



**Michigan
Technological
University**

Michigan Technological University
Digital Commons @ Michigan Tech

Michigan Tech Publications

7-2022

Towards codes of practice for navigating the academic peer review process

Benjamin K. Sovacool
Aarhus Universitet

Jonn Axsen
Hong Kong University of Science and Technology

Laurence L. Delina
Simon Fraser University

Hilary Schaffer Boudet
Oregon State University

Varun Rai
The University of Texas at Austin

See next page for additional authors

Follow this and additional works at: <https://digitalcommons.mtu.edu/michigantech-p>



Part of the [Social and Behavioral Sciences Commons](#)

Recommended Citation

Sovacool, B., Axsen, J., Delina, L., Boudet, H., Rai, V., Sidortsov, R., Churchill, S., Jenkins, K., & Galvin, R. (2022). Towards codes of practice for navigating the academic peer review process. *Energy Research and Social Science*, 89. <http://doi.org/10.1016/j.erss.2022.102675>
Retrieved from: <https://digitalcommons.mtu.edu/michigantech-p/16217>

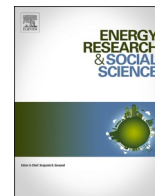
Follow this and additional works at: <https://digitalcommons.mtu.edu/michigantech-p>



Part of the [Social and Behavioral Sciences Commons](#)

Authors

Benjamin K. Sovacool, Jonn Axsen, Laurence L. Delina, Hilary Schaffer Boudet, Varun Rai, Roman Sidortsov, Sefa Awaworyi Churchill, Kirsten E.H. Jenkins, and Ray Galvin



Perspective

Towards codes of practice for navigating the academic peer review process

Benjamin K. Sovacool^{a,b,c,*}, Jonn Axsen^d, Laurence L. Delina^h, Hilary Schaffer Boudet^e,
Varun Rai^f, Roman Sidortsov^{b,g}, Sefa Awaworyi Churchillⁱ, Kirsten E.H. Jenkins^j, Ray Galvin^k

^a Center for Energy Technologies, Department of Business Development and Technology, Aarhus University, Denmark

^b Science Policy Research Unit (SPRU), University of Sussex Business School, United Kingdom

^c Department of Earth and Environment, Boston University, United States

^d Simon Fraser University, Canada

^e Oregon State University, United States

^f University of Texas-Austin, United States

^g Michigan Technological University, United States

^h The Hong Kong University of Science and Technology, Hong Kong

ⁱ RMIT, Australia

^j University of Edinburgh, United Kingdom

^k Cambridge University, United Kingdom



ARTICLE INFO

Keywords:

Editors

Authors

Peer reviewers

Codes of conduct

Misconduct

Academic practice

ABSTRACT

Peer review is the bedrock of modern academic research and its lasting contributions to science and society. And yet, reviewers can submit “poor” peer review reports, authors can blatantly ignore referee advice, and editors can contravene and undermine the peer review process itself. In this paper, we, the Editors of *Energy Research & Social Science (ER&SS)*, seek to establish peer review codes of practice for the general energy and social science research community. We include suggestions for three of the most important roles: peer reviewers or referees, editors, and authors. We base our 33 recommendations on a collective 60 years of editorial experience at *ER&SS*. Our hope is that such codes of practice can enable the academic community to navigate the peer review process more effectively, more meaningfully, and more efficiently.

1. Introduction

Peer review is the bedrock upon which modern academic research and its lasting contributions to science and society are founded. Specifically, peer review operates as the predominant process for assessing the validity, quality, and originality of scientific articles for publication. Wiley, a global publishing firm, reports that 84% of researchers believe that without peer review there would be no control in scientific communication, and 90% feel that peer review improves the quality of their published papers [1]. As Riley (p. 629) concluded, peer review “is a crucial component to publishing and the progression of science.” [2] Peer review also serves as an independent evaluation of research so that integrity is maintained and poor-quality contributions are filtered out of the publication process. In many ways, science advances one paper at a time, and each paper, in turn, is the *collective* outcome of a complex, often intense and protracted peer-review process that involves many actors, including authors, reviewers, editors, and even the readership

audience. Science would not be science without peer-review [3].

The peer review and publication process is especially important in light of the very high and consistently growing numbers of submitted and processed manuscripts. For instance, when looking only at a search of research on “climate change”, the volume of publications and evidence in need of synthesis has grown from a mere 1452 articles in 1992 to more than 87,000 articles in 2020 [4]. And that is only one sub-field or area of focus. Consider the case of Elsevier, the world’s largest academic publisher by volume. In 2013–14, when we launched *Energy Research & Social Science (ER&SS)*, Elsevier received about 1.3 million submissions a year. Of these submissions, 365,000 were accepted. This means that 72% of academic articles submitted to a peer-reviewed journal within Elsevier’s system were rejected that year. This represents just a small fraction of the 69 million articles already available on Scopus at that time, and which were downloaded 700 million times annually by 11 million researchers across 120 countries [5].

Given the importance of peer review amidst this high volume, it is

* Corresponding author at: Science Policy Research Unit (SPRU), University of Sussex, Jubilee Building, Room 367, Falmer, East Sussex BN1 9SL, United Kingdom.
E-mail address: B.Sovacool@sussex.ac.uk (B.K. Sovacool).

crucial to understand what factors lead to publication versus rejection. Rejection may occur for myriad reasons, including poor research design, unclear article or argument structure, limited engagement with the target journal and/or relevant theory, and lack of originality (see Fig. 1). In fact, most papers submitted for peer review are unlikely to even be put to peer review: editors must decide which of those studies are most likely to enter the process to advance science. As Fig. 1 indicates, for each submission the editor needs to identify if quality of the paper is sufficient for further consideration, including scientific quality or rigor, originality of analysis, adequate research design, and conclusions that are supported by the presented analysis. Unfortunately, for the typical international journal, the “unready” submissions vastly outweigh those that are ready to even begin peer review.

Conversely, solid articles that excel in these areas are typically much easier for editors (and reviewers) to spot. Annette Lareau captured some of these themes in a recent guide to research methodology noting that strong research tends to require at least *nine* different ingredients [7]: a clear contribution to new knowledge, succinct assessment of previous literature that shows the holes in the literature, a research question that can be answered with the data in hand, breadth and depth in the data collection, clear exposition of the results, deep analysis that links the evidence to the interpretation, acknowledgement of disconfirming evidence, a discussion that uses the case as a springboard to reflect on more general concerns, and discussion of implications for ideas and practices. Clearly, this is a tall order—both for authors to construct, and for editors and reviewers to evaluate.

Throughout this process, publishing can generate emotional and physical fatigue. The experience can be likened to a challenging quest that requires persistence and diligence on both sides of the editorial desk. It is true that one of the joys of being a researcher is the opportunity for independent thought and meaningful contribution to the frontiers of science. However, the mechanics of the process for doing so can be arduous and confusing and can require a lead author and authoring team to play many roles and develop many different skillsets. It is not nearly sufficient for the author to be an expert in their topic or method. A contemporary researcher (or at least their team) must have the skills of a historian (diving into literature), creative scientist (asking and systematically addressing a novel question), project manager (steering collaborations), creative artist (skilled and dedicated writer and creator of visual aids), detail-oriented data entry clerk (for the submission system), diplomat (when dealing with collaborators, editors, and peer reviews), and promotor (to disseminate results from a successful publication). Even after acting as promotor, authors must also perform for relevant audiences to attract their interest, to see research findings translated into impact, all while also handling rebuttals or critical questions that often ultimately span their careers.

The actual submission of the manuscript can be the most opaque step of all. Once they have a coherent product, lead authors must identify the “right” target journal, learn (or re-learn) the many editorial platforms, which keep evolving, and sometimes recommend a slew of potential arms-length reviewers. (Not to mention the often-excruciating step of remembering one’s login and password information.) Once in the system, the product must pass editorial screens, while authors are left hoping to avoid the “desk reject” (while also recognizing that a quick death is often merciful compared to rejection after a long peer review stage). If their manuscript makes it to review, authors must then brace themselves for negative feedback, working to manage their own emotions and often to “battle” peer reviewers, especially that really annoying and negative reviewer who “just doesn’t get it” and “moves the goalposts,” sometimes over third, fourth, and fifth rounds of revision.¹

To shed more light on how each key actor (author, reviewer, editor) could better play their part in the peer review process, this Perspective

¹ As an example of commiseration in this area, see the Facebook group: “Reviewer 2 must be stopped!”

draws from our collective 60 years of experience as Editors at *ER&SS*. We propose three sets of codes of practice that can enable editors, reviewers, and authors to navigate the peer-review process more effectively, meaningfully, collegially, and efficiently. This collective experience is a microcosm and we think a fairly diverse representation of the larger scientific community, where scholars rotate to the different “sides” of this publishing “battle” by virtue of being responsible and respected members of this community.

Although not a focus of this article in any way, there are many shortcomings and criticisms of the peer review process, such as personal bias, power imbalance, variations in quality, and timeliness, to name a few. There are also many criticisms that such flaws lead to weak research that ends up being published, i.e., papers that lack novelty, have poor rigor, and/or bad quality of writing. Some research may even deserve to be labelled “scholarly bullshit” [12]. While these issues are salient, we avoid engaging centrally with them here, for reasons of length, and to focus on the practical aspects of the publishing process. We are acutely aware of the vagaries of the peer-review process and that outputs can be of poor quality [8]. Peer review is far from perfect—inevitably some low-quality work will sneak through the system, and some high-quality work will be rejected. This awareness is, in part, a motivation for this Perspective. We believe that the integrity of the process can be further improved through the following codes we spell out for reviewers, editors, and authors.

We divide our recommendations into three sections. We first address peer reviewers with 12 suggestions for a code of practice, then present 11 for editors, and then 10 for authors. We begin with the consideration that published journal articles should score highly in terms of novelty, rigor, and style [9]. By novelty, we mean significant advances in theory or conceptual frameworks, empirical applications, or methodological techniques. By rigor, we mean the quality of being meticulous in executing robust, appropriate, and valid research designs. And by style, we mean taking care to write well and make a case compellingly. We argue that peer reviewers, editors, and authors should all consider these three elements.

2. Twelve codes of practice for peer reviewers

Many peer reviewers conduct their evaluations having little to no guidelines on what approach they should take—what exactly to evaluate, and in how much detail. Some reviewers only have the experience of being a reviewed author, and the vast majority do not have experience as an editor. Is it any wonder that reviewer reports can vary so widely in scope, quality, and tone? To help improve the process, we propose the following 12 codes of practice for referees.

