

A good idea gone bad in the service of cultural globalization: measuring the impact of publications in the psychological disciplines

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Bibliometrics deals with the analysis of bibliographic information and is said to come close to an “objective” measure of research activity. Highly accessible and easy to use, it is increasingly called upon in the academic community to measure the impact of scientific journals and/or quantify the development of the research output of universities, research units, groups of researchers and individual researchers themselves – as a means of evaluating these groups and individuals and allocating the funding and other means that they deserve.

Bibliometrics could be defined as the use of mathematical and, in particular, statistical methods to analyze sets of bibliographic references in order to arrive at a quantitative assessment of scientific output. While a content analysis of this output is conceivable (e.g. in the field of information science), bibliometrics, first and foremost, makes it possible to carry out a quantitative assessment of publications and their dynamics over time based on characteristics such as the number of articles published and the number of times an article has been cited. First used in the field of library science by documentalists and librarians, where it provided useful data on the sociology of scientific publishing, during the second half of the 20th century, bibliometrics suddenly (Rostaing, 1996) evolved into a set of measures used to *evaluate* scientific output. Its relevance has henceforth been found in the process of comparison that it facilitates: it allows for a quick ranking of publications without it being necessary to read their content. What is measured is not the intrinsic scientific value of these documents (i.e. their content and the added value they bring to a field of research), but rather the statistical trends of factors such as citations or the algebraic formulae associated with them. In other words, bibliometrics does not take into account the actual content of publications, as this is supposed to have been evaluated *upstream* through a peer review process determining whether the article should be accepted or refused for publication. Given that bibliometric evaluation takes place *downstream* from the peer review process, it could be seen as a complement to it, in accordance with the philosophy of science and epistemology. Unfortunately, as will be seen, it may in fact contravene this philosophy and epistemology, being based on indicators that are foreign to them.

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As an evaluation tool, the first phase of bibliometrics focused on *scientific output*. This involved counting up the number of scientific documents (articles, books, reports) produced by a given research source (S) (country, laboratory, researchers, journals, etc.). At that time, research institutions and researchers were evaluated according to the *number* of documents published. However, under the influence of Garfield and the Institute for Scientific Information (ISI; see in particular Garfield, 1972), bibliometrics took on quite a different form, shifting its focus to the *impact* of scientific publications. Thus, the goal became assessing the impact of scientific work by measuring the number of times the work in question had been cited, which is assumed to reflect its consumption by peers. Thus, indicators of visibility or notoriety emerged based on the principle of consumption (i.e. consumption of documents by researchers) which could imply, when impact and scientific significance are considered to be one and the same, that scientific and commercial logics are isomorphic. The famous IF (Impact Factor: Garfield, 1972, 1987; Hansson, 1995; Trayhurn, 2002) and the Hirsch index or h-index (Hirsch, 2005, 2007; Baldock, Ma & Orton, 2009) are typical of this type of bibliometrics[†] and, obviously, the types of bias to which they have given rise (Bhatia & Gandhi, 2003; Lehmann, Jackson & Lautrup, 2006). It is this type of bibliometrics, citation analysis, which is being imposed as the sole source of reference in the scientific community.

Quantifying scientific output is not the same as focusing on the impact of scientific publications and the respective effect of each focus is also very different. When country rankings based on scientific output (i.e. number of scientific documents produced) are compared to those based on citation impact (i.e. number of citations per document), it can be observed that the two rankings differ considerably (see, <http://www.scimagojr.com/countryrank.php>). Thus, all research fields (i.e. all subject areas and all subject categories) combined, China is in fourth place in terms of the number of documents produced but finds itself behind countries such as Belgium, Canada, Germany, France or Italy when it comes to the number of citations per document, even though the latter countries in fact produce markedly fewer documents.[‡] The same trend exists when the number of citable documents is used as the measure of output. Indeed, based on this criterion, China is in second place. Thus, there is a significant gap between scientific output and citation impact. A quick scan of Table 1, which includes just eight countries, shows that this gap is too highly significant to be interpreted effectively in terms of scientific quality alone, unless one accepts the idea that only Westerners, and perhaps the Japanese, put out quality research. Thus, whereas China produced 1,217,169 documents, with each of its documents being cited a mere 4.61 times, the Netherlands showed an output of only 346,852 documents, with each of its documents being cited, on average, 16.88 times. These examples attest to the gap between the production and consumption of documents, a gap which mainly works to the advantage of Western countries, in particular English-speaking countries. It goes without saying that the

[†] These days, with just a few clicks, it is possible to access online databases which will calculate the impact of a journal, an article or a researcher using indexes based on the number of publications and citations (e.g. impact factor, h-index).

