Critique of Some Vulnerability and Resilience Papers

By Ilan Kelman http://www.ilankelman.org
Suggestions to the author via http://www.ilankelman.org/contact.html

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Introduction

Four papers have been frequently cited as key references when discussing vulnerability and resilience, especially in the context of climate change, other environmental changes, and related topics:

Adger, W.N. 2006. "Vulnerability". Global Environmental Change, vol. 16, pp. 268-281.

Adger, W.N., T.P. Hughes, C. Folke, S.R. Carpenter, J. Rockström. 2005. "Social-Ecological Resilience to Coastal Disasters". Science, vol. 309, pp. 1036-1039.

Folke, C. 2006. "Resilience: The emergence of a perspective for social-ecological systems analyses". Global Environmental Change, vol. 16, pp. 253-267.

Janssen, M.A., M.L. Schoon, W. Ke, K. Börner. 2006. "Scholarly networks on resilience, vulnerability and adaptation within the human dimensions of global environmental change". Global Environmental Change, vol. 16, pp. 240-252.

The amount of referencing received by these papers is surprising, because I believe that I have found numerous factual and conceptual errors in them along with unscientific approaches and analyses throughout them. This document details these concerns.

If I have misinterpreted any aspects or made any mistakes, please accept my deepest apologies and please contact me, as listed above, to point out my errors so that I could update this document. Thank you very much.

Adger 2006 GEC

Adger, W.N. 2006. "Vulnerability". Global Environmental Change, vol. 16, pp. 268-281.

The article is published in a journal edited at the time by the author, the author's wife, and the author's institutional director. Therefore, the author cannot claim that this article has undergone an objective scientific peer review process, because any such attempt would entail a conflict-of-interest. The consequence is that this article could be considered to be an editorial or viewpoint, but it should not referenced as being a scientific paper. Given that the criticisms here are scientific, these criticisms might not be valid, unless claims are made that the article is scientific; however, it is also important to remember that even an editorial or a "viewpoint" should check basic facts and should demonstrate an awareness of the scientific literature.

This paper's abstract states "This paper reviews research traditions of vulnerability to environmental change and the challenges for present vulnerability research in integrating with the domains of resilience and adaptation." The paper's first sentence (page 268) states "The purpose of this article is to review existing knowledge on analytical approaches to vulnerability to environmental change in order to propose synergies between research on vulnerability and on resilience of social-ecological systems". Many of the comments below refer back to these statements of the paper's purpose.

1. Knowledge of the literature

Despite the paper stating that its purpose "is to review existing knowledge on analytical approaches to vulnerability to environmental change" (page 268), the author does not consider the work of the authors Eric Waddell, Anthony F.C. Wallace, William Torry, Graham Tobin, Robin Spence, Douglas Paton, Anthony (Tony) Oliver-Smith, Betty Morrow, Andrew Maskrey, Graham Marsh, James Lewis, Allan Lavell, David Johnston, Susanna Hoffman, Michael Glantz, Thomas Glade, Anne Eyre, Elaine Enarson, Zenaida Delica Willison, Fred Cuny, David Crichton, Jean Copans, Andrew Coburn, Philip Buckle, and Virginia García Acosta. That work, and the work of many others, extends from the present back to at least the 1970's, using field-based practical evidence to match with theoretical ideas to better understand and evidence the vulnerability of individuals and communities--including different definitions, interpretations, and understandings of "vulnerability"--to different environmental, often linked to social, changes and challenges. Many comments below refer back to this list of authors, indicating that the author has missed key publications on the topic of vulnerability.

It could be argued, perfectly fairly, that no paper can cover all literature. Yet with such a high level of omissions, the paper has not matched its own mandate. In particular, the author finds room to reference 8 of his own publications (6 of them for which he is listed as first author), all of them from 2000 or later. That suggests that space would have been available to diversify the sources used in the paper, both historically and regarding different authors, in order to demonstrate deeper and broader knowledge of the literature.

Some specific examples of the author having poor knowledge of the literature on this topic are:

1(a) The paper states "The concept of vulnerability has been a powerful analytical tool for describing states of..." (page of 268). That comment is accurate for some of the literature, but fails to recognise the literature that looks at vulnerability as being more than a state, instead viewing vulnerability as being a long-term process, covering the ongoing, chronic, underlying conditions, that has led to the observed state. See, for example, the work of James Lewis and Tony Oliver-Smith. The failure of the author to distinguish between views of vulnerability as a state and as a process is even more surprising given that the next paragraph and other sections of the paper do discuss processes with regards to vulnerability, suggesting the author's awareness of the notion, at least to some degree. Yet there is a large body of literature discussing the vulnerability process which is not reflected by or acknowledged in this paper.

1(b) Page 268 of the paper states "In the context of these social-ecological systems, resilience refers to..." followed by a definition that is represented in only one portion of the literature, thereby failing to acknowledge the in-depth discussion of resilience given by the authors listed above.

1(c) The paragraph on page 269 beginning "Evolving insights into the vulnerability of social-ecological systems..." makes sweeping statements which are not always supported by the literature of the authors listed above. Certainly, the author presents one view in the literature which is worthy of discussion and critique and that view is sometimes reflected in the literature of the authors listed above along with those referenced in this paper--but that is only one view. Without presenting other views, the paper's mandate is not met.

1(d) Page 269 states "In a world of global change, such discrete events are becoming more common" yet no data, no evidence, and no references are provided. Does the author claim that all elements of environmental change including earthquakes and meteorite strikes are becoming more common? This question is appropriate to ask because, on page 276, the author refers to the relevance of "scientific communities in geological hazards". Even so, the geological hazards literature is not referenced in the paper and the results from that research appear to have been forgotten by the author, with specific examples being:
-For landslides, Thomas Glade, Mike Crozier, and David Alexander.
-For earthquakes, Andrew Coburn, Anthony Oliver-Smith, and Robin Spence,.
-For volcanoes, Douglas Paton, David Johnston, Chris Dibben, and David Chester, Russell Blong (one of Blong's heat wave papers is referenced).

1(e) On page 273, the author twice refers to "social capital" and social capital is implied in the statement on page 269 referring to "other forms of capital (human, social and physical)". Given the paper's mandate and given the critiques of many other concepts throughout the paper, it is surprising that no critiques of social capital are given, with two examples (there are many more) being:

Baron, S., J. Field, and T. Schuller. (eds.) 2000. Social Capital: Critical Perspectives. Oxford University Press, Oxford, U.K.

Navarro, V. 2002. "A Critique of Social Capital". International Journal of Health Services, vol. 32, no. 3, pp. 423-432.