2.1. Assess novelty, rigor, and style

Many reviewer reports focus on only one aspect of the manuscript, such as its fit for the journal or its research design. As previewed in the *Introduction*, a broader, more helpful way for assessing the quality of the manuscript is to include *all* three elements of novelty, rigor, and style.² Sometimes, a reviewer will focus exclusively on the method or just the quality of writing. Because all three elements are needed to make a high-quality manuscript, editors appreciate feedback from reviewers on all these aspects. To be fair, there are some exclusions to the “review on all three dimensions;” if a reviewer judges that the paper is so weak in one of the dimensions to be sufficient to reject. In particular, a paper may be

² To reiterate, novelty refers to the uniqueness and significance of the work’s contribution to the overall field (this includes replication studies for high-stakes research outcomes). Rigor refers to the validity of results based on the robustness of the methods used and analysis conducted. Style refers to the quality of writing as well as the structure and presentation of data (including images, graphs, and charts).

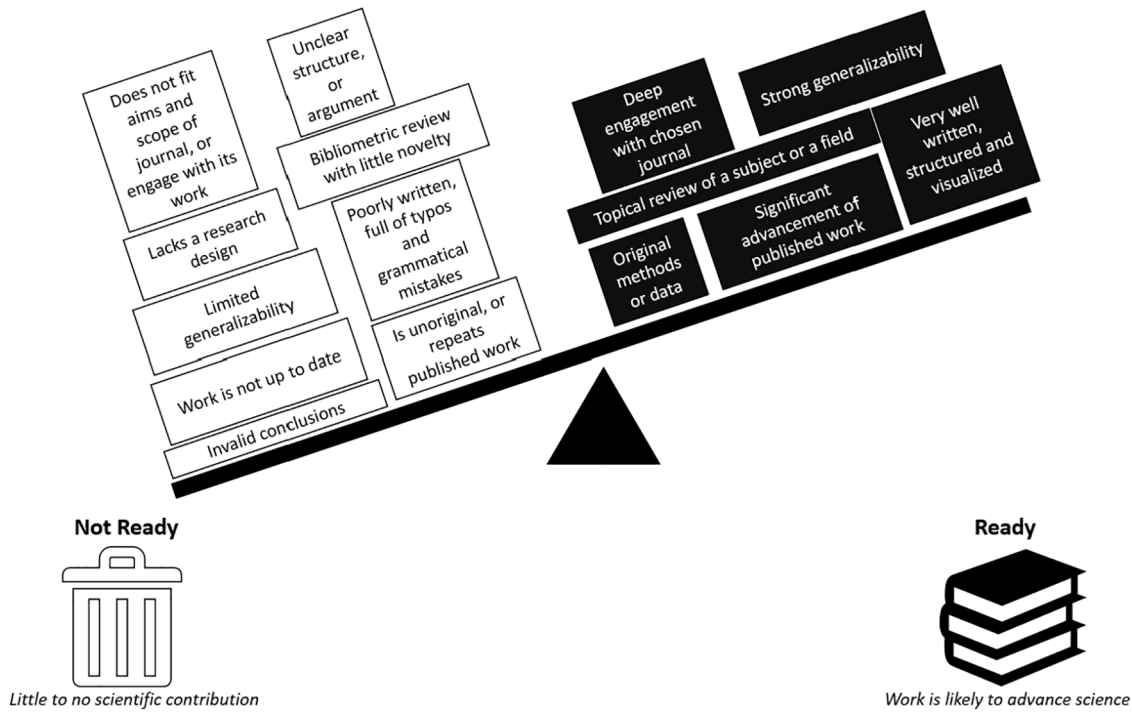


Fig. 1. Determining when authors are ready to submit for peer review and publish. Source: Authors, inspired and modified from [6].

so badly written that it is not possible to assess novelty or rigor. In such cases, reviewers don't necessarily need to focus on the other elements.

2.2. Judge a paper according to its own objectives

Reviewers should evaluate a paper for what it is, within the research objectives, conceptual framework, and research design that the authors set out. This may sound obvious, but many reviewers review the paper an author *didn't* write, rather than the one they did. They should judge a paper according to its own stated objectives, and also consider whether those objectives are novel (or worthy of replicating). This should evaluate the study according to the rigor of its method (using common standards where available), while also considering whether that method is appropriate to the research question being addressed. Further, reviewers should evaluate whether the analysis performed in the paper does the job of actually answering the question asked (while also considering whether that question was actually worth asking). Core questions to consider are as simple as: Does the question address a real gap in the literature and is answering it a genuinely useful contribution? Is the research design and scope tied to and built on the current state of knowledge in the area? Does the analysis follow ongoing norms and standards of analysis within that category of method, be it qualitative, conceptual, statistical, mixed-method, or otherwise? Make these evaluations clear and accessible, because editors may not be as knowledgeable as reviewers about a specific topic, method, or sub-discipline. In short, a review is more useful if it focuses on evaluation of what is actually done, rather than the Reviewer focusing on "here is what I would have done".

2.3. Embrace interdisciplinarity and avoid disciplinary dogma

The spirit of our broad discipline, and our journal, is one of interdisciplinarity. Yet some academics and, thus, peer reviewers, are overly dogmatic, largely owing to the dominant theories and methodological approaches within the disciplines where they received most or perhaps all of their academic training. This is one of the reasons why, for example, most neoclassical economists cannot judge the work, from a peer-review perspective, of conventional sociologists; and vice-versa. Because energy social science research is an inherently interdisciplinary space, reviewers must bring an openness, awareness, and tolerance to the review of work that includes other disciplines. A reviewer with a background in neoclassical economics should not evaluate interdisciplinary or non-economics papers according to the standards or best practices of economics. Further, it can be tremendously helpful to authors and editors if the reviewer acknowledges their disciplinary loyalties and biases, and more importantly, have the self-awareness to recognize and communicate the shortcomings of their evaluation. If the reviewer is unable to appreciate interdisciplinarity in this way, then they should not agree to review papers outside of the core academic discipline that they identify with.

2.4. Avoid territoriality

A separate problem is often seen with reviewers that see themselves as the "top dog" for a given topic or method. If combined with hubris, such reviewers insist that their own publications are the leading sources of information in that literature, and either dismiss the reviewed paper as lacking novelty or importance relative to their own work, or require

that the author add in multiple citations to their work. In doing so, such reviewers often shamelessly give away their anonymity as well. Instead, reviewers should be able to bring a humbleness to their review, even for work that sits squarely within their research area. They should not allow their closeness and vested interest to cloud their judgement, nor should their review be self-serving by either boosting their own citation count or eliminating a competitor. Rather, in the real world of energy social science, we should celebrate and support alternative perspectives, the addition of research attention that can expand our collective understanding, and ultimately the further expansions of any given literature.

2.5. Don't focus on the ability of the manuscript to "surprise" you

Quite often, reviewers will be too focused on their own small piece of a research area, and evaluate a paper based on the apparent immediate value (or lack thereof) that such a paper provides for themselves. Such reviewers often see a good paper as one that "surprises" them. However, relying on whether a manuscript "surprises" a referee is a poor and inconsistent metric that is impossible for authors (and even editors) to predict. Instead, the question isn't necessarily "did the study surprise a reviewer?" Rather, editors want to know: "Is this study novel, rigorous, and of suitable style? Or more simply, does the study contribute broadly to science?" And in the broad, applied, and interdisciplinary space of energy social science, this should include the consideration of whether the paper will educate a broad pool of readers about something new? Focusing on these components will help to minimize subjectivity in reviews, prompting referees to view the paper through the lens of whether a general reader would find the study valuable, in addition to whether the reviewer themselves did.

2.6. Aim for short and quicker reviews

Reviewers need only a half day or less to evaluate some papers, or perhaps up to a full day if the reviewer takes the time to further examine a provided data set, data code, qualitative interview coding files, or even a particular literature in more depth. Generally, most reviewers only need to provide one or two pages of comments, but the report can be shorter, say just a couple of paragraphs, provided it captures the key structural elements of what should be in a review (see Sections 2.1 and 2.2). A few reviewers have provided a beautiful set of 5–10 pages or even 20-page reports, but such a mix of quantity and quality are rare and truly extraordinary! It is not expected that reviewers will check citations or

directly edit the text, although they can if they wish. Normally, there is a requirement for the author to conduct a thorough proofreading of the paper once the peer-review process is headed towards its acceptance. Appendix I has a sample of actual reviews from earlier articles to give a sense of the appropriate length and tone of reviewer comments at Energy Research & Social Science.

2.7. Use your judgement, based on the science

Appendix I provides a very limited sample of reviews at ER&SS. These should demonstrate that there are judgements involved throughout the peer review process: by authors, by reviewers, by editors, and ultimately by the readers and users of the research. That shouldn't be a surprise to any serious participant in the scientific process. The real art of the science, though, is to base those judgements upon the current state of the science in the relevant field. That's the crux of the whole peer-review process: to evaluate and enable evolution of the scientific frontier based upon frontier scientific knowledge.

2.8. Respect editorial screens about fit for journal

If an article is sent to you for peer review, try to avoid questioning whether the paper topic is a fit for the journal. If an editor sent it to you, and it passed their screen, this is already a signal it fits the aims and scope of the journal. The editor wouldn't have sent it to you otherwise.

2.9. Be collegiate and constructive with your language

Avoid offensive language and do not attack authors. Evaluate the work, not the person. Stick to the facts. It's important to realize that some authors are early career researchers or even graduate (or sometimes undergraduate) students; and that everyone is human, including scientists. This may be their first article, or a part of the mentoring process for their Ph.D. (Reflect upon your own evolution through the scientific process.) Reviewers should avoid statements that may cause needless offence. Reviewers should provide explicit and clear explanation of what needs to be done to make the paper read better towards the critical objective of systematically building up science. They should express their suggestions not by simply stating what is incorrect or illogical, but rather in terms of how it can be improved towards a more scientifically-based knowledge base. Doing so increases the validity of a review, especially of an interdisciplinary piece.

Table 1
Harnessing more careful and descriptive language in peer review comments.

Comments with poor language	Example	Comments with active and descriptive language	Example
Merely echo the decision	<i>The innovative aspects of the paper are poor.</i>	Explain the decision	<i>This paper is not convincingly innovative in X and it does not properly take [xxx] into account.</i>
Are unclear or ambiguous	<i>The research methods for this paper are inappropriate.</i>	Are clear	<i>The research design for the paper is overambitious, given the complexity of the activity proposed and the nature of the research question.</i>
Are vague and subject to interpretation	<i>I think the conclusion is probably inadequate.</i>	Are precise	<i>The Conclusion is inadequate. It does not include a clear description of findings; it also lacks a future research questions or agenda subsection.</i>
Are inaccurate and provide an opening for complaints	<i>There is no discussion of key findings. The authoring team is not experienced.</i>	Are authoritative and close the question	<i>The paper fails to advance key findings at the appropriate level of detail, nor does the paper embed its findings in existing research gaps identified via a literature review. The authors do not demonstrate in the paper an adequate level of knowledge about the state of the field or the generalizability of their findings.</i>
Include words such as ...	<i>... perhaps, think, seems, assume, maybe, probably.</i>	Include words such as ...	<i>... because, specifically, for example, definitively.</i>

Source: Authors, substantially modified from [10].

