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IS IT WORTH DISCUSSING (OR IS IT NECESSARY TO DISCUSS) VARIOUS ASPECTS OF PRACTICING PSYCHOLOGY IN POLAND?

Responding to the comments submitted in the discussion, the author addresses two issues. The first issue concerns the possible peculiarities of psychological research and the transfer of its results to the sphere of social practice. This refers particularly to the publication of psychological works in the language of a given country (in this case, Poland) despite the fact that psychology effectively communicates in English. The author therefore reminds his distinction between two levels of doing psychology. The first level is scientific research. Psychology is an empirical science and has an intersubjective character. The second level is psychological practice. Practice is derivative to the achievements of psychology regarded as a science. What is more, only scientific achievements make actions undertaken in the sphere of psychological practice sensible (as well as ethical). The author voices his opposition to various popular works of pulp psychological literature. The second issue addressed is scientific misconduct, increasingly (and excessively) widespread in the research practice of psychologists, which includes: *HARKing*, *p-hacking*, hiding negative results, *ghostwriting*, *guestwriting*, etc. The author also comments on the new publication-related initiative aimed at combatting the phenomenon of *HARKing* – *pre-registration*. The author stresses that what is important in eradicating misconduct is formative work with graduate and doctoral students, aimed at instilling in them the ethical norms of research work.

Keywords: psychology, practice, publishing, ethics.

Even though the Professors taking part in the debate on the assertions made in my focus article (Brzeziński, 2014a) represent various scientific specialties and differ in the store of research experience they possess, and even though they work not only in Poland, our opinions on basic issues are very similar. This, in my opinion, is yet another argument in favor of the global and universal character of our discipline. Differences manifest themselves in emphasis rather than in principal points. When it comes to the latter, we concur. Despite the high level of agreement – which I gladly admit – certain problems connected with the cultivation of our “garden of sciences” occupy different places in my fellow debaters’ hierarchies of importance than they do in mine. These are not significant differences, though. I will therefore try to organize our views around the two main issues that, I believe, our discussion has focused on. **The first issue** concerns the possible peculiarities of psychological research and the announcement of its results as well as the transfer of the results to the sphere of social practice in Poland (though I think instead of Poland one could write, for example, Italy). This refers particularly to the publication of psychological works in the language of a given country (in this case, Poland) despite the fact that psychology communicates in English (the universal language of science) without any problems. **The second issue** concerns the increasingly frequent (and more and more effectively detected) cases of misconduct in the research work of psychologists (let Diederik Stapel serve as the “icon” in this case) and the measures that should be taken by the scientific community to minimize the activities of pseudoscientists and fraudsters that destroy our discipline.

**Does it make sense to speak of any (justified)
specificity of psychological research in Poland
and should psychologists publish (also)
in their native language?**

Let me start by reminding that I distinguish **two levels** of doing psychology.

The first level – the basic one – means focusing on the **accumulation** of scientific knowledge (in this case, psychological knowledge) by research psychologists. That knowledge – as I wrote – derives from scientific cognition, which has an **empirical** and **intersubjective** character; otherwise, the results of scientific research are not **replicable**. This feature of research allows to subject the activity of researchers to **effective social control**.

Built upon the results of thus defined psychological scientific cognition (**not any different in terms of fundamental methodological principles from cognition in other empirical sciences**) is a particular social practice (diagnostic, the-

rapeutic, expert, etc.) generated by public demand (as understood by Jerzy Kmita, 1969). This is the **second level** of doing psychology, derivative from (dependent on) the first one.

As I also wrote, I support the view that **first-level psychology has a global, universal character**, and in this sense of the term it makes no sense to speak of a specificity of Polish or, say, Italian psychology. **There is, quite simply, one psychology (in the scientific sense)** – just like there is one logic whose laws have a universal character, there is one way to understand and carry out an experiment, and there is one statistics whose methods research psychologists (and not only psychologists) use. The Yerkes–Dodson law (Yerkes & Dodson, 2008) means the same everywhere in the world. In this sense, it is “universal” (and I think Janusz Grzelak will agree with me that it is). If, at this basic level, we speak of cultural peculiarities, we mainly speak of those that stem from the fact that research participants use their ethnic language, have a distinct cultural experience, customs, and lifestyle, follow different rules of the social game, etc. On this point there is almost complete agreement between me and my fellow debaters. For example, Janusz Grzelak (2014) writes:

Science should be global, not national, and it should make it possible to communicate with other researchers rather than only with oneself. The method and results should be available for everyone in a commonly known language in order to serve replication purposes. What is more, the collected data should also be available for reanalyses (p. 639).

