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Commentary

Influencing the perception of what and who is important in ecological economics

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ABSTRACT

In a recent article using citation analysis Hoepner et al. (2012) make strong claims to have identified the most influential works, authors, journals and institutions in a hypothetical field they call environmental and ecological economics. This paper shows that their work is biased by its framing, far from the non-subjective approach they claim and highly sensitive to minor data errors. More than this the exercise can be seen as another example of how ecological economics is susceptible to suppression by a dominant mainstream economic perspective which attempts to override, belittle and dismiss a whole range of alternative thought, including that which is heterodox, interdisciplinary, from the natural sciences and based in the non-economic social sciences. Ecological economics is not the same as agricultural, resource or environmental economics, and if it were it would be pointless. Neither can it be understood or treated as a subfield of resource and environmental economics, as done by Hoepner et al. (2012). I argue that the study promotes a limited perspective on social, environmental and economic problems that includes an implicit political and ideological framing. What is most influential, important or high quality in ecological economics is certainly not defined by such work.

1. Introduction

Hoepner et al. (2012) present an analysis that they claim identifies the most influential articles, journals, authors and institutions in a hypothetical field they term environmental and ecological economics. They state: "We follow an increasing literature that frequently employs citations for their virtues as non-subjective, reasonably comprehensive measure of study influence". This non-subjectivity presumably implies an objective or at least scientific approach for their analysis. There are, however, several problems with such claims and, indeed, both flaws in the analysis and extreme sensitivity of their results which go unanalysed. Even more fundamentally, the "sample selection process" consists of criteria which exclude outlets for much of the interdisciplinary work done by ecological economists and specifically work from outside of mainstream economics.

The critique presented here will address the framing of the Hoepner et al. (2012) study as well as the calculations employed. The argument made is that the study is clearly biased towards environmental and resource economics and has little to do with the much broader and heterodox field of ecological economics. This is symptomatic of a trend amongst resource and environmental economists for treating ecological economics as a subfield of their own (for example see Auffhammer, 2009: 259, who also places Herman Daly under this mainstream neoclassical specialisation). The relative citation strength of the journal *Ecological Economics*, especially compared to most economic journals, proves an especially tempting territory for them to claim.

Before going further, a general health warning on the increasing use of citations is necessary. Citations are susceptible to being employed in highly discriminatory ways within academia and by those trying to 'manage' academia through quantitative indicators and ranking exercises. As noted by Costanza et al. (2004), there are numerous issues making citation analysis of limited use as a means for identifying or assessing influence in a field of knowledge. The problems include: ignoring the influence of work outside academia, focussing on journal articles to the exclusion of other publications (e.g. books, book chapters, reports, policy briefs), bias against more recent work, and confusing the quantity of citations with the quality of work. Hoepner et al. (2012) cite the first three of these issues but ignore the last, which, amongst other things, raises the need to distinguish between popularity and importance.

In addition, regardless of the problems noted, Hoepner et al. (2012) go on to concentrate exclusively on citations to academic journal articles, and further restrict their analysis to those published between 2000 and 2009. Ecological economics has some foundational books (e.g., Daly, 1977; Georgescu-Roegen, 1971) and more generally the significant role of books within ecological economics has been noted by other citation analyses (Costanza et al., 2004; Ma and Stern, 2006). However, books also often take time to mature and a study which focuses on immediacy would in any case hardly be predisposed to consider the influence of such work. The study setup therefore already seems troublesome before we even get into the details.

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This comment is specifically concerned with the misrepresentation of ecological economics, although several of the criticisms also bring into question using the study as representative of influence in environmental economics. The next section argues that the way in which these two distinct fields are merged is highly misleading. In addition, the prescriptive selection of source journals by Hoepner et al. (2012) is shown to be in effect a means of unscientifically censoring data. In Section 3, the details of calculating what and who is influential is brought into question and the sensitivity of the method used exemplified with my own work. Just like other numerical indicators, the creation of summary single number citation indexes can easily obscure more than it reveals. Section 4 turns to the claims made for influential organisations and argues that here the study enters into implicitly recommending a political and ideological perspective on what is meant to be good.

