Citation Metrics: Serious Drawbacks, Perverse Incentives, and Strategic Options for Heterodox Economics

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Abstract. This article reviews strategic suggestions for heterodox economic journals and heterodox economists relating to quantitative indexing. It contains a critique of Thomson Scientifics "Journal Impact Factor" as well as an integrated discussion of general strategic guidelines and specific strategic suggestions accounting for the special paradigmatic position of heterodox economics.

Introduction

Quantitative indexing and evaluation is more and more being taken for granted within the scientific community. Meanwhile, it is an established practice to evaluate researchers, departments, or proposals for research grants by relying on the "Journal Impact Factor" of their publication outlets. Consequently, also the European Commission (directly[1]) and the British Research Assessment Exercise (indirectly[2]) rely on quantitative indexing to measure the quality of research output in economics. Individual academic careers, proposals for research grants, or the future of specific departments thus depend on the impact factors gathered by the particular researchers (cf. Lee and Elsner 2008). This standard procedure, somehow surprisingly, doesn't lead to skepticism of researchers. On the contrary, many scientists seem to internalize the rules of the "ranking game" and try to succeed within a given set of institutional mechanisms:

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> That scientists . . . try to achieve as much impact-factor-capital as possible has, from my point of view, to be understood as a fundamental law . . . ." (Statement from an anonymous German medical scientist, cited according to Dobusch (2009), translation JK)

This attitude is surprising for various reasons: First, it principally accepts the separation of content from the evaluation of academic texts, since the impact-factor calculations or the rankings based upon these calculations only count citations and are not directly concerned with the "intrinsic" quality of a certain contribution. Second, there are various biases incorporated in and numerous problems associated with the standard approaches of quantitative indexing, such as the indices provided by Thomson Scientific (TS), constituting a general problem rarely discussed in the economic community. The relative discrimination of heterodox economics within such an evaluation process is on the contrary a specific problem, only partially related to the general biases incorporated in the TS indices.

These mere technical problems have to be understood as part of a larger discussion aimed at the journal culture in economics and other scientific disciplines. Since these topics are obviously related, a few remarks on this debate seem to be helpful to contextualize the arguments presented in this article. Generally, the journal culture in mainstream economics often relies on informal channels: "Top" authors often do not even "submit" their "submissions" but hand them in privately (cf. Shepherd 1995). Many authors anticipate criticism and a priori withhold or change arguments to please the editors or referees ("preference falsification"; see: Davis 2004; Bedeian 2003). Heterodox submissions seem to be, at last partially, rejected due to their methodological or political orientation (Reardon 2008). It is for these reasons that 60 percent of North-American economists agree in a survey that "a 'good-old-boy' network in the profession influences the probability of article acceptance, expressing the same strength and consensus of opinion as for school or business affiliation" (Davis 2007). "Old-*boy*" hits the point in this context since women are massively underrepresented in mainstream's editorial boards (Green 1998).

Moreover, there are documented cases of uncorrected errors in mainstream economic journals (cf. Jong-a-Pin and de Haan 2008),

strengthening the impression that review processes and editorial decisions are arbitrary to some extent. This is also evidenced by the noteworthy amount of hot papers in economics that were rejected by the peer reviewers at their first attempt to get published (cf. Gans and Shepherd 1994).

Based on these considerations, the structure of this article is the following: First, I review and discuss several drawbacks of the most important quantitative indexing and evaluation standard—Thomson Scientifics "Journal Impact Factor" (JIF) and the often perverse incentives related to this method of quality measurement (second and third sections). This illustrates that quantitative evaluation encourages the production of specifically framed articles (from the perspective of the individual author) as well as the emergence of specific citation patterns (from a paradigmatical perspective). Second, I sketch general strategic options for heterodox economists (fourth section). Based on these and other findings, related to different aspects of the scientific publication process, I try to develop a handful of specific suggestions for conscious strategic behavior of heterodox economists (fifth section). As a last step I merge the general strategic options from the fourth section with the specific and more concrete suggestions from the fifth section to develop a well-structured picture of potentially useful strategies for heterodox economists.

The Drawbacks of the Journal Impact Factor (JIF)

> Impact factors, as one citation measure, are useful in establishing the influence journals have within the literature of a discipline. Nevertheless, they are not a direct measure of quality and must be used with considerable care. (Amin and Mabe 2000: 6)

Most research assessments based on quantitative evaluation refer to Thomson Scientifics impact factor. This section provides an overview about the general problems and specific methodological biases of this mode of measurement. This critique can be used in three ways, namely, (1) to challenge the authority of the TS data and their implications, (2) to exploit the biases to improve one's performance in the ranking game, and (3) to consider the failures when designing an alternative indicator.

The standard calculation of the impact factor is relatively simple (Garfield 1994): The export citations of a certain journal in one year referring to articles published in the two preceding years is divided through the number of (citable) articles published in the two preceding years. The calculation of some impact factor in 2009 is thus equal to:

$$JIF_{2009} = \frac{Citations_{2009}\ to\ articles_{2007-2008}}{(citable)\ articles_{2007-2008}}.$$

A general reflection of this evaluation procedure can be based on the question whether such a method is a reliable and valid measure of scientific quality. The fact that citations do not only depend on the quality of the cited items but on other factors, like trends in the scientific discourse, the influence of the "Matthew Effect in Science" (Merton 1968), citations set to criticize an article or argument, or the convention to cite "standard references" generally question the validity of the JIF. For example, the introductory quotation above suggests that the JIF measures the relative power ("influence") of outlets, not their quality. A provocative but highly significant example of missing validity is provided by Woo Suk Hwang's papers on stem cell research published in *Science*: While these papers are the source of the biggest recent scandal relating to fraud in the scientific community and, thus, surely hit rock bottom of scientific quality, these papers have been highly cited (over 450 times within the TS database) and pushing the JIF of its publication outlet (*Science*). Moreover, the JIF is not a nonreactive measurement procedure, since authors and editors may anticipate the rules and biases incorporated in the calculation of the JIF and therefore change their publication behavior in order to improve their performance in the ranking game. Authors, for example, face the incentive to split their contributions in as many articles as possible in order to maximize their impact-factor capital subject to the "least publishable unit" in a particular discipline.[3] In other words, the reliability and validity of the JIF, which are understood as basic criterion of any scientific research, are highly questionable.