Still, I do not disregard the cultural context, which Gian Vittorio Caprara (2014) very aptly pointed out by writing that replications of psychological studies are not always as unambiguous (one-to-one) as those performed in neuro-cognitivist laboratories (not to mention studies carried out by physicists or chemists):

Yet, replication cannot be only a matter of translation or repetition of the same procedures. One cannot underestimate the fragility and volatility of previously observed regularities concerning cognition, affect, and behavior when brought to test in a different cultural context. Indeed, the major awareness of the flexibility, variability, and potentiality of human psychological functioning that derives from a better appreciation of culture and epigenesis should invite broader openness to novelty and diversity and warn against premature conclusions about what should be viewed as universally valid or true (pp. 521-522).

Janusz Grzelak believes, however, that I fail to notice the **intercultural differences** that will make certain findings – also at the level of scientific law formulation, I would add – differ a little depending on the population and culture. For example, the Yerkes–Dodson law that I mentioned operates in the natural

world and is one of the basic laws of psychology as well. Still, the “V-shaped” relationship between the level of task performance and the level of organism activation will have different specific features in different groups. In psychology, as I will proceed to explain, one must not disregard the role of the cultural factor – especially when it comes to the second level of doing psychology. **The laws of science have an idealizing character** (this is one of several features of science – cf. Nowak, 1980; also Andrzej Malewski, 1964, whose work is mentioned by Janusz Grzelak). When developing theories, researchers perform successive **specifications** of idealizing statements. One of the specifications is the introduction of the cultural factor to the specified statement (unless the researcher is only interested in the cerebral or even molecular level). When it comes to determining the specific validity scope of the results of an empirical study, it is important to determine **external validity** (cf. Cook & Campbell, 1979; Brzeziński, 2004). Unfortunately, it happens that psychologists (also in Poland) seem to forget about this and erroneously ascribe universality to the obtained results – without respecting the limitations of external validity when these in fact occur (and in such a case I do agree with J. Grzelak).

My next point was that **psychology cannot be reduced to brain sciences** because adopting a reductionist view on the ultimate form of sciences (including psychology) will lead to the **elimination of psychology** as a distinct scientific discipline (cf. Brzeziński, 2014b)¹. The specificity of the object of study in psychology is its twofold determination: **biological and cultural**. I cannot imagine a state of research on human psyche in which psychological theories will have disappeared, having been replaced, for example, by the findings of biochemists concerning the state of neurotransmitters or “molecule movements” (such a view can be found in the works of the famous reductionist John Bickle, 2006a, 2006b). Let me quote G. V. Caprara’s (2014) opinion again:

nothing can be more useful than good psychological theories in times in which pressure of various kinds discourages from thinking psychologically and in which new technologies may nurture the illusion that a better knowledge of the functioning of the brain may fully replace the study of mind, subjectivity, and individuality (pp. 523-524) [emphasis mine].

For instance, the fact that psychologists using personality inventories or David Wechsler’s Intelligence Scales in various countries subject those instruments to cultural adaptation beforehand does not make it impossible to perform comparative analyses. Consequently, IQ = 70 means the same in Poland and in

¹ Arguments in favor of the antireductionist position are provided by Katarzyna Paprzycka (2008).

France – provided that adaptation works have been carried out properly. It is also possible to compare, for example, the results of Aaron T. Beck's Depression Inventory (BDI) obtained by people in different countries. This makes James R. Flynn's studies meaningfully interpretable. The fact that scientific research in psychology is relatively often done in the participant's ethnic language **does not invalidate the universal character of such research**. Moreover, I still maintain that, compared to "pure" humanities, psychology is the least culturally charged discipline and the results of correctly performed cultural adaptations of questionnaire measures (especially measures of this kind!) make it possible to take your research conclusions beyond the limitations of the local cultural context, to perform comparative analyses, and to determine specific variance (related to a particular context – which, it seems, J. Grzelak had in mind).

