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# ON WHAT IS IMPORTANT WHEN WE THINK OF PSYCHOLOGY IN POLAND

Sharing one's knowledge and work with the public is the main experience sustaining the sense of a deeper meaning of intellectual effort
*Grzegorczyk, 2001, p. 61*

The author takes up the problem of the condition of psychology as an academic discipline. He narrows down the scope of his reflections to answering four questions concerning the context of the country in which psychology is developed – in this case: Poland. The author answers Question 1 – *Does it make sense to speak of any specificity of research carried out by psychologists in Poland?* – in the negative. In his opinion, it is possible (and advisable) to speak of the cultural context of psychological practice but not research practice. Psychological cognition is intersubjective and has a global character. The author's answer to Question 2 – *Should psychologists publish (also) in their native language and should they publish their work also in Poland?* – is positive. In the context of Question 3 – *What indices are used (or should be used) to determine a psychologist's position in world science?* – the author comments critically on the practice of overusing various not always well thought-out bibliometric indices and is in favor of increasing the role of peer review assessments. Answering Question 4 – *What conclusions should we draw from the scientific pathologies that have come to light and what should our reaction to those pathologies be?* – the author suggests that psychologists should make their raw data available and share them with other researchers so as to make external replications of empirical studies possible.

**Keywords:** psychology, scientific research, publishing, replications, misconduct in science.

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There is nothing particularly original in the observation that science has a **global** character – that, except in countries ruled by totalitarian regimes, it knows no geographical or political boundaries and is, in essence, **pluralistic** and **tolerant** (see: Nowak, 1998; Brzeziński, 2001). The point is for proposals deriving from various schools of science to be considered on equal terms in debate. The point is also to use only scientific arguments, not ones referring to economy, ethnicity, religion, sex, age, etc.

This is also what university education is (or should be) like – which was clearly articulated by the authors of *The Magna Charta of European Universities*, signed at the University of Bologna, in 1988, on the 900th anniversary of the establishment of that university (Europe's oldest one), by rectors of European universities. Among other things, we read the following in that document:

> […] 2. A university is the trustee of the European humanist tradition; its constant care is to attain universal knowledge; to fulfil its vocation, **it transcends geographical and political frontiers**, and affirms the vital need for different cultures to know and [to] influence each other [emphasis by J. M. B.][1].

It is universities that guarantee the regeneration of elites (including those that will continue the research initiated by their predecessors). If only for this reason, they ought to be specially cared for by governments. It is the investments the state makes in them (as well as in the education of its citizens at all levels) that turn out to be the most profitable ones – even though a return on that investment is not immediate. This is because the elites determine the survival of a country and nation, in the cultural sense above all. The politicians who fail to see this are very short-sighted. The development of research and education is a task of the utmost importance to the development of a democratic society, characterized by a high level of well-being. The authors of the report entitled *The heart of the matter. The humanities and social sciences for a vibrant, competitive, and secure nation*[2], prepared in 2013 by the American Academy of Arts and Science (AAAS), realized this well. What – in the context of that document – is particularly worth noting? The relevant fact is that AAAS point to the alarming need for the humanities and social sciences to get out from the shadow and break free from the domination of exact and technical sciences. It is them that, to a great extent, determine the development of civil society. In Poland, already before the *Round Table of the Humanities*, the ministry responsible for science and higher education forcibly promoted enhancing the status of technical educa-

[1] Cited from: http://phys.chem.elte.hu/forum/MagnaCharta.pdf
[2] See: http://www.humanitiescommission.org/_pdf/hss_report.pdf

tion at all costs while simultaneously deprecating education in the humanities and social sciences. It is fortunate that our society is now leaving this path of "development" and that we intend to restore proper status to the humanities and social sciences as well as to education in these fields at universities.

Science is – as Kazimierz Ajdukiewicz put it (1983; see also: Frankfort-Nachmias & Nachmias, 2001) – **intersubjective** (which makes it possible to distinguish the products of scientific, rational cognition from those of nonscientific, irrational cognition), and it is such in two senses: it is **intersubjectively communicable and intersubjectively verifiable (controllable).** This means that every researcher competent in a given field may repeat the research carried out by another researcher. Correctly done research (e.g., carried out by a psychologist) whose results have been – precisely in the spirit of the intersubjective communicability principle – properly communicated to the community of researchers can be **replicated**. Thanks to replications (Wojciszke, 2011a), it is possible also in social sciences (and thus in psychology as well – see thematic sections in the issues of the following journals: *Psychologia Społeczna*, 2012, and *Perspectives on Psychological Science*, 2012) to eliminate those findings from the corpus of scientific knowledge that emerged either as a result of methodological errors or as a result of fraud (such as that committed by the infamous perpetrator of data fabrication in studies in social psychology, Diederik Stapel from Tilburg University – see: Klebaniuk, 2012; Levelt, Drenth, & Noort, 2012).