A more detailed critique of the JIF could focus on four different dimensions: (1) missing control variables in the JIF formula, (2)

selection problems and other data-related biases, (3) problems regarding the application of the JIF as an instrument for evaluation, and (4) a lack of transparency in combination with irreproducible results provided by Thomson Scientific.

Missing Variables

The formula shown above lacks some important control variables like the number of authors (how many people contributed to the cited articles[4]), the circulation of a certain outlet (how many people will read it or have access to the publication), the length of individual articles, or the number of self-citations. These missing variables are even more problematic when the JIF is used to compare the performance of individual researchers.

Technically, it would also seem necessary to correct for article type, since review articles or "data-rich" empirical analysis naturally attract more citations than methodological or theoretical articles (Garfield 1994; Amin and Mabe 2000: 3).

Sample Selection Biases

Scientific literature consists of different modes of publication, like books, journal articles, research reports, working papers, and so on. Thomson Scientific primarily includes international journals in its database, thereby excluding the vast majority of scientific publications. While the total number of academic journals is estimated between 50,000 and 500,000 (cf. Fröhlich 2008) only about 11,700[5] journals (along with very few books and some book series) are covered in the TS database, thereby neglecting that books are normally the publication mode with the highest citation impact (Hooydonk and Milis-Proost 1998; Cronin et al. 1997). Moreover, research reports or similar "grey literature" are not included at all. This is decisive since a "full-option-method," implying the usage of a sample of publications as big as possible, delivers results completely different from Thomson's (Hooydonk and Milis-Proost 1998).

In addition to this selection bias there are several technical problems related to the automated citation filtering process, which is based

on scanned reference lists of the included publications.[6] Generally, the whole TS system has a language bias, which discriminates against non-English publications (Adler et al. 2008: 8). Furthermore, typos in reference lists appear very often[7]—especially in names or a title stemming from languages different from the author's—and distract the automated citation filtering process. The software used by TS in this case seems not very flexible: Neither authors nor journals should change their names—otherwise all the credits gathered before the change would not be considered in the TS evaluation system.[8] For journals changing the name of the publication is comparable to a sentence of death since the impact factor of such a journal will drop down to zero for at least two years.

Another problem is the short time-span considered when computing the JIF of a certain journal: In economics this two-year time-span on average covers less than 10 percent of all citations to a certain article (Adler et al. 2008: 7). However, the alternative five-year impact factor, which covers roughly 25 percent of citations per article in economics, has recently received a slightly more prominent role within TS' Journal Citation Reports. While TS rightly points out that the five-year impact factor on average correlates well with the standard two-year impact factor (see also Garfield 1998), the differences for the individual journal might be substantial. Especially journals publishing relatively few articles per year have a higher short term volatility of citations.

Problems Regarding the Application of the JIF in Evaluation Processes

One of the most significant problems is the inappropriate usage of the JIF in comparing individual articles or researchers. This is highly problematic because the JIF gives absolutely no information about the success of a single article: JIF values are mostly driven by a few articles, which are cited very often, while most articles receive a much smaller amount of citations than one would expect when solely looking at the JIF (Seglen 1997; Adler et al. 2008). In other words: Citations per article are far from equally distributed, but exhibit a power-law distribution, indicating that equating the quality of an individual article with the JIF of its outlet is highly misleading. This is

well known and even acknowledged by TS (Thomson Scientific 2008). More precisely, TS even appeals to the scientific community to abandon the practice of interpersonal comparisons based on the JIF, but these postulates remain unheard by those using the JIF of an individual article's outlet in order to determine its quality.

It is of course particularly problematic in this context to compare different disciplines by using the JIF, since different disciplines also exhibit different citation cultures, which make comparisons across disciplines totally senseless (cf. Adler et al. 2008: S. 9–12).

Lack of Transparency and Irreproducible Results

While TS offers the Web of Science database for individual usage, JIFs cannot be reproduced using the data delivered by Web of Science. This unpleasant feature led to the case reported by Rossner et al. (2007, 2008), where a publisher (Rockefeller Press) bought citation data from TS and still failed to replicate the JIF calculations even by using the bought data. In standard scientific discourse no irreproducible quantitative result could be accepted, since intersubjectivity is a necessary precondition for any kind of scientific statement. This raises the question why scientists accept a criterion for scientific evaluation that does not fulfill scientific standards by itself. It seems to be a problem of transparency highly critical not only for the scientific community but also for the public, which is often relying on the exactness of scientific propositions.

Another matter of transparency is related to the classification of "citable" and "notcitable" articles as apparent in the formula stated above. The idea is that only "substantial articles" (Garfield 2005) should be considered in the JIF calculation, implying that the TS staff has to categorize all articles appearing in a certain journal into "substantial" (and thus "citable") and "unsubstantial" articles. This indexation is in turn affecting the denominator of the JIF calculation and may have drastic implications for the JIF of a certain journal; a phenomenon best illustrated by the high JIFs but low numbers of "substantial" articles of journals such as *Science, The Lancet*, or *Nature*. Here are two aspects of transparency at stake: First, we do not know the guidelines for differentiating "substantial" and "unsubstantial"

articles, and thus we may only hope that this kind of separation follows reliable and valid criterions. Second, we do not know whether it is possible for individual editors and publishers to successfully complain about a certain routine of indexing. Such complaints may be motivated by an incentive to cheat or by sound and respectable arguments regarding content, but in any case we do not know and can again only hope that TS is honestly trying to separate the careful editors from the venturous cheaters. Rossner et al. (2007: 1091) give the following example for such an odd case: "*Current Biology* had an impact factor of 7.00 in 2002 and 11.91 in 2003. The denominator somehow dropped from 1032 in 2002 to 634 in 2003, even though the overall number of articles published in the journal increased."

Heterodox Discrimination?

Summing up the drawbacks of the JIF is thus leading to quite a long list, which includes missing variables, serious selection biases, wrong interpretations and applications, and, eventually, a disputable practice in terms of the usual scientific standards of data handling and integrity. On the whole, it seems that the JIF is an evaluation instrument that is at best slightly misleading but in most cases applied (especially in cases where grants, tenure, or hiring are discussed) highly arbitrary and completely unrelated to the questions at hand.