When educating psychology students, we teach them one universal psychology – the same in every country. And, essentially, it makes no difference whether the textbook used is Bogdan Wojciszke's (2011) *Psychologia Społeczna* (*Social Psychology*) or David G. Mayers's (2011) *Social Psychology*. Textbooks of research methodology, psychology, statistics, or psychometrics are similarly or even more strongly grounded outside the cultural context. For instance, the methodology textbooks by Santo Di Nuovo (1993, 1996) work fine in teaching in Italy, and if they were translated into Polish they would work fine in Poland, too. I fully subscribe to Janusz Grzelak's (2014) position and suggestions concerning the education of university students:

At some universities [unfortunately not at all of them! – J. B.] it is assumed that by the third year of studies the student knows the language well enough to have English-language items included in the obligatory reading list. . . . This direction of change is a good one, and it is worth making much effort to maintain it or even accelerate it (p. 641).

and to Michał Harciarek's (2014):

In my opinion, apart from excellent textbooks prepared in their mother tongue, psychology students in Poland should be able to use and learn from works published in English in a comfortable way (p. 542).

Publishing in English is very important if **Polish psychologists are to join the global discourse**, which takes place mainly in top specialist journals (those from the JCR database are regarded to be top quality). This, again, is a point on which I fully agree with the debaters.

In the context of publishing in the native language, let me now add a few words about **the second level** of doing psychology in Poland (and, I suppose,

also in any other non-English-speaking country). If at the first level ignorance of English generally excludes a researcher from serious scientific life (not only when it comes to publishing in English-language periodicals), ignorance of that language is not a significant obstacle to doing psychology at the second level, when **the practicing psychologist establishes professional contact with the beneficiary of his or her psychological services**. I wrote in a different article (cf. Brzeziński, 2013a) that at the second level, using the scientific findings arrived at by research psychologists, the practicing psychologist builds **a program of practical intervention** (diagnostic or therapeutic) with regard to his or her client. Thus, the findings of academic psychology permeate into the sphere of social practice (for more, see Brzeziński, 2013a). While showing concern for the state of “our” psychology, its development, and the extension of the limits of scientific cognition, we should not forget that the knowledge and methods developed at the first, **academic** level will spread to psychologists (as well as other specialists) working at the second, **practical** level. This is why I emphasized so strongly that, apart from strictly academic works, Polish-language publications (textbooks, methodological studies, tests, etc.) for Polish practicing psychologists are also needed. The latest findings in the sphere of science will not spread into the sphere of social practice by themselves. We must “help” them spread. It was therefore with satisfaction that I read the opinion expressed on this issue by Gian V. Caprara (2014):

Practitioners should be helped and mentored to update their knowledge, to communicate efficaciously with their colleagues and their clients in the **language they habitually use**, and to deal with specific problems whose solution largely depends on their experience and their capacity to contextualize knowledge (p. 523) [emphasis mine].

Santo Di Nuovo (2014) expresses a similar view. He believes – and this is where I fully agree – that “[t]heoretical and experimental research regarding processes (cognitive, emotional, neuropsychological, interpersonal) that are basically universal” (p. 589) **has no linguistic limitations**. If we wish to go beyond our localities (Italians have a good word for this: *campanilismo*, which could be metaphorically used here), such research should be disseminated in the *universal English language*, the *lingua franca* of our times. By contrast (Di Nuovo, 2014),

[e]xperimental and quasi-experimental studies regarding specific applicative aspects, more interesting and useful in a particular country, should be conducted according the methodological guidelines indicated above, but they may be published in a place and in a language accessible to the psychologists living in that specific country (e.g., Polish or Italian). . . . Scientific publication in the native language, regarding the themes outlined above, is useful for preventing all the scientific knowledge of the psychologists working in a specific country, as well as the

updating of the acquired information, from being based on the reductive translations of Wikipedia (or similar) entries into the language of that country (pp. 590-591).

Very well said, I could not agree more! I would only strengthen this view by pointing out that the withdrawal of internationally renowned Polish researchers from presence among Polish practicing psychologists and psychology students will be conducive to **the spread of pulp psychological literature** [my own expression – J. B.], **which brings dishonor to academic psychology**. I would like to draw the attention of academic psychologists, hiding in their ivory tower, to an interesting text: “How to Tell a Specialist From a Shaman?” (“Jak odróżnić specjalistę od szamana?”) posted as an entry in a blog entitled *NeuroBigos*².

Let us not leave a crack through which authors of pseudopsychological texts and tests try to find their way in and, unfortunately, accomplish that from time to time. I spoke about this in the lecture inaugurating the 35th Scientific Convention of Polish Psychological Association (see also: Brzeziński, 2014b):

. . . we should strongly reject those pseudoscientific products of shamanic minds and just as firmly stigmatize their use. A considerable amount of pulp psychological literature should also be mentioned as belonging to this kind of “offer.” Luckily, unless I am mistaken, those products of pseudoscience have only negligible influence on the awareness of our students. Unfortunately, the proportion of those products of “real” psychology is by no means small in the offer of various postgraduate courses and trainings organized by organizations of suspicious reputation with psychology written on their signboards.