That said, be direct—we strongly encourage reviewers to plainly state their position regarding a manuscript. Table 1 attempts to differentiate active and “good” comments versus passive and “poor” comments. We come across many wonderful reviews every day: supportive, constructive, and helpful. These kinds of reviews help authors to transform their promising, if flawed, drafts into strong manuscripts. And for truly flawed research designs to reconsider new, more appropriate designs. Overly critical remarks that provide little substantive feedback reflect poorly only on the reviewer and often persuade the editor to give the entire review lower weight than it might deserve.

2.10. Commit to reviewing all revisions

If you agree to review the first version, there is an expectation that you will also review the first revision, and second revision, and even third or fourth (!) revisions. This remains true even if you suggested “reject” the first time around. When you agree to review, you agree to the full process. Understand that the editor often needs to have at least two “reject” evaluations to reject a paper (if there is a split decision). Otherwise, the paper typically stays in the running in the review process. If the original “rejecting” reviewer refuses to review this first revision, then they lose their vote for “reject”, and as result poor quality papers could pass through the peer review process because the more critical reviewers refuse to reevaluate a paper.

We have found most reviewers don’t realize the importance of sticking with a manuscript. While it can be annoying to have to return to a paper, keep in mind that subsequent reviews require far less time than the first rounds. Most journals request that authors provide detailed responses to reviewers’ comments and highlight all revisions or submit a tracked-changes version. If you feel that you have to go hunting for the revised text, please let your editor know. As editors, we have sent half-marked up manuscripts back to their authors and we expect your handling editor to do the same. Ultimately, peer review is a process based on mutual respect for each other, but more importantly for science. Editors value the work and time that reviewers put into the process and are generally happy to make the process as easy as it can be for reviewers. In return, we ask reviewers to stick with handling editors until the final decision is made.

2.11. Manage a fair reviewer workload

There are two sides to this item. On the one hand, do not accept too many review invitations. Set a “reviewing budget” or quota, and stick to it. That said, do offer to review for journals, especially those you submit to, especially in proportion to the amount of manuscript submissions you make. See more on this point in Section 4.9.

2.12. Assiduously meet deadlines

Do your best to meet the journal deadlines, realizing that some journals expect reports back in a matter of weeks rather than months. Meeting these deadlines can be supported by the other items suggested here, i.e., keeping reviews as succinct as possible and sticking to your reviewer budget. If you cannot meet deadlines, let the editors know as far in advance as possible. We implore you: do not just let deadlines expire. This is especially important around holidays, as missed deadlines often lead to editorial systems automatically uninviting reviewers and slowing the entire process to a halt. (We also discuss deadlines and elements of time management in Sections 3.1, 4.1 and 4.9.)

Granted, as the last couple years have taught us, “stuff happens”, and life poses unseen challenges in ways that derail our professional

routines. When push comes to shove, reviewers often drop reviews from their plate first. While not ideal, this is at times unavoidable and a reality in the practice of peer review. However, reviewers have the responsibility to proactively let editors know as early as possible that their plan has derailed, and to contribute to a solution, e.g., mutually agreeing upon an extended deadline, or helping to identify new reviewer(s) who are willing to review with a short turnaround time. This is a very important point. Flouting this principle is one of the main reasons for delay in the peer-review process. There’s a flipside for editors, too. They shouldn’t wait for too long beyond the due date, but should rather touch base with reviewers at the earliest signals of delay.

3. Eleven codes of practice for editors

Peer reviewers are not the only actors who need to follow codes of practice. In addition to recognizing novelty, rigor, and style, here we suggest the following 11 items for editors. Our target audience includes potential future editors of this journal, but also those in the field more broadly. Of the three roles in the peer review process, the role of editor is least likely to be familiar to a given researcher. We believe that this section will help to round out authors’ and reviewers’ understanding of the peer review process.

3.1. Promptly process articles

Just like reviewers and authors, editors are expected to promptly process articles, or let authors know why they cannot (if they are on leave or are having trouble finding peer reviewers). At *ER&SS*, we generally expect an editor to desk screen an article within one to two weeks of receiving it, and then either desk reject it or send it to peer review shortly afterwards. We expect our editors to send an article for review within one week of it being resubmitted.

3.2. Provide direction, don’t just cut and paste reviews

Some editors take on passive roles of merely tabulating reviewer scores and sending them along to authors. Authors may then be confused about what next steps are expected, or how to address a split decision (some mixture of “reject”, “accept”, “minor revisions”, and “major revisions”). Instead, we encourage editors to be more active and helpful by evaluating the reviews as well as the reviewers. Editorial decisions are not necessarily a matter of counting votes or numerical rank assessments, and editors do not have to always follow a majority recommendation. Editors should evaluate the strength of the arguments raised by each reviewer and by the authors, and may also consider, for example, other information not available to either party, such as potential biases within the field, disciplinary training of the reviewers, the quality of other reviewer reports, or even the nature of the expertise of the reviewers.

The primary responsibility of editors is to the scientific community at large, not to a particular author or reviewer. We therefore encourage editors to actively interpret and communicate to authors about the reviewer reports, by giving them an editorial “steer” towards how they themselves evaluated the reports and offering critical advice. This can even include tips on which reviewer concerns the editor found most important, as well as those with which the editor might disagree. This takes time, as it requires editors to engage with each article they handle, but the peer review process improves because of this engagement and has substantial potential to *save time* later on by helping steer the authors down particularly fruitful pathways more likely to succeed.

3.3. Identify and seek out any missing perspectives

While it is challenging to balance out conflicting reviewer reports, it can be even more difficult to identify when a given reviewer perspective is missing altogether. This could be a lack of insight specific to the topic or utilized method, or the need to seek out a second opinion on one referee's opinion that might be biased or overly strong in one direction. Editors should identify what perspectives are missing from the reviews and seek them out as needed.

3.4. Recognize that controversial papers might be the most innovative

Sometimes reviewers will never come to agree on a paper—where consensus proves impossible. Because editing is more than vote counting, editors should not be afraid to override harsh and unconstructive reports of good or promising papers. The Nobel Laureate, Elinor Ostrom, once told the lead author (BKS) that “the more impactful or significant a paper is, the more *likely* it is to get rejected.” She spoke about one of her most significant, highly-cited papers being rejected *five times* before it was accepted the sixth time. We can speak from experience that many of the most highly cited papers in *ER&SS* had referees suggesting “reject”, which the editors had to override.

It takes a strong understanding of the field balanced with good foresight and a determination to encourage potentially disruptive piece of work (editorial risk taking) for an editor to recognize an innovative article that goes against the grain of convention and to advocate for it. It is not uncommon to see such work produced by interdisciplinary scholars who do not have the constraints and directives of disciplinary departments. Editing these pieces requires creativity in reviewer selection and assessment of their comments. Editors should actively consult with other editors, especially those who do not come from the same discipline, when they see a promising, yet seemingly untraditional, article. (That said, more often than not, a review of “reject” should be seen by the editor as a red flag that the manuscript is of low quality, rather than being an indicator of innovativeness.)

3.5. Be responsive

Proactively communicate with authors and reviewers (to the extent possible). This may be difficult for very high-volume journals, where editors handle hundreds to thousands of submissions a year. But for most editors at *ER&SS*, it's entirely possible for editors to keep in contact with the authors of the papers they manage (which usually range from 3 to 10 submissions or resubmissions a week). This includes an automated email from Elsevier notifying authors when their piece is assigned to a particular Editor or Associate Editor, but also more personalized updates from editors letting authors know their piece is going out to peer review or introducing themselves to authors (which some of our Associate Editors do after a submission passes the editorial screen). The same is true for communications between editors and reviewers; editors should always interact with reviewers in an understanding, accommodating, and respectful manner. Timeliness and quality of feedback to authors are the ultimate goals. By default, editors and reviewers should aim for and assume mutual respect around this goal, establishing this as a norm.

3.6. Respect the peer review process and integrity of reviewer reports

In our collective experience as authors, rather than editors, we have encountered situations where an editor has rejected an article that has attracted favorable peer review. That is, a situation where the

manuscript was submitted, passed editorial screen, went to peer review, was determined to be publishable after revisions, was revised, was peer-reviewed a second (or third) time to confirm it improved, was accepted by all referees, only to *then* be rejected by an editor. The reasons for such a stark conclusion are manifold, and can include the piece being assigned to a new editor (or that the editor handling the first submission went on leave, or worse, passed away); the editor identifying mistakes that the peer reviewers missed or that they didn't see before; or just not “liking” the manuscript, or some other form of possible bias. Although we maintain that editors should make their own judgement and not be absolutely beholden to peer reviewers, the editorial practice of rejecting or dismissing a consensus of favorable reviews should be discouraged. Such editorial practice circumvents the peer review process, wastes the time of authors and reviewers, and can be viewed as capricious. We argue against it in the vast majority of cases—and any exceptions should be made with careful consultation with other editors.

3.7. Avoid mixing reviewer and editorial responsibilities

Editors should help steer the peer-review process but should avoid becoming reviewers themselves for an article they are handling. Doing so mixes the roles and responsibilities of an editor, given their role is to arbitrate among peer reviewers and act as intermediary. Moreover, editors should also avoid becoming *authors*. We encourage editors to avoid over-editing papers; instead, focus on high-level elements, such as novelty, rigor, and style. In particular, our editors tend to provide insight on higher level aspects of communication and accessibility, notably the wording of the title, abstract, and introductory sections. But not more than this.

3.8. Provide the full, unedited reviewer reports

As tempting as it may be, editors should not edit the language of referee reports. Unless they use extremely offensive language or hate speech (which is rare), reviewer reports should not be sanitized, though cases of inappropriate language should be flagged for potential action regarding the reviewer (see [Section 3.11](#) below for more detail). As already noted, we encourage editors to make their own editorial comments that help frame and interpret reviewer reports. Although problematic language and poor word choice often speak for themselves, it is important for an editor to insert themselves as a moderating and mediating figure and to protect the collegiality of the peer review process. Shifting the focus to substance is instrumental for a successful peer review and the creation of knowledge.

3.9. Recognize possible bias or conflict of interest

Appreciate when an expert disciplinary/method perspective is needed and recruit reviewers accordingly. But also recognize possible bias, including various types of conflicts of interest: a referee working for an institution being criticized by an article, having their work or character criticized by an article, or having a financial or professional stake in the findings of an article. Such possible biases could justify the possible exclusion of a reviewer report, or justify an appeal (noted next).