What are the implications of these arguments for heterodox economics? The answer to this question is threefold: First, the general insight that the JIF is invalid as an evaluation instrument seems to be valuable for every scientist—if any scientist was aware of this fact, the JIF might soon lose its institutional power. Second, one may speculate whether the outlined biases could be exploited in a Machiavellian sense, a question asked in the following section. Third the biases already discussed seem not to be responsible for the relative discrimination of heterodox economics—a problem demanding a more specific answer relating to the network effects of paradigms in terms of citation networks. One main determinant of this asserted discrimination is of course related to the sample selection problem sketched above, implying that many important heterodox economics journals are simply not included in Thomson's Social Science Citation Index

(SSCI) (cf. Lee 2008a).[9] Thus the heterodox community is weakened, since its network is split in two parts—those included in the SSCI and those not included—implying a significant reduction of heterodox JIFs when compared to a "full-option-method," that is, a calculation including a sample of publications "as big as possible." The other main source of discrimination is that big and dominant paradigms obviously exhibit positive network effects in terms of citations: more outlets for orthodox or mainstream economics lead to a higher number of (potentially citable and potentially citing) articles, which in turn lead to a higher number of citations. The JIF thus favors a dominant paradigm in any case, since it has a much bigger citation network at hand, thereby further strengthening the discrimination of heterodox journals.

Perverse Incentives Associated with the Journal Impact Factor

As has been mentioned already, the JIF is a reactive measurement procedure, meaning that the units of analysis—the researchers—may adapt their behavior in order to maximize their results. Of course, this kind of adaptation to the rules of the game—relating to the incentives created by the JIF—can across the board be understood as a "manipulation" of the JIF, that is, as a serious violation of the moral standards of the scientific endeavor (as in Reedijk and Moed 2006). While in most cases we cannot observe whether individual scientists yield to the perverse incentives embodied in the ranking game, there seems to be consensus on a more general level. Most scientists probably would agree that in theory the only incentive science should follow is its curiosity related to observable phenomena. But in practice, as it is the case here, most things look different. The examples given in the following paragraphs are, thus, not to blame the individual's "manipulating" the JIF, but to show its working routine. I hereby simply follow an economic tradition, which is not to blame the selfishness of the agent, but the structural defects of the principal's rules.

In this case we find incentives on three different levels: the individual scientist's, the editor's, and the general paradigm's level. The following table is based on the preceding section and gives an overview of the variety of perverse incentives associated with the JIF.

Table 1

Perverse Incentives Associated with the Journal Impact Factor

Drawback	Incentive	Level	Perversity[10]
Missing variable: length of articles	Publish articles as short as possible.	Individual	++
Missing variable: number of authors	Publish articles with as many authors as possible.	Individual	+
Missing variable: article type	Publish primarily review or "data-heavy" articles.	Individual	+
Missing variable: circulation	Publish only in outlets with high circulation.	Individual	+
Sample selection bias: mainly journals	Publish only in SSCI-listed journals (no books or book chapters!).	Individual	+
Sample selection bias: language	Publish only in English.	Individual	−
"noncitable articles"	Cite your journal articles heavily in editorials.	Editor	++
Missing variable: self-citations	Encourage submitting authors to cite your journal (as a condition for publishing).	Editor	−(++)
"noncitable articles"	Introduce noncitable commentary sections in your journal(s) to increase citations.	Editor	−
Lack of transparency (coding of articles)	Intervene at TS to change the coding of your articles related to "citable" and "notcitable."	Editor	?[11]
Automated scanning process	Cite only working paper versions of articles from a rival paradigm or abstain from such citations.	Paradigm	++
Lack of transparency (journal inclusion)	Try to "anticipate the rules" and cite a related journal heavily before it applies for inclusion in TS Web of Science.	Paradigm	+

On the individual level there is a series of clear-cut incentives, namely, to publish articles that (a) are review or "data-rich" *articles* (no books or book chapters, never!), (b) as short as possible with (c) as many authors as possible (d) written in English only and (e) in SSCI-listed journals with a high circulation. These are certainly somehow perverse imperatives for researchers who want to completely adapt to the rules of the "JIF game."

Also, editors of journals face noteworthy incentives mostly related to the word "citable" in the denominator of the JIF calculation. Since "unsubstantial" and, thus, "noncitable" contributions are not counted in the denominator, this is obviously one possibility to influence the JIF. Moreover, since citations in such "noncitable" texts are also counted in the enumerator of the JIF calculation, it is possible to manipulate the JIF by heavily citing own articles in editorials and commentaries. The *Journal of Gerontology: Medical Sciences* constitutes an example of excessive self-citing in editorials, gaining more than one impact factor point in 2003 (4.1 instead of 2.9) due to such self-citations (cf. Reedijk and Moed 2006: 188–189). Another, more obvious and much less corrupt, way to increase the number of (journal-)self-citations is of course to simply encourage authors to cite articles from the journal they are submitting to (in sharp contrast to imposing journal self-citations as a *condition* for publishing).

Another incentive editors face is to include "noncitable" commentary sections in their journals: While contributions to such a section are "free," that is, not counted in the denominator, citations to such articles are still counted in the enumerator. Since commentary sections are often very inspiring to read this incentive does not seem as perverse as most other incentives associated with the impact factor mechanics. However, generally speaking, there is always a potential to manipulate the JIF by influencing the coding of articles conducted by Thomson Scientific. In the individual case this may be justified ("hey, you coded all our book reviews as full articles!") or not, but eventually the whole process is a black box.

Furthermore, incentives can also be identified from a paradigmatical perspective: One may abstain from citing articles from a paradigmatic rival at all[12] or only refer to working paper versions as substitutes for the original articles so not to strengthen one's paradigmatic competitor

in terms of citation metrics. Moreover, as a reactive measurement procedure the Thomson's impact factor game invites to anticipate its rules. According to various sources (Garfield 1990; Testa 1998; Dolfsma and Leydesdorff 2008; Testa 2009) the entry of a certain journal is, aside from other criteria such as internationality, timeliness, availability of English titles and abstracts, or the use of a peer review system, based on a "quasi-IF," which is calculated by filtering out citations from journals already included in the SSCI to the "applicant" that is the applying journal. The calculation procedure follows the same routine as defined in the second section. So journals already included in the SSCI could cite applying journals associated with the same paradigm more frequently within the year(s) their application will be based on.[13] They could initiate this secretly (by requesting authors to cite this or that journal), openly (by encouraging contributions from this or that field), or really subtly (by providing special issues devoted to the pet subjects of other journals associated with the same paradigm).