[‡] When field of research is considered, however, these differences are more or less pronounced and can even be reversed.

relationship observed here between scientific output and citation impact for all research fields combined will vary for each individual research field.

| | Number of Documents Produced | Number of Citations per Document [§] |
|--------------------|------------------------------|---|
| United Kingdom | 1,242,464 | 14.78 |
| Japan | 1,220,415 | 10.12 |
| China | 1,217,169 | 4.61 |
| Canada | 628,843 | 14.84 |
| Russian Federation | 405,278 | 4.42 |
| India | 391,687 | 5.77 |
| Netherlands | 346,852 | 16.88 |
| New Zealand | 80,095 | 12.27 |

Table 1. Scientific output and citation impact of eight countries, all research fields (i.e. all subject areas and all subject categories) combined (Source: SCImago, Journal & Country Rank)

The aim of this chapter is not to contest the principle of citation analysis. Indeed, the impact of scientific publications appears to be a good basis for understanding how scientific work is communicated and avoiding the trap of self-publication or scattered publications. *A priori*, the idea that the impact of a scientific source of documents (S) on a target (T) can be estimated by the number of citations of S in T seems relevant. But this implies correctly defining S, which is not so difficult (e.g. a scientific journal, a group of researchers, a researcher, etc.), but also T, which is not so easy (e.g. all researchers from a particular country or group of countries, researchers from around the world in a given discipline, English-speaking researchers from around the world in a given discipline, etc.). As will be seen, based on the example of the Impact Factor (IF) in the psychological disciplines, the lack of clarity with regard to the definition of the targets concerned (T) can lead to bias. Thus, what is criticized here is not the principle of citation analysis itself, but rather the way it is carried out, which is conducive to bias and creates a situation whereby it no longer measures what it is supposed to measure, or at least what evaluators or research administrators maintain that it measures. Indeed, rather than measuring the impact of publications on international research, citation analysis is actually a measure of S's participation in the process of ideological globalization (or domination).

In the psychological disciplines, what does the IF measure? The impact of practices

Let us examine the generally accepted idea that an important bibliometric measure of scientific impact should measure the impact of a source (S) (an article, a researcher, etc) on

[§] The number of citations per document appeared more representative of impact since it lessens the influence of articles that are cited very often and whose impact may represent an epiphenomenon.

international research, with T being defined as researchers from around the world in a given discipline or group of disciplines. This is in fact how those involved in research policy evaluation present the IF, to which they often refer. On this basis, they rank journals, laboratories and – at least implicitly – researchers, maintaining that they are taking into account the impact of these sources on the process of international research. Unfortunately, the IF does not measure what it is supposed to measure, that is, the impact of a source (S) on international research. As will be seen, the target in question (T) does not correspond to the concept of “international research.” In our opinion, the problem does not lie in the basic tenet of citation analysis itself, but rather in (1) the empirical way that the databases used to calculate this influence are set up and (2) the weight given in the final score to the appraisal and review practices of groups of researchers that are not defined on the basis of scientific criteria. Let us take up an analysis by Beauvois and Pansu (2008) which refers to the psychological disciplines but which could be applied more broadly to other disciplines in which ideological choices are essential – in particular, other disciplines in the humanities and social sciences (history, sociology) but also geography (Milhaud, 2005). This analysis essentially shows that some basic principles regarding the “purest” possible evaluative measure of S’s impact on T are not in fact fulfilled. Let us review five such principles which can be said to be essential and the reasons why they are not fulfilled.