3.10. Carefully consider author appeals

Although uncommon, authors do try to appeal editorial decisions or challenge peer review comments. We note that authors should only appeal in extreme circumstances. When it does occur, editors should be

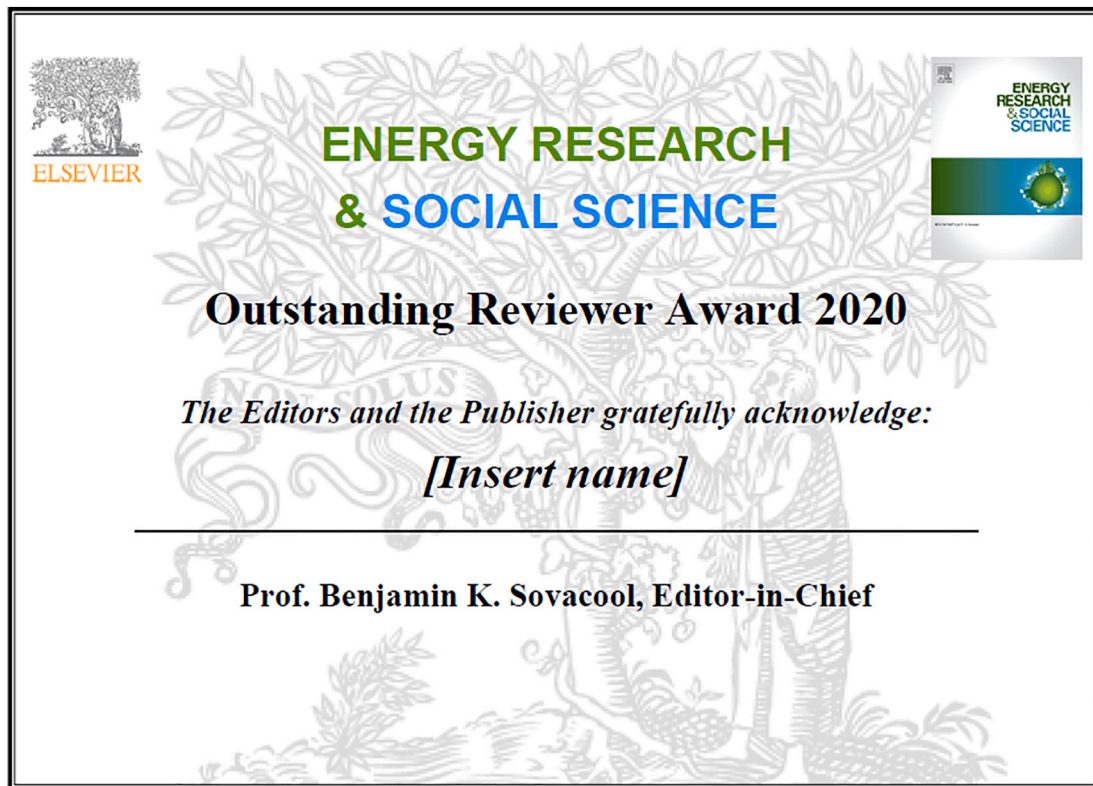


Fig. 2. An Energy Research & Social Science “Outstanding Reviewer Award” certificate for 2020. Source: Authors.

aware of any limitations in the initial review process, including possible missing or incomplete reviews, as well as potential bias among peer reviewers. Further, additional editors should be consulted in the appeal process.

3.11. Blacklist “bad” reviewers and recognize “good” ones

Although we do not have a formal list of “bad” and “good” reviewers, if we do find that a particular referee has exhibited bias, given a poor-quality review, or used offensive language, we tend not to invite them to review for the journal again. We also recognize a small list of “best” and “outstanding” reviewers every one to two years with a certificate (see Fig. 2). This serves as a way of acknowledging excellent reviewers and building morale and support for the journal. Of course, neither of the lists formally exist and so are not public or published, to protect the identity of our colleagues. Moreover, a case can be made that “bad” referees need training, mentoring, and perhaps encouragement about how to become “good” reviewers. Rather than being outright banned, editors could offer them constructive suggestions for improvement.

4. Ten codes of practice for authors

As noted, we believe the codes we identify for reviewers and editors should already be enlightening for authors seeking to understand the full

peer review process. In addition, this section offers ten codes of practice specific to authors.

4.1. Make life very easy for the reviewers and editor

Consider that while the author is hoping for publication, the reviewers and editors are largely volunteers who do not share in the glory of a final product. With this in mind, authors should do all they can to make their response very easy to read. Your cover letter should clearly and concisely detail what you have done—make it extremely easy for the editor to see that you have taken the revision seriously, so that they can pass it along to reviewers (or even accept as is). As noted further below, your responses to reviewers should be systematic, well-written, and easy to follow. At the first glance, the reviewer should be quickly reminded of their original comments (several months may have passed!), and they should quickly get a sense of what changes you have made. To accomplish such a revision document, authors need to set aside plenty of time to not only make revisions, but to also carefully write (and polish) their cover letter and reviewer response documents.

4.2. Don't put off revising, get started immediately

Delaying the process can lead to further delays, and often tends to hurt the quality of peer review. The longer the process takes, the more

likely are referees (and editors) to forget details of the paper or to become involved in other commitments. Delaying also signifies to editors that an article isn't a priority for the author, and, importantly, runs the risk that others can publish similar articles and erode the novelty of the piece. A series of long reviewer reports and line-item edits (say, more than 8000 words), or a high number of referees (say more than four), can seem daunting, and even insurmountable. But even these formidable obstacles can be overcome by breaking down reviewer suggestions into more discrete, solvable problems to be tackled. It may help to schedule one to two hours per day to revising a particular work that will see substantial progress made each week. Ultimately, this decision is a matter of each scholar's approach to work. But regardless of whether you are a "planner" or "crammer," accept that the revision process is not only labor intensive, but can also be emotionally draining. Getting started as soon as possible helps divide this mental and emotional toil over a greater number of days.

4.3. Be respectful to your referees, and editors

Be courteous in tone, even when you disagree, and realize that most of the time reviewers are trying to help you, even if they are being critical. Authors should avoid walking into the process assuming it's an argument and pushing back on everything. Save pushback, or rigidly standing your ground on something, for only what you see as essential points. In your response to reviewers, recognize the constructiveness of reviewer comments, saying things like "good comment" or "astute point" when they provide feedback that is particularly constructive or even creative. If you disagree, go on to say why you respectfully couldn't incorporate that particular suggestion. Elaborate upon your points, defend your responses with evidence, or elucidate how you responded to their criticisms earnestly or comprehensively. Remember our points about collegiality — be careful not to lose an ally just because you let emotions cloud what could otherwise be a clear, grounded response. Moreover, be respectful to the editors as well, even in the case of desk rejects.

4.4. Be systematic and complete in your response to reviewers

Write a short cover letter to the editor, explaining how you have addressed the suggestions, followed by an anonymous, detailed response to the reviewers' comments. This means being "systematic" (i.e., point-by-point) and "complete" or "exhaustive" (covering all points). Rather than providing a general summary of revisions, we require authors to generate a table or a list showing each and every comment and their response to it. If not done in a table, this can be done as a list of items, with the reviewer's original text in bold, and each follow-up response in plain text. This format will enable editors to quickly see what you've done and will make it easier for us to decide the next step for the manuscript: whether it will go back to the reviewer(s), be accepted for publication or, in some cases, rejected. If it does go back to the reviewers, such a detailed list is even more helpful. The review comments should be tackled one by one, systematically, and thoroughly. Also resist the impulse to combine your responses to similar comments across reviewers — better to repeat yourself a bit for each reviewer making the comment, if it efficiently shows the reviewer that you have addressed their specific comment.

We recommend following the tabular form of responses shown in [Appendix II](#). As you can see, these include a high-level summary of revisions *as well as* a point-by-point response to each and every comment, even grammatical ones. This actual reviewer response document was

used for the article by Sovacool and Walter [11]. As we have already emphasized, for a double-blind journal such as *ER&SS*, some editors may send an article back to an author if their responses are incomplete and difficult to locate in text, and/or if they reveal author identities to the reviewers in this document.

4.5. Focus on substance to win over hostile reviewers

Whenever you encounter a reviewer hostile to the article or who suggests "reject", calmly, courteously, and clearly address each comment, drawing focus to the substance of the work. Assume that they have read the entire manuscript and supporting documents (give them the benefit of the doubt) and help them to understand what they have missed or misunderstood. If they have misunderstood something, carefully review your work to see if it can be revised to be clearer. If your text is already crystal clear, then simply show that clarity to the Reviewer by pasting relevant sections of the text into your response. In doing so, you should remain considerate and collected. In short, use this as an opportunity to further improve the clarity of your text. Do your best to show (and persuade) the reviewer that your explanations are there already — nice and clear. If applicable, reference how other reviewers have complimented sections that this reviewer didn't understand.

4.6. Realize you may not be able to agree to every comment

This is far from a general rule, but we find most authors, most of the time, agree with 70%–95% of reviewer comments. That said, it may not be possible to agree to all comments, especially if reviewers themselves disagree with one another. It is acceptable to defend what the paper is trying to do at various points. The reviewers are meant to guide and critique you, but they should not become proxy co-authors, nor should their comments lead to fundamental changes in the paper unless fundamental errors were identified. Authors can also use a "future research directions" or "limitations" section to hedge some reviewer comments. Anything authors cannot do—that they do not have the space, resources, time, or skills to do, or may want to instead address in a future paper—can be put into this forward-looking section. This connects to the very first point addressed to reviewers above: to make it easier for reviewers to evaluate your paper for what it is and what it *does*, you need to make these two points as explicit as possible. Elaborating on the limitations of your research and explaining why you are addressing a particular slice of a larger problem can go a long way with reviewers and editors.

4.7. Do not use reviewer comments verbatim in your revised manuscript

It can be tempting to use reviewer suggestions for new arguments, sentences, analysis, and the like directly in your manuscript without changing them. We recommend that if authors do add text suggested by the reviewer, they should paraphrase or otherwise adapt the suggestion rather than directly copying any suggested text into the manuscript. The latter could be considered a form of plagiarism.

4.8. If you are given the chance to revise, take it

It is almost always easier to face even a large stack of negative reviewer comments, than to start afresh at a different journal. Thomas Edison once wrote that, "Genius is 1 percent inspiration and 99 percent perspiration." That is, invention, or innovation or research in the scientific arena, is not only having a good idea, but also working hard to

Table 2
Towards 33 codes of practice for reviewers, editors, and authors.

Peer reviewers	Editors	Authors
1. Assess novelty, rigor, and style	1. Promptly process articles	1. Make it easy for editors and reviewers
2. Judge a paper according to its own objectives	2. Provide direction, don't just cut and paste reviews	2. Don't put off revising, get started immediately
3. Embrace interdisciplinarity and avoid disciplinary dogma	3. Identify and seek out any missing perspectives	3. Be respectful to your referees, and editors
4. Avoid territoriality	4. Recognize that controversial papers might be the most innovative	4. Be systematic and complete in your response to reviewers
5. Don't focus on the ability of the manuscript to "surprise" you	5. Be responsive	5. Focus on substance to win over hostile reviewers
6. Aim for short and quicker reviews	6. Respect the peer review process and integrity of reviewer reports	6. Realize you may not be able to agree to every comment
7. Use your judgement, based on the science	7. Avoid mixing reviewer and editorial responsibilities	7. Do not use reviewer comments verbatim in your revised manuscript
8. Respect editorial screens about fit for journal	8. Provide the full, unedited reviewer reports	8. If you are given the chance to revise, take it
9. Be collegiate and constructive with your language	9. Recognize possible bias or conflict of interest	9. Offer to review for the journal
10. Commit to reviewing all revisions.	10. Carefully consider author appeals	10. Diligently meet deadlines
11. Manage a fair reviewer workload	11. Blacklist "bad" reviewers and recognize "good" ones	
12. Assiduously meet deadlines		

Source: Authors.

push it through the peer-review process. Publishing is hard work. And given the rejection rates discussed in the Introduction, we all frequently face rejection and an almost constant risk of failure. Perseverance and a thick skin are rarely discussed but critically important characteristics of successful academic researchers. Your handling editor sees and recognizes your efforts, and sometimes this is exactly what it takes to convince the editor that your position is correct, provided the effort is meaningful and genuine.