Most of the incentives discussed in this section clearly exhibit a tendency towards bad scientific practice. One could argue that by discussing these incentives one, therefore, also encourages the bad scientific practice. While I would agree that the whole ranking game rests on a doubtful logic, I think a transparent and accurate description of the current situation is a precondition for developing reasonable alternatives. Thus, the bad scientific practice is clearly on the side of the JIF and its adherents.

General Strategic Options for Heterodox Economists

Hirschman (1970) identifies three main possibilities of action in the context of a social conflict, that is, a state of affairs not acceptable to a certain group or individual. Basically he differentiates between exit and voice, where the former indicates the possibility to "stop buying the firm's products or [to] leave the organization" (Hirschman 1970: 4), whereas the latter option emphasizes "any attempt at all to change, rather than to escape from, an objectionable state of affairs" (Hirschman 1970: 30). These two mechanisms, based on economic competition (exit) and political discourse (voice), do not work in isolation,

but in a complex dynamic interplay allowing for a complementary or substitutive relationship between these two modes. Loyalty, on the contrary, is introduced as a relevant variable influencing the decision between exit and voice prolonging the former and intensifying the latter. However, Hirschman also develops a concept of "unconscious loyalist behavior," which is "by definition free from felt discontent, it will not lead to voice"[14] (Hirschman 1970: 91) but still prolongs a potential exit. Therefore, the three broad categories introduced have a lively interplay mutually influencing each other. In relation to the matter in hand, systems of quantitative evaluation will be interpreted as the referential product. The basic modes of action suggested by Hirschmann provide a conceptual framework for discussing the specific suggestions introduced in the subsequent sections. In this context, compatibility, not origin, is the main criterion for assigning suggestions to the different strategic levels. In this context, the nonexclusiveness of exit, voice, and loyalty favors such an approach. Thus, reinterpreting this framework for addressing the specific problem of the relative discrimination of heterodox economics within the TS standard leads to the following three general options:

- EXIT: Heterodox economists completely refuse to accept quantitative indices, especially the TS standard, as a measure of quality (nevertheless anticipating that *others will do*).
- VOICE: Heterodox economists try to alter the status quo by designing *and establishing* their own (quantitative) indicators or criterions of research quality.
- LOYALTY: Heterodox economists accept the TS standard as a measure of quality and try to compete at the best in the given evaluation system.

These three categories broadly illustrate the scope of action available to heterodox economists. Thus it seems reasonable to consider the compatibility of all the specific suggestions discussed later in the article with these three general categories. Some suggestions will fit into all categories—heterodox economists can adhere to them without even agreeing on a general orientation towards quantitative indices (which is, eventually, an individual decision). Other suggestions will fit into only one or two categories and, thus, could be interesting for

discussing the general strategic orientation. Such a discussion seems necessary to realize the potential network benefits associated with a situation, where heterodox economists could agree on this point in terms of a certain commitment.

Specific Strategic Suggestions

A Methodological Note

In order to give a clearly arranged account on all the specific suggestions I will code them according to the following scheme: #Number—Topic, for example #1-NET is indicating the first suggestion related to the citation behavior of heterodox economists. All suggestions coded this way will be incorporated in a common framework showing which of the specific suggestions go along with the different general strategic orientations introduced in the preceding section.

Establish Stronger Networks

Citation networks are an important factor in discussing the performance of heterodox economics as measured by the JIF. We can view citation dynamics in terms of network effects and ask how the disciplinary citation behavior is distributing citations among different journals. The following table, which analyzes the top-10 orthodox and top-10 heterodox journals according to the TS Journal Citation Report 2007, might help to focus on problems associated with this perspective.

Table 2 is based on a 20-year sample (1989–2008) of all citations between 20 economics journals. The sample selection rests upon the Journal Citation Report (JCR) 2007; it includes the top 10 journals of the JCR (=top 10 orthodox[15]) and the top 10 heterodox[16] journals identified in accordance with Frederic S. Lee's heterodox directory (Lee 2009a).[17] Two important properties or restrictions of the sample utilized above are noteworthy: First, all the citations related to *articles published between 1989 and 2008* have been counted (i.e., citations to articles published before 1989 have been excluded) and second—due to the idiosyncratic operating of the Web of Science database, which does not precisely show the references but only the citing articles—an

article that cites two or more articles from another journal counts only as one citation ("citing article"). Nonetheless the above comparison clearly demonstrates that the neoclassical citation network is much tighter, since heterodox journals cite each other less frequently—they import relatively more citations from orthodox journals than vice versa and have a much higher amount of self-citations.

The same conclusions apply when relying on a different sample of heterodox journals provided by Lee. The following table, a modified version of one of Lee's tables (Lee 2009b: 53, 153–154) based on the years 1993–2003, includes 11 heterodox journals. On grounds of the content-oriented selection of journals in this sample emphasizing radical, post-Keynesian, and socioeconomic approaches, only one of these journals, the *Cambridge Journal of Economics*, is also part of the above sample of the "JCR top 10 heterodox." Interestingly, while the sample used in Table 2 is much more diverse in terms of paradigmatical viewpoints (Marxist, post-Keynesian, ecological, evolutionary, feminist journals, and a journal very close to the mainstream are included), the results derived from Lee's much more coherent sample are very similar.[18] Moreover, it gives an intuition about the structural reasons for the loose heterodox citation network.

Table 2

Orthodox vs. Heterodox Citation Networks

	Average percentage of citations from top 10 heterodox journals	Average percentage of citations from top 10 orthodox journals	Average intra-network (heterodox/ orthodox) citation percentage excluding self citations
in top 10 heterodox	60.35% (intra-network)	39.65% (inter-network)	**19.3% (intra-network)**
in top 10 orthodox	4.89% (inter-network)	95.11% (intra-network)	**64.22% (intra-network)**

Taken from Dobusch and Kapeller (2009); see also Cronin (2008).