Against the social and methodological backdrop outlined above, highlighting the global and supralocally intersubjective nature of psychology, where does its research practice stand? Let us ask directly:

### Question 1. Does is make sense to speak of any specificity of research carried out by psychologists in Poland?

Psychology as a science, let us repeat, has a global character (it seems no one questions this), but we must not forget (and this is where unanimity ends) that it is practiced in a particular country, not only in the English language. Whereas exact sciences assimilated English (being the contemporary equivalent of Latin) a long time ago and use it comfortably and intersubjectively now, this can hardly be said of the humanities or social sciences. There are disciplines (including psychology, I believe) that are – to various degrees – burdened by the cultural factor.

Let us abandon reflections on the cultural uniqueness (expressed in language) of such humanistic disciplines as Polish literature studies, certain subdisciplines of history, art history, or ethnology, whose researchers can more fully

express themselves in their native language or in a foreign language other than English (for art scholars – besides English – Italian or German are such languages, while lawyers researching the issues of Polish law mainly do that in Polish) than in English (strongly favored without regard for the tradition and specificity of a given discipline). Let us stick to "our" psychology seen in the broader context of social sciences rather than the humanities.

When we speak of psychology (as well as of medicine, sociology, or any other discipline of science that has direct impact on the shape of the social practice "built upon it"), we must (!) distinguish two layers in it: (a) the strictly scientific layer and (b) the practical layer. The quality of the latter (as well as the ethicality of the practical activities it comprises – see Brzeziński, 2013) is, of course, determined by the quality of the former. Let us bear in mind that psychology is not only research but also the preparation of professionals capable of handling the vast sphere of social practice. The "intervening variable" in this chain is the standard of university education of future psychologists. When striving to ensure, ideally (idealization?), the world-class level of the research done by Polish psychologists – both those from the "junior" generation, being the potential recipients of PRELUDIUM grants from the National Science Center (NCN), and those from the "senior" generation, receiving European grants as well as NCN's MAESTRO grants – and the best possible publication of their results, we must not forget that this is not only about enriching the scientific picture of the world described in the language of psychology.

This "psychological cake" has three layers. The first one, located at the very base, is the scientific layer (the building of psychological models of the world by psychologists). The second one is the educational layer (the university formation of specialists to handle psychological practice). The third one is the layer of psychologists and other specialists introducing first-layer solutions into social practice (this is what some consider to be the "real" psychology).

A parallel phenomenon is the self-regeneration of scientific elites (in university studies and laboratories), meaning those who will ensure that psychology not only does not die but keeps developing. Let me clearly stress at this point that it is not possible to take active part in this phenomenon, vital for psychology itself, while remaining outside the scientific mainstream of the academic world. Today, being outside the mainstream means, above all, publishing in obscure journals (which sometimes pretend to be scientific) and having one's work printed as "bookbinder's syntheses" by publishing houses located "in a brother-in-law's shed" (to use Professor Maria Lewicka's expression). Thus practiced, psycholo-

gy will not be noticed worldwide and will not inspire other psychologists to take up scientific dialog.

Of course, there are no methods of scientific cognition that are specific to a given local psychological community. These methods are universal – as can easily be seen by flipping through a number of "top" psychological journals that publish the work of psychologists from all over the world. The Wechsler Intelligence Scale or the Beck Depression Inventory (BDI) do not essentially differ between the countries where they are used, except in language. Consequently, a psychology student who got acquainted with one of those diagnostic methods at a Polish university will have no problems (except, perhaps, problems with the language of the country where he or she will continue studies) in joining the discussion during a presentation of a case in which a practitioner psychologist has used Wechsler's Scale. In this sense, I can see no justification for speaking of Polish psychology as distinct from world psychology. A good theory, meeting the high methodological standards of contemporary psychology and announced in the universal English language (see e.g., Jan Strelau's temperament theory; Strelau, 1998), will be, and is, successfully used in many countries and thus it will be independent of the context of the language of a particular country. And this is the only road that leads Polish psychologists, simply speaking, to the high society of psychology. At any rate, today, in the time of omnipresent Internet and electronic communication with the entire world, it is an anachronism to say that someone is a Polish psychologist because he or she works at a Polish university (i.e., one located in Poland) and teaches students there (the students not necessarily being Polish). One is, first of all, a psychologist with certain scientific achievements, recognizable in psychologists' academic world – and thus one to whom "geographical and political frontiers" (to use the expression from *The Magna Charta* again) are alien.

It is, however, possible and necessary to speak about the conditions (legal, economic, political, cultural, or institutional) in which universities function in Poland and in which research in psychology is done there. The same is true for Germany, the United States, or the Czech Republic. The context of a particular country refers primarily to the education of new generations of psychologists as well as to the quality of psychological practice, to the conditions in which academic knowledge is transmitted to practice, and to translation from the language of psychological theory into the language of activities – also psychological. The former is a no less important sphere in which psychologists fulfill themselves in the modern world. In fact, it is psychological theory that – in the long-term perspective – makes their research legitimate (Brzeziński, 2011). Every research

finding will eventually find its application, since, when all is said and done, what is left is the social utility criterion – and its being sometimes filtered through other factors makes no difference. Yet, we should bear in mind that every social practice will be effective and ethically responsible when it is built upon empirically (intersubjectively) verified theory. Outside the context of psychological theory, psychological practice does not exist. Theory precedes practice.