4.9. Offer to review for the journal

Volunteering to review for the journal is a welcome practice most editors like to see. It shows interest, helps distribute workload, and enables authors to become much more familiar with the journal. It will help authors to avoid typical framing, structuring, and style mistakes that we see far too often. Try to review for a journal commensurate to how frequently you submit to it, or even twice or three-times that (as most papers require a minimum of two reports). That is, if you submit an article to a journal such as *ER&SS*, consider that it needs two to four reports for an editor to make a decision, and offer to reciprocate as a reviewer of two to four articles to offset this.

4.10. Diligently meet deadlines

As with our suggestions for referees, do your absolute best to meet journal deadlines. Good time management is essential to being an academic. If you cannot meet deadlines, let the editors know as far in advance as possible, and ask for extensions only in emergency situations. Unless well justified with acceptable reasons (health issues, career change, institutional change, etc.) authors' delays can also signify to us their article isn't a priority for them. Conversely, authors who meet deadlines reduce editorial workloads substantially, improve publication speeds, enhance journal production planning and contribute to the timely advancement of energy research and policy. They also gain more time to disseminate their work and achieve impact.

5. Conclusion

The peer review process involves a synergistic interaction of authors, referees, and editors. Together, these actors form an ecosystem that

creates different selection pressures, with the aim of arriving at the best possible research the scientific community can advance. Table 2 offers a high-level summary of the 33 suggestions we have discussed here. To be clear, these are only our collective and iterative suggestions, not journal policy. Our suggestions may work well for energy, interdisciplinary fields, and social science, but surely will vary field-to-field, and journal-to-journal—they are not universal for all contexts and disciplines.

Admittedly, there are tensions between some of our principles. We tell peer reviewers to aim to be quick, but also to assess novelty, rigor, and style (three very different, and time-consuming, criteria). We tell editors to respect the peer review process, but to override outliers; to steer authors, but avoid mixing responsibilities. We tell authors that they can turn down some suggestions, but also to try to win over hostile reviewers. This shows there are no hard and fast rules that work in all cases, and instead a looser collection of norms and principles that constitute the art of the peer-review process, an art that varies by disciplines and by journals even within the same discipline.

Nevertheless, there are also some common codes of practice that transcend the roles and responsibilities of peer reviewers, editors, and authors. All three should treat each other with respect and avoid offensive and counterproductive language. All three should seek to practice sound time management and meet expectations about deadlines. All three should also try to persevere and have the fortitude not to give up—including reviewers that stand by their reject (or accept) until the final decision, editors that intervene with firm advice and strong steering (even if they override some reviewers), and authors who should take the chance to revise if given the opportunity.

Despite these tensions, complexities, and synergies, the entire academic community, and the science that it produces, have much to benefit from more balanced, timely peer-reviewer reports, proactive and efficient editors, and respectful, thoughtful authors. It is with this goal in mind that we have offered our collective thoughts towards codes of practice in producing scientific knowledge through the peer-review process.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix I. A sample of peer review reports

Example 1.

Ms. Ref. No.: [REDACTED]

Reviewers' comments:

Reviewer #1:

- * This paper is novel and timely, and is relevant to the Journal's scope.
- * The literature review is rather solid, and serves to present a very structured introduction to the subject matter.
- * Section 1.1 Last para: the link between personal change in consumption/behaviour mod. (e.g. smoking/weight) and energy consumption is a bit vague. In this reviewer's opinion: e.g. personal change in eating/smoking habits does not ultimately contribute to an obvious greater good for the community; this is in contrast to Earth Hour where energy savings -> climate awareness. Perhaps build upon this argument with e.g. lit review of campaigns on recycling, which is far more relevant to the topic at hand.
- * Section 2.1: the sources of the collected data need to be provided for the reader's reference/further evaluation. This reviewer understands that it is not entirely feasible to provide all sources in the References; perhaps an Appendix with the main URLs/descriptions/pers. comms. of the sources for the energy data?
- * Section 2.1: also, the "methodologies used to calculate percent electricity demand change..." needs to be detailed and backed up with examples, if feasible. e.g. "...compared observed demand to a normal or average demand experience" -> what constitutes "normal" or "average", is it e.g. 'weekday consumption over the past year', or over a 'sliding window' of 7 days, etc?
- * Section 4 Para 4: consider providing examples of differing methodology / reporting entity. e.g. for Australia, NEMMCO switched operations to AECO in the past couple of years - was there any impact to the obtained data, if any?
- * Section 4 Para 6: re: social media (e.g. Facebook/Twitter) and real-life engagement (e.g. media campaigns, ads, Earth Hour gatherings) for Earth Hour throughout the past few years. Consider elaborating if there were any changes (positive/negative/neutral), and what effects such engagement might have had on participation throughout the past few years.
- * Minor typo: Pg 1 Section 1 Para 2: "compilation" instead of "compliance".
- * Minor typo: Pg 3 Section 1.3 Para 1: "presence" instead of "presense"

Reviewer #2:

I don't think the paper matches the journal's mandate of providing a social science perspective, though it is on an interesting topic. The piece primarily seems to be about what earth hour has achieved, with no theoretical frame of the problem. It would be interesting if the author used a theoretical frame, and connected the results of the study with some type of social science concept. I am thinking something about symbolic or material public policies. Although this paper is about people and energy, I don't think it should be accepted on just that basis.

Moreover, I would make the title a bit longer, perhaps "The Electricity Impacts of Earth Hour: The Limits of Behavioral Change."

Additionally, the authors treat the 4% drop in energy consumption caused by Earth Hour as a big deal, but there are two issues with this. First, Earth Hour promotes a somewhat "negative" view of energy efficiency as doing without - as turning off lights or being in the dark, voluntarily, for an hour. This is closely in line with the classic conception of conservation, or sacrifice. This is a different notion of modern energy efficiency efforts, which tend to prioritize not sacrifice, but enhanced services at lower cost (better efficiency, or productivity). It is not doing without, but doing more with less. The authors should discuss these different paradigms of energy efficiency, and what they mean. Second, is 4 percent really that significant? Don't we need double digit drops in energy consumption to meet carbon and climate goals? The authors need to put this 4% number in context. Is that the most EH can achieve? Is it significant, and why? Draw out more the implications of this study for the wider audience of energy scholars and analysts.

Example 2.

Ms. Ref. No.: [REDACTED]

Reviewer #1:

This paper addresses some of the outcomes of an energy efficiency retrofit of 26 social housing units focusing on the resultant interactions between the new technologies and occupant behaviours. It is theoretically informed and within the scope of the journal. I found it a useful paper and can imagine citing it in the future.

There are a few minor issues which I think could be addressed to aid clarity.

The first stated aim of the research, in the introduction, is 'to identify through comparison of pre- and post-retrofit interviews, interactions between personal factors and contextual factors'. Before reading the rest of the paper I found it hard to know what was meant by this. I wonder if it would be good to add something about the impact of these interactions on energy use, as certainly this paper seems to be interested in that as the outcome, not just on the interactions themselves. I also thought that in the paper at least, it was mainly the technologies as the contextual factors that were the interest, not all / any contextual factors.

In the last sentence of the introduction, 'contribute to a greater understanding of behaviour-related barriers to retrofit energy saving effectiveness' I think this might be better framed rather more openly as not just barriers but also enablers or opportunities. The paper itself finds that in some cases the interaction resulted in behaviour changes that enabled energy saving; it is not just focused on finding barriers. Indeed improved understanding about where the best opportunities lie is very valuable.

Reviewer #2:

This paper investigates a significant topic, i.e. the interaction between housing energy refurbishment and occupant behaviors, which is relevant to this journal. It's also targeted at social housing, an important yet less studied sector. The semi-structured interview the authors adopted seems also interesting and has the potential to provide insights to such topic.

Reviewer #3:

General comments:

The article presents a case study of occupant perceptions of energy consumption behavior in 25 households 12 months after a thermal retrofit of their homes. These perceptions are apprehended through semi-structured interviews which are analyzed according to a schema the authors call 'template analysis'. The data obtained in the research appears to be very rich and to provide valuable insights into why occupants consume as they do, therefore a fair interpretation of it deserves to be published.

However the current version of the article falls short of the standard required for a scientifically credible account, for a number of reasons, and these need to be properly addressed and dealt with if it is to reach that standard.

1. The conceptual framework (i.e. the authors' understanding of the sociology, psychology and phenomenology of energy consumption) is not clearly developed or expressed. The authors rightly mention the rational choice theories, the norm-based approach, habit, social practice theory (SPT) and socio-technical systems theory as the dominant frameworks in extant literature, but then fail to establish a position of their own from which their data is analyzed. Their analysis therefore jumps around between different and at times contradictory standpoints. At times they seem to be favouring SPT, but only in the sense that they see superficial resemblances between SPT and some of their findings. This lack of clarity makes their results a smorgasbord of ideas that in places lack sufficient critical reflection, and they end up telling us what is already known in the literature.

2. There is a similar muddle in the article's methodological framework, i.e. its approach to conducting and analyzing semi-structured interviews. E.g. p. 7 para 3:

a. The authors seem confused as to whether or not it is acceptable to approach their data with pre-determined theories (as they admit they do in the next paragraph);

b. They seem to deny that quantitative studies (as well as qualitative) can capture people's social construction of reality (it could be argued that that is all they capture);

c. They seem unaware that the difference between qualitative and quantitative is NOT the same as the difference between socially constructed and physically existent.

d. They seem unaware that the difference between inductive and deductive is not the same as the difference between starting with and not starting with preconceived ideas.

The authors need to clarify their thinking on these points, and make a clear, credible case for choosing qualitative interviews as their research method. They do not need to apologise for using qualitative methods or a social constructivist ontology. A simple way forward might be to assert that the advantage of a qualitative methodology is that it enables us to find out all the different things that are going on, regardless of how dominant or non-dominant any particular one of these is, while the advantage of a social constructivist ontology is that it gets the story from the consumer's point of view - while the disadvantage of qualitative methods is that it cannot tell us how much of each phenomenon is present, and the disadvantage of constructivism is that it does not provide a check on how well the consumers are construing their reality.