Table 3 gives a more precise picture of what has basically been already said: A stereotypical heterodox economist publishing in the journals depicted in Table 3 follows a rather standardized citation routine, which can be summarized as follows:

(1) First: Cite your enemies, that is, mainstream economic journals.
(2) Second: Cite yourself, that is, the journal you are submitting to.
(3) Third: Cite your buddies, that is, the two journals with the strongest connection to the journal you are submitting to.
(4) Lastly: Cite your allies, that is, heterodox economic journals except the three already mentioned (that is, the 17 remaining journals within this sample).

This fatal routine is subtly, but not in full detail, also present in Table 2 and can be (roughly) read as 40 percent mainstream citations, 40 percent self-citations, 20 percent citations of allies for heterodoxy compared to 65 percent citations of allies, 30 percent self-citations and 5 percent heterodox citations for orthodoxy. These differences in network density are striking especially when remembering that the sample of *orthodox* journals used in Table 2 is less homogenous in terms of content than the *heterodox* sample used in Table 3 (the orthodox sample in Table 2 includes the *Journal of Accounting and Economics* or the *Journal of Economic Geography*). In a nutshell: The orthodox citation network is much tighter—consequentially also heterodox journals have relatively more journal self-citations and import more citations from orthodox journals than vice versa.[21]

From a pluralist perspective this leads to a rather surprising result: Under the assumption that a pluralist attitude, as heterodox economists often invoke it, implies *talking to each other* (otherwise it would not be pluralism, but some kind of careless ignorance), which in a scientific context implies *citing each other*, we find that heterodox economics—as compared to its paradigmatic rival—is actually very pluralistic (according to Table 2, roughly 40 percent of the citations in heterodox journals stem from mainstream journals). On the contrary, the analysis suggests that the economic mainstream is theoretically closed, that is, not open for alternative theoretical approaches and thus not pluralistic (according to Table 2, only about 5 percent of the citations in mainstream journals stem from heterodoxy).[22] While this

Table 3

Citation Behavior Among a Content-Oriented Selection of Heterodox Economic Journals

Journal	Total Citations	% Mainstream[19]	% Self	% Buddies	% Allies[20]
Cambridge Journal of Economics	21,363	9.7%	2.6%	1.6%	1.7%
Contributions to Political Economy	2,204	9.1%	1.4%	2.3%	1.0%
International Papers in Political Economy	2,164	7.1%	0.3%	2.9%	3.0%
Journal of Economic Issues	22,917	4.9%	7.1%	1.0%	1.2%
Journal of Post Keynesian Economics	10,918	13.1%	7.6%	2.7%	1.6%
New Left Review	10,451	0.0%	3.2%	0.2%	0.1%
Review of Black Political Economy	3,886	6.1%	3.2%	0.6%	0.1%
Review of Political Economy	9,580	9.3%	1.5%	3.0%	2.6%
Review of Social Economy	9,067	5.5%	2.3%	2.4%	1.9%
Review of Radical Political Economics	9,391	4.2%	4.1%	1.8%	2.9%
Science & Society	7,735	0.2%	3.2%	2.1%	1.4%
	Average:	**6.29%**	**3.32%**	**1.87%**	**1.59%**

The categories "self" and "buddies" have been added by the author.

observation goes well along with a series of complaints about the discrimination of heterodox ideas within the mainstream journal culture it only holds from bird's eye view interpreting heterodox economics as a single paradigmatical alternative to mainstream economics. If we remove the tendency to paint opposing paradigmatical fractions by using a "broad brush" (Backhouse 2004: 268) and focus on intra-fractional citation behavior it comes clear that heterodoxy is more pluralistic in its relation to the mainstream than in its internal discourse: Only about 20 percent of citations in heterodox journals stem from the *other* heterodox journals in the same sample (that is, the sample Table 2 is based on). Thus heterodoxy imports twice as many citations from mainstream literature as it produces domestically, that is, within the heterodox paradigm. This indicates that heterodox economists should try to partially reorient their pluralist attitude from mainstream journals to other heterodox branches, not at least to intensify the theoretical discourse between different heterodox journals and schools of thought, potentially leading to a "Win-Win-Situation." While an intensified discourse on theoretical or methodological questions between different heterodox schools of thought might improve the theoretical and empirical standards as well as the applicability of heterodox economics in general, it would probably also lead to a significant improvement in terms of the ranking-game (#1-NET).

Practically speaking, heterodox scholars should always check 15–20 heterodox journals for potentially useful sources before submitting an article (the idea to rank journals according to their contribution to pluralism as presented in Lee (2008b) is in fact a kind of carrot to pursue this task). Under the assumption that citations are a reciprocal phenomenon the same argument applies to related disciplines such as economic sociology, management studies, political science, economic geography, and women's or development studies (#2-NET; cf. Reardon 2008). If mainstream economists are not willing to cite their heterodox counterparts maybe "neutral" researchers from other fields might well do so if the heterodox economists' work proves to be interesting. Again a potential "Win-Win-Situation" with characteristics very similar to those described above might arise between heterodox economists and economics' neighboring disciplines.

In any case it seems necessary to alter the current situation, which is characterized by the fact that heterodoxy comparatively strengthens the orthodox position in the content-avoiding JIF logic, as is evident from the following table based on the same sample as Table 2.

Table 4 examines the "cross-paradigmatical-border" citation behavior between the heterodox and the orthodox citation community. To fully clarify this situation it should be mentioned that the majority of the 385 citations that are exported from heterodoxy to orthodoxy are created by the respective "outliers" of each side: While 201 are exported by the *Journal of Economic Behavior and Organization*, another 111 are imported by the *Journal of Economic Geography* (excluding those from the former to the latter journal). Thus only 73 export citations within 20 years remain for the nine "nonoutliers" on each side. Technically, this implies that heterodox economists strengthen the neoclassical paradigm in terms of citation metrics (Factor 10 in this sample!), since they import many more citations from orthodoxy than vice versa. This is somehow paradoxical since heterodox economists often cite mainstream journals to criticize neoclassical theory or to demarcate themselves. It's a wonderful example of the content-blind "logic" of simple citation counting. Consequentially this analysis implies that heterodox journals should decrease their presence in the TS database, which is a rather radical and potentially self-damaging option mainly compatible with a strong rejection of any quantitative quality measurement (#3-NET). Tables 2 and 3 and the argument that the relative discrimination of heterodox journals regarding the JIF rests partially on the exclusion of some heterodox journals, on the other hand, would imply to try to increase the presence of heterodox journals in the SSCI (#4-NET).