2. Framing the Study

A key assumption made by Hoepner et al. (2012) is that ecological economics can be treated as just another specialist area of mainstream economics and is interchangeable with resource and environmental economics. They appeal to Ma and Stern (2006) for support on the overlap, but ignore that study's approach and it's findings on differences. Ma and Stern use the *Journal of Environmental Economics and Management* (*JEEM*) and *Ecological Economics* as representing two separate fields which they then compare and contrast. On this basis they note “major differences” between the two fields (Ma and Stern, 2006: 499). They show that *JEEM* cites more journal articles while *Ecological Economics* pays greater attention to other publications (e.g. books, chapters, government reports). Resource and environmental economics is credited with being far narrower in focus than ecological economics and note is made of references by articles in *JEEM* concentrating on specific economics journals and individual publications. They also find little overlap amongst the most cited articles in the two journals with just four held in common out of 56 listed. They conclude that “the emphasis given to different topics is very different in the two fields and there is even more difference at the level of specific papers and authors who are most cited” (Ma and Stern, 2006: 505).

Despite this the analysis by Hoepner et al. (2012) proceeds to address a hypothetical, and I would say non-existent, field called environmental and ecological economics. Thus, ecological economics is, from the outset, merged by the framing into being identical with or at least highly similar to neoclassical environmental and resource economics. They reference two studies as having conducted citation analysis of journals in “environmental and ecological economics” (Hoepner et al., 2012: 194), when in fact these studies explicitly cover environmental and resource economics (Auffhammer, 2009; Rousseau et al., 2009). This again indicates scant regard for the differences. The assumption of a single field allows the authors to then restrict their entire analysis to a handful of journals which are presented as representative, but are mainly concerned with agricultural, resource and environmental economics. An influential journal must already be in their preselected list of journals and an influential article, author or institution must be published in one of these journals.

Elsewhere, I have criticised this type of misclassification of ecological economics (Spash, 2012), and explained why the prevalence of the tools and methods of resource and environmental economics within ecological economics is not something to celebrate (Spash, 2011). Indeed, from a heterodox economic perspective, this is just another case of territorial annexation by the mainstream, and is far from being either unique or new (see Lee, 2009). This is just how the political battles within economics operate and how, until now, the non-conformist minority of blasphemous economists has been overridden by the conformist majority.

Consistent with such an approach, the analysis of Hoepner et al. (2012) is based on a narrow set of qualifying criteria for selecting the shortlisted journals. Indeed, the first framing criteria is that a journal be affiliated with the word “economics” either in its title or under the Thomson Reuter's Web of Knowledge (WoK), formerly ISI, classification category of that name. So that helps remove such journals as *Science*, *Nature*, *Ecology & Society* and *Environmental Values* along with a range of disciplines, and their journals, such as conservation biology, environmental studies, planning, geography, philosophy, ethics, political science, social psychology and so on. Highly relevant work in these areas is quite simply ignored.

The second and third qualifying criteria use the journals *Ecological Economics* and *JEEM* as definitional. For a journal to be selected to the next stage it must be cited by and make reference to one or both of these two journals. An arbitrary threshold level is employed to classify significance of the associations. In this process of judging the cited journals to include, Hoepner et al. (2012) do not conduct original up to date analysis but use that of Ma and Stern (2006), which only covers the period 1994 to 2003.

Fourth, the final hurdle for a journal to make the list is having a focus on environmental and ecological economics. The authors state that they do so if the journal carries an environmental or ecological concept in its title. What this means in practice is justifying journal inclusion/exclusion on a set of four keywords (besides environmental and ecological), namely: agriculture, land, resources and energy.

