note the conspicuous absence of reference to national or regional knowledge needs. They continue: 'The evaluation committee may also suggest that researchers submit their work to higher impact journals, in order to increase the visibility of their research'. Williams and Morrone's response is an articulate confirmation of my point.

They describe, as did I in my original article, that outreach, training of students, and other activities contribute to SNI evaluation as well. But, if anything, their narrative suggests that editorial choices of publishers and commercial decision making by the parent company of WoS products and foreign editorial boards are *more* central to evaluation processes than I asserted.

I make a point of acknowledging in my article that there have been debates in the literature on the relative importance of WoS products and journal IFs in the SNI evaluation processes and attempted to be fair to both sides of that debate. Williams clearly is familiar with other authors who share my position within that exchange since he co-authored a response to one of those contributions as well. In response to my article, Williams and Morrone cite only Williams' own contribution to the debate (Williams & Aluja 2010) rather than acknowledging that other Mexican ecologists have published critiques compatible with my own (e.g. Ricker et al. 2009, 2010).

And, as I argued in my original article, scientists perceive (apparently correctly) that JCR-listed journals and journal IF matters in their evaluations. As I document, they act accordingly. Williams and Morrone assert, without evidence, that it 'is not and has never been the case' that scientists would actively avoid nationally- or regionally-important topics based on expectations of what is publishable in SNI-recognized journals. As these respected and prolific scientists know, such an empirical claim should come with some form of evidence, especially since it contradicts what sociologists of science have long known about scientific problem choice. When the success of scientists' careers is adjudicated based on their

quantitative productivity and the 'quality' of the journals in which they publish, of course those factors contribute to their selection of research questions (Busch et al. 1983; Ziman 1981, 1987; Zuckerman 1978). The focus on IF internationally is a problem that countless international scientific bodies, publishers, and individual scientists have in recent years recognized and begun to organize against because of its distorting influence on science (Alberts 2013; Hicks et al. 2015; Monjeau et al. 2013; Moustafa 2015; The PLoS Medicine Editors 2006).

More problematic to Williams and Morrone's assertion that scientists do not change their research foci because of the need to publish in particular journals, *Mexican ecologists with whom I spoke told me that they themselves and their colleagues do select research topics in this way.* As I documented in my original article, one ecologist reported a switch to a very different subfield based on more promising citation patterns in journals in the new area, and many other researchers reported actively balancing work on research subjects that yield rewards under SNI with research that they feel is more important to their regions and communities but that may not be of interest to the often-foreign editorial boards of the English-language journals in which they are expected to publish. Mexican ecologists report needing the financial incentives associated with SNI and crafting their work choices to achieve those pay increases. Many of the ecologists with whom I spoke actively chose to sacrifice the SNI financial incentives in order to work on topics they deemed to be important to local, national, and regional users of science. I am not critiquing Mexican ecologists by pointing out that SNI incentives influence their choices. Quite the opposite, I admire that many are bucking the system at great personal cost and am sympathetic to those who cannot do without the SNI bonuses and voiced with frustration that they have to focus on international scientific interests to earn them.

Far from being a serious error, my assertion that WoS and journal impact factors play a central role in researcher evaluation under SNI Area II criteria is well supported in my original article, in the broader literature, *as well as by Williams and Morrone's response to my article*. As I systematically document in my article, the format of these criteria does shape ecologists' problem choice processes.

2. Claimed error 2

Williams and Morrone point out that my article stated that 'Researchers need SNI-3 status to be eligible for membership on the evaluation committee, and current *committee* members elect members as vacancies occur'. They are right to assert that the italicized word 'committee' is incorrect. It should read 'SNI'; all SNI members are eligible to vote on candidates to fill (most, but not all) vacancies. *My point stands*: only those with the highest success within SNI are eligible to serve on the committee that determines the evaluation criteria, and only those who have achieved SNI-level I are eligible to vote. This creates a self-reinforcing system: those who have succeeded under the system are the ones most empowered to alter it; any scientists who believe that the SNI criteria inadvertently divert attention from legitimate topics, for whatever reason, are likely to direct at least some of their research attention to those topics even at the cost of SNI status. And, numerous scientists told me that they 'play the game' of pursuing international scientific priorities only as much as they need to in order to achieve financial security; they spend the rest of their time working on knowledge needs as identified by local, regional, and national knowledge users. Moreover, other scholars have documented a range of legitimate and beneficial reasons why researchers opt to publish in journals that do not yield rewards under the SNI system, regardless whether the work is publishable in WoS JCR-listed journals (Chavarro et al. 2017b). Any such researchers are less likely to achieve adequate status in the SNI system to alter it.