3. The article seems to continually contradict itself as to whether the knowledge intervention (the handbook) made any significant difference, with statements both ways throughout, and a negative statement in the abstract. In any case, it would not be possible, to tell whether the difference was 'significant', as this was a qualitative study, not a quantitative study. Further, since the literature cited in the article already strongly suggests that this type of intervention makes little or no difference (i.e. the effects of such interventions are already known), we wonder why the study needs to include this intervention at all.

4. In many places the article is poorly expressed, and the meaning of some sentences is obscure (see detailed comments attached to the text). More generally, the argument of the article needs to be streamlined and clarified, as it tends to ramble and lose focus.

5. The conclusions need to be streamlined and focused, and not allowed to ramble as at present.

6. The article uses a difficult-to-grasp interpretive schema regarding technology and behaviour (sub-headings in Section 4), but in the Conclusions it refers to Midden et al.'s (2007) much more punchy schema, showing how its results accord with Midden et al. (2007). A better approach might have been to introduce Midden et al. earlier in the article, and use their schema as the interpretive framework for the results, showing where and to what extent this framework is adequate for such a study. This would make an important contribution to science, as it would build step-wise on already-existing knowledge.

7. A similar approach could be taken with SPT as the social-behavioural framework. Since most of the important findings do seem to accord with SPT theorizing, the article could use this as a parallel framework throughout (which it almost does de facto) and offer critical comments on SPT in the conclusions.

More detailed comments:

An annotated version of the article, with reviewer's comments in pop-up boxes, is attached (note that the code rga is not the reviewer's initials). Some of these have already been referred to above, but many are additional.

Example 3.

Ms. Ref. No.: XXXXXXXXXX

Reviewer #2:

Thank you for the opportunity to review this submission which I greatly enjoyed reading. The article draws on focus group data to explore different forms of public engagement with demand side management (DSM). In so doing, it distinguishes between two types of public - energy consumers and energy citizens - and shows how each responds to the DSM agenda in different ways. In so doing, the article generates important implications for how DSM should be framed and approached in future. Whilst there are one or two minor points of clarification that I feel are required (see below), overall I feel the article makes an important and timely contribution to debates in this area and thus am happy to recommend it for publication.

The main points of clarification I would recommend are as follows:

First, in the discussion section (especially p26) there is a danger of romanticising community-led forms of energy generation. Many have previously claimed that community schemes might be a good route to a low-carbon transition for reasons of trust etc., but there is also very little evidence as to how effective such an approach might be e.g. in terms of delivering large amounts of low-carbon energy quickly as climate change imperatives suggest

is essential. It would be good for the author(s) to sound a small note of caution here so as not to over-romanticise the contribution community groups are making.

Second, the concept of 'alterity surfacing' is interesting but needs to be defined much more clearly when it is first introduced on p19. The paragraph that begins 'It is important to emphasise...' (p19) is very unclear - especially given the importance of the concept to the article as a whole - so demands some re-writing.

Third, it would be good to provide a few more details next to the quotations to show which focus group they stemmed and even, if possible, to allow distinctions to be drawn between individual participants. Were the two quotes on p22, for example, from the same group or the same person? This information would help inform interpretation of the data.

Finally, there were a few areas where I felt greater clarity of expression would help draw out the argument, these were as follows:

- p5, the sentence beginning 'It is argued that the current...' could be clearer.
- P6 - it would be good to be clear at the outset that these were four distinct focus groups each with 18 participants, and not 18 participants spread across four groups.
- P16 - re: self-selection of participants in dynamic pricing schemes - I don't argue with your point here, but a practice-based reading would also suggest that the success or otherwise of the scheme hinges fundamentally on the current state and dynamics of the system of practices into which it is introduced. Hence, we should not necessarily expect the same scheme to be similarly successful every time in different places - or even in the same place at a different time. In short, your argument could be strengthened here.
- P16-17 - the sentence beginning 'At a household level...' and to the end of the paragraph was a little hard to follow. Please clarify.
- P17 - the sentence beginning 'The energy citizen persona became...' was unclear - especially the final clause.
- P17-19 - the sentence beginning 'Those who did have a heightened...' doesn't make sense to me.

In addition to these minor revisions, one broader point that the author(s) might like to consider in their future work (but that is not necessary to address here as the article makes a valuable contribution as it stands), is the relationship between concepts of 'energy citizens/consumers etc' and a practice-based framing. In many recent formulations, practice theory tends to take the focus away from 'energy' itself and towards developing understandings of 'what energy is for' e.g. showering, cooking, communicating etc. In these arguably 'strong' interpretations of practice theory, it perhaps makes little sense to talk of the 'energy' citizen or consumer, and still less sense to suggest that an 'energy citizenship' framing is one that we should seek intentionally to invoke. Rather, primary focus should instead be placed on evolving practices in ways that reduce their energy intensity without energy necessarily needing to be 'surfaced'. Personally, I favour the approach adopted by the author(s) here and think the strong interpretation of practice theory being outlined by some risks generating the sort of distrust of big brother type interventions so nicely highlighted in this contribution. Nonetheless it would be helpful, in future work, for these discussions to be drawn out more explicitly and for the author(s) stance to be made clear.

Overall, however, I greatly enjoyed reading this submission and, with a few minor revisions, think it will make a valuable contribution to debates in this area.

Reviewer #3:

The paper addresses an important, currently under researched area and makes good use of contemporary social theory to investigate the emergence of smart grids and DSM. While it has the potential to make a contribution to the field it is not currently ready to be published. The authors are encouraged to develop the paper further in three areas:

- 1) It is unclear whether the energy citizen and consumer personae come from analysis of the data or from a priori conceptual categories. The paper would benefit from further discussion of the origin of these 2 identities in terms of the research process; please explain where these concepts come from, how they were developed and what other concepts you considered but did not use and why.
- 2) these 2 personae seem to be a rather neat binary pair of identities which makes them feel somewhat constructed rather than 'actually existing'. The authors should comment on how they relate to the multiple, overlapping, complex and changing dispositions toward energy that their participants exhibited. They remark on how people move from one to the other identity but do so only in passing. More analysis of these complex and changing dispositions may help a reader understand the utility of the citizen and consumer identities.
- 3) The citizen-as-solution message in the conclusion does little to challenge the status quo. The industry and governments are already looking to establish such forms of active citizenship, and several research projects (notably the LCNF ones in the UK, and the academic work of Shove, Strengers and others) are already working with these ideas. The authors have the opportunity to go beyond chiming with the sentiment of these initiatives but do not do so in this draft.

I would recommend that the authors revise the text to fulfil the paper's potential. I attach a PDF with some comments for their consideration in relation to specifics in the text.

Reviewer #4: I really like this piece—I found it well written and well argued. I suggest it be accepted as is.

Appendix II. Example of a detailed response to reviewer document

We thank the editors and three peer reviewers for extremely helpful suggestions. We have taken almost all of them to heart. In terms of major revisions, the resubmitted manuscript has:

- A new title and better International Political Economy reframing in the Introduction and Conclusion, as well as engagement with other Special Issue articles (e.g. Kuzemko et al., Newell, Van de Graaf and Bradshaw) plus a chapter from Keating;
- Updated the dataset (and analysis) to the latest from the World Bank (2017) for good measure, now testing three time periods 1985–1994; 1995–2004; 2005–2014;
- Added an entirely new (sixth) hypothesis on environmental degradation and carbon emissions;
- Cross checked and added specific page numbers for all direct quotes

In terms of specific point-by-point responses, please see the Table below.

Sincerely,

The Authors

Reviewer comments	Authors' response
<p>Reviewer 1: Article addresses a very important topic and makes a clear case in the introduction as to why such an analysis is useful and timely.</p> <p>My first reaction to the stated goals of examining how hydro states “do” across a variety of indicators is that many of the impacts (both positive and negative) resulting from large-scale hydropower development may not be captured by national statistics. For instance, the authors claim to “analyze the effect of countries’ hydropower construction and production rates on X, Y, and Z” (p. 3, line 19), but it seems that parsing out the specific hydro effects on those things is a different story (and far more difficult) than simply looking for correlation between levels of hydro development and, say, corruption. The authors clearly address these concerns at the start of Section 3.2, but I still find myself wondering whether the results might be similar if the independent variable were investment in roads or some other major infrastructure. And again, the authors address this concern in the conclusion (p. 32).</p>	<p>Thank you (no action required)</p> <p>Excellent point. In our revision, on p. 3, we mention three things that help better situate our approach to minimize this limitation as much as possible:</p> <ol style="list-style-type: none"> 1. We’ve redone the regression analyses (and indeed the entire analysis) to focus only on hydropower supply/generation of electricity, rather than dam construction (which as the reviewer points out can be similar to roads) 2. Our research design involves looking at reference classes of countries, at “major hydropower” states as a whole, precisely so we can see how they perform as a class, moving away from “dam-centric” or single case approaches to more comprehensive analysis 3. We now make our reference classes of countries mutually exclusive, so the hydropower class can be compared with the OPEC class and all other countries.
<p>Overall the paper is very clearly written and thoroughly argued, and does fill an important gap in the literature. By the end I wondered whether more attention might have been given to hydro’s green credentials, including its ability to balance (or firm) variable renewables given its “instant-on” ramp rate, thereby making those renewables more attractive in the long run.</p>	<p>Also, and this is critical, is that the mixed methods nature of the study was intended to actively preempt this criticism. This is why we have both a mix of country class comparisons <i>and</i> regression analyses. Regarding the independent variable, we have a metric variable (hydropower production rate), and then we scale it down to a nominal variable (hydropower states), which means the country class comparison has less statistical vigor than the regression analysis.</p> <p>Thank you—this is a valid point about the green credentials. To address it, we have added an entirely new (sixth) hypothesis about the environment where we summarize evidence about the potential high carbon footprint of dams (and other environmental impacts on water). We then test this hypothesis with data on carbon dioxide emissions. We find that major hydropower states do in fact perform positively on this metric (lower emissions), contrary to the hypothesis. In the section explaining why, we offer a few sentences about storage, ramp up, and integration of renewables.</p>
<p>I’ve provided some comments below (page/line): 2/38 “major thinkers” is somewhat inelegant and could stand to be refined/clarified</p> <p>2/50 Zeitoun, not Zeitour</p> <p>9/19 Brown et al. (2008) is not in the ref. list</p>	<p>(We would have stated the hypothesis the other way, that dams decrease emissions, and we prove it true, except all of our other hypotheses were in the ‘negative’ direction, looking for the so-called resource curse, so we wanted to maintain that).</p> <p>Good suggestion, replaced with “a body of research”</p> <p>Corrected</p> <p>Added – it was Brown Philip H, Darrin Magee, and Yilin Xu. 2008. Socioeconomic vulnerability in China’s hydropower development <i>China Economic Review</i> 19 (2008) 614–627</p> <p>Correct, proper reference (Now cited in text) is Zhao et al. (2012):</p>
<p>9/24 Xingang et al. (2012) is not in the ref. list. Xingang is likely the given name (2 syllables), not surname (usu. one syllable) - suggest double-checking.</p>	<p>Zhao, Xingang, Liu Lu, Liu Xiaomeng, Wang Jieyu, Liu Pingkuo, A critical-analysis on the development of China hydropower, <i>Renewable Energy</i> 44 (2012) 1-6</p> <p>Done</p>
<p>10/40 Jun 2011 should be Ma 2011 (Ma is his surname); also fix in ref. list</p> <p>10/43 What do the authors mean by “gross hydropower facilities”? Really dirty ones? I doubt it!</p> <p>17/32 How did the authors arrive at 30% as the cutoff for the “hydro construction” class? This figure strikes me as high, and seems to create a class almost exclusively comprised of lower-income countries that would fall into the LDC category. Countries that I would consider as having significant hydro (e.g., China, Canada, US) don’t make it into this class or the hydro production (>80%) class. In fact, these cutoffs seem more a statement of the state of a country’s power sector as lacking non-hydro resources, rather than a comment on the role of hydro there.</p>	<p>;) This was the term the author used to imply “as a whole” or “in aggregate” but we agree we can simply remove “gross” from the sentence</p> <p>A valid point—we admit here any cutoff would have been arbitrary (60, 70, 80, etc.). We went with 70% merely because we wanted to test how “major hydropower states” performed and 60% seemed not enough, whereas 80% would have created too narrow a reference class. Although it varies by timeframe, this class (at 70%) still has 25–33 countries in it, and it includes many non-LDC countries including Albania, Brazil, Costa Rica, Iceland, Norway, New Zealand, Peru, etc.</p> <p>We would also note that our (new/refined) analysis makes the reference classes mutually exclusive, and has removed the LDC class, to depict a hopefully clearer relationship among the variables.</p>
<p>20/40 “there” should be “they” I think</p> <p>21/5 “a built dam can only generate electricity” – this might be a bit too simplistic. Opportunities for corruption arise in processes like inspection and maintenance, in resettlement, and in operation. But I get the point that the sums are likely smaller than those from the construction process.</p>	<p>Lastly, as already noted, the country class comparison is an important part of our analysis, but the regression analysis is there as well, so the 70% cutoff point is not that dramatic, since it is not relevant for the regression analysis.</p> <p>Good eyes, corrected</p> <p>Very good point. As we now say in this paragraph:</p> <p><i>Perhaps the reason we find no significant support for the hypothesis that hydropower countries have more conflict is because the resources involved dams (materials, money, labor) require skills and knowledge that are fairly particular—they are not easily fungible or applicable to say a coal mine or paper mill. Then, once built, a dam can only (directly) generate electricity. Opportunities for corruption may arise in processes like inspection and maintenance, in resettlement, and in operation, but such sums are likely smaller than those from the construction process</i></p>
<p>28/48 Beware the “small is beautiful” trap with hydro. In China, for instance, smaller means less oversight. Moreover, Kibler and Tullos (2013?) showed that the cumulative</p>	<p>Excellent suggestion. To accommodate it, we have made three changes. First, in the “small is beautiful paragraph” we have added more nuance (and references) to back up our</p> <p style="text-align: right;"><i>(continued on next page)</i></p>