In order to understand the bias inherent in this four stage process consider, for example, that a journal like *Energy Policy* is excluded while one called *Energy Economics* is included. More to the point, the field of ecological economics is not the same as environmental and resource economics or agricultural economics, or energy economics. What turns out to be the focus of this study is a set of outlets primarily aimed at or suited to neoclassical economists. Yes, that includes *Ecological Economics*, which has itself published much neoclassical and mainstream work despite its title, affiliation to the international society and the original aims of its community. As I have noted elsewhere (Spash, 2009 especially pp.19–20), the journal *Ecological Economics* is in itself a poor and misleading guide to what ecological economics is about, exactly because it has devoted so much space to mainstream methods, studies and approaches.

A journal network analysis illustrates the bias in adopting the approach of Hoepner et al. (2012). Such an analysis has been undertaken by Spash and Ryan (2012) for the ecological economics community and compared with that for environmental and resource economists. Results published to date concern primary data that were collected from participants at conferences of the European Society for Ecological Economics (ESEE), the Association for Heterodox Economists, and the European Association of Environmental and Resource Economics. As part of a survey, they were asked to state the three journals that they read most often. The data show that ecological economists read a far wider and more diverse set of primary journals than do resource and environmental economists. In addition, the journals are different between the two groups. Both findings are consistent with Ma and Stern (2006). For the ESEE the core journal held in common is *Ecological Economics* with secondary nodes being *Science*, the *Journal of Industrial Ecology*, *Environmental Values*, and *Ecology & Society*. Other journals such as *Nature*, *Land Use Policy*, *Energy Policy*, *Environment & Planning C*, and the *International Journal of Sustainable Development* are also popular. In contrast the resource and environmental economists read relatively few journals and primarily *JEEM* and then *Environmental & Resource Economics* and *Ecological Economics*, which, as noted, has become a recognized outlet for their work. The core mainstream journal, the *American Economic Review* appears as a secondary node. What they do not regard as very important are the type of interdisciplinary journals favoured by ecological economists, or heterodox economic journals or natural science journals. Indeed, Spash and Ryan (2012) found only a dozen journals held in common between these communities, besides *Ecological Economics*, and even those often had a minority readership amongst respondents.

In summary, ecological economists cannot be expected to pay attention to or publish in the same journals as resource and environmental economists, while the latter have been allowed a significant presence in the journal *Ecological Economics*. There is then a straight forward bias in the sampling method employed by Hoepner et al. (2012). Many highly cited ecological economists publish in non-economic and non-neoclassical economic journals, such as those in the larger fields of natural science and energy policy. However, their work is excluded from being regarded as influential by Hoepner et al. (2012) precisely because it is regarded as being published outside the bounds of relevance to neoclassical environmental and resource economics.

3. Calculation Errors and Sensitivity

Besides the framing problem, the actual calculations in Hoepner et al. (2012) appear flawed and highly sensitive to design choices. The period of the study is from 2000 to 2009 for publication selection. Although this study was first submitted to the journal on the 10th January 2012, the authors claim they chose to "end our sample as recent as end of 2009 to ensure that our citation analysis is as timely as possible". In addition, they both cut-off eligible citations to those articles and stopped collecting those citations on the same day. This was not at year or even month end date, but for some unknown reason the 19th September 2010; one exception is made for the journal *Environmental & Resource Economics*, for which the period was extended to 28th September. The fact that the closing date applies for both limiting and stopping data collection leads to problems with the analysis (as illustrated below). On the divergence between sources the authors state: "This nine day delay appears inconsequential but to ensure good conservative research practice the subsequent age adjusted citation statistics have been adjusted to reflect this nine day delay." (meaning they add nine days to their time denominator). While this may at first appear inconsequential, just like the choice of 19th September, this is far from clear (as will also be exemplified below), because of the extreme sensitivity of the results of their method to the inclusion/exclusion of even one or two cites (which might easily appear within such a nine day period).