Wiesław Łukaszewski (2014) wrote in a similar vein:

In the face of antiscientific attitudes growing stronger and in the face of shamanism of all descriptions spreading, it seems strikingly short-sighted to dismiss the popularization of science (I mean science, not personal beliefs). . . . A refusal to publish in your mother tongue appears to be acting to the detriment of your own society. It certainly amounts to leaving your potential readers exposed to news about so-called “discoveries” of scientists published by tabloids – the kind of news that no one is usually even able to straighten out (p. 555).

This has been understood, to the benefit of psychological research and practice development in our country, by such eminent scholars as Jan Strelau, Bogdan Wojciszke, Maria Lewicka, or Dariusz Doliński. Their important monographs and academic textbooks have been published (also) in Polish (which J. Grzelak approvingly notes). **I hope (for what else could I do?) that psychologists will develop their practice under the influence of these scientific texts and will not consider it in the light of pseudoscientific texts.** John B. Nezlek

² Retrieved Oct 14, 2014, from <http://neurobigos.wordpress.com/2014/10/03/jak-odroznic-specjaliste-od-szamana/>.

(2014) looks (from the American and international perspective) at the popularization of monographs and textbooks by Polish authors (and, I believe, also by Italian, Czech, or Swedish ones, for instance) in a similar way:

Polish psychologists need to publish both in Polish and in English, the default language for international science. . . . Teachers, students, and practitioners need to have access to native language materials to teach, learn, and practice. Such a dual responsibility places an additional burden on Polish psychologists however, and this additional burden needs to be recognized when evaluating Polish psychologists and their accomplishments (p. 580).

Katarzyna Markiewicz (2014) observed that “a Polish author who writes only in English loses a considerable part of his or her native audience” (p. 569). I agree. Publishing in Polish is also popularization of psychology and of “good Polish research among psychology students” (Harciarek, 2014, p. 543) and practicing psychologists. Let us not be ashamed to publish in our native language. It is also our duty to society to do so. After all, it is to society that we owe our education.

The important thing is – and Michał Harciarek (2014) agreed with me on this – that

. . . one of the main reasons that makes it necessary to publish works also in Polish is the concern for our language, including the language of psychology. I am strongly opposed to borrowing numerous scientific (and other) terms from the English language when these terms have their equivalents in Polish (p. 542).

I would like to stress firmly that one of the burning problems of our scientific discipline as practiced here in Poland is **the cluttering of the Polish language** and its pseudoscientific enrichment through the introduction of new, Polish-sounding “terms” (Łukaszewski, 2014, stigmatizes this as well, writing that the language of Polish publications is sometimes “riddled with Americanisms” [p. 557]). Sadly, there are more and more of those. And something must be done about it! I would therefore like to make an appeal to the editors of our psychological journals: do not allow authors to use those language monstrosities; return the texts that contain them to their authors to correct the language, regardless of their scientific value.

**What conclusions should we draw
from the scientific pathologies that come to light
and what should our reaction to those pathologies be?**

I suppose that the solution to this problem has two levels.

The first, surface level comes down to seeking more and more effective anti-missiles against the enemy's missiles, to use military rhetoric. In other words, it comes down to more and more sophisticated analyses of statistical data, aimed at detecting undesirable and highly reprehensible behaviors of researchers (technological progress in the construction of computers as well as progress in writing more and more advanced statistical software are certainly conducive to this). There are more and more of such analyses, and they are less and less clear to research psychologists, who do not spend their time on following these "games" only. **I do not believe these procedures to be absolutely effective.** This resembles a struggle between two ambitious and highly determined Internet hackers. No one wins this struggle "for good."

Nevertheless, without disregarding those innovations (I do have reservations but, to make it clear, I appreciate them and I try to popularize them!), I think what is far more reliable, tough – let us say this right away! – laborious and demanding, is **formative work** done from scratch, so to speak. What I have in mind is instilling a **science ethos**. It is not enough to perfect the codes of ethics (e.g., successive editions of one of the best codes for psychologists, prepared by APA, 2010). Nor does the passing of the *Code of Ethics for Researchers* (cf. KSEN, 2012/2013) by the Polish Academy of Sciences solve the problem.