(continued)

Reviewer comments	Authors' response
effects (in terms of habitat destruction) of many small dams can outweigh those of one large dam.	specific points about micro-hydro, emphasizing they perform well only when properly designed and governed.
	Secondly, we have read and now cite Kibler and Tullios.
	Thirdly, we expand on this point from the reviewer explicitly in our new hypothesis/section on the environment, with three paragraphs all pointing out the ways that dams can degrade the environment (though mostly focused on carbon). This is where we cite Kibler and Tullios (2013), but we also write:
	<i>Table 4 summarizes other negative impacts that hydroelectric dams can have on habitats, water quality, and environmental sustainability. Both Brismar (2004) and Wang et al. (2013) warn that much of the time, only lower order impacts at a single dam are evaluated, whereas in reality lower and higher order impacts occur and they can cascade across multiple dams—meaning impact assessments can underestimate the true extent of environmental damage. Kibler and Tullios (2013) even show that the cumulative effects (in terms of habitat destruction) of many small dams can outweigh those of one large dam. Separate still ...</i>
32/15 The “impacts at what scale?” question is a crucial one, one with which I have wrestled myself	We agree, and have added here (in the conclusion) that questions of timing and temporality are also important
Reviewer 2: This is a well-organised paper setting out and testing a number of hypotheses about the political, economic, and developmental effects of hydroelectricity.	Thank you (no action required)
I think they need to explain two fairly basic things in more detail to justify the paper. First, in what way is this an IPE paper? This is rather taken for granted, and there is plenty of things relevant to IPE as the paper proceeds, but the overall framing of the paper is rather generic, focused on testing various specific claimed benefits or problems of hydroelectric development, and what this specifically has to tell scholars in IPE is never really directly addressed. It could readily be a paper in Energy Policy or similar journal.	This is a fair criticism—in a sense the article is really about bringing energy data (and insights) into the IPE field (about the biggest source of renewable energy globally) rather than bringing IPE concepts to the energy domain.
Second, the opening set-up of the paper makes much of the reason for promoting hydropower in relation to greenhouse gas emissions mitigation, and yet none of the hypotheses really deal with this explicitly. In the discussion (e.g. p32) we then get a conclusion that there isn't really a justification for extensive promotion of hydropower, because it doesn't promote growth, alleviate poverty, etc. But these are not the reasons given for promoting hydro at the beginning of the paper so it seems rather misplaced to give them as reasons for why hydropower is problematic. See especially the passage on p34.	Still, we try to make an explicit link to IPE both in the Introduction (see paragraph starting “Second, for an IPE audience ...”, which we have doubled in length in our revision) and in the Conclusion (where we return to notions such as the resource curse, energy transitions, and winners/losers/tradeoffs). We agree, and have taken this criticism to heart. We have therefore added an entirely new hypothesis on the environment and carbon dioxide emissions, which we now test alongside the other results.
Smaller comments	
p3 line 49. impound is an odd word here. ditto 'fragment' later on in the sentence.	Impound is definitely the word we want (this part of the dam phase is even called impoundment) but we realize fragment is odd, so we've replaced that with “operate on”
p11 line 53. phlegmatic is not the right word here - 'sluggish' perhaps?	Agreed, changed to sluggish
p13 line 43. not uniquely susceptible. the table contradicts the text here. nuclear reactors have significantly higher overruns. sentence needs adjusting.	Fair enough – we tried to hedge this by noting that nuclear were in the same category (see original sentence with nuclear reactors in brackets):
	<i>As Table 3 indicates, compared to other types of infrastructure, hydropower projects (and nuclear reactors), are uniquely susceptible to cost escalation ...</i>
	But we agree this is a poor use of unique. We have taken the adjective out and revised the sentence.
p18 line 26. Is there not the possibility of using time-series data here? i.e. I would have thought it would make sense to try to analyse things across time explicitly to try to get at causal connections - i.e. if dam is built at time T1 you might expect (e.g. for H3) to see changes in economic growth rates from T0 to T2 or beyond (depending on the lag effect you might expect. is the data not up to being able to do this? a bit of explanation for why this couldn't be done or wouldn't be analytically useful might be in order.	We essentially try to inject some temporal complexity into the analysis by having the three (previously two) timeframes. In doing this revision to incorporate the most recent 2017 data, we are now able to test performance of the reference class countries across three equal blocks of 10 years (1985-1994, 1995-2004, 2005-2014). However, in doing so we have had to abandon the construction dimension (the hydropower building hypotheses) given that the World Bank changed the format of the data, and stopped collecting it. We now only report hydropower generation data, and as previously explained, our entire point was to move away from dam-centered analysis to nation-state analysis.
line 51. I'm not quite clear how the comparison cross the groups works when you have some countries that appear in more than one group ... could be explained explicitly.	Great point – we agree, and in our re-analysis have made the three country reference groups mutually exclusive, so a country can only be in one class.
p19 table 5. The countries here are clearly not a full set of all countries in the world. There are obvious hydropower countries like Canada missing. Why isn't this a complete set of countries? I didn't see an explanation for this. If it's a question of missing data, then we might reasonably expect a discussion of how this affects the results - the n in each group is pretty small so would be affected considerably by missing data I would think.	Correct—the total number of countries listed for each phase is in the 110–140 range rather than the 190+ range, due to data availability. The new data (from 2017) does now include Canada, but notably not all countries. As we now note in the text:
Reviewer 3: Comments to the Author	<i>In the first timeframe, our data analysis encompassed 113 countries, followed by 137 countries in timeframe 2 and 140 countries in timeframe 3. Lower case numbers in the first two timeframes are due to missing data in the hydropower dataset of the World Bank. Furthermore, our analysis does not encompass all countries of the world, since for approximately 75 countries there was no data in the hydropower dataset of the World Bank.</i>
This is an interesting and thought-provoking paper that employs regression analysis to test five hypotheses regarding the impacts of hydropower on various socio-economic indicators at state level, using primarily, but not exclusively, World Bank data. The authors hypothesise that major hydropower states exhibit 'more conflict, see more poverty, have lower growth rates in economic development, and greater rates of public debt, and experience more corruption'. The regression analysis uses two timeframes	Thank you! (no action required)

(continued on next page)

(continued)