As the discipline from the field of social sciences that, I believe, is the least charged with the cultural factor (language and custom), psychology is nonetheless somehow identified with the country of origin of its products. This, of course, is more conspicuous when we delve into the sphere of application of scientific findings to the regulation of social practice, and less conspicuous when we speak of basic research only. Yet, there is something to it. Does this have any deeper meaning – apart from the national identification of a given psychological laboratory of journal (in spite of the fact that the authors who publish their papers in it are psychologists of various nationalities)? Does it not resemble the prestige-driven ranking games for universities, study programs, etc.? None of that makes science or the practice based on it any better or any more advanced.

Despite the international character of psychology and the correspondingly international character of psychological research standards, psychologists do not abandon indicating the country where a given journal is published (not even in the case of those journals whose impact factor values are high). The locality of a given title is emphasized by psychologists in a number of countries – e.g., *American Psychologist* (a very high position on the JCR list), *British Journal of*..., *Scandinavian*…, *European*…, *Australian*…, *Canadian*…, or *Československá psychologie*. Incidentally, there are as many as 135 journals in various disciplines with *American*… in their titles on the JCR list and 171 with *European*; seven psychological journals have *British*... in their titles. There is also a Polish title there: *Polish Psychological Bulletin* (Poland's oldest English-language periodical, published since 1969). Are the authors of articles published in those journals not engaging in "global" psychology? Of course they are. What is characteristic of those psychological journals is that nearly all of them are published in English. In a "neighboring" field, the philosophy of science, there are also, for example, worldwide book series: *Boston Studies in the Philosophy and History of Science*, *Minnesota Studies in the Philosophy of Science*, or the book series edited in Poland but circulating worldwide (published by Rodopi in the Netherlands), entitled *Poznań Studies in the Philosophy of the Sciences and the Humanities* (published since 1975). When it comes to psychology, an example of a "geographically" identified book series of this kind, recognized worldwide, is

*Nebraska Symposium on Motivation*, published since 1953. I believe that these names – names of regions, countries, or cities – play a sentimental role only and constitute an opportunity to emphasize the scientific position of the center where the series is published. In the time of globalization in science, the scientific quality of a journal is not likely to be judged based on the national identifier in its title. This role is performed much better by the *impact factor* (IF).

**Question 2. Should psychologists publish (also) in their native language and should they publish their work also in Poland?**

My brief answer is: yes, they should. I can see several arguments in favor of such a view. Let me start by drawing the reader's attention to the important civic duty imposed by society (which finances its universities by paying taxes) on psychologists professionally associated with the academia. The psychologists' duty is not only to develop their academic discipline by participating in research programs and supplying new research results, but also to educate their students well. Only a small fraction of students will replace the outgoing generation of active researchers by taking over their research and teaching roles. Most psychology graduates (Masters of Arts in psychology) will join the ranks of professionals who, having been well-prepared – trained in state-of-the-art scientific theories and methods of psychology – will work effectively (and in an ethically responsible manner) in the sphere of social practice.

While university psychologists should (in the name of globally conceived science) comfortably use the English language, which is universal and fundamental to psychology, we cannot expect this from those who work in the sphere of practice. For the time being, we educate and train students and psychologists in their native language – and I can see no reason why this should change. It is in this language that psychology graduates as well as those who have completed postgraduate studies or in-service training courses will communicate with their clients in psychologists' offices, clinics, schools, counseling centers, etc. By publishing in Polish journals – even though a majority of them represent a lower scientific level compared to most psychological journals listed in JCR or SCOPUS – and providing Polish publishers with our (Polish) textbooks (for different levels of education), we also disseminate the research findings of Polish psychologists. An example of such an attitude is Bogdan Wojciszke's *Psychologia społeczna* (*Social psychology*), a textbook by one of Poland's most eminent social psychologists – original and referring to the research findings of Polish psychologists, too. I followed the same idea as an editor of a series of academic

textbooks in psychology: *Wykłady z psychologii* (*Lectures in psychology*), published by Wydawnictwo Naukowe Scholar (Scholar Scientific Publishers). Professors Jan Strelau and Dariusz Doliński (2010) were thinking along similar lines, too, when, instead of undertaking to translate one of many good American textbooks for psychology students, they decided to involve 38 Polish psychologists in the project, the collaborators representing all the subdisciplines of psychology and all the important Polish university centers. In this way, a two-volume textbook came into being, with a total of 1,800 A4 pages: *Psychologia akademicka. Podręcznik* (*Academic psychology. A handbook*). Its editors also decided that, apart from the achievements of psychology contributed by foreign university centers, it was important to make Polish students acquainted with the achievements of Polish researchers as well.