(1) *As an evaluation tool, an impact measure should not influence the choice of research themes.* This principle is far from being fulfilled. A researcher’s choice of research theme should (a) *a priori* be independent from dominant trends and fads and (b) be based on heuristic theoretical hypotheses and methodological feasibility. This is less and less the case, however, as it is very much in the interest of researchers, who are anxious to be evaluated positively, to choose themes which are highly valued (or trendy) in order to increase their chances of being cited. Why explore new issues in articles that will not be cited? In this way, competition creates an impetus for researchers to make sure their work fits into one of the major themes that are currently popular and which, within their discipline, are likely to lead to citations. Thus, the impact measure as it is applied today (IF or h-index) condemns young researchers, and even not-so-young ones, to avoid taking risks and to make a name for themselves through the path of performance framed by conformity to theoretical and conceptual trends (see also, Aalbers, 2004).

(2) *As an evaluation tool, an impact measure should not, in and of itself, act as an obstacle to the publication of articles which are relevant from a theoretical and methodological standpoint.* This principle is also far from being fulfilled. In the present context, it is very much in the interest of the editor of a journal to ensure that the journal he/she manages will receive an excellent IF in the hope that, consequently, the articles that it accepts for publication will be cited. Under these conditions, the editor has no choice but to make editorial decisions accordingly. It would never be suggested by a philosopher of science that the consumption factor (or IF) of an article represents the major criterion of its contribution to science. There are many other criteria which need to be taken into account in deciding whether to publish an article. Indeed, this is why the review process involving peers who are generally considered to be competent in their fields remains indispensable. Nevertheless, at the rate things are going, might we soon expect the evaluation grid given these experts to include a question regarding the probability that the article under review will be cited? That would sound the death knell for the epistemological justification of the peer review process.

(3) *As an evaluation tool, an impact measure should provide two journals of equal scientific quality the same chances of being indexed in a given database.* Once again, this principle is not fulfilled. We will not comment extensively here on the supremacy of English as the language of publication, since this is obviously the case and clearly creates a bias, albeit one that is seen by the ISI as being perfectly “natural” (Vauclair & Piolat, 2004). We will, however, refrain from naming it explicitly as a definitive rule. This fact inevitably favors English language journals and contributes to keeping the IF of journals written in other languages low. And yet, this language-based criterion has nothing to do with scientific quality. Moreover, there is at least one other selection criterion used in deciding which journals to index which also has nothing to do with scientific quality, that is, a journal’s *publication frequency*. Indeed, rather than functioning as a measure of quality, this criterion – which was supported by Garfield himself (Garfield, 1990) and continues to be listed as an essential criterion on the Thomson-Science web site (Thomson Reuters since 2008) – in fact relates to *power* considerations (i.e. potential size of readership, financial power). Only journals with a relatively solid financial base, large readership and strong portfolio of authors have the ability to publish regular issues. From this perspective, as regards the psychological disciplines, the American Psychological Association, simply by virtue of the *power* it holds, has an advantage over other national psychology associations which publish scientific journals.

(4) *As an evaluation tool, an impact measure should provide two articles of equal theoretical and methodological relevance, which are published in indexed journals and thus offer the required guarantees of quality, the same chances of being read, regardless of which of these journals they appear in.* This principle is also not fulfilled. The reason is simple: in deciding which articles to read, most American researchers, who make up the largest reference population, choose among a select few journals, very often those published in the United States as well as perhaps one or two English language journals covering themes that are close to those dealt with in American journals. The same article appearing in *Cognition*, or *l’Année Psychologique*, both indexed journals, would likely be eagerly read by these researchers in the case of the first journal and completely ignored in the case of the second, even if the article, in both cases, was referenced in *Current Contents* (or PsychINFO). Even when they are written in English, articles appearing in non-American journals (e.g. French journals) are hardly cited by the vast majority of researchers in the United States. Some researchers have been quoted as saying, quite frankly: “everything of interest in my field is published in three or four American journals.”