It is strange that a paper purporting to give an overview of a topic does not mention Robert Putnam with regards to social capital.

1(f) Page 276 states "If a goal of sustainable development is to eliminate risks to the most vulnerable, then this suggests that application of the precautionary principle should be central to decision processes." Where are the references that state that to be the articulated goal? Is it ever possible "to eliminate risks" completely for any group? How can the conclusion suddenly be reached on the importance of the precautionary principle without providing a definition of it or referencing the literature that critiques it?

1(g) Appendix 1 is developed without any reference to the previous literature on calculating vulnerability in a proportional manner. In fact, that approach has been used as a basic definition long before this paper. See, as one example, the definition of "vulnerability" in:

UN DHA. 1992. Internationally Agreed Glossary of Basic Terms Related to Disaster Management. UN DHA (United Nations Department of Humanitarian Affairs), Geneva, December 1992.

1(h) Appendix 1 is developed without any reference to the previous literature on the vulnerability gap, for example:

La Cava, G. and R.Y. Nanetti. 2000. Albania: Filling the Vulnerability Gap. World Bank Technical Paper no. 460. Europe and Central Asia Environmentally and Socially Sustainable Development Series. The World Bank, Washington, DC.

Wisner, B., Blaikie., P., Cannon, T., and Davis, I. (2004), At Risk: Natural Hazards, People's Vulnerability and Disasters, 2nd ed., Routledge, London, U.K. mention the "vulnerability gap" on page 22 and reference the notion to page 297 of UNEP's third Global Environmental Outlook report from 2002.

2. Analytical approach and scientific merit

The author's poor knowledge of the literature leads to several problems in the author's attempt to analyse and interpret the literature. These problems undermine the paper's value because, in the end, little new is presented and the discussion is frequently not scientific.

2(a) Page 269 states "Livelihoods research remains, I argue, firmly rooted in social systems rather than integrative of risks across social-ecological systems", repeated on page 270 by stating that livelihoods "largely ignore physical and biological systems". No evidence is presented to back up these statements, such as direct quotations from the literature or field examples, and these statements directly contradict some of the literature on this topic. The definition of "livelihood" (from Chambers, R. & Conway, G.R., 1992, although dated December 1991, Sustainable Rural Livelihoods: Practical Concepts for the 21st Century, Discussion Paper 296, Brighton, University of Sussex, Institute of Development Studies) is that "a livelihood comprises the capabilities, assets (stores, resources, claims and access) and activities required for a means of living". When people use natural resources for livelihoods, by definition, a livelihoods approach must consider the combination of society and the environment. The only possible way for the author's statements quoted above to be true would be if natural resources are not part of the environment--an obvious contradiction. In fact, the Chambers and Conway (1992) reference and definition are given by the author on page 272, again in claiming that the livelihoods approach does not factor in the integration of society and the environment, so it is unclear why the author believes that people interacting with natural resources to develop and maintain livelihoods is irrelevant to interaction between society and the environment. The statement is made on page 272 that "While livelihoods are conceptualized as flowing from capital assets that include ecosystem services (natural capital), the physical and ecological dynamics of risk remain largely unaccounted for in this area of research". In the first clause of that statement, the author accepts that ecosystems are explicitly included in livelihoods, yet provides no logical connection to the second clause nor provides any evidence, discussion, argumentation, or references to support the contention in the second clause. This analysis of livelihoods research is not supportable with the evidence available. It may be that this view of the author is simply his opinion or his interpretation, for debate. To make this opinion have scientific merit, further evidence should have been provided to support the contention. At best, the author neglects proper scientific argumentation and evidencing of a viewpoint. At worst, the author's point is fundamentally and insupportable.

2(b) Figure 1 on page 271 and the discussion regarding this figure fail to account for the various views and schools of literature covered by the authors listed above. As such, the paper fails to meet its own mandate and is conceptually and analytically inaccurate in its generalist statements about the vulnerability literature appearing throughout this section. Interestingly, on page 270, the author refers to the ideas he espouses regarding the vulnerability literature with the qualifications "I believe" and "based on my reading of this literature". First, it is based on the author's reading of only a small portion of the literature, because it does not include the authors listed above. Second, the paper's purpose is presented as being a research piece presenting scientific results, not as an opinion piece with limited scientific value. The author's own qualifications suggest that this paper is the latter.

2(c) Page 271 references "Burton et al. (1978 and 1993)" and suggests firm conclusions resulting from it, followed by some critiques of those publications. The author references Hewitt's critique, but not another significant review of the 1978 edition which lambastes Burton et al.'s (1978) findings:

Torry, W. 1979. "Hazards, Hazes and Holes: A Critique of *The Environment as Hazard* and General Reflections on Disaster Research". Canadian Geographer, vol. 23, no. 4, pp. 368-383.

These reviews indicate that the conclusions and outcomes from Burton et al. (1978) are not as firm or as accepted as the author implies on page 271. Also, given that the author feels it appropriate to reference both the first and second editions of Burton et al. (1978 and 1993), why is the second edition of Blaikie et al. (1994)--which was published with the same title and same publisher, but as Wisner et al. (2004)--not considered to be important? It is a serious omission not to discuss updates between the 1994 and 2004 editions which affects the paper's analytical approach and scientific merit.

2(d) Page 271 suggests that Hewitt (1983) "emphasizes the role of economic development" even though social development is also highlighted in Hewitt (1983) as part of "adapting to changing exogenous risk and hence differences in class structure, governance, and economic dependency in the differential impacts of hazards".

2(e) Page 272 states "Impacts associated with geological hazards often occur without much effective warning and with a speed of onset of only a few minutes". If volcanoes are considered to be "geological hazards", as they should be, then it would be important to note that volcanoes often give weeks or months of effective warning, as shown by cases such as Mount Pinatubo (1991), Montserrat's Soufrière Hills (which started rumbling in 1995), Mont Pelée on Martinique (1902), and Nevado del Ruiz in Colombia (1984-1985). Even though the warnings were not fully heeded by everyone in these two latter cases, the warning signs were there and identified by some.

2(f) Page 275 states "However, specific variables do not necessarily measure vulnerability directly. Hence a leap of faith is required between vulnerability of a key variable (whether physical or social) and other elements such as ecosystem services or well-being. Unless the variable and causal links are well established, the relationship may not hold." Many of the authors listed above along with many of the references given in the paper consider aspects of ecosystem services and well-being to be (a) variables in measuring vulnerability and (b) key variables with vulnerability. Many ecosystem characteristics are directly and explicitly linked to aspects of vulnerability in this literature. In whichever manner the author's quotation is read and interpreted, it does not appear to be reflective of the literature.