> There are two kinds of textbooks. The first kind is a single-author textbook, in which a scientist presents all the issues. The second kind is a work where each specific issue is discussed by a different author (or authors). . . . In a way, the development of science – or, more broadly, of civilization – made narrow specialization a necessity for scientists. It therefore seemed natural to us that the latter solution should be chosen – namely, that a large group of distinguished researchers, most of whom had internationally recognized achievements, should be invited to collaborate on the textbook. Each researcher writes on the issues he or she specializes in. We asked them to present the achievements of world psychology **with particular focus on the achievements of Polish researchers** (Strelau & Doliński, 2010, pp. 23-24) [emphasis mine].

We should also try to get our academic textbooks published (particularly the single-author ones, not being mere compilations of foreign studies) rather than translations only as well as to construct Polish tests, not only adaptations. What should also reach the Polish reader is Polish scientific monographs. Publishing a monograph through a prestigious foreign publishing house – certainly a boost to the author's status! – results, of course, in the author entering international circulation but leaves him or her obscure in Poland (especially among students and psychologists working outside research centers). The awareness of the multitude of students and practitioners will be shaped by second- and third-rate authors of popular "second-hand" compilations and by authors of translations selected on the basis of their market attractiveness, or by the "psychological pulp" that sells well in railway station book kiosks. Jan Strelau understood this well when, following the American release of his important monograph *Temperament. A psychological perspective* (Strelau, 1998a), he decided to publish its Polish translation (Strelau, 1998b) the same year. His fundamental study, summing up decades of work on the psychology of individual differences, is now being brought out in Polish (Strelau, 2014). Likewise – which is yet another example – Maria Lewicka's important monograph containing results of original

psychological research, *Psychologia miejsca* (*The psychology of place*; Lewicka, 2012), was also first released by a Polish publishing house, with the author simultaneously publishing important articles in professional journals with a worldwide readership. Let those two examples serve as a model of how a Polish professor of psychology performs his or her role. In pursuit of points (which will be discussed below) we should not forget about the Polish reader of psychological studies.

By publishing original results of Polish psychologists' research as well as by popularizing the latest solutions concerning the application of statistical methods and in the area of psychological assessment (tests!), we develop the Polish language, including Polish psychological lexicon – and this is a very important argument. We enrich the corpus of the Polish language. Years ago – as the Polish state was coming back to life after World War I – the professors who rebuilt and built Polish universities (Cracow, Lwów, Vilnius, Poznań, Lublin) after the time of the partitions as well as set up departments of psychology there and opened study programs, thought along similar lines. In the first issue of the newly established yearbook, *Nauka Polska. Jej potrzeby, organizacja i rozwój* (*Polish science. Its needs, organization, and development*), Stefan Błachowski, appointed by Kazimierz Twardowski to set up a psychology department at the new university in Poznań, published an article entitled "W sprawie potrzeb naukowych w zakresie psychologji" ("On the scientific needs in psychology"). In that article, he wrote, among other things:

> . . . 1. One of the most acute ailments of Polish psychology is the lack of a textbook that would present all the important achievements of this discipline in a strictly scientific form, using uniform terminology consistent with the spirit of the Polish language. Neither Polish translations of foreign works nor Polish textbooks, usually representing the metaphysical approach . . . or having a popular character fail to meet the needs of psychology.
> 2. The non-uniform terminology, showing considerable discrepancies depending on whether a given psychologist lives in Warsaw, Lwów, or Cracow, and the lack of Polish terminology in entire branches of psychological knowledge considerably hinders communication between psychologists and leads to plenty of effort and time going to waste as a result of purely verbal misunderstandings. What ought to put a stop to those inconveniences is an encyclopedic dictionary of psychology, containing all the terms used in psychology and in related sciences and explaining the meaning of those terms . . . (Błachowski, 1918, pp. 487-488).

In recent years, several dictionaries and encyclopedic publications have been brought out in Poland. Let me mention two of them: the Polish-language publication by 15 Polish psychologists, edited by Jerzy Siuta (2005), and the translation of the English work authored by Artur S. Reber and Emily S. Reber (2005). The advantage of the latter is an English-Polish index of entries. Still, the dynamic

development of some research subfields in psychology, particularly those from the borderland between psychology and, for example, neuroscience or cognitive science, as well as from the area of advanced statistical methods, makes S. Błachowski's suggestion no less valid after all these years. I would very much like to eliminate the language monstrosities introduced by researchers who professionally use the English language. Paradoxically, it is Polish that becomes their second language, which in turn leads to murdering rather than enriching the native language. Here are a few examples of such monstrosities (what is interesting, some of them already have proper equivalents functioning in Polish): *kowariata* (*covariate*), *zmienna supresyjna* (*suppressive variable*), *analiza rezydualna* (*residual analysis*), *metoda machingu* (*matching method*), etc. Developing language culture and enriching the Polish language with new scientific terms – these are also important tasks to be fulfilled by academic psychologists, and ones that cannot be overestimated. There is still a need for a good encyclopedic dictionary, putting the chaos of Polish psychological terminology in order. This was pointed out during Psychological Colloquia, an event organized in June 2012 by the Committee of Psychology of the Polish Academy of Sciences (PAN) and the Institute of Psychology of the University of Silesia.