Williams and Morrone suggest that membership on the evaluation committee is not problematic for ecologists because there are several current members with ecological expertise. I would point out that the committee evaluates not only the relatively well-cited ecologists who pursue genetics- and remote sensing-oriented ecological questions, but also the relatively poorly-cited areas (particularly within the two-year citation window captured in journal IF calculations) of systematics and taxonomy. Project completion times and two-year citation rates in these various subfields determine pay bonuses for researchers in those areas, not the importance of the work to Mexican society. Scientists and ministerial knowledge users and funders of science conveyed to me that these low-citation subfields are understudied in some of the most biologically and culturally diverse parts of Mexico, and they themselves cited SNI incentives as part of the problem.

Even if it were true that the current makeup of the Biology and Chemistry committee is not problematic for *any* area within ecology, which I cannot believe, the strong ecological representation would necessarily come at the cost of coverage of other scientific disciplines that the committee evaluates and thus introduce additional problems elsewhere. A single committee evaluates the applications several hundred candidates annually across all of the disciplines and subdisciplines of biology and chemistry. As I articulate in the original article, each committee's remit includes a wide range of disciplines, methodological approaches, and publication and citation cultures.

3. Claimed error 3

Williams and Morrone argue that my assertion that evaluation rules within a committee apply uniformly to all disciplines within its remit is incorrect. The SNI Area Committee II rules that were available as I drafted my manuscript (CONACyT, n.d.) specified the number of publications in JCR-listed journals and the number of citations from which types of sources researchers must achieve to attain each level of SNI—and this applied across all disciplines and subdisciplines within that committee's remit. As I write this response, the most up-to-date criteria ('Criterios Especificos AII', n.d.) does create room for more flexible application to the various disciplines within Area II and this is an improvement over the former requirements.

Given the new criteria document, it is possible that the committee has an appropriately nuanced understanding of publication patterns associated with the disciplines and subdisciplines they review. Countering that optimism, though, Willimas' and Morrone's narrative itself articulates that *there is a uniform minimum productivity standard of three Web of Science Journal Citation Reports-indexed publications in journals with a minimum IF of 0.5*, regardless of a researcher's discipline.

4. Claimed error 4

Williams and Morrone suggest that the evaluations are not points-based 'which is why numbers of publications or citations required for each level (category) within the system do not appear in the online guidelines for each area of the SNI'. As I noted above,

publication and citation counts were specified under recent guidelines; I agree that moving away from minimum publication and citation counts for the higher SNI levels is an improvement. Whether evaluations are based on points is unimportant to my critique. What matters is that it is built upon an expectation of publication in JCR products. And as Williams and Morrone themselves argue, minimum publication counts remain in place and impact factor remains central to evaluations.

5. Further critiques

After presenting these supposed errors, Williams and Morrone critique narratives conveyed by my interview participants and my analysis thereof. Just as was the case when they challenged my factual understanding, my critics inadvertently validate the concerns of my interview participants even as they seek to offer counterevidence. They identify and attempt to counter four main findings in my article; I will address each of these critiques in turn.

(1) To evaluate my assertion that SNI incentives discourage scientists from doing biological surveys and other first-order research in Mexico's more biodiverse regions because of the need for frequent publications in journals with minimum IFs, Williams and Morrone wrote to a high-level administrator within Mexico's National Council for Biodiversity (CONABIO). They report her response as follows: CONABIO 'delays public access to the results for a two year period to give researchers time to publish' (P. Koleff, as cited in Williams and Morrone). *This confirms rather than debunks my assertion that publication mandates interfere with public availability of relevant scientific knowledge.*

Williams and Morrone summarize the remainder of the administrator's comments in a way that is wholly compatible with my argument: 'she felt that SNI evaluations give little weight to teaching, books, popular science articles… and those evaluations s*hould include a broader vision of applied scientific research and include products that informed the public and decision-makers* (emphasis my own).' This is a reasonable summary of my argument rather than a refutation of it.