2(g) Page 276 states "Vulnerability is manifest in specific places at specific times" which is given as a truism, yet which fails to account for the richness of the vulnerability literature, especially those authors referenced in the list above, in terms of understanding vulnerability's existence and manifestation in space and time.

2(h) Page 276 states "A second challenge arises from the tension between objective and perceived elements of vulnerability and risk." This quotation assumes that vulnerability and risk have objective elements, but that assumption is challenged by many of the authors listed above along with a large body of scientific work on risk perception.

2(i) Page 276 states "Thus technological risks that create new vulnerabilities (from nuclear power to genetically modified agricultural crops) are ignored in the name of progress". This statement reveals a misunderstanding of the definitions and discussion of "vulnerability" throughout the literature, including references given by the author but especially in references that the author does not provide, such as the authors in the list above. Many would argue that new hazards and new risks might be created by technologies such as nuclear power and genetic modification, but the same vulnerability is (or "vulnerabilities are", depending on the word definition used) exposed by these new technologies without changing the vulnerability nor creating new vulnerabilities.

2(j) Page 277 states "In many situations and examples it appears that the incidence of vulnerability within the social and natural systems is not central to decision-making and adaptive action." No explanation is given why the expectation should exist that vulnerability would be central to decision-making, adaptive action, and governance. As the vast literature on governance and government demonstrates, many decision-makers focus on issues such as short-term economic consequences, personal gain, and retaining power. Thus, it is frequently naïve to expect that "many situations and examples" will defy that trend. It is odd that the author does not discuss such basic aspects of governance, instead seeming to imply--intentionally or not--that vulnerability should be central to decision-making and adaptive action. This weakness in the text suggests poor analysis of governance issues.

2(k) Appendix 1 does not properly define the variables used in the equations, with the main item not addressed being how to quantify well-being. If that definition is not agreed upon or if robust data are not available, then the equation does not produce a meaningful result. Page 276 states "It could be objective material measures such [sic] indicators of mortality, income, wealth, or freedom from crime or access to education, depending on the nature of the vulnerability being measured. In addition, vulnerability as experienced could be measured directly through perceptions of those that are vulnerable." That quotation raises more questions than it answers, because none of these options are as simplistic as the author implies. Why are "indicators of mortality" considered to be an "objective material measure" considering the vast literature on development- and disaster-related mortality which makes it clear the strong subjectivity of many of these calculations? If annual mortality rate per age group is the indicator being implied, then that could be considered to be objective, but that provides minimal explanatory or predictive power regarding vulnerability. One example of the subjectivities that result in mortality explanations is how long after a disaster to attribute deaths to that disaster. Moreover, if the "well-being" variable is so generic as to cover all aspects of living, then this discussion falls into the same traps that it criticises without providing any advancement of knowledge or methods. Also important, is it necessary to quantify vulnerability and to use an equation, or are there more practical ways of approaching the question of understanding and describing vulnerability in order to act to reduce vulnerability? The assumptions underlying the construction of this Appendix and the related discussions are not necessarily acceptable, yet these assumptions are not debated. Instead, the reader is left with an equation for which the parameters are not clearly defined and the result from which does not have a clear meaning. That does not advance knowledge, is not scientific, and shows limited analytical capability.

2(l) Page 278 states "Say the farmers acted to reduce (but not eliminate) their vulnerability through hard coastal defenses that changed coastal processes and displace the risk of flooding down the coast such that the owners of beachfront coastal properties were now more vulnerable than previously." [sic; either "displace" or "were" is in the wrong tense in this sentence.] The literature on the use of structural flood defences is clear that these defences alter the hazard, but do not reduce vulnerability, instead tending to increase vulnerability. The author's statement displays a gross misunderstanding of the definition of "vulnerability" and of the literature on structural flood defences. Three examples of references (there are many more) are:

Criss, R.E. and E.L. Shock. 2001. "Flood Enhancement Through Flood Control". Geology, vol. 29, no. 10, pp. 875-878.

Etkin, D. 1999. "Risk Transference and Related Trends: Driving Forces Towards More Mega-Disasters". Environmental Hazards, vol. 1, pp. 69-75.

Tobin, G.A. 1995. "The Levee Love Affair: A Stormy Relationship". Water Resources Bulletin, vol. 31, no. 3, pp. 359-367.

3. Accreditation of ideas

In numerous instances, the author accredits ideas and concepts to literature which was published long after others had published similar thoughts. In science, it is customary to give credit to those who were first with an idea or approach.

3(a) Page 268 states "The concept of a social-ecological system reflects the idea that human action and social structures are integral to nature and hence any distinction between social and natural systems is arbitrary. Clearly natural systems refer to biological and biophysical processes while social systems are made up of rules and institutions that mediate human use of resources as well as systems of knowledge and ethics that interpret natural systems from a human perspective (Berkes and Folke, 1998)." This quotation includes two sentences, the first of which is a truism and the second of which covers basic definitions. Page 269 similarly references a truism to Berkes and Folke (1998) in stating "As Berkes and Folke (1998, p. 9) point out, 'there is no single universally accepted way of formulating the linkages between human and natural systems'." Referencing a 1998 source for these concepts does not account for the decades-long list of research publications on indigenous societies that accept as a main premise the links and interchanges between society and the environment. Such ideas, of course, have been prevalent in many societies for millennia. If considering indigenous cultures, two examples of journals with many references illustrating the view of society-environment links are "Human Ecology" which started in 1972 and "Pacific Affairs" which started in 1927.

3(b) Page 269 has a sentence starting "Vulnerability, by contrast, is usually portrayed..." followed by a discussion of vulnerability which appears to focus on IPCC work. The explicit focus on climate change as viewed through the IPCC neglects the vulnerability discourse related to all other forms of environmental change, in contrast to the paper's given purpose. If the sentence on page 269 starting "In all formulations..." is referring back to only the IPCC definition, it is unclear why the author thinks that the IPCC defines or starts the debate on vulnerability to environmental change given the vast works from long before the IPCC started, from many of the authors listed above. In full, this part of page 269 states that "In all formulations, the key parameters of vulnerability are the stress to which a system is exposed, its sensitivity, and its adaptive capacity" repeated on page 270 " vulnerability is most often conceptualized as being constituted by a components [sic] that include exposure and sensitivity to perturbations or external stresses, and the capacity to adapt". These statements do not match the evidence given by the publications of the people listed above, many of which do not use the phrases "exposure", "sensitivity", or "adaptive capacity", instead referring to concepts such as "susceptibility", "underlying conditions", and "ability to adjust". It could be argued that most of these concepts are parallel, because different authors use different terms to cover the same idea. In that case, the earlier work should be considered before the IPCC, rather than starting with the IPCC approach and trying to fit the earlier, founding work into later formulations. The same critiques apply to the sentence beginning "Thus, vulnerability research and resilience research have common elements of interest..." (page 269).