Reviewer comments	Authors' response
<p>and compares states that are large hydropower producers and constructors with OPEC states, low income states and non-hydro states. The analysis supports all hypotheses to some extent except for the hypothesis that suggested hydropower increased corruption.</p>	
<p>This is a well written paper that is clearly argued and undertakes sophisticated data analysis to test the hypotheses.</p> <p>There are, however, a few areas where the paper could be improved, particularly in the relationship between the development of the hypotheses and the data analysis itself. For instance, the data analysis is divided between hydropower construction and generation in each country and it therefore seems logical that the hypotheses sections should be similarly clearly delineated along these lines. At the moment, the review of the literature for each hypothesis doesn't really distinguish between hydropower construction and generation so it is sometimes difficult to assess whether the literature is blaming hydropower construction, generation or both, for the impacts observed.</p> <p>There is a further mismatch in the arguments of some of the literature and what is being tested. For instance, there is a disconnect with regards to the relationship between hydropower and poverty. Most of the literature cited in 2.2 focuses on the poverty-creation impacts of hydropower in marginalised or rural communities directly affected by the dams – e.g. 'In China, hydroelectric dams in Yunnan Province were promoted as mechanisms of poverty alleviation and local employment but, in practice, have benefitted primarily urban centers and industrial clusters hundreds of kilometers away (Magee 2006)' – while the quantitative analysis in the calculations assesses the increase or decrease in poverty at a state level. In the literature, there may well be an overall reduction of poverty across the state – the hydropower might reduce poverty for a multitude of distant urban dwellers who receive its electricity while sending a lesser number of displaced indigenous or rural populations near the dam into poverty – but that is not their argument. Their argument is that this is causing injustice to the local peoples adversely affected. The paper, on the other hand, assesses increases or decreases in poverty at a national level. Again, a tighter relationship between the literature and the data analysis would help.</p> <p>The last few sentences of the conclusion seem to pick up on the points made in the literature on poverty cited in the hypotheses section:</p>	<p>We agree, and in our attempt to make the article more up to date, this challenge was solved for us. In using the most recent 2017 World Bank data for our (re)analysis, they no longer compile the construction data, so we have focused the entire analysis on only hydropower supply/generation/production. We still talk about how construction impacts can manifest themselves (relocation, accidents and dam failure, etc.) but assess that impact through the lens of electricity generation. The idea here is that construction impacts would manifest themselves over time even after dam completion.</p> <p>We agree, and this comment forced us to rewrite some of the ways we introduced and framed the hypotheses, especially the one on poverty. We hope we succeeded, but are certainly open to suggestions from the reviewer about how to improve this even further (if needed).</p> <p>This is an apt and valid criticism – and it is (admittedly) one we didn't see until this reviewer pointed it out. We agree – and while we still believe our reference class country approach has value (and sheds more light on hydropower and political economy than single country cases) this aspect of our research is in tension with the way we formed the conclusions. In that vein, rather than redo the entire research design (which we have already sought to improve vis-à-vis updating the data, making the reference classes of countries more coherent and mutually exclusive, and adding a sixth hypothesis), we have instead attempted to make our conclusion more reflective of our findings (and more nuanced). We've also tried to better justify both a national-state and international comparative focus in this (revised) conclusion.</p>
<p>'We need to consider not only the magnitude of costs and benefits, but the equitable nature and timing of their distribution. In sum: the political economy of hydroelectricity is also about choices that are being made between which forms of (low carbon) electricity to invest in, and why. Who builds dams, who benefits from them, and who suffers their costs—who wins and loses—must remain a central part of examining the promise—and peril—of hydropower.'</p> <p>This may be a call for scholars to investigate these questions elsewhere but it seems somewhat strange to highlight in the conclusion how important these questions are, yet not really having addressed them in the paper itself, despite looking at poverty levels overall in each state. Indeed, this inability to disaggregate the data is often one of the key problems with quantitative analysis, particularly at a state level. The paper finishes by effectively extolling the very virtues of the detailed 'single country/project case studies' that the paper purports to move beyond.</p> <p>Some specific queries are listed below:</p>	
<p>P.1.L.21 Need page numbers in citations for specific facts. There are quite a few citations where page numbers should be added throughout the paper.</p>	<p>Good catch – page numbers now added for all direct quotes. For some specific claims of fact, e.g. P.1.L.21, these claims come from multiple pages (across many chapters), so the citation style stands at just a reference to the book, not any individual page.</p>
<p>P.1.L.45 Quotation doesn't have page numbers in citation.</p>	<p>Citation added for P.1 L 45</p>
<p>P.2.L.21 Quotation doesn't have page numbers in citation.</p>	<p>Page number added for P.2 L21 (Haas, p. 86)</p>
<p>P.2.L.10 (Lindquist 1998) – should be Lundquist.</p>	<p>Good eyes – we had it correct in the reference list, but not in the text – corrected.</p>
<p>P.3.L.38 Don't use 'even' here for emphasis. Just state your point.</p>	<p>This seems like a minor stylistic point but we have removed "even" from the sentence</p>
<p>P.4.L.38 'Of the \$11.1 trillion the world is anticipated to spend on energy infrastructure from 2005 to 2030, \$1.9 trillion is expected to go exclusively to hydropower (Haas 2008)' – We are already a decade into this timeframe. Any update?</p>	<p>Alas, no – the Haas 2008 source was the only place we found this estimate, although we have changed the verb tense to emphasize that this estimate was made years ago, i.e. "Of the \$11.1 trillion the world was anticipated ..."</p>
<p>P.7.L.28 To update this table perhaps include the capture of Mosul Dam in Iraq by Islamic State in 2014.</p>	<p>Excellent suggestion—done</p>
<p>P.9.L.39 The quote by Imhof – not Imjof – and Lanza seems to apply to 2.2, focusing on poverty, not 2.3, inhibiting economic growth.</p>	<p>Noted (on correct spelling of Imhof) and agreed about moving this to 2.2</p>
<p>P.10.L.27 \$9.7bn doesn't sound like a large amount of damages for over a century of global accidents. Compare with one oil spill: Deepwater Horizon – ~\$60bn.</p>	<p>True – it's all a matter of perspective I suppose. But it would depend on where those \$9.7 bn in damages befell, i.e. if on chronically poor populations then the impacts would have been terrible. I would surmise that most of the monetized damage from dams is human or</p>

(continued on next page)

(continued)

Reviewer comments	Authors' response
P.11.L.40 'As Table 3 indicates, compared to other types of infrastructure, hydropower projects (and nuclear reactors), are uniquely susceptible to cost escalation.' – if there is more than one, it's not unique, particularly since the cost overruns for nuclear reactors are so much more than for dams.	socially related, whereas much of the BP damage was likely environmental remediation and focused on natural habitats Agreed—and this was also picked up by the other reviewer. Sentence rephrased, “unique” removed.
P.14.L.7. “Government officials stole \$50 million of resettlement funds appropriated for the Three Gorges Dam in China, leading to ‘the largest such corruption scandal on record’ (Haas 2008).” - \$50 m doesn't sound overly corrupt for a \$28bn project.	Again, all a matter of perspective (and why we give the precise financial figures, rather than more vague phrasings such as “substantial sum of money” or “millions of dollars.”
P.15.L.55 About a third of OPEC countries are high income countries in the Persian Gulf, particularly for Timeframe 2. These are not usually defined as those associated with the resource curse so the assumption does not necessarily apply. The comparison appears to be of limited use	Fair point—and while we don't disagree entirely, some of the resource curse literature has nonetheless framed OPEC countries as being susceptible. We now support this claim with two references – adding in direct quotes from each study here (though not in the text) to illustrate the arguments taken by such authors: Robinson, JA, R Torvik, and T Verdier. Political foundations of the resource curse. <i>Journal of Development Economics</i> 79 (2006) 447 – 468 “The resources we have in mind are especially those that are publicly owned such as oil, gas and other minerals. For such resources the resource rents accrue to the public sector, and the government decides how much of the resources to extract. For instance, for practically all main oil exporters this is the case” And “Resources are a curse for currently developing countries. This claim is supported both by some basic facts, for example, for OPEC as a whole GDP per capita on average decreased by 1.3% each year from 1965 to 1998” Hammond, John L. (2011). The Resource Curse and Oil Revenues in Angola and Venezuela. <i>Science & Society: Vol. 75, No. 3</i> , pp. 348-378. “Angola, a classic case of the resource curse, has experienced corrupt and authoritarian government since independence in 1975. Venezuela appears to have avoided the resource curse under President Hugo Chávez. The concept of resource curse, and accordingly its remedies, are multidimensional, encompassing honest government, sound economic management, and public welfare. The case of Venezuela shows that sound economic management is not sufficient to overcome the resource curse” Lastly, we reference another article in the Special Issue (Van de Graaf and Bradshaw) talking about the value of examining oil exporters as a country class. Interestingly, one other reviewer thought we had too few countries here. Apparently the UN currently has 193 member states and it looks like our (current) data set, updated to 2017, has around 113-140 countries in total. So we're missing some.
P.18.L.18 The ‘Non-hydro’ countries (N = ...) plus the hydro production and construction groups add up to around 220 countries in both timeframes. Is this correct? That's a lot more than at the UN.	As we now clarify in the text: <i>In the first timeframe, our data analysis encompassed 113 countries, followed by 137 countries in timeframe 2 and 140 countries in timeframe 3 (to put these numbers in perspective, the United Nations currently has 193 member states). Lower case numbers in the first two timeframes are due to missing data in the hydropower dataset of the World Bank. Furthermore, our analysis does not encompass all countries of the world, since for approximately 75 countries there was no data in the hydropower dataset of the World Bank.</i> Correct, though with us dropping the construction reference class, we no longer need this superscript (as we only have one reference class of major hydropower states). Good catch, we had moved this into the text and forgot to delete the reference to the footnote. Corrected Done—all corrected.
P.18.L.21 Superscript for note 1 is missing – presumable it applies to ‘Hydropower capacity’.	
P.18.L.30 There is no footnote for Table 3. References: Ensure references are in alphabetical order: Kumar and Koch not in alphabetical order. Leslie and Lerer not in alphabetical order. etc.	

References

- [1] Wiley, Principles of Peer Review, Available at, 2018, https://authorservices.wiley.com/asset/Principles_of_Peer_Review.pdf.
- [2] B.J. Riley, R. Jones, Peer review: acknowledging its value and recognising the reviewers, *Br. J. Gen. Pract.* 66 (653) (2016) 629–630, <https://doi.org/10.3399/bjgp16X688285>.
- [3] R. Smith, Peer review: a flawed process at the heart of science and journals, *Journal of the Royal Society of Medicine* 99 (April, 2006) (2006) 178–182.
- [4] M. Callaghan, C.F. Schlessner, S. Nath, et al., Machine-learning-based evidence and attribution mapping of 100,000 climate impact studies, *Nat. Clim. Chang.* 11 (2021) 966–972, <https://doi.org/10.1038/s41558-021-01168-6>.
- [5] Henri van Dorssen, Publishing your article in a scholarly journal, in: *Pre-conference Workshop on Publishing and Career Development for Young Researchers*, 2017. Oxford, April 2.
- [6] Henri van Dorssen, Publishing your article in a scholarly journal, in: *Pre-conference Workshop on Publishing and Career Development for Young Researchers*, 2017. Oxford, April 2.

- [7] Annette Lareau, *Listening to People: A Practical Guide to Interviewing, Participant Observation, Data Analysis, and Writing It All Up*, University of Chicago Press, 2021.
- [8] Ioannidis JPA, Why most published research findings are false, *PLoS Med* 2 (8) (2005), e124. <https://doi.org/10.1371/journal.pmed.0020124>.
- [9] Benjamin K. Sovacool, Jonn Axsen, Steve Sorrell, Promoting novelty, rigor, and style in energy social science: Towards codes of practice for appropriate methods and research design, *Energy Research & Social Science* 45 (2018) (2018) 12–42.
- [10] Research Executive Agency, *MSCA-IF Evaluation Step by Step Manual for Evaluators*, Available at, European Commission, Brussels, 2018, https://ec.europa.eu/info/sites/default/files/msca_if_2018_manual_for_evaluators_0.pdf.
- [11] Benjamin K. Sovacool, Götz Walter, Internationalizing the political economy of hydroelectricity: security, development and sustainability in hydropower states, *Review of International Political Economy* 26 (1) (2019) 49–79, <https://doi.org/10.1080/09692290.2018.1511449>.
- [12] J. Kirchherr, Bullshit in the sustainability and transitions literature: a provocation, *Circ. Econ. Sust.* (2022), [https://doi.org/10.1007/s43615\[HYPHEN\]022-00175-9](https://doi.org/10.1007/s43615[HYPHEN]022-00175-9).