(5) *As an evaluation tool, an impact measure should provide two articles of equal theoretical and methodological relevance the same chances of being cited by the researchers who have read them.* Again, this principle is not fulfilled. Let us take the example of a Filipino psychology researcher who, after conducting a pertinent research project to explore a concept put forward by one of his/her Filipino colleagues, decides to send an article to a journal with a “prestigious” reputation (one that has a high IF and, most certainly, is either American or published in English). This researcher would probably receive responses from several reviewers very likely suggesting one or two concepts which are “better known to readers and just as relevant” accompanied by ten or so references to be cited in the revised manuscript (it is a sure bet that these references would not be taken from a Filipino journal!).

This is common practice, at least in the psychology disciplines. In order to publish his/her paper in this "prestigious" journal, the researcher would very likely be encouraged to abandon the concept put forward by his/her Filipino colleague which led to the research in the first place, and replace it by that proposed by the expert reviewers. The researcher may even end up not citing his/her colleague at all, or other work by Filipino scholars that this colleague inspired. It is even possible that following such a revision, the manuscript will be refused by the journal, in which case the researcher may decide to submit it again to a less prestigious journal with the new references, thus failing, once again, to cite the sources that initially led to his/her research. This type of practice, whereby researchers must submit unconditionally to the demands of expert reviewers, widens *the gap between what is actually read and worked on and what is cited*, a gap which seriously distorts the logic of the IF.

Thus, these five principles regarding the validity of a measure of scientific impact, which seem so logical, are not fulfilled by usual practices or, *a fortiori*, by the IF. The conclusion that appears to impose itself is this: the IF *is not* a valid measure, or even an approximate measure, of the scientific impact of a journal, team of researchers or individual researcher on *international* research. What does it measure then, assuming that it does indeed measure something?

In the psychological disciplines, what does the IF measure? An intellectual network, essentially representing an American point of view, sets the tone and decides what is "in."

The previous section leads to a very direct answer to this question, that is, it suggests that an impact measure such as the IF activates, and thus guarantees, the activity of a group of researchers which appears to be "dominant." Currently, this group is geographically situated in the United States, and is sustained by "private" and powerful associations there, as is the case in most disciplines in the humanities and social sciences. It is in fact a geographer, Milhaud (2005), who used the term *intellectual network* to describe this group. Certainly, the exact boundaries of this American network are debatable given that it is far from representing the numerical majority in all psychological disciplines. It would also be possible to digress here and ask whether researchers from other countries (e.g. Europe, Japan) who are determined to integrate into this group are in fact pioneers, disciples, new partisans, opportunists, or even collaborators. However, as regards the question of what the IF measures, the answer seems obvious: *it primarily and directly measures a researcher's integration into this group or intellectual network*. This is the only conclusion that can be drawn from the observed non-fulfillment of the principles reviewed above.

It would be tempting to suggest that this group represents the cream of the crop among researchers in the psychological disciplines. Accordingly, the IF could be seen to represent, somewhat indirectly, a mark of scientific quality conferred by integration into this prestigious group of scholars who conduct the very best quality research. This, however, at least in the psychological disciplines, represents an act of faith. It would nice if it were true. We even believed it ourselves for a time. But we now have at least two reasons to suggest that, in our disciplines at least, scientific quality hardly features in this equation at all. The first reason relates to the serious distortion of research activity noted above, caused by an impact measure based on an index of consumption. Popular trends have gradually

replaced heuristic considerations in guiding scientific practices. Research follows fads, and, rather than basing their work on decisive theoretical critique or crucial experimentation (*as should be the case*), scholars are following these trends or, a practice which is not much more mature from a scientific standpoint, are simply putting forward a few clichés taken from the body of public theories. Theories and experimental paradigms that proved to be rich from the 1960s through to the 1990s have thus been dropped without us really knowing why or being able to explain the *scientific* reasons to young researchers embarking upon careers in our fields. One sometimes hears it expressed in small research circles in countries off the beaten track that these paradigms and theories “no longer interest the Americans” and that it is therefore better to drop them. The need to be up to date takes precedence over that of adding new knowledge to the already accepted body of scientific knowledge, which again represents a major distortion of research activity.** Yet that is not all. Let us consider the second reason referred to above.