3(c) Page 269 states "A number of traditions and disciplines, from economics and anthropology to psychology and engineering, use the term vulnerability. It is only in the area of human-environment relationships that vulnerability has common, though contested, meaning." It is unclear what the author is trying to state. Is he suggesting that anthropology and psychology are not about human-environment relationships? If so, then he might wish to examine the fields of environmental anthropology and psychological response to environmental stimuli. Regarding areas with a common meaning for "vulnerability", many English dictionaries have similar meanings for "vulnerability", so the fields of linguistics (especially historical linguistics) and semiotics (depending on how symbols have been used to represent "vulnerability") might of interest to seek "common, though contested, meaning".

3(d) Page 272 states "there is a newly emerging synthesis of systems-oriented research attempting, through advances in methods, to understand vulnerability in a holistic manner in natural and social systems". By many definitions of vulnerability from some of the authors listed above, it is not a "newly emerging synthesis" to understand vulnerability within the context of interactions between society and the environment. Similarly the statement on page 272 that "Portraying vulnerability as a property of a social-ecological system, and seeking to elaborate the mechanisms and processes in a coupled manner, represents a conceptual advance in analysis (Turner et al., 2003a)" is incorrect. In contrast, it is a step back because it bypasses the literature from the authors listed above that have moved beyond vulnerability as a property or a state, instead viewing vulnerability as being a long-term process, covering the ongoing, chronic, underlying conditions, that have led to the observed state.

3(e) On page 273, the discussion of gender and vulnerability does not reference the key source on this topic:

Enarson, E. and B.H. Morrow (eds). 1998. The Gendered Terrain of Disaster: Through Women's Eyes. Praeger Publishers, Connecticut, USA.

Similarly on page 273, it is unclear why Eakin (2005) is given credit for the insight that commercial high-yield agriculture exacerbates vulnerability given the vast development literature on how globalisation, neo-liberalism, cash cropping, and multinational corporate control of agriculture exacerbate vulnerability. Three examples (there are many more) of such references are:

Susman, P., P. O'Keefe, and B. Wisner. 1983. "Global disasters: A radical interpretation", pp. 263-283 in K. Hewitt (ed.), Interpretations of Calamity from the Viewpoint of Human Ecology, Allen and Unwin, London.

Wisner, B. 2001. "Capitalism and the shifting spatial and social distribution of hazard and vulnerability". Australian Journal of Emergency Management, vol. 16, no. 2, pp. 44-50.

3(f) Page 273 "Vulnerability research in climate change has, in some ways, a unique distinction of being a widely accepted and used term and an integral part of its scientific agenda." Which term is being referred to here? If the term is "vulnerability" or "vulnerability research", then climate change is not unique or distinct, since both "vulnerability" and "vulnerability research" have been widely accepted and have been an integral part of research and application in many other fields for decades, from earthquake engineering to psychology.

3(g) Page 278 states "Ultimately insights through newly emerging interdisciplinary understanding of vulnerability and resilience demonstrates [sic; the verb should agree with the subject "insights"] the co-evolutionary nature of social and natural systems". The co-evolutionary nature of social and natural systems has been a truism for many indigenous societies for millennia and has pervaded fields such as anthropology and human geography for decades, as evidenced by some of the authors in the list of above, such as Tony Oliver-Smith and James Lewis, along with authors referenced in the paper such as Ken Hewitt and Ben Wisner.

4. Verbiage

The paper contains numerous sentences that demonstrate verbiage more than useful commentary. A few examples from amongst many:

4(a) Page 268 states "This current research can potentially contribute to emerging resilience science through methods and conceptualization of the stresses and processes that lead to threshold changes, particularly those involved in the social and institutional dynamics of social-ecological systems." This statement effectively says that research might or might not assist in understanding resilience by examining change, society, and the environment. That is not a particularly helpful remark.

4(b) Page 273 states "Hence vulnerability assessment incorporates a significant range of parameters in building quantitative and qualitative pictures of the processes and outcomes of vulnerability." The two mentions of "vulnerability" make this statement circular. By definition, assessment "incorporates a significant range of parameters in building quantitative and qualitative pictures of...processes and outcomes". This statement should be a starting point, not a result from the previous discussion.

4(c) Page 274 states "Vulnerability is a dynamic phenomenon often in a continuous state of flux [sic; there should be a semicolon or new sentence starting here] both the biophysical and social processes that shape local conditions and the ability to cope are themselves dynamic (O'Brien et al., 2005)." By many definitions of vulnerability, that statement is a truism. Why is a reference given--and it is a recent reference, as if O'Brien et al. (2005) were the first to provide this truism?

4(d) Page 276 states "But vulnerability may be differently perceived or experienced by the vulnerable themselves (Kasperson et al., 2005)." By many definitions of vulnerability, that statement is a truism. Why is a reference given--and it is a recent reference, as if Kasperson et al. (2001) were the first to provide this truism? The same comment applies to the statement "security and insecurity themselves are not easily measured (Kasperson and Kasperson, 2001)." (page 276). The same comment also applies to the statement on page 276 that "Trends in environmental change, technologies and other social and demographic processes make individuals and social systems are always [sic] vulnerable to surprise and susceptible to unforeseen consequences of action (Cutter, 2003; Schneider et al., 1998). Two recent references are given for a statement which (apart from the typo) is a truism by any usual definitions used for the words. Yet the phrase immediately following that one, also on page 276, "While policy-makers always express surprise at events" is strange. Does the author believe that no policy maker has ever said "I told you so" or "I am not surprised" or "I knew that would happen"?

4(e) Page 276 states "While Norwegians may be concerned with snow for skiing, English gardeners worry about the early arrival of spring..." Such patronising stereotypes could arguably have a place in an undergraduate class to assist in teaching, but they have no place in what is purported to be a peer-reviewed scientific paper in an international academic journal. The same point could easily have been made by writing more succinctly and scientifically "While skiers may be concerned with snow conditions, gardeners [or farmers] worry about when spring arrives..."

4(f) Page 277 states "Resources to reduce vulnerable [sic] in times of crisis are largely latent in social institutions." That sentence says only that institutions have resources and those resources could be used during a crisis, an obvious truism. The next sentence begins "They are..." but it is unclear to what "they" refers. It could be "resources", "resources to reduce vulnerable [sic] in times of crisis", or "social institutions". It is unclear why any of those "are usually involuntary" as the paper states. Neither references nor evidence is given to support this statement, whatever it means.