**Question 3. What indices are used (or should be used) to determine a psychologist's position in world science?**

In the last few years, the presence of Polish psychologists in the scientific circulation worldwide has been the subject of particularly intensive debate in the academic community of psychologists. I cannot recall such debate taking place ever before, and I have been on the psychological scene for many years now. This in turn translates into a specific question: How (read: in what kind of journals) is it advisable to publish the results of one's empirical research? This debate is not at all about reaching the widest possible group of potential readers, who are also creatively dealing with the same research problems that one is dealing with. The background of the debate is much broader. The background is the parametric evaluation of academic units (university faculties and institutes, institutes of the Polish Academy of Sciences, and research institutes), which has been carried out for many years – first by the Polish State Committee for Scientific Research (KBN), later by the Council for Science of the Polish Ministry of Science and Higher Education, and currently (for the second time already) by the Committee for the Evaluation of Scientific Units. In connection with the evaluation, the staff of scientific institutions were (and still are) obliged to contribute

the largest possible number of points that publications were converted into (the rules of conversion not always being clear and rationally acceptable). In this pursuit of points, interest in actual creative achievements has vanished. The question of what a given author has written about and what new contributions he or she has made to psychology lost its importance; the much more important question became how many points the author has published a paper for. A kind of market started to function, and those who scored the highest number of points in a given year were rewarded by rectors. The rules of evaluating research units were also applied, somewhat automatically, to the evaluation of researchers. Moreover, promotion criteria began to be introduced based on the number of points collected by researchers over a certain period (e.g., after obtaining a doctoral degree and before the opening of postdoctoral degree conferral procedure). Rather arbitrarily, lists of journals were introduced where publication was awarded with different numbers of points. It became desirable to have one's work published in journals that were high on the so-called Philadelphia List (Thomson Reuters JCR list today). Under pressure from the community representing natural and exact sciences, a devaluation of monographs took place, monographs being very important to the large and culture-creating community of the humanities as well as – to a smaller extent perhaps – to the community of social sciences. A caricatural definition of a monograph was decreed and all monographs were put on a par, regardless of their scientific importance or the amount of time and effort put in by the author. For instance, a book the length of only six publisher's sheets (a publisher's sheet being the standard unit of text length, equivalent to 40,000 characters) was awarded 20 points if published in Polish, and only little more – 25 points – if published in one of the so-called congress languages. A monograph published by *Oxford University Press* brought its author the same number of points as any third-rate barely bound compilation printed "in a brother-in-law's shed"[3]. This, however, is merely a caricature of truly merit-based evaluation of research achievements. I find it hard to imagine that in 2017 (which is when the next parametric evaluation of research units is to take place) we will not be referring to peer reviews when assessing monographs. Anyway, university psychologists have begun to take part in "the numbers game" (I am referring here to the critical article entitled "Stop the numbers game"; Parnas, 2007).

The idea of "absolute" conversion of all achievements into points – the points being somewhat arbitrarily determined – came from the community of

[3] For critical comments on this evaluation system, see Antonowicz and Brzeziński (2013).

natural and exact sciences. On December 16, 2012, an important declaration was published, drawn up in the… biological sciences community. The American Society for Cell Biology (ASCB) announced the now famous *San Francisco Declaration on Research Assessment* (abbreviated as DORA). What should be of interest to us in it is the critical opinion on the use of the *impact factor* in the assessment of individual research achievements. DORA (2012) reads as follows:

> . . . Do not use journal-based metrics, such as Journal Impact Factors, as a surrogate measure of the quality of individual research articles, to assess an individual scientist's contributions, or in hiring, promotion, or funding decisions.

It is remarkable that the Declaration was signed by nearly 11 thousand people (including numerous Nobel Prize winners) and 467 scientific organizations from all over the world[4]. In Poland the Declaration was also signed by the authorities of the Foundation for Polish Science (FNP, 2014). They decided to do that

> . . . bearing in mind the alarming fact that a tendency has been developing in the academic community to reduce the quality of research work contributed by scientists to the rating of journals in which their achievements have been published. Scientific communities do know which journals in a given discipline are the most important ones, but this should be auxiliary knowledge and should not render the assessment of individual research achievements redundant. . . . In the case of experienced scholars, an additional important parameter in the assessment of research achievements is the number of citations that their publications have (depending on the specificity of a given discipline or field), or the Hirsch index (h) derived from it. For various reasons, *impact factor*, the number of citations, and the h-index are not applicable in most disciplines of the humanities and social sciences. . . . [They are not applicable, either] in the assessment of small units, and much less in the assessment of individual research teams and their leaders. In this case, peer review evaluation should definitely be used, in which carefully selected academics assess the originality of individual scientific contributions of other researchers, while the h-index and the number of citations may play an auxiliary role.