For an article, and so author, to make the influential list they must first have been published in one of the 14 selected journals within the set nine year time horizon. Next the articles must have been cited five or more times per annum since being published. As they do not use an annual cycle, this results in employing a formulae to calculate the time period over which an article has been in print, i.e., the time that has expired from the date (month) of an article's publication to the date (day) they selected to download cite information. This means that the maximum period would be 10.72 years for an article published in January 2000. Note, the over precision on closing date is not matched by the use of monthly data for publication; so whether an article appeared in press at the start or end of a month makes no difference, but if it slips into the next month it does impact the calculations and due to the sensitivity of the method this can impact the results.

Instead of this overcomplicated approach the authors could easily have taken the year end as a cut-off date for citations. This would have simplified the analysis. If, in addition, they had delayed collecting citation data, for say six months, they would have avoided a second problem; that is the inaccuracy in their data resulting from the fact that the Thomson Reuter citation data takes many months to catch-up with journal publication. So another arbitrary element has been added; namely what Thomson Reuter happen to have put online by the 19th (or 28th) of September 2010.

In addition, the citation analysis is restricted to what Thomson Reuter regards as worthy of inclusion in their commercial database. Alternative databases by other publishers favour different journals. In my personal experience Scopus, for example, appears slightly more inclusive, giving higher citations. This means citations for the same author and article can vary between databases and again this can be enough to substantively influence the main results from the study by Hoepner et al. (2012) due to the sensitivity of their method.

While the authors do make some qualifying remarks about subjectivity, the formulae used and data presentation give all the appearance of a rigorous scientific approach. Thus, we get precise numbers to two decimal places which appear as if objective and high quality, and which are used to create tables giving highly ordered rankings. Just as Funtowicz and Ravetz (1994) note in another context, this type of precision is not necessarily a sign of quality and over precise figures can be used to hide inaccuracies and uncertainties.

In order to explore the problems with the calculations I take my own case as an example. This is obviously motivated by convenience and personal interest, but the case is also illustrative of general problems and informative with regards to the detailed calculations underlying the results of Hoepner et al. (2012). The issues raised are clearly generalizable and relevant for any number of other authors. In my case I published 17 articles in peer reviewed journals between 2000 and 2009, but only seven of these articles appeared in two of the 14 journals selected by Hoepner et al. (2012); five in *Ecological Economics* (Spash, 2000, 2007a, 2007b; Spash and Vatn, 2006; Spash et al., 2009) and two in *Land Economics* (Spash, 2006, 2008). So, for a start we can see how the majority of ecological economists' work can be readily excluded, even if they are trained economists publishing relevant work in peer reviewed WoK accredited journals.

Table 1 shows the various data, for the seven publications, necessary for calculating the time-adjusted citation rate, which is key to the results of Hoepner et al. (2012). Here the maximum time (Max Time) for a publication to gather citations is 10.72 years; the publication time (Pub Time) is a point between 0 and 10.72 calculated as the sum of the publication (Pub) year (yr) measured from a zero base in 2000 and if relevant the month (mth) as a fraction of a year; when this is subtracted from the maximum time this gives the time in print in years (yrs). Cites are the total number of citations up to and including September 2010, and when divided by time in print (cites / time in print) this gives the time-adjusted cite score.

The pseudo-scientific character of the results can be illustrated in different ways. Consider the two decimal place designation, copied from Hoepner et al. (2012). If this is reduced to one decimal place then suddenly the first article listed becomes valid as a "most influential study". Dropping the pretence of accuracy further (no decimal places and rounding-up) would clearly add more publications and require grouped ranking categories (e.g., all authors scoring 5, 6, 7 and so on) rather than the long precisely ordered list presented by Hoepner et al. (2012). This would also limit the susceptibility of the results to influence from minor changes in the number of citations, a problem discussed further below.

In fact, the results in Table 1 do not match those given by Hoepner et al. (2012) in their Table 2 showing the most influential studies. They have included only one of the three eligible articles as most influential, in their Table 2, namely Spash and Vatn (2006). This raises the question as to the grounds for the divergence and exclusion of apparently eligible articles.