Adger et al. 2005 Science

Adger, W.N., T.P. Hughes, C. Folke, S.R. Carpenter, J. Rockström. 2005. "Social-Ecological Resilience to Coastal Disasters". Science, vol. 309, pp. 1036-1039.

The article is labelled as "Viewpoint". Therefore, it does not claim to have scientific value and it should not referenced as having scientific value. As such, given that the criticisms here are scientific, these criticisms might not be valid, unless claims are made that the article is scientific; however, it is also important to remember that even a "viewpoint" should check basic facts and should demonstrate an awareness of the scientific literature.

The statement "The concept of resilience is a profound shift in traditional perspectives, which attempt to control changes in systems that are assumed to be stable, to a more realistic viewpoint aimed at sustaining and enhancing the capacity of social-ecological systems to adapt to uncertainty and surprise." (page 1036) shows a lack of knowledge of indigenous societies and of the literature on indigenous societies. Authors such as Eric Waddell, William Torry, and Tony Oliver-Smith have published extensively, dating back to at least the 1970's, on how societies, including ecosystems, can deal with aspects of uncertainty and surprise. To suggest that "the concept of resilience" is responsible for this viewpoint is disingenuous, neglecting those societies who practice the concept espoused while ignoring the literature that documents this aspect.

The statement "Globally, 1.2 billion people (23% of the world’s population) live within 100 km of the coast" is a meaningless factoid that has no place in an international scientific journal. The reason is that no statement is made regarding the topography of all land within 100 km of the coast. An area could be 80-100 km from the coast, but 500 m in elevation which does make that area overly susceptible to tsunamis or storm surges. The issue here is how many people live in a coastal environment or, potentially, a coastal ecosystem. The issue is not drawing an arbitrary line at 100 km, when a kilometre itself is an arbitrary unit of measure, and miraculously declaring these people to be "coastal".

The first paragraph on page 1037 compares the impacts of a 1992 hurricane in Florida with the impacts of a 1991 cyclone (incorrectly labelled as a "tropical typhoon"; see http://www.aoml.noaa.gov/hrd/tcfaq/A1.html for the correct term) in Bangladesh. The comparison fails to account for the large body of literature on Hurricane Andrew which details significant weaknesses in what the article terms "social resilience from strong institutions, early warning systems, and a high capacity to deal with the crisis confined the impact to manageable proportions". Two examples (there are many more) are:
-In 1992, Dade County, Florida had one of the toughest building codes in the U.S.A., but much of the damage caused by Hurricane Andrew occurred because buildings were not designed in accordance with the code and because poor enforcement practices failed to uncover the problems, according to N.K. Coch, 1995, Geohazards: Natural and Human. Prentice-Hall, New Jersey.
-Elaine Enarson writes at http://understandingkatrina.ssrc.org/Enarson that "Following hurricane Andrew, the broad-based women's coalition Women Will Rebuild Miami (born the day funds were directed toward the Chamber of Commerce and away from child care) struggled for months, and unsuccessfully, to earmark just 10 percent of relief funds for girls and women". She references E. Enarson and B.H. Morrow, 1998, "Women will rebuild Miami: a case study of feminist response to disaster," pages 185-200 in The Gendered Terrain of Disaster.

In this Bangladesh-Florida comparison, the authors contradict their own analysis by focusing on "social resilience" rather than considering both society and ecosystems. The difference between Florida's and Bangladesh's ecosystems, and the differences in society's interactions with those ecosystems, must be addressed in any such comparison. Additionally, the authors failed to take into account that the worst of Hurricane Andrew just missed Miami. If the full force of the hurricane had struck Miami, it is conceivable that casualties and destruction would have been several orders of magnitude greater.

Finally for Hurricane Andrew, the authors report that "23 people lost their lives" which contradicts the literature on the topic. See, for example:

CDC. 1992. "Preliminary Report: Medical Examiner Reports of Deaths Associated with Hurricane Andrew--Florida". JAMA (Journal of the American Medical Association), vol. 268, issue 13 (7 October), p. 1644. This paper reports 32 known deaths plus one person missing and presumed dead.

Combs, D.L., R.G. Parrish, S.J. McNabb, and J.H. Davis. 1996. "Deaths related to Hurricane Andrew in Florida and Louisiana, 1992". International Journal of Epidemiology, vol. 25, no. 3, pp. 537-544. This paper lists 44 deaths in Florida officially associated with Hurricane Andrew (15 "directly related to the hurricane" and 29 " indirectly related"), plus 11 resident deaths in Louisiana and 6 non-resident deaths in the Gulf of Mexico.

The statement "The resilience (or conversely, the vulnerability) of coastal societies..." (page 1037) makes assumptions about the definitions of "resilience" and "vulnerability", that the two are opposites, even though these definitions are not fully supported by the literature. The view that resilience and vulnerability are direct opposites is a minority view. Two examples (there are many more) of literature presenting other views of the relationship between resilience and vulnerability are:

Klein, R.J.T. and R.J. Nicholls. 1999. "Assessment of Coastal Vulnerability to Climate Change". Ambio, vol. 28, no. 2, March 1999, pp. 182-187.

Lewis, J. 1999. Development in Disaster-prone Places: Studies of Vulnerability. Intermediate Technology Publications, London, U.K.

Page 1037 includes the phrase "global tourism (an ecosystem service)". How is cultural and historical tourism an ecosystem service? S3 tourism (sun, sea, and sand) could arguably be considered an ecosystem service, especially with climate considered to be part of the ecosystem, except that the definition starts to break down when the beaches are artificially constructed and maintained--unless it would be considered to be a service from an artificially constructed ecosystem.

The authors state "Crucially, the causes of vulnerability are embedded in the political economy of resource use and the resilience of the ecosystems on which livelihoods depend" (page 1037). What about non-resource related oppression (including sexism and racism), corruption, power, control, injustice, and inequity being crucial causes of vulnerability? For example, see the work of Eric Waddell, Anthony (Tony) Oliver-Smith, Betty Morrow, Andrew Maskrey, James Lewis, Allan Lavell, Kenneth Hewitt, Susanna Hoffman, Maureen Fordham, Elaine Enarson, and Jean Copans, none of whom is referenced in the article.

The statement that the 26 December 2004 earthquake was "the second-largest earthquake in the instrumental record" (page 1037) contradicts the USGS data at http://earthquake.usgs.gov/eqcenter/eqarchives/year/mag8/magnitude8_1900_mag.php which places this earthquake third amongst earthquakes since 1900.