Similarly critical opinions were voiced by participants in the "Evaluations of Science" debate, held in Mogilany near Cracow, organized by the Polish Academy of Arts and Sciences (PAU) in November 2013 (Białas & Biliński, 2013), as well as by Zbigniew Błocki and Karol Życzkowski (2012).

Summing up, I believe that the evaluation of the scientific quality of publications in which the results of empirical research or theoretical analyses are presented cannot be reduced to the values of scientific metrics: (a) the total number of points collected for publications, patents, or funds obtained from grants; (b) the summary IF value of journals in which papers have been printed. What accu-

[4] See DORA website at: http://am.ascb.org/dora/

rately captures the evaluation of institutions is not necessarily accurate in describing the achievements of an individual researcher (as numerous specialists point out and as I tried to demonstrate above). There remains the evaluation using the peer review system, plus – if we consider quantitative metrics – the number of citations and Hirsch's h-index. Special care should be given to the evaluation of monographs (those with a truly capital M). I can see only one solution: heads of academic units themselves should select a not-too-large set of monographs for expert assessment of scientific merits. Next, these monographs should be assessed by reviewers known for their objectivity and competence in the field. A community is usually able to draw up such a list of independent experts (independent in their opinions and not pliable or easily influenced). I realize that such a procedure is time-consuming and costly. Still, if we do not want to abandon the evaluation of research achievements, should we not follow this path? The alternative solution – at least when it comes to the evaluation of monographs – is already familiar to us, and it is nothing to be proud of[5].

## Question 4. What conclusions should we draw from the scientific pathologies that have come to light and what should our reaction to those pathologies be?

The common denominator of pathological behaviors in empirical research is the violations of so-called good practice observed in the scientific community. These can be briefly referred to as FFP violations: **f**abrication, **f**alsification, **p**lagiarism[6]. This is consistent with the taxonomy of such violations of research work standards to be found in the Polish "Code of Ethics for Researchers" (KSEN, 2012/2013).

Ordinary plagiarism, perpetrated using the copy-and-paste technique, is usually detectable. Perhaps this is because we have access to increasingly modern Internet search engines, because authors and publishers of journals place electronic versions of papers on the Internet, because electronic anti-plagiarism systems have been created, and because, finally, this dishonest practice is more and more strongly stigmatized (and it is, at least potentially, punishable by law). A more serious problem (whose scale of occurrence is difficult to estimate) is:

[5] We wrote about this problem in Antonowicz & Brzeziński, 2013.

[6] See the following document: FPRM (2002). US Federal Policy on Research Misconduct. Retrieved May 4, 2014, from www.federalregister.gov/articles/2000/12/06/00-30852/executive-office-of-the-president-federal-policyon-research-misconduct-preamble-for-research#h-16 See also Brzeziński & Doliński (2014b).

(a) making up data (e.g., filling in the missing data or making up all data in such a way that they confirm the hypothesis); (b) "altering" data in a direction suggested by the hypothesis, and (c) stealing raw data and processing that stolen set of results "on one's own." The last of the above is a case of plagiarizing from the deepest level of the original study. It amounts to plagiarizing a study that has never been written before in the form proposed by the plagiarist. It is more difficult to detect than "patchwork" plagiarism or plagiarism consisting in "intelligent" changes to the structure of sentences. The already mentioned Diederik Stapel's case, presented in detail in many publications, falls into two FF categories. Stapel made up data and altered (falsified) them. He acted with thought-out premeditation. What was the fraudster's *modus operandi*? As Jarosław Klebaniuk (2012) writes:

> . . . He [Stapel – J. M. B.] acted in three ways: he either completely made up data, or fabricated such an extension of their basis as to obtain a confirmation of the hypothesis, or modified data – complete or incomplete. His activities usually started with the preparation of research, very thorough and carried out in consultation with collaborators. The idea, the hypotheses, the ways of operationalizing the variables, and the type of experimental manipulation were discussed. All materials were prepared, including questionnaires, sweets for participants, and other tangible manifestations of research being carried out. It was possible to finance many of those thanks to grants, which the "distinguished" scientist received without difficulty. The materials were usually placed in the trunk of Stapel's car and that is where they were last seen. What followed had nothing to do with academic honesty (pp. 213-214).

In this ingenious way, he reached an impressive number of several dozen articles containing (referring to) data that he either wholly or partly made up or subjected to "creative" statistical processing. A question must arise, however: What were reviewers actually doing, especially those in prestigious periodicals such as *Journal of Personality and Social Psychology*? Why were nearly all of them – except the three honest ones – taken in, as the already quoted Klebaniuk wrote (2012, p. 214), by data that "were obviously 'too good to be true'"? Despite the high position Stapel held in institutional science (he was a dean for some time), the falsification was revealed. That was possible thanks to the persistence of three young researchers.

There are, I believe, several factors that determine the occurrence of undesirable and – there is no denying it – shameful behaviors in the community of researchers.