A primary issue is the failure of accuracy in accounting for actual citations. The authors chose an arbitrary cut-off date for citations of 19th September 2010, while the data reported here are for the complete month of September 2010 (as of May 2012) showing in the WoK database. Indeed, even the one article they include has a lower score (5.92) than calculated here (i.e., they miss three citations). Perhaps the missing citations appeared in journals published later in September than the 19th, or the database was updated later by Thomson Reuter. In either case, the Hoepner et al. (2012) data does not represent the actual citations existing in journals according to WoK.

Table 1
Example of the Hoepner et al. method for judging influential articles.

	Authors	Cites	Max Time (yrs)	Pub (yr)	Pub (mth)	Pub Time (yrs)	Time in Print (yrs)	Cites/Time in Print	Fractional Cites
Spash (2000)	1	49	10.72	0	8	0.67	10.05	4.88	
Spash (2006)	1	16	10.72	6	11	6.92	3.80	4.21	
Spash and Vatn (2006)	2	25	10.72	6	12	7.00	3.72	6.72	3.36
Spash (2007a)	1	22	10.72	7	9	7.75	2.97	7.41	7.41
Spash (2007b)	1	14	10.72	7	9	7.75	2.97	4.72	
Spash (2008)	1	11	10.72	8	8	8.67	2.05	5.36	5.36
Spash et al. (2009)	6	6	10.72	9	2	9.17	1.55	3.87	

These problems are particularly relevant for articles on the borderline of eligibility. For example, Spash (2008) is marginal and would be dropped if it loses just one citation (going down to a score of 4.88). So now we see why database choice, cut-off dates and number of decimal places become important to the results for the entire study. Any author who has articles on the borderline of the acceptability criteria can see their work included or excluded on a number of arbitrary grounds which need only affect a single citation.

However, the other excluded article, namely Spash (2007a), cannot be explained away in terms of inaccuracies and arbitrary assumptions. By the start of 2010 this article had already surpassed the criteria of acceptance for the entire time period. Indeed, this is the most cited time-adjusted article of the seven papers. The authors state: “we retrieve citation data for any document published in one of the 14 journals which is classified by WoK as journal article” (Hoepner et al., 2012: 195). This article meets that criteria. So the exclusion of the work appears to be a straight forward omission on no good grounds. As I will show next, these problems bring into question the claim to having identified the most influential authors.

Influential authors are presented by Hoepner et al. (2012) in their Table 4. In order to qualify an author must have been included in their Table 2 (i.e., been deemed to have at least one influential article). They then take the scores for time-adjusted cites and divide this by the number of authors. Next the results are summed to give a “fractional cites” score for each author. To be amongst the most influential authors again requires a score of five or more. As before the results are given to two decimal places.

Due to the above noted two missing papers, my only included work is co-authored, and the score once divided is below five, so this means exclusion from the most influential author list. If the most highly cited paper, which inexplicably does not appear in their study, was included, and the actual cites now in the Thomson Reuter database for the study period had been counted, then the score would be 10.77 or a ranking in their Table 4 as 25th in the hypothetical field. If, in addition, the other erroneously excluded paper was added the score would be 16.13 and the result would mean being ranked 10th in the hypothetical field. This is merely illustrative, because the overall outcome depends upon taking into account how the problems noted here affect all other authors' work.

Just to show how susceptible the results for the entire study are to analyst choice (e.g. cut-off dates) consider if the authors had done their calculations on the basis of year end citations and allowed the database time to be up-dated correctly. In this case Table 1 would become Table 2. While now more of the seven eligible papers meet the criteria to qualify as influential those that do have changed! The article Spash (2008) is dropped and the articles Spash (2000, 2007b) are now included. As these inclusions are single authored papers the fractional cite score over time increases to 21.43 (a ranking of 4th in Hoepner et al.). This also exemplifies another sensitivity of the results which is to single as opposed to multiple authored work. Basically according to Hoepner et al. (2012) an author has more influence if they work alone.