The statement "Coastal areas in parts of Indonesia, Thailand, and Malaysia closest to the epicenter received little or no warning" (page 1037) is obviously incorrect for the reason that those areas closest to the epicentre received significant warning through the immense ground shaking. Subsequent research backs up this point showing that this form of warning saved numerous lives, where the shaking was heeded as warning. For example, see:

McAdoo BG, Dengler L, Prasetya G, Titov V (2006) "How an Oral History Saved Thousands on Indonesia's Simeulue Island during the December 2004 and March 2005 Tsunamis". Earthquake Spectra 22(3): 661-669.

The authors acknowledge this example on page 1038, mentioning Simeulue Island explicitly, so it is unclear why they believe that the shaking was not a warning. In discussing this example, however, the authors give no evidence and no detail for their claim of "institutional preparedness for disasters" on Simeulue (page 1038).

Consider the statement "A key lesson is that resilient social-ecological systems reduced vulnerability to the impacts of the tsunami and encouraged a rapid, positive response" (page 1037). The identified "key lesson" is a truism within the article's framing because it stems directly from the definitions provided earlier in the article. That sentence is meaningless circularity. Another example of similarly meaningless circularity is "wherever ecosystems have been undermined, the ability to adapt and regenerate has been severely eroded" (page 1038). By definition, an "undermined" ecosystem will have diminished or eroded "ability to adapt and regenerate".

Table 1 (page 1038) places two statements in the "National and international action" column which should have been placed additionally in the "Local action" column:

(i) "Promotion of early warning networks and structures". The early warning literature is clear that warning processes start at the local level. Two examples of references (there are many more) are:

Glantz, M.H. (2003), Usable Science 8: Early Warning Systems: Do's and Don'ts, Report from the workshop held 20-23 October 2003 in Shanghai, China. National Center for Atmospheric Research, Boulder, Colorado, U.S.A., download from http://www.ccb.ucar.edu/warning/report.html

Wisner, B., Blaikie., P., Cannon, T., and Davis, I. (2004), At Risk: Natural Hazards, People's Vulnerability and Disasters, 2nd ed., Routledge, London, U.K. The older edition of this reference is provided in the article, but the authors should have updated their knowledge by reading and referencing the newer edition.

(ii) "Mitigation of human-induced causes of hazard" must occur at all levels, including the local level. Two examples of references (there are many more) are:

Wisner, B. (2001) "Risk and the Neoliberal State: Why Post-Mitch Lessons Didn't Reduce El Salvador's Earthquake Losses". Disasters 25(3), pp. 251-268. In addition to international and national influences, this paper discusses local development decisions that caused most of the earthquake deaths. The links amongst all spatial scales regarding hazard mitigation is poignant.

Twigg, J. 1999-2000. "The Age of Accountability?: Future Community Involvement in Disaster Reduction". Australian Journal of Emergency Management, 14(4): 51-58.

No references or evidence are given for the statement "The hidden success story of the tsunami was the prevention of widespread secondary mortality of injured and traumatized victims from infection and disease, due in large part to the unprecedented scale of national and international responses." (page 1038). No connection is made to the previous statements regarding "the sources of resilience of societies and their life-supporting ecosystems." It is unclear why the point regarding infection and disease is suddenly made here without any supporting material.

"Hurricanes, typhoons, and their related impacts affect societies throughout the world" (page 1038). How many arctic societies have been affected by "Hurricanes, typhoons, and their related impacts"? How many societies in central and northern Africa, central Europe, Tibet, Turkey, Saskatchewan, and Bolivia have been affected by "Hurricanes, typhoons, and their related impacts"?

The statement "The implementation of these activities followed economic and ecological impacts of three major hurricanes" (page 1038) seems to suggest that the hurricanes had no social impacts, other than the economic ones.

Page 1038 refers to Hurricane Michelle happening in 2000, but the year was 2001 http://www.nhc.noaa.gov/2001michelle.html

The statement "Even today, the lessons of implementing post-disaster planning to increase adaptive capacity do not appear to have been learned by many of the states that were impacted by Hurricane Mitch" (page 1039) is given without any references or evidence. The Wisner (2001) reference listed above would have suitable to support the authors' statement for El Salvador. Where is the evidence for Honduras and Nicaragua which are also mentioned in the article?

The references to hurricanes and typhoons on pages 1038 and 1039 neglects the consideration of other similar storms. See http://www.aoml.noaa.gov/hrd/tcfaq/A1.html The authors could have used the phrase "tropical cyclone" to cover all such storms or, if they preferred, perhaps "tropical and extra-tropical cyclones" (possibly too jargonistic) or "cyclone-type storms". The focus on only hurricanes and typhoons shows a lack of understanding regarding basic terminology in this field.

The statement "Two-thirds of the coastal disasters recorded each year are associated with extreme weather events, such as storms and flooding" (page 1039) is given without any quantitative data, references, or further supporting evidence. This statement seems to be another meaningless factoid with no place in an international scientific journal.

"The capacity of coastal ecosystems to regenerate after disasters and to continue to produce resources and services for human livelihoods can no longer be taken for granted" (page 1039). When could it be taken for granted? When has it been taken for granted?

Folke et al. 2006 GEC

Folke, C. 2006. "Resilience: The emergence of a perspective for social-ecological systems analyses". Global Environmental Change, vol. 16, pp. 253-267.

The paper's abstract states "This article presents the origin of the resilience perspective and provides an overview of its development to date". That is further elaborated in the paper as "The purpose with this paper is to provide an overview of the emergence of the resilience perspective and the context within which it has developed. It will not be a paper for those that look for simple, clear-cut explanations about resilience in a technical sense. The paper is more of a narrative that starts with presenting the ecological or ecosystem resilience perspective, and its early influence on other disciplines and how it contrasts with more narrow interpretations of resilience in ecology." (page 254). Many of the comments below refer back to this statement of the paper's purpose.

Despite the paper stating that its purpose "is to provide an overview of the emergence of the resilience perspective and the context within which it has developed" (page 254), the author does not consider the work of the authors Ben Wisner, Eric Waddell, William Torry, Douglas Paton, Anthony (Tony) Oliver-Smith, Phil O'Keefe, Betty Morrow, Andrew Maskrey, James Lewis, Allan Lavell, David Johnston, Michael Glantz, Kenneth Hewitt, Susanna Hoffman, Maureen Fordham, Anne Eyre, Elaine Enarson, Zenaida Delica Willison, Ian Davis, Fred Cuny, Jean Copans, Terry Cannon, and Virginia García Acosta. That work, and the work of many others, extends from the present back to at least the 1970's, using field-based practical evidence to match with theoretical ideas to better understand and evidence individuals and communities showing characteristics of resilience in the face of different environmental, often linked to social, changes and challenges, including within the context of "sustainable development" (e.g. page 260) as that literature evolved. Since the author specifically references anthropology and culture (page 255), the authors listed in this paragraph must be directly relevant to the paper's discussion and perhaps the paper's author does not fully know the resilience literature.