The first one is pride, competition, and the desire to remain among the best. Working conditions or financial pressures are of no importance here. The desire to be one of the best means living in constant stress. People such as Stapel (who, after all, was a member of the elite and had no financial worries) are driven ex-

clusively by constant concern not to leave the top and to be always present at prestigious conferences as well as to publish (and to be cited!) in top professional journals. When such people run out of ideas their resistance to temptations becomes weaker and they enter a slippery slope. "Professor" Stapel painfully found out where that slope ends. A majority of psychologists would like to publish (and be cited) in the best journals. Social psychologists would be glad to see their work in top journals such as *Journal of Personality and Social Psychology, Journal of Experimental Social Psychology*, or *Personality and Social Psychology Bulletin*. The trouble is that not everyone can have their paper published there and not everyone is willing to come to terms with this.

The second factor is pressure from the employer: head of the department, director of the institute, dean of the faculty, or rector of the university. In recent years, Poland has seen intensified pressure exerted by heads of academic units on their staff to score points for publications, to speed up collecting scientific achievements necessary for opening postdoctoral degree (habilitation) conferral procedures or professorial nomination procedures. After all, the accumulation of a large number of such points by a unit will enable it to obtain a satisfactory category (A, B, perhaps even A+, but not C!) in the parametric evaluation carried out every four years by the Committee for the Evaluation of Scientific Units. This excessive bibliometricization of scientific output evaluation may, in extreme cases, lead to the choice of shortcuts: adding one's name to publications authored by others (especially by those to whom the "coauthor" is a superior), artificially fragmenting and repeating publications, plagiarizing, buying studies or their parts (e.g., advanced statistical analyses), etc. Also academic units, striving to come out as well as possible in parametric evaluation, resort to buying publications (which is favored by leaky law and the tendency to bend it to one's needs), the only condition being that authors affiliate them with that particular unit.

The third factor is social consent to and negligible consequences of misconduct. What is also conducive to pathological behaviors is the lack of an unambiguously strong reaction of the academic community to the violations of academic standards; this refers in particular to university authorities, especially in situations when "their own" employee is implicated. An example? Fairly often – also at large universities – we encounter the phenomenon of scandalous behaviors and ones that damage the good name of the university (usually, cases of plagiarism) being "swept under the rug." It happens that a case which is shameful and problematic to a given university is prolonged until it has expired and until its subject has retired or left the university "in a direction unknown." It also happens

that the penalty imposed on the guilty party (e.g., reprimand) is incommensurately light compared to the social harm. State institutions ignore the problem, too. Let the case of the prosecutor dropping charges against a university professor suspected of plagiarism serve as a fairly characteristic example of state institutions dismissing the violation of the principles of ethics in the academic community – on the grounds that "the social harm of the act is insignificant." It is very alarming that such a signal is sent: nothing is going to happen to you if you get caught.

The fourth factor is the excess of higher education institutions (as well as students), and also – despite the reduction of staff requirements for opening new programs and maintaining (one is often tempted to use the term "reanimation" here) those that ought to be closed, if only for the sake of decency – the necessity of employing scholars with a postdoctoral degree, or habilitation (the acceptance of second full-time employment of staff members included in the so-called minimum academic staff complement for departments offering undergraduate programs), which results in academic promotion being sought by people who do not feel the need to for such promotion, who are not talented, and who are not capable of writing a decent academic paper. Let us bear in mind that the normal distribution law works here, too. What can they do, then? They can either leave the university (but where to?) or try taking a kind of "shortcut." This often means plagiarizing, stealing other people's texts or parts of texts and compiling their own "study" out of them. It is easier to steal or to buy something ready. I believe we, psychologists, are also beginning to experience the negative consequences of such generous and at the same time insouciant policy of the ministry. It manifests itself in the excess of centers educating psychology students (today, there are already more than thirty institutions offering graduate studies in psychology and half of them are non-public schools). Is the state able to guarantee adequate staff to the thousands of psychology students educated in those institutions? No, it is not. The problem is that the state does not seem to care. As a result, we – as a country – provide lower quality education.

Can this alarming phenomenon be prevented? When it comes to plagiarism, thieves must be aware of the severity and inevitability of punishment. As regards cases of fraud that consist in fabricating data and manipulating them (twisting them to fit the hypothesis), I can see only one solution (and I am not the only one to recommend it): disclosure of the set of raw data by authors. By doing that, we put into effect – to a full extent – the weapon of **replication**. Commenting on Stapel's case, J. Klebaniuk (2012) wrote:

> Greater transparency of research – that is, ensuring **unconditional availability of raw data** as well as information concerning procedural details (those concerning the place where research was carried out and the group of participants have to be known at least to collaborators and reviewers, since they are not always of the kind that can be made public) – should be standard practice (p. 216) [emphasis by J. M. B.].