Disseminate Your Papers

As already mentioned in above, circulation is an important criterion influencing the presence and availability and, thus, also the citation frequency of a certain article. Hence the following section is devoted to the question how to increase the visibility and circulation of heterodox articles to increase citations and impact factors of heterodox articles and journals.

Table 4

Citation Imports and Exports Between Top 10 Orthodox and Top 10 Heterodox Journals

Top 10 heterodox journals	Citations in top 10 orthodox (export)	Citations of top 10 orthodox (import)	Difference	Proportional Factor
Economy and Society	16	49	−33	3.06
Ecological Economics	10	681	−671	68.10
Work, Employment and Society	5	29	−24	5.80
Review of International Political Economy	26	70	−44	2.69
Journal of Economic Behaviour and Organization	201	1,884	−1,683	9.37
New Political Economy	1	38	−37	38.00
Cambridge Journal of Economics	47	463	−416	9.85
Journal of Development Studies	43	487	−444	11.33
Journal of Evolutionary Economics	31	395	−364	12.74
Feminist Economics	5	133	−128	26.60
Total	385	4,229	−3,844	10.98

Taken from Dobusch and Kapeller (2009).

Looking, for example, at the two-year time-span used to calculate the JIF, it is obvious that the availability of articles is crucial for the development of one's JIF—preprint publication is thus simply a must (#1-DIS), otherwise the outlet is "hurting itself" in terms of the JIF calculation. The reason for this is very simple: An article published in December (say, 2009) will be counted in the denominator for the 2010 JIF and citations to this article in 2010 will be counted in the enumerator. But most articles being published in 2010 will already be under review at the end of 2009. So most articles appearing 2010 could not even consider an article published in December 2009, if it was not accessible earlier via preprint channels.

Another aspect of this perspective refers to the amount of heterodox journals in total and the number of journals included in the SSCI. These data combined with some knowledge about the rejection rates in different heterodox outlets should make it possible to consciously found new journals in order to fully utilize the capacities of the heterodox economic community. In the case of founding new journals, the possibility of online open access journals should be taken into account, since this kind of publication is accessible all over the world and thus combines low costs with high circulation. For example, an open access journal for good heterodox review articles on topics relevant for developing countries might achieve quite a popularity, since most of the classical heterodox journals are simply not accessible in many universities of developing countries, while heterodox approaches might prove very useful for their students and staff (#2-DIS).

A further feature of the digital sphere is that research that is freely available on the Internet, for example, by downloading from the author's homepage, or disseminated along digital research platforms (like RePEc or SSRN) or mailing lists, gathers significantly more citations (Bergstrom and Lavaty 2007). A simple conclusion is therefore that heterodox economists should be allowed to post their scholarly articles as working papers on the web—a short reminder on the title page (published in this or that journal Vol. x(y); pp. a–b) could guarantee that the relevant Journal also gathers the citation, if the particular working paper is cited by someone (#3-DIS). This observation may also serve as a further incentive to create new

journals as open access journals, which should suffer from the same "positively distorting" bias. Furthermore, this suggestion is strengthened by the observation of Novarese and Zimmermann (2008) that heterodox articles posted on the RePEc platform and distributed via the "New Economic Papers" (NEP) mailing lists are on average downloaded more often than mainstream articles. Thus it seems reasonable to consciously improve the dissemination of heterodox work through digital channels as research platforms or mailing-lists (#4-DIS; for example, there are still no heterodox research networks within the SSRN[23]).

Lastly, heterodox economists should consider some general network effects influencing citation behavior. A general effect well known is that scientific publications, which appear (prominently) in the media, are cited more often than other papers (Fröhlich 2008: 73). So the imperative is: be interesting (to get cited)! Another "back-door option" to get on the citation lists is to enter the political debate. Publicly well known persons also tend to be cited more often and heterodox economists may act politically as commentators, experts, advisers, or advocates related to some topic, politician, or postulate. There are also mutual network effects between these societal fields: Political activities tend to be reported more often in the media than scientific results, but citations still grow through presence in the media. Thus another and related imperative is: be political (to get cited)! (#5-DIS) By the way, being political often goes along with good scientific practice: Since value judgments are hardly avoidable in social science, especially in economics (Myrdal 1963), one should treat them with the greatest transparency possible instead of hiding them behind (more or less) complex battlefields of algebra.

Advocate for Alternatives

Another perspective on this complex of problems is related to the general question of the validity of the JIF. It basically asks if we can find more valid instruments or proxies for measuring the quality of an outlet or an article. A possibility for heterodox journals to differentiate themselves in terms of associated quality and influence from the "old-boys-network" could be the introduction of a so-called triple

blind review system as a nonquantitative argument in a discourse on quality. "Triple blind" is in this context referring to the fact that not only authors and reviewers do not know each others name, but also editors do not know the identity of the authors. The German *Zeitschrift für Soziologie* has introduced triple blind review and consequentially it also rejected manuscripts from "star authors" (Fröhlich 2008: 68). This system could represent some kind of discoursive Unique Selling Proposition, especially since some TS journals are not even peer reviewed, not to speak of triple blind reviewed (#1-ALT). Another, quite different, possibility to achieve a qualitative difference from mainstream journal review practices would be to make review processes more transparent, e.g., by posting submissions and reviews on the web and allowing visitors to comment on both—the submission and the respective reviews (something similar is proposed by Earl 2008; #2-ALT). If submissions and reviews are posted on the web this could also induce authors to be even more careful in their manuscript preparations.