It could be argued, perfectly fairly, that no paper can cover all literature. Yet with such a high level of omissions, the paper has not matched its own mandate. In particular, the author finds room to reference more than 30 of his own publications (6 of them for which he was the first author), suggesting that plenty of space would have been available to diversify the sources used in the paper and to demonstrate deeper and broader knowledge of the literature.

Interestingly, after presenting his own view of the history of the field, the author complains that "all this work, especially in the early days, was largely ignored or opposed by the main stream body of ecology" (page 256). He makes the same mistake by ignoring (or, it could be possible, opposing) the work of the authors listed above.

Similar comments regarding the author's lack of knowledge of the literature could be made about the final paragraph on page 256 and the following text regarding the superficial discussion on "engineering resilience". Plus, as epitomised in Table 1, but as is evident throughout the paper, the work on psychological resilience is overlooked in the paper, despite its evident application to addressing "social-ecological resilience" (pages 259 and 263), with two examples (there are many more) being:

Paton, D. and D.M. Johnston. 2001. Disasters and Communities: Vulnerability, resilience and Preparedness. Disaster Prevention and Management, vol. 10, no. 4, pp. 270-277.

Ronan, K.R. and D.M. Johnston. 2005. Promoting community resilience in disasters: the role for schools, youth, and families. Springer, New York, New York, USA.

The paper states "In 1991, the newly established Beijer International Institute of Ecological Economics initiated a research program on the ecology and economics of biodiversity loss (Perrings et al., 1992), in particular, the role and value of biodiversity in supplying ecosystem services (Barbier et al., 1994), without which civilization could not persist (Ehrlich and Ehrlich, 1992)." (page 258). Why is this statement made, but references to people who were studying such topics earlier are not given, with two examples (there are many more) being:

McNeely, J.A. 1988. Economics and Biological Diversity: Developing and Using Economic Incentives to Conserve Biological Resources. IUCN, Gland, Switzerland.

Norgaard, R.B. 1987. Economics as mechanics and the demise of biological diversity. Ecological Modelling, vol. 38, nos. 1-2, pp. 107-121.

As noted above, it would be impossible for all work to be covered in a single paper, but also as noted above, when the author clearly focuses on his own work at the exclusion of other important material, then there is definitely room to diversify sources and to ensure that the paper matches its stated mandate. The same concern applies to the paper's statement that "The resilience perspective was revived in the early 1990s through research programs of the Beijer Institute" (page 260) as if none of the authors referenced above had worked on resilience at all before the Beijer Institute started their own work "in the early 1990s".

Page 260 states "despite the huge literature on the social dimension of resource and environmental management, most studies have focused on investigating processes within the social domain only, treating the ecosystem largely as a 'black box' and assuming that if the social system performs adaptively or is well organized institutionally it will also manage the environmental resource base in a sustainable fashion." No references or quotations are given to support that statement. That statement does not account for the decades-long list of research publications studying indigenous societies accepting as a main premise the links and interchanges between society and the environment. Some (not all) of the authors in the list above would be in this category, with two specific examples being Eric Waddell and William Torry. The rich and lengthy literature on indigenous islanders is particularly poignant with regards to societal-environmental connections and integrations (see also the two journals mentioned in the next paragraph).

Similarly, pages 261-262 state "Berkes and Folke (1998) used the term social-ecological system to emphasize the integrated concept of humans-in-nature and to stress that the delineation between social and ecological systems is artificial and arbitrary". These ideas have been prevalent in many indigenous cultures for millennia (two examples of journals with many references supporting this statement are "Human Ecology" which started in 1972 and "Pacific Affairs" which started in 1927). It is unclear why the author attributes to himself what many others knew and implemented long before 1998--and, in fact, many others such as the authors listed above have published on exactly this topic long before 1998.

Social capital receives three mentions, on pages 260-261, but not critiques of social capital, with two examples (there are many more) being:

Baron, S., J. Field, and T. Schuller. (eds.) 2000. Social Capital: Critical Perspectives. Oxford University Press, Oxford, U.K.

Navarro, V. 2002. "A Critique of Social Capital". International Journal of Health Services, vol. 32, no. 3, pp. 423-432.

It is strange that a paper purporting to give an overview of history does not mention Robert Putnam with regards to social capital.

The paper states "A vulnerable social-ecological system has lost resilience. Losing resilience implies loss of adaptability" (page 262). These statements appear to be given as truisms, even though there are many views and much discussion by the authors listed above, that provide different perspectives on such claims. As such, the paper has not fulfilled its mandate. The same concern arises with the sentence on this page that begins "In resilience work adaptability is referred to as the capacity of people in a social-ecological system to build resilience through collective action". Only in some resilience work, not resilience work in general. Later on this page, the sentence starting "The resilience perspective emerged from..." would be fair if written as "One resilience perspective emerged from..."

The paper states "The implication for policy is profound and requires a shift in mental models toward human-in-the-environment perspectives" (page 263). Does the author believe that "human-in-the-environment perspectives" have never been involved in research, policies, practices, and advocacy relating to reducing chemical pollution including persistent organic pollutants, stopping ozone depletion, reducing greenhouse gas emissions, reducing disaster risk, and tackling harmful infrastructure development, and protected area management?

Janssen et al. 2006 GEC

Janssen, M.A., M.L. Schoon, W. Ke, K. Börner. 2006. "Scholarly networks on resilience, vulnerability and adaptation within the human dimensions of global environmental change". Global Environmental Change, vol. 16, pp. 240-252.

1. Choices of references

The paper states (p. 241) "our goal is to objectively identify major research topics, experts, papers, etc., in the three knowledge domains of interest" which are vulnerability, resilience, and adaptation. Yet in neither the paper nor the supplementary material nor the full online database of references, could I find references to major experts and publications on this topic from those experts including people such as Gustavo Wilches-Chaux, Eric Waddell, Jean Tricart, Anthony (Tony) Oliver-Smith, Claude Meillassoux, Andrew Maskrey, Elisabeth Mansilla, James Lewis, Allan Lavell, Susanna Hoffman, Elizabeth (Betsy) Hartmann, Robert

Geipel, Maureen Fordham, Elaine Enarson, Zenaida Delica Willison, Jean Copans, Virginia García Acosta.

In the database, I could find only one publication from each of Phil O'Keefe and Michael H. Glantz despite their decades-long careers and numerous publications on exactly the topics being covered. Neither of these authors is referenced in the paper.