In the psychological disciplines, what does the IF measure?
In the service of cultural globalization

Variations of these trends in the dominant psychology remain limited to the possibilities offered by the individualistic, liberal culture which predominates in the United States and its satellite countries (Beauvois, 2005). This observation alone would be sufficient to contest the universality of the dominant psychology. However, this challenge has also been supported since the 1970s by the growing popularity of the *indigenous psychologies* movement (see Allwood & Berry, 2006). In its early days, this movement was particularly strong in the Philippines under the leadership of the late Virgilio Enriquez (see Pe-Pua & Protacio-Marcelino, 2000). From the outset, the indigenous psychologies movement was associated with the *geopolitical* protest against the American cultural influence and the fight for recognition of the value of dominated cultures. Several Asian psychologists saw the importation of Western (and particularly American) psychology as a form of cultural imperialism which they believed to perpetuate the colonization of the mind.† This movement rapidly spread to Southeast Asia, and then, via Hong Kong and Taiwan, to China, and later to Africa and South America. It is based on the following two propositions:

(1) American psychology and its European satellite contain postulates borrowed from American culture, in particular, American liberal individualism. These postulates are disseminated around the world under the cloak of “psychological science” which, therefore, despite its claims of universality, is unfit to grasp the psychological realities that exist in heterogeneous cultures. It could even be said that the dominant psychology is really only an *American indigenous psychology*.

** Psychologists have good theoretical reasons to suggest that this type of conformity to popular trends, while it may facilitate conformity-based performance, actually impedes innovation (Zajonc, 1965). Moreover, as has been shown by the work of social psychologists, the path from accepting pressure to conforming to it, and then to rationalizing practices and subsequently naturalizing them, can be all too short.

† As recently as 2006, San Juan expressed the goal of “decolonizing” indigenous psychology in the Philippines (Sokolohiyang Filipino).

(2) The cultures of the countries that are invited to import the dominant psychology most certainly have propositions of their own likely to provide postulates for their respective psychologies – which are surely just as “scientific” as Western psychology and more meaningful when it comes to dealing with local problems. Thus, indigenous psychology is defined as the study of human behavior and mental processes within a cultural context that relies on values, concepts, belief systems, methodologies, and other resources indigenous to the specific ethnic or cultural group under investigation (Ho, 1998, p. 94). The indigenous psychologies movement should not be confused with intercultural psychology which, using Western categories, often seeks to identify, in the cultures examined, variations of a process defined in the dominant culture.##

The intellectual network whose importance was discussed above thus appears to be the bearer of a Western (and especially American) indigenous psychology, which is typically *individualistic* and *liberal*. Through this indigenous psychology, a dominant and broader cultural model is conveyed to Western countries and other countries around the world. Indeed, it is this cultural model which provides – for example to social psychology, the most socially committed of the psychological disciplines^{##} – its active and intransgressible postulates, taken as basic truths. For example, again in the field of social psychology, the literature is largely focused on the individual level of analysis, often concentrating on motivational interpretations, implicitly assuming that the individual level is more authentic, meaningful and “true” than the collective level. Several “important” research fields in this discipline would lose their *raison d’être* were this purely ideological assumption to be refuted. Again, this process does not appear to be limited to the psychological disciplines, having been denounced in the field of geography as well (Berg & Kearns, 1998).

The intellectual network referred to above does not therefore simply represent an “excellent and particularly brilliant” group of pure and true researchers working in a universal realm to build a science. This group has its own cultural and ideological perspective. Moreover, researchers around the world who wish to achieve a high IF must first, in order to integrate into this network, accept and internalize this perspective. Citation analysis, and all the scientific practices that it prescribes, participates in the process whereby this perspective is increasingly becoming the sole source of reference. It thus acts as a mechanism for exporting around the world the liberal and individualistic cultural model that permeates Western psychology. This process could be seen as a new form of crusade. Consequently, beyond integration into this network, what IF indirectly measures is not scientific quality, as is so earnestly claimed, but rather one’s position in this geopolitical process of cultural globalization.

Having observed that Western subjects tend to explain their actions and what happens to them in terms of so-called “internal” causes (personality traits, intentions, previous behavior; cf. Dubois, 2003; Dubois & Beauvois, 2008; Pansu, 2006; Pansu, Dubois & Dompnier, 2008), intercultural psychology seeks to discover whether or not a given culture diverges from this Western tendency by providing “external” explanations (the importance of a situation, the power of others, etc.), considered to be the “opposite” of internal ones.