A further important advice referring to a better estimation of the quality of a specific scientific piece of work is to analyze the absolute impact of a certain publication in terms of total citations. Different databases or instruments can be used to measure the volume of citations to a certain publication, from Google Scholar over Scopus to TS Web of Science.[24] One should, thus, always calculate the concrete publication impact if an individual's work is evaluated in quantitative terms, no matter whether it concerns hiring or promotion decisions or the application for diverse grants. Especially for heterodox economists concrete and substantial "article impacts" are important since they represent an instrument less invalid and less unreliable as compared to the standard JIFs; moreover article impacts needn't suffer from the implicit discrimination embodied in the JIF (#3-ALT).

Lastly, of course, the various problems related to the exclusion of potentially relevant variables and the sample-selection bias associated with the JIF highlight avoidable problems, which could be eliminated by design through the creation of a new, alternative quantitative index for evaluating economic research (#4-ALT). The relevant biases in this context are mainly the small sample size and the two-year time-span for calculating the JIF, as well as the missing control variables for the

number of authors, the length of articles, the circulation of the outlet, article type, and the number of self-citations. Of course, the weight of these variables has to be interpreted with respect to the task at hand: When evaluating journals the length of the article seems to be a less crucial variable as compared to the evaluation of single authors. However, in the long run it seems necessary to establish alternative and superior measures of scholarly quality and visibility and, thus, new standards in research evaluation (see also Frederic Lee's contribution to this special issue). In this spirit alternative indicators could also be based on different "basic principles" instead of mere citation counting. For example, these could include the building of specific knowledge (journal self-citations), the ability to combine insights from various fields of research within a certain discipline (network centrality) resp. to connect different schools of thought (pluralism) or the interdisciplinary openness of a given outlet (citation trails to other disciplines). Moreover, the introduction of peer review or the inclusion of download statistics for individual articles (if accessible) could broaden the horizon of quantitative evaluation. More generally speaking, this would imply to reform the methodology of quantitative evaluation in favor of a multi-method approach. Thereby the related and potentially allied neighboring disciplines, already mentioned, could operate as international and interdisciplinary partners in a project applying for official funding to create a better alternative index (#5-ALT).

Another preliminary suggestion to compensate for the relative discrimination of heterodox economics within the SSCI is to develop a complementary index, which is correcting for some factor of pluralism (#6-ALT; as suggested by Lee 2008b).

Conclusion: Putting the Pieces Together

Why is it interesting for a philosopher of science to discuss the impact of citation metrics on heterodox economics? First, the scientific community's institutions are often a blind spot in theory of science, which thus seems all too detached from the practical process of science in many cases; this is the *theoretical* motivation. Second, philosophers of various epistemological camps agree that critique is a basic

prerequisite of scientific discourse (Popper 1934). This is the reason why many of them are skeptical regarding dominating paradigms in the social sciences and constitutes the *normative* motivation for writing this article. Third, citation ranking in philosophy is even more disastrous since the English-speaking bias is a much more serious problem when compared to economics (cf. Stekeler-Weithofer 2009), which constitutes the *affective* motivation. Fourth, I think that heterodox economics has great things to offer for improving the understanding of our economic environment, which is my *personal* motivation. It is for these reasons that I give a pragmatic outlook here mainly by providing an overview and discussing some general implications.

As a first step, the following table evaluates the above suggestions along the lines of the three general strategic criterions exit, voice, and loyalty. Thereby an "X" signifies the compatibility of a certain suggestion with the respective general strategic criterion. Suggestions printed in *italics* are signaling that the relevant suggestion has, from the author's point of view, a serious strategic drawback.

It is immediately observable that most suggestions are compatible with all three general strategic orientations. This is good news, since it implies a kind of flexibility for heterodox economists: Most activities listed above can be pursued without a general strategic consensus. Of course, there is still a high demand for further coordination and cooperation between different heterodox journals and schools of thought, but it is not primarily necessary to go for a "great debate on strategy." The differences between the broad strategic orientations consequently concentrate on a few points: Exit asks the fundamental question of boycotting the whole system in suggestion #3-NET. But this option seems to exhibit self-damaging properties and thus renders suggestion #3-NET more or less invalid. This observation illustrates that the quasi-monopoly power of TS is rendering this strategy obsolete, since exit relies on competitive mechanisms simply not available in the light of TS' dominance. Thus, without an alternative or competitive product to choose, exit is hardly justifiable. Lastly, the exclusive characteristic of voice is the creation of competitive (or complementary) indices for evaluating (economic) research.

An important observation embodied in the first suggestions depicted in Table 5 is that the different heterodox schools of thought

Table 5

A matrix of suggestions

Dimension	Suggestion	Exit	Voice	Loyalty
Etablish stronger networks	#1-NET: Reorienting Pluralism I: Consciously increase (current) citations of other heterodox communities, journals and scholars.	X	X	X
	#2-NET: Reorienting Pluralism II: Increase citations of related and maybe "allied" disciplines as economic sociology, political science, development studies, social/economic geography, organization/management science . . .	X	X	X
	#3-NET: Decrease presence of heterodox journals in the SSCI.	X		
	#4-NET: Increase presence of heterodox journals in the SSCI.		X	X
Disseminate your papers	#1-DIS: Pre-print publication is a strategical 'must' in order to succeed in the ranking game.	X	X	X
	#2-DIS: Fully utilize the capabilities of the heterodox economic community: Found new (Open Access) heterodox journals if rejection rates are considered too high.	X	X	X
	#3-DIS: Allow heterodox economists to distribute working paper versions of their publications via digital channels.	X	X	X
	#4-DIS: Access and create mailing-lists and online communities; disseminate research via RePEc, NEP, SSRN . . .	X	X	X
	#5-DIS: Enter the non-economic discourse (media, politics).	X	X	X
Advocate for Alternatives	#1-ALT: Introduce triple blind review as stand-alone criterion for objectivity.	X	X	X
	#2-ALT: Increase transparency in review processes as a stand-alone criterion	X	X	X
	#3-ALT: Calculate concrete 'publication impacts' if you are – directly or indirectly – affected by an evaluation process (partially) relying on data from TS.	X	X	X
	#4-ALT: Create an alternative and superior index to Thomson's JIF.		X	
	#5-ALT: Try to forge an alliance between heterodox economists and potential allies from other disciplines and apply for funding to create a better index.		X	
	#6-ALT: Create an index of heterodox journals complementarity to the SSCI correcting for pluralism (cf. Lee 2008b).		X	X

should consciously try to implement a common heterodox paradigm. More work should be devoted to discuss theoretical and empirical connections and complementarities between these different schools in order to tighten the theoretical and citation-related network of heterodox economics. Discussing each other's theoretical and empirical results—i.e., talking with each other—naturally leads to reading and citing each other.