As well, in the paper, in the supplementary material, and in the database, perhaps I missed key publications of authors who are cited? A few examples, and please correct me if I missed any listed references, are:
(i) Hewitt's 1997 book is cited, but not K. Hewitt (ed.), 1983, Interpretations of Calamity from the Viewpoint of Human Ecology, Allen & Unwin, London, UK.
(ii) Blaikie et al. (1994) is cited, but not the second edition, B. Wisner, P. Blaikie., T. Cannon, and I. Davis, 2004, At Risk: Natural Hazards, People's Vulnerability and Disasters, 2nd ed. Routledge, London, UK. The need to consider second editions is exemplified in the paper's footnote 4 where the authors "combined citations referring to the 1978 and 1993 editions" of Burton et al.
(iii) Cannon is cited as part of Blaikie et al. (1994) but not his other work on vulnerability and dealing with vulnerability.

I acknowledge the statements in the paper that a complete literature search and compilation "seems to be impossible" (p. 241) and that "we might have missed important contributions" (p. 243). In that case, the paper's suggestion that it covers the seminal literature is inaccurate, because so many seminal pieces and authors were missed making it appear as if the paper's stated goal might not have been met.

These concerns could potentially suggest that the method used to determine the "seeds" for the paper's analysis did not yield a robust result (and hence starting point) due the significant bias shown in Table 2 of the paper. The bias is evident in some authors appearing multiple times while several authors mentioned above do not appear at all--even though the authors mentioned above are certainly categorized as "referred to frequently by scholars publishing on resilience, vulnerability and adaptations in HDGEC" (p. 241) and would be identified by "experts in the field" (p 241) depending on which experts were asked. I make that assertion based on evidence, because the list above of authors absent from the paper was extracted from my own informal poll of "experts" on key authors in this same field. Some other authors not listed overlapped with the paper's suggestion for authors, while some of the more prominent "experts" mentioned in the paper were disparaged by my "experts".

I am not suggesting that one of us is right and that the other is wrong. Instead, I am noting that the process of expert elicitation here is fundamentally subjective and highly prone to bias. Any conclusions from an analysis based on these expert views could not be claimed as being objective and, due to the flawed starting point, might have limited robustness and usefulness.

I also note that many of the "seeds" are papers from authors who appear in the paper's acknowledgements as "providing suggestions on relevant literature". Again, that is not exactly the "objective" approach stated in the paper. I comment further on self-citations below in point 3.

Furthermore, statements such as "Liverman's (1990) work connected the term vulnerability to global environmental change" (p. 248) are interesting, considering that, the year before, James

Lewis (who is not mentioned at all) connected Tuvalu's vulnerability to the threat of sea-level rise in three similar publications:
Lewis, J. 1989. "Sea-level Rise: Some Implications for Tuvalu". Ambio, vol. 18, no. 8, pp. 458-459; Appropriate Technology, vol. 16, no. 2 (September), pp. 26-28; and The Environmentalist, vol. 9, no. 4, pp. 269-275.

2. The history of the terms

The statement "The concept of resilience was introduced by Holling (1973) in the field of ecology" (p. 241) seems rather odd to me. See Paul L. Errington's review of the book "Natural Communities" by Lee R. Dice in "Science", vol. 117, no. 3028. (Jan. 9, 1953), p. 43. Errington writes "As concerns various aspects of community equilibria, population dynamics, etc., I am disappointed that Dice has not recognized the increasingly voluminous evidences of resilience and compensatory or automatic adjustments that represent substantial departures from conventional ideas of the impact of living things upon each other". Is it possible that resilience was embedded in the field of ecology long before Holling (1973)?

The statement that for vulnerability, "there has been little...focus on mathematical models" (p. 241) is also strange to me, as would be evident from speaking to relevant people in the insurance and reinsurance sectors along with companies such as RMS and ABS who make a substantial portion of their income from developing, selling, and supporting mathematical vulnerability models. They also publish some work in academic journals, as shown by the publications on insurance and/or vulnerability calculations of, for example, Andrew Coburn, Robert Muir-Wood, Robin Spence, and Russell Blong (one of Blong's papers is listed in the database). I am curious why this extensive work on mathematical models of vulnerability was considered to be peripheral?

It is good to see, though, that the paper somewhat acknowledges the long pre-1990 history of work on adaptation.

3. Equating citations with influence

It is always important to be cautious about equating citation numbers with quality, influence, or importance of the publication.

In particular, perhaps I missed discussion in the paper regarding:

(i) The removal of self-citations; that is, considering the effect of an author citing themselves which is hardly an unbiased or objective judgement of a paper's influence, importance, or quality. I would also suggest that any paper authored by the editor of the journal in which it is published should automatically be eliminated because there is an obvious conflict of interest with regards to the so-called unbiased and objective peer review process. See also the extensive academic literature discussing biases in and limitations of the scientific peer review process.

(ii) Citing a paper to criticize it, which can increase the paper's citations while diminishing the paper's influence.

(iii) The self-perpetuation trait of citations. For example, as above, Holling (1973) was perhaps not the first publication to introduce resilience into the field of ecology, but the more people who make that claim, the more that Holling (1973) will appear to be the relevant reference on this topic and the more that Holling (1973) will be re-referenced. Similarly, in choosing to cite certain authors in the paper, but not the authors in the list given above, is it possible that the paper might affect the authors' relative influence irrespective of the authors' importance and quality?

I appreciate that, arguably, the above three points might not be a significant proportion of citations and might not affect the paper's results. Without discussing the issues and running appropriate analyses, we will never know.

4. Influence vs importance

Reliance on citation indices to determine the influence of publications and the influence of authors has potentially diminished significantly with the growth of the internet as a scholarly networking and scholarly debate approach. I am curious why the paper (published in 2006) did not factor in, or at least mention, the internet in terms of developing scholarly networks and creating authors and publications of influence--especially given the number of websites mentioned in the paper and the (entirely appropriate) use of email lists to try to increase the influence and citation count of the paper? Perhaps the internet does not create more scholarly influence than citations, but without considering it, we will not know the answer.

In any case, as is hopefully evident from comments throughout this critique, the influence of a publication is not the same as that publication's importance or quality. An author might be vocal, send a lot of emails, frequently self-cite, and/or publish mainly in the journals which they edit, but that does not indicate the importance or quality of their work. Naturally, importance and quality are subjective and qualitative criteria. Could they be more useful to analyze than the quantitative but nonetheless still subjective determination of influence through specific citation indices and citation numbers?

As well, the reliance on citations in peer reviewed academic literature neglects the influence which a publication might have by being cited in course material, policy documents, consultancy reports, and websites amongst other such material. Is academic influence or non-academic influence more important?