^{##} It can even be suggested, without fear of being contradicted, that the attempts at social disengagement often observed in this discipline are in fact the expression of these individualistic and liberal tendencies.

A less biased measure. For a truly internationalist approach

As repeatedly stated above, we do not contest the principle of citation analysis as such, but rather its implementation which is essentially empirical, guided solely by a few journal selection criteria (e.g. publication frequency, language) within the realm of the consumption of scientific articles, as seen from the viewpoint of the United States. This realm is described as “international” simply out of linguistic laziness (or because it is confused with the political and economic power of the dominant country). We consider a group of supposedly without *a priori* researchers who communicate among themselves, if possible in English, and claim that they are the international scientific community. It is this empiricism in constructing the concepts – whereby the international scientific community is defined through the eyes of the citation analyst, who therefore does not have to construct it – which explains the types of bias identified earlier. Only an *a priori approach*, deliberately designed to measure the impact of publications on a new concept of international research, a concept which thus needs to be *constructed*, will make it possible to avoid this bias. We will illustrate this approach within the psychological disciplines.

We will start our discussion with the work of a think tank put together by the European Science Foundation a few years ago, undoubtedly to come up with the famous ERIH (European Reference Index for the Humanities), but whose work, to our knowledge, was interrupted by European officials as soon as this think tank pointed out that the existing databases were insufficient. The initial idea was that, since research in the psychological disciplines is carried out in many countries around the world, it would be necessary *a priori* to take this research into account in developing a truly international database. It was an excellent idea! It involved accepting *a priori* the inclusion of documents coming out of various countries appearing in journals published in any country in which psychology research is carried out, regardless of whether this research explores subjects that differ from those explored in the West, uses different tools or is based on different underlying values, and even whether the results obtained may seem *strange* to Western eyes. These journals *must*, on principle and even for ethical reasons, be included in the database. The proceedings of the 28th International Congress of Psychology in Beijing, organized and edited by Qichen Jing, offer a few rich examples of such intercultural “strangeness.” Here is one such example: in the West, where individualism reigns, the teleology of child development is seen as leading towards independence and autonomy. Hence the “stages of development” studied in the West. Professor Chi, on the other hand, describes child development from the perspective of a child growing up in China who is immersed in Confucianism rather than liberal individualism, with the child’s development leading towards harmony with others, this being the final stage of development.*** Let us hope that the standardization we are denouncing here will never eliminate such differences! Otherwise, it could be said that the tools of domination (rather than science) had done their job. Is it possible to envision a bibliometric tool that could curb this standardization? We propose the basis of a tool that rests on three requirements:

- Meeting scientific quality criteria.
- Constructing a truly international database using an *a priori* approach.
- Fostering the development of research that is itself truly international.

*** Could Professor Chi publish in an American journal such as *Child Development*? Would he even want to?

Journals. We accept the following postulate by Jeannin (2005): “we suppose that every community is competent when it comes to evaluating the scientific quality of journals in which members of the community publish.” (our trans.) In the main countries in which scientific journals are published, for each discipline or sub-discipline, a survey should make it possible to find a few journals which are recognized as “high quality” journals by the community of peers in this country. It should also be ensured that these journals meet the following basic criteria which could be said to be “universal”:

- A *peer review process* which involves recognized experts from several countries (a committee could judge the quality and the international character of the peer review process)
- A *national editorial board* made up of researchers recognized in their own countries for their work.
- An *international editorial/advisory board* which truly represents several countries.
- Articles in English or accompanied by sufficiently informative summaries in English (for example 1000 or 2500 words in length, like the short reports or research reports in *Psychological Science*). We thus accept English as the “language of scientific communication.”
- *The desire* to be included in an international database and the *commitment* to meet the requirements related to the guarantees of scientific quality.

It goes without saying that online journals that meet these criteria must also be considered.