(2) SNI incentives, as Williams and Morrone articulate, emphasize publication in international outlets, in English. I reported hearing from some Mexican professors that they occasionally translated articles from English back to Spanish so that they and their peers could use them in classroom settings to teach students who are not sufficiently proficient in English to read technical scientific writing. Williams and Morrone assert that this would only be the case in low-quality programs, as graduate students in approved programs must meet English proficiency standards. I can assure them, ecologists reported to me, that they do this. Williams and Morrone go on to lament the low number of people employed in R&D and the low educational achievement of Mexican primary and secondary students. Resource availability and any number of other factors undoubtedly contribute to these poor outcomes, but current science policies—which signal to students that success in science means dedicating a substantial portion of their effort to publication in a foreign language in inaccessible journals on topics without local relevance—seem unlikely to contribute toward a solution. Science policies that systematically depriving Spanish-speakers of the results of the research they help to fund are unlikely also to inspire allocation of additional resources to science.

(3) In my original article, I wrote: 'In order to publish adequate numbers of articles in 'top-tier' journals, many researchers with whom I spoke reported that they were forced to collaborate with one another and with scientists in wealthier countries' in order to gain access to relevant journals, equipment, and linguistic that otherwise serve as barriers to publication in those outlets. I wrote that while this is seemingly beneficial, my participants

> reported that they end up trading co-authorship with one another when in fact there was little true collaboration. And they reported deliberately committing 'salami science'… splitting what should be one manuscript into several that are then submitted to different journals. … Further, several of my participants reported surrendering their own research agendas to those of their better funded collaborators'

in order to achieve publication in top-tier journals.

Williams and Morrone respond to the content of this section (though I cannot imagine they meant to condone all aspects of the above) with 'This is hardly unusual and applies to scientists across the world. … There is a clear correlation between international collaboration and publication impact. … Mexico has done well in this respect, with 46 per cent of Mexican-authored papers in JCR-indexed journals published in collaboration with scientists from other countries.'

Their response again equates journal publication 'impact' (read: IF and citation counts) and societal benefit. The increase in international collaboration is one of the metrics of success common in dominant scientific culture. If that has come at the cost of research on Mexican knowledge needs, that suggests a failure. Exactly how these international collaborations play out when they involve asymmetric access to necessary resources is a subject that merits future research. Simply pointing out that Mexican scientists increasingly collaborate internationally does not undermine my argument.

(4) Williams and Morrone write:

> One of the complaints cited by Neff seems particularly farfetched, such as ecologists that bemoan their requirement for a 3-5-year period of minimal productivity in order to establish new research sites in culturally and biologically diverse regions of the country. It is difficult to envisage a modern country in which scientists can avoiding productivity for such an extended period.

Suggesting that a problem is universal, however, does not undermine the claim that it is a problem.

6. Concluding thoughts

My interest in the SNI system stems from two motivations. First, because it is a uniform national system, it serves as a natural experiment in which it is possible to systematically investigate impacts of researcher evaluation schemes on the content of science. Mexico's SNI system is similar to many others in basing researcher evaluation on publication in and citation from a particular set of scientific journals even when the stated goals of science funding programs and academic institutions are societal benefit. Such systems can lead to success by the metrics, but failure by the stated goals (Bozeman and Sarewitz 2005; McNie et al. 2016). In the USA, we do not have a uniform nation-wide researcher evaluation scheme; rather, each institution (and often department or college) develops its own evaluation approaches. Though many prestigious institutions around the world utilize metrics similar to those that I critique in my SNI study, the variability between and even within institutions in many countries makes a study like this much more difficult.

My second motivation for this project was an experience at an ecological science conference at which I was helping to run a symposium on 'usable science'. We were reporting the results of a

five-year long US National Science Foundation-funded project exploring ways to reconcile the supply of and demand for scientific understanding. Science, we found, is most likely to be utilized when scientists are attuned to and respond to the needs of potential users throughout the scientific process, from project conception and design through to analysis and dissemination of the results (SPARC 2010). This necessitates slow processes of relationship-building, and participating scientists must be sufficiently flexible to surrender some control over research problem choice and design; projects that are co-created or co-produced by scientists and knowledge users are more likely to yield usable and utilized results.

A Mexican ecologist in the audience where we presented these ideas spoke up to say that this approach is a common goal among Mexican researchers, but that publication expectations make it nearly impossible for him and his colleagues to pursue. A second—and well-respected—researcher from the region stood up and told this ecologist that his concerns were not valid because good research is published in good journals. By definition.

I firmly believe that science can and should serve to advance societal goals. This anecdote cemented in my mind that we—the scientific and research community—are responsible for some of the most substantial barriers to that aspiration. We, after all, define for one another what counts as prestige and success within our respective fields. We thus are in positions of power to change these systems, if we are willing to use our analytical skills to think critically about ourselves.

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