It is the researcher who should want to find out how things really are and develop natural curiosity rather than try to "prove" – unfortunately, at all costs (read: unethically) – that he or she was right. The formation of researchers is a method that belongs to **the second, deep level. Doing science is not a "cops-and-robbers game,"** as Dariusz Doliński (2014) points out, and it is hard not to agree with him. **Doing science is constantly searching for truth out of internal motivation (not for points!).** Let us ask, together with D. Doliński: "What is crucial for science, then?" I fully agree with him that the crucial thing is "Self-awareness! The belief of the entire community and of each researcher that dishonesty is pointless" (p. 530). I think a psychologist with such a well-formed "personality" will be resistant to financial temptations, to the delusive splendor of fleeting fame, and to the bibliometric pressures of the institution he or she works for (as K. Markiewicz, 2014, pointed out: "I believe that, beside the deceptive personality of a pseudo-researcher, the most frequent cause of scientific fraud is the pressure of time as well as the necessity of collecting points").

Science is harmed (and this should not be tolerated by society, from whose taxes education is largely financed) by dishonest behaviors such as: **plagiarizing, data fabrication, data manipulation, ghostwriting, or guestwriting,** as well as more "subtle" **ways of cheating by means of advanced methods of**

statistical data analysis, available only to “advanced” researchers! It is worth stopping for a while at the phenomenon of **guestwriting**. K. Markiewicz (2014) drew attention to it in her comment: “A good example of a while lie is the practice of authors adding one another as coauthors to their papers in order to multiply their scientific output” (p. 568). The scale of this phenomenon remains unknown. However, I think that some academic teachers use their position of thesis supervisors or heads of departments to force their graduate and doctoral students to grant them the coauthorship of theses. They kindly “allow” the students to add their names on the title pages of theses or dissertations. This does not refer to obvious coauthorship, as when the supervisor has made a real (rather than merely virtual) contribution to the writing of a dissertation. What prevents the aggrieved from protesting? The answer is simple: they want to work or to be employed. It is necessary to talk about this to graduate students (I do this during my methodology lectures in the 3rd semester) and doctoral students.

Let us now focus on more sophisticated kinds of data manipulation – discussed by Santo Di Nuovo (2014) and John B. Nezlek (2014) – and on the methods of coping with them (their authors are convinced about their effectiveness). These manipulations include: (1) **HARKing** (short for: *Hypothesizing After Results are Known*), (2) **p-hacking** (also called *p-fishing* – selecting and manipulating results to be statistically significant at the “sacred” level of $p = .05$), (3) **publication bias**. The coping methods (how effective?) are (1) **meta-analysis** and (2) **pre-registration research**.

In my opinion and according to some of the debaters (especially D. Doliński and S. Di Nuovo), an extremely important thing is to change the attitude and publication policy of editors-in-chief. This means journals should be opened broadly for papers whose authors found “nothing” and obtained a statistical significance of $p > .05$, which is researchers' bad dream. Why should the level of $p = .05$ be treated as “sacred” (Skipper, Gunther, & Nass, 1970: “the sacredness of .05”)? The consequence of such a conservative attitude is quite commonly known as the file drawer effect and publication bias.

Incidentally, it is worth noting that a method heavily burdened by the file drawer effect is meta-analysis – a method very popular in the community of psychologists, consisting in modern-style review of the literature and used in order to find empirical confirmation (rather than falsification) for a particular hypothesis. This is so because scientific journals – being the sources that meta-analysis is based on – tend not to publish papers reporting the nonoccurrence of the effect sought or its negligibly small size (non-significant values of effect size indices). As a result of this publication bias (cf. Franco, Malhotra, & Simonovits, 2014;

Song, Hooper, & Loke, 2013), the findings of meta-analyses are overestimated. To what extent? Nobody knows. The policy of publishers would have to change: they would have to start publishing “negative” articles as well. What is optimistic is that “something” has already begun to change in the right direction.

In my opinion, **pre-registration** is a method that goes in the right direction – though it will take some time to find out about it better. Following this procedure, a researcher (in this case, a psychologist) proposes an empirical study project at a given journal; after the project has been provisionally approved, its author may begin the research (cf. Chambers & Munafò, 2013; NeuroChambers, 2013). According to Chris Chambers and Marcus Munafò as well as a few dozen other academics who signed a special appeal concerning this issue (Chambers & Munafò, 2013):

Unlike traditional scientific publishing, in which manuscripts are peer reviewed only after studies have been completed, registered reports are reviewed *before* scientists collect data. If the scientific question and methods are deemed sound, the authors are then offered “in-principle acceptance” of their article, which virtually guarantees publication regardless of how the results turn out.