A final remark on calculations concerns the treatment of data implying that longevity of citations has no value. That is, a publication achieving 4.99 cites per year for 10.72 years is treated as worse than one achieving 5.00 cites for one year. The former would be excluded and the latter included as influential by Hoepner et al. (2012). I would argue that this also has little to do with establishing what is the most influential in a field of study and conflates immediacy with longevity.

Clearly all ecological economists' work is as susceptible as my own to the vagaries of the authors' choices and calculation methods. So the outcome for whom might be influential in the authors' hypothetical field, let alone ecological economics, on the basis of such citation analysis is far from clear, even if the caveats noted in the introduction are ignored. What is clear is that this work is seriously flawed from top to bottom and the outcomes highly sensitive to some rather arbitrary choices. As demonstrated using my own publications a potentially 4th ranked author could just as easily not appear at all! Yet, the point here is not that such a ranking is more valid, but rather that such distinct rankings are not warranted at all.

4. Influential Organisations and Political Ideology

Finally, there is the claim about organisational influence. The authors state: “We find that University of Maryland, Resources for the Future and University of East Anglia are always in the top 3 … they can truly be regarded as the most influential institutions in environmental and ecological economics.” These are followed by The World Bank, which they conclude is also to be grouped with these three. This basically shows that the study is identifying organisations which maintain a core of mainstream environmental economists consistently over time. These “most influential institutions” may have little or nothing to offer in terms of content for ecological economists seeking alternatives to the current political economy, because the study is totally biased towards mainstream approaches in terms of the journal selection and those approaches support a neo-liberal status quo. Thus, the University of East Anglia is famed in environmental economics for twenty years of research championing environmental cost–benefit analysis. Likewise and for much longer Resources for the Future, which also lobbies for regulation by market-based instruments. It is strategically located near the centre of government in Washington D.C., as is the World Bank which is famed for its free-market pro-growth development policies. There are also close links between the University of Maryland, the World Bank and Resources for the Future with entire careers consisting of moving from one to the other or holding joint posts. There is certainly power within these American institutions, but what of content or meaning, or indeed hope for the future? I would suggest institutions strong in mainstream economics, like these, are the least likely places to find the radical alternatives ecological economists have been calling for over several decades or in which to make them successfully operational.

The World Bank provides a good example. Herman Daly, as a widely acknowledged influential ecological economist, worked as a Senior Economist at the World Bank in the early 1990s, but left, for

Table 2
Sensitivity of Hoepner et al.'s approach to time frame and data cut-off.

	Authors	Cites	Max Time (yrs)	Pub (yr)	Pub (mth)	PubTime (yrs)	Time in Print (yrs)	Cites/Time in Print	Fractional Cites
Spash (2000)	1	53	11	0	8	0.67	10.33	5.13	5.13
Spash (2006)	1	19	11	6	11	6.92	4.08	4.65	
Spash and Vatn (2006)	2	27	11	6	12	7.00	4.00	6.75	3.38
Spash (2007a)	1	23	11	7	9	7.75	3.25	7.08	7.08
Spash (2007b)	1	19	11	7	9	7.75	3.25	5.85	5.85
Spash (2008)	1	11	11	8	8	8.67	2.33	4.71	
Spash et al. (2009)	6	8	11	9	2	9.17	1.83	4.36	

the School of Public Affairs at Maryland, stating that the organisation suffered:

> "top-down management, misguided by an unrealistic vision of development as the generalization of Northern over consumption to the rapidly multiplying masses of the South, [which] has led to many external failures, both economic and ecological. These external failures, due to faulty vision and hearing, will be considered later, but for now I just note that external failure also undermines internal morale. The unrealistic vision of development should be blamed at least as much on academic economic theorists as on World Bank practitioners." (Daly, 1999: 61)

He also later returned to give a critical address in which at one point he made clear the divergence between mainstream thought, as institutionalised at The World Bank, and his own ideas:

> I am afraid I will be told by some of my neoclassical colleagues that frugality (or sufficiency) is a value-laden concept, especially if you connect it with redistribution of scarcity rents to the poor. Who am I, they will ask, to impose my personal elitist preferences on the democratic marketplace, blah, blah, etc. etc. I am sure everyone has heard that speech. The answer to such sophistry is that ecological sustainability and social justice are fundamental objective values, not subjective individual preferences. There really is a difference, and it is past time for economists to recognize it.[1]

Try publishing that perspective in *JEEM* or see how far it gets you today in a job interview at The World Bank or Resources for the Future or in any mainstream economics department. I cite Daly to exemplify the divergence between the type of organisations and journals claimed by Hoepner et al. (2012) to be the most influential, in their hypothetical field, and what many ecological economists actually do in their work and believe in. The fact is that the influence of The World Bank, and other organisations pushing traditional economic growth and neo-liberal political ideologies, is regarded by many ecological economists as a negative and not something with which to be proudly associated. What Hoepner et al. (2012) present as a non-subjective analysis is in fact politically and ideologically loaded; whether they realise this or not is a separate issue. Their paper conflates opposing worldviews and, with an array of statistics and numbers, they quickly lose the plot completely as far as ecological economics is concerned.

5. Conclusions

The journals in the analysis by Hoepner et al. (2012) are selected on a basis which biases the whole study. A set of disputable criteria is used by the authors to narrow their sample down to 14 journals suited to those publishing mainstream neoclassical economic perspectives. The claim to be analysing influential journals across a supposed field of environmental and ecological economics is not credible. The study is far from being a "non-subjective, reasonably comprehensive" analysis, but rather limited from the very outset by unspecified preanalytic positions. The authors' selection process favours agricultural, environmental and resource economics in an approach which excludes the broader fields of interest to ecological economists (e.g. the natural sciences, heterodox economics and non-economic social sciences) and is biased against the more alternative social ecological economics movement. The authors create a hypothetical field of knowledge in which ecological economics is by definition a minor sub-area, and then employ a loaded empirical analysis to reify this position.

In addition, the calculations presented suffer from serious problems. The calculations to two decimal places are not justified by the inaccuracies in the data and their sensitivity to minor changes. Using my own publications as an example reveals several flaws in their work. Eligible papers are excluded, citation totals are inaccurate for the time period chosen, and the time period for data collection is closed without allowing data correction, but despite all this the results are presented as highly precise strictly ordered rankings. Such work cannot be taken as a credible representation of what is influential when a few cites here or there can include or drop work from being amongst the most influential papers or people from amongst the most influential authors. In addition, the credibility must be questioned further when claiming work which is cited well for one or two years is equivalent to work consistently cited over a decade (or more), even if at a lower rate. Yet, getting caught up in the academic fashion for, and accountancy culture of, citation statistics is also besides the point. Influence here is merely popularity which can be just superficial academic name dropping. A more interesting question, than how many cites is deemed to make you popular enough to be considered 'influential', concerns whom is trying to influence what and to what end?

This type of work would not be particularly interesting if there were no contention over the meaning of ecological economics and the direction of social, ecological and economic systems. However, there is a very serious point of contestation here. That the current mainstream is continually imposed upon us, as all that matters, has long been problematic for economics as a discipline and societal guide. Contesting that dominance was once the raison d'être of ecological economics. The readiness to dismiss non-conformist diversity and blasphemous difference can then be seen as either strategic or deriving from ignorance, but in either case it is unscientific misrepresentation with political consequences. This is not a reflection of a healthy debate within ecological economics, but rather a sign of attempted domination by mainstream thought to the detriment of interdisciplinary and radical alternatives. A subliminal political message being transmitted here is that, 'if you want to be influential as an environmental or ecological economist, conform to the mainstream, publish in its journals, then move to Washington D.C., get a professorship in economics at Maryland and work for organisations like Resources for the Future and the World Bank'. So perhaps in the end the article does say a lot about influence, just not what the authors claim.

[1] http://info.worldbank.org/etools/docs/voddocs/269/553/essd_hdaly.pdf.

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