In terms of a general strategy a concrete suggestion could be based on Heinz von Förster's famous two imperatives concerning smart behavior in complex, nondeterministic situations (Förster 1993: 49). While the *aesthetical imperative* ("if you are eager for knowledge, learn how to act") leads, as in most cases, to a merely trivial conclusion, namely, "do something, just do," the *ethical* imperative ("if you act, act in a way which increases your options!") gives a deeper advice: Choose the strategy with the most options. In the collection of suggestions above this strategy would be voice. Moreover, voice has also a substantial net advantage when compared to the other strategic orientations: While the institutions of scientific evaluation do not seem to be very responsive to the exit option and the possibility of influencing the system postexit is probably even smaller, unconscious loyalty on the contrary has another severe drawback as it would lead to basically accepting what is essentially wrong, namely, the JIF system. Therefore, in the given context, voice is the most promising route to change in the spirit of Hirschmann, since the creation of a rival system offers the possibility to empower the other options associated with exit and voice (cf. Hirschmann 1970: 55–56, 120–126).

Notes

1. Based on EconLit and the Social Science Citation Index (SSCI); see European Commission (2004).

2. The evaluation process of the Research Assessment Exercise is based on the Diamond List, which is again based on the SSCI; cf. Diamond (1989), Lee (2007).

3. These "perverse incentives" resemble the effects of governmental specifications in centrally planned economies: For example, the specifications regarding Christmas trees in the former Soviet Union were communicated in tons, which led to the production of massive and oversized trees,

These were very heavy and thus helped to fulfill the relevant specifications, but surely did not meet the standards expected by the consumers (cf. Fröhlich 2008).

4. The more people contribute to an article the higher is its probability of being cited, either by the authors themselves or by their related networks.

5. Roughly 2,700 of these are included in the SSCI. see: http://science.thomsonreuters.com/cgi-bin/jrnlst/jlresults.cgi?PC=SS (dl. 17-06-09).

6. If a journal contains no reference lists, but only footnotes, TS seems to count citations within the footnotes, i.e., the automated scanning system counts every single citation set to a certain publication and not only its appearance in the reference list (cf. Klein and Chiang 2004).

7. Typos appear in roughly 10 percent of all references according to Opthof (1997), while Evans et al. (1990) report that 48 percent of the references in their sample, consisting of three medical journals, were incorrect.

8. Individual researchers can register at the platform www.researcherid.com, where they can mark their articles within Web of Science, giving the opportunity to manually correct this bias.

9. Some examples of important heterodox journals not included in the SSCI are: *Journal of Institutional Economics, Review of Radical Political Economics, Contributions to Political Economy, Review of Social Economy, Journal of Socio-Economics, and Review of Political Economy.*

10. This column shows a proxy for the "degree of perversity" embodied in the given incentives, based on the way I see the relation between the incentive and what one might call "good scientific practice" (++: very perverse incentive, +: perverse incentive, −: perversity hinges on the context of the JIF—while the action as such might be reasonable, the fact that it is based on the anticipation of an evaluation mechanism still causes some headache; this of course resembles the well-known area of tension between deontological and consequentialist ethics).

11. As outlined in the preceeding section this depends heavily on the editor's concrete motivation.

12. As exemplified by mainstream economics.

13. According to the TS homepage, journals applying for inclusion have to submit three consecutive issues of their journal to TS (http://science.thomsonreuters.com/info/journalsubmission/; dl. 04-05-09). So if some journal applies in 2011 by sending three 2011 issues to TS, the calculation of the quasi-IF will most probably be based on the years 2010 and/or 2009. This implies that other paradigmatically allied journals should cite the applying journal more frequently in 2009 and 2010 and these citations should relate to articles from 2007–2009 (most interesting are of course citations relating to articles published 2008, since they are relevant in any case). However, this outline is still quite speculative as the journal inclusion process is eventually a black box as rightly argued by Klein and Chiang (2004).

14. Very similar to this point is the notion of passive loyalty described in Hirschmann (1970: 78).

15. *Journal of Political Economy, Journal of Economic Literature, Quarterly Journal of Economics, Journal of Accounting and Economics, Journal of Financial Economics, Econometrica, Journal of Economic Perspectives, Journal of Economic Geography, Review of Economic Studies, Journal of Economic Growth*.

16. *Economy and Society, Ecological Economics, Work, Employment and Society, Review of International Political Economy, Journal of Economic Behaviour and Organization, New Political Economy, Cambridge Journal of Economics, Journal of Development Studies, Journal of Evolutionary Economics, Feminist Economics*.

17. The acquired dataset is accessible via http://www.dobusch.net/pub/uni/citation-data.xls.

18. A stronger content-oriented selection would intuitively imply stronger relationships in terms of citations.

19. All citations to 12 different mainstream journals have been counted: *American Economic Reviev, Economic Journal, Economica, Econometrica, International Economic Review, Journal of Labor Economics, Journal of Political Economy, Journal of Monetary Economics, Oxford Economics Papers, Quarterly Journal of Economics, Review of Economics and Statistics, Review of Economics Studies*.

20. All citations to 20 different heterodox journals have been counted (see Lee 2009b: 153–154).

21. Journal-specific evaluations help to precisely analyze the citation routines of a certain outlet in order to provide journal-specific suggestions for possible improvements from this perspective. See Starr (2010) or Kapeller (2010) for two recent examples.

22. This is clearly an epistemological fallacy of mainstream economics, since the standard epistemological routine suggests to be highly interested in and concerned with critical articles or asserted falsifications relating to an established theory.

23. See http://ssrn.com/ern/index.html (dl. 04-05-09).

24. See Neuhaus and Daniel (2006) for an overview of different data sources for citation analysis. Ann-Wil Harzing's Software "Publish or Perish" is a small and useful tool for counting citations based on GoogleScholar; see http://www.harzing.com/pop.htm (dl. 06-07-09).

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