This is supposed to be an effective method – “the great white hope” – of combatting the phenomena of HARKing (cf. Kerr, 1998) or *p*-hacking (cf. Lombrozo, 2014; Scot, 2013). It is unsettling to look at the results of the study on the occurrence of QRP (Questionable Research Practices) in the community of psychologists (cf. Leslie, Loewenstein, & Prelec, 2012), showing a significantly high percentage of psychologists resorting to unethical research practices that fall into the HARK type. As regards critical comments on the pre-registration method, they can be found in Jamie Monogan’s study (Monogan, 2014). The literature, both positive and negative, is rapidly expanding. For example, the editors of the *Journal of Experimental Psychology* have announced the publication of a *Special Issue on Pre-Registered Research* in 2015, devoted to articles prepared in the new format. More and more major psychological journals declare their intention of joining this project.

I believe, however, that the fascination with the new format of empirical research (not only in psychology, to be sure) will diminish with time. And those who strive for success at all costs (the currency being the points for publications in JCR-listed journals), if only they are motivated strongly enough to cheat (and become new “Stapels”) – will do it anyway and continue their criminal activities, only perhaps in a more refined way. Let us remember that progress in statistical data analysis, new techniques such as bootstrapping, and the development of

high-speed computer technologies has resulted in the widespread use of those and similar increasingly advanced procedures by nearly everyone. This is not the first time in history that the Latin adage *Quis custodiet ipsos custodes?* becomes relevant. I therefore agree with John B. Nezlek's (2014) judgment of the method:

Calls to require registration of studies before they are conducted also provide a false sense of security. Who will watch the watchers? Distrust breeds dishonesty. . . . Cheating at science is like cheating at solitaire – you are only fooling yourself. Now, if you are playing solitaire to impress someone else that is a different story; other motives are involved (pp. 581-582).

The Macdonaldization of the university, the attempts to make it similar to a production enterprise (cf. the idea of an entrepreneurial university, Clark, 1998) – all this leads to the erosion of academic norms and to the large-scale occurrence of pathological phenomena: FFP misconduct (fabrication, falsification, plagiarism – cf. FPRM, 2002). Rushing employees to harvest points (without paying attention to how those points are obtained) is the simplest way to pathologize the entire institution (Markiewicz, 2014). What happens is that only papers printed in highly-scored JCR-listed journals begin to count (the number of points that can be scored ranges from 15 to 50). These are often multiple-author papers. The policy of granting 100% of points to all the coauthors of a given publication, provided that they are affiliated with different scientific institutions, has resulted in the emergence of “cooperatives” – publishing jointly and multiplying points! The adopted system of point-based assessment has relegated the writing of monographs to the margins – particularly synthetic monographs, very important to the development of a given discipline (cf. W. Łukaszewski, 2014; Caprara, 2014). It is hard to understand why a scholar can get a mere 25 points for a large monograph (written in English and brought out by a prestigious publishing house), which takes even a few years to write. I know that the system of awarding points was designed by researchers working in the field of exact and natural sciences (I witnessed its development in Poland), who do not usually write monographs. Fine, let them evaluate scientific activity in this manner: it is their playground and their toys. But why has this practice been imposed on the humanities and social sciences? We have reached the point when **science has turned into bookkeeping**. It is really hard not to agree with John B. Nezlek (2014), who wrote:

raw bibliometric data are replacing judgment. One does not need to read a scholar's articles to evaluate what he or she has done. Simply calculating the H-index suffices. . . . **Bibliometric indices can and perhaps should be part of the evaluation of scholarship, but they need to be part of evaluations; they cannot be the evaluations themselves** (pp. 580-581) [emphasis mine].

There is one more thing. The **parametrization system based on scientometric data** (points for everything) that has been introduced in Poland by the Committee for the Evaluation of Scientific Units (KEJN) works well in the evaluation of large scientific units but **fails in the evaluation of the research work of individual academics**. It is good that the **Declaration on Research Assessment (DORA, 2012)** was adopted and that an institution as important in Poland as the Foundation for Polish Science (FNP) signed it. The next thing to be expected is that **KEJN will considerably modify its rules** as well.

What I have written above can be summed up in one sentence. The principles of ethical conduct, also in science, should be **internalized**; they must be our own, not institutionally imposed, and we must begin – together with our graduate and doctoral students – to put these ethical solutions into practice.

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