Addressing this issue (in the context of the Stapel affair), we wrote (Brzeziński & Doliński, 2014a):

> Commenting on those shocking events in the prestigious journal *Nature*, Jelite M. Wicherts (cf. Wicherts, 2011) wrote: "To scientists in other fields, not sharing data may seem extraordinary; to psychologists it is sadly **common practice"** [emphasis by J. M. B. & D. D.]. A way out of this situation could be the reanalysis of data that researchers would make available for reanalysis. However, not all psychologists would subscribe to this idea. Why not? Collecting empirical data is often a laborious and time-consuming thing – as well as costly, sometimes even very costly. . . . Someone with access to such data would not have to make the effort of conducting a study. . . . The asymmetry of work input between the one who has collected the data and the one who has only used it is obvious. . . . Nonetheless, we believe it is worth taking the risk that someone putting in a little amount of work (not doing their own research) will achieve the same or greater scientific success. than the author of research. . . . Psychology has gained much by adopting the methodology of natural sciences. Let us therefore be consistent: let us adopt natural scientists' practice of making raw data universally available (p. 797).

Making raw data available plus the requirement of replicating research before publishing its analyses is, in fact, the only remedy for FF frauds. It would also be helpful – across the entire field – if editorial teams of psychological journals changed their publication practices and decided to publish not only "positive" reults but also those that did not confirm hypotheses (see, e.g., Klebaniuk, 2012; Brzeziński & Doliński, 2014a; Sun, Pan, & Wang, 2010). After all, that is interesting information too, and it could be useful in planning further research. The acceptance of a paper for publication should not depend only on obtaining the value of $p < .05$ – as Jacob Cohen aptly noted in his highly cited paper (1994/2006).

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The reflections presented above, addressing important aspects of Polish psychologists' research work can be reduced to the following question: does the fact that research is carried out in the field of psychology and that it is carried out in Poland have any significance to the methodology of that research, to the education of psychology students and to the training of professionals as well as to the improvement of social practice, which should draw on that research in order to increase its effectiveness?

When doing empirical research in psychology – just like in any other empirical disciplines – psychologists should respect the fact that scientific cognition is rational and intersubjective, which means that psychological studies are replicable (Ajdukiewicz calls this the intersubjectivity principle). This is very important because it allows to eliminate from the body of empirically verified scientific knowledge those pseudo-findings (Stapel's case) that – despite following all the strict methodological and technological rules – were not successfully replicated by independently working researchers: at a different time, in conditions as close to the original ones as possible but still different, and with different participants. It is therefore essential that the results of original studies carried out by psychologists reach different research centers worldwide without any hindrance (linguistic or other).

The effective communication of research results – on a global scale, across "geographical and political frontiers" – takes place by publishing interpreted results in journals, accessible in the classic (paper) form or electronically (the Internet). In the mass of information, whether we want it or not, we have to select the source of information: we have to choose between psychological journals that differ in terms of scientific prestige. The journal's IF index will be helpful in this choice. Journals with high IF values tend to be reviewed more thoroughly and by highly competent specialists. Is it not possible to make a mistake and overlook a really important journal? Admittedly, it is – but even if no mistakes are made, one cannot possibly browse all the pages that have been printed. Having your work published in truly intersubjectively available journals, with a high likelihood that other research psychologists – the proportion being difficult to estimate – will get acquainted with your results is tempting indeed. What is more, we should seek opportunities for as many competent researchers as possible to get acquainted with our findings.

Apart from striving to ensure the widest possible availability of original findings (meaning those that contribute "something" to science), psychologists should also be concerned about the state of social practice, which largely depends also on the state of psychological research. It is important that results of valuable research (ideally, interpreted in a proper manner) be assimilated by psychological practice so that professionals can develop their diagnostic and treatment practice on their basis. This means that psychologists should publish their work also in the Polish language. A "side effect" of such conduct is the enrichment of the corpus of the Polish language with psychological terms and a considerable impact on the quality of translations of psychological papers from foreign languages into Polish.

The assessment of the academic prestige of researchers (including psychologists, naturally) in today's "parametrized" world of science, engaging in a "numbers game" is done by means of various indices, sometimes structurally sophisticated (and sometimes having the appearances of intersubjectivity): Hirsch's h-index, summary IF index, the number of citations, etc. Also in this area degeneration happens, and therefore DORA's reaction – warning researchers not to fall into bibliometric madness and advising them not to forget about the important role of peer review evaluation – is a healthy one.

The various forms of pathology (e.g., the already mentioned Stapel's case or the very recent case of another social psychologist, Jens Förster from the University of Amsterdam, reported in *Science*, no. 344 of May 9, 2014) spreading in the world of science (also in psychology) demand that the community of psychologists actively seek preventive measures. It is not only about stigmatizing the individuals who violate the ethical standards of the scientific community (which, of course, is important but would merely be a "day after" kind of reaction); it is about seeking measures that prevent this kind of behavior. It seems that the dissemination of raw data may be an effective measure of this kind. Community consent to the violation of ethical standards will be (has already been?) destructive to our discipline.

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