Books by authors that make a tangible contribution in terms of theoretical and/or empirical knowledge^{†††} would be governed by the same criteria. These play a greater role in the humanities and social sciences than in the so-called “hard” sciences (40% of citations, according to Hicks, 1999). These books could be integrated into the database of a given discipline or sub-discipline as long as the *collections* in which they appear meet the minimum quality criteria listed above – in particular, a peer evaluation process and a large book acquisition committee. If these conditions are met, the collections could be evaluated on the same terms as journals and thus be included in an international database.

Chapters in collective books that make a tangible contribution in terms of theoretical and/or empirical knowledge^{†††} would also be governed by the same criteria. Like books, these chapters could also be integrated into the database of a given discipline or sub-discipline as long as the *collections* and their guest editors meet the minimum quality criteria listed above – in particular, the intransgressible criterion of peer evaluation. Chapters published in a language other than English should, like journal articles, be accompanied by a sufficiently informative summary in English.

The database could be comprised of two levels. The first would include indexed journals, collections and books.^{§§§} It would include, by sub-discipline, the best journals, collections

^{†††} Excluded from this category are overviews with a pedagogical aim or intended for the general public, which considerably lessens the number of books to be considered.

^{†††} The preceding footnote concerning books also applies to book chapters.

^{§§§} The second level (listing journals (and collections) that are only cited) would include all the sources cited by the former but not integrated into the database.

and books published in the United States (given the huge number of researchers there), the best journals, collections and books coming from groups of countries or confederations (in particular, journals and collections from Europe, Asia and South America, given the large number of researchers working in these regions as well) and at least one, or perhaps two or three of the best journals, collections and books coming from other individual countries.

However, which countries should be included? This choice is obviously essential in order to create a truly international database. Again within the psychological disciplines, three criteria can be retained:

- The first is the *reputation criterion*, which would lead us to consider countries in which visible research is known to take place in the psychological disciplines: the United States, Canada, the countries of "Old Europe," Russia, China, India, Brazil, South Africa, the Philippines, etc. This first criterion, we hope, would involve putting together an international panel of experts, which would not be necessary in the case of the next two criteria, which are already available.
- The second is the *national research criterion*, which would lead us to retain the countries ranked among the top 20 for scientific research, all disciplines combined. The most suitable measure of scientific output would probably be the number of citable documents.
- The third is the *specialty research criterion*, which would lead us to retain the countries ranked among the top 20 for the discipline (in our case, psychology).

There could be variations on these three criteria. Nevertheless, the use of citation impact factors for evaluative purposes, based on a truly international database such as this one, should raise fewer objections and be more conducive to new international publication practices. In terms of evaluation, the h-index and its variants seem to be the simplest choice (h articles or books cited at least h times****). It would also be possible, if desired, to establish an $h_{(\text{international})}$ -index, taking into account only citations found in journals outside of the country in which the article was published, regardless of the nationality of these journals, of course.

Obviously, the above points serve as a basis for discussion and would require a more thorough and detailed examination.

The value of science

One of the basic values of descriptive science (which is oriented towards establishing deterministic or functional relationships) is universality. A scientific law should be universal. In the psychological disciplines, this value is far from being established. Certainly, it is not impossible that some psycholinguistic or psychophysical laws may be considered to be universal, but there is no way of proving this, except by accumulating identical data from around the world, barring the appearance of an exception which would bring everything

**** "A scientist has index h if h of his/her N_p papers have at least h citations each, and the other ($N_p - h$) papers have fewer than h citations each" where N_p is the number of papers published over n years (Hirsch, 2005, p. 1)

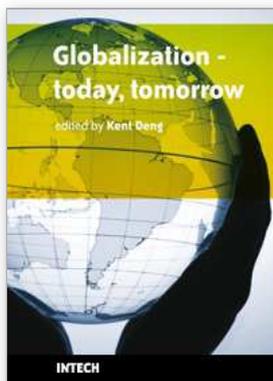
back to square one. Since Lewin, even psychologists know that the exception does not confirm the rule, but rather invalidates it.

Internationalism indisputably appears to be a less attractive value than that of universality, although it can sometimes give the impression of coming close to the latter. This is why the idea of international research is so often put forward. However, once again, there is a need to develop tools to evaluate researchers and research that will lead the latter to move towards a true internationalism rather than simply disseminating cultural clichés from one part of the world.

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