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Methodology through a cultural lens[1]. The Poznań Approach to Philosophy of Humanities Against Alternative Meta-Methodological Orientations

> Methodology of science ... embraces a broad realm of research associated with interpretation of the historical activity of man, which is ascribed to the field of what is termed symbolic culture.
>
> *Jerzy Kmita*

Introductory remarks. On the intellectual background of the Poznań Methodological School

The idea of methodology through a cultural lens stems from the milieu of Poznań Methodological School which is an inherent part of the Polish (and not only) intellectual landscape. Its works have contributed to the original academic achievements of the Polish humanities of the 20th century. The Poznań Methodological School was one of the more unique and creative philosophic-methodological *Denkkollektiv* in the post-war Europe. It was founded in the mid-60s of the XXth century by such scholars as Jerzy Topolski (1928–1998), Jerzy Kmita (1931–2012) and Leszek Nowak (1943–2009). The rise of this School was accompanied by publishing the first issue of the journal of *Dissertationes Methodologicae* subtitled *Studies for the integration of science*. Since the mid-70s Poznań Methodological School had developed in three parallel ways: one was set by Leszek Nowak with his idealizational theory of science [Nowak 1980]; the second — by Jerzy Kmita with his historical epistemology [Kmita 1988] and socio-regulative theory of culture [Kmita 1996]; and the third one — by Jerzy Topolski with his theory of non-

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-source-based knowledge [Topolski 1976]. The thought style distinctive to the Poznań Methodological School is still present in numerous works which, explicitly or implicitly, refer to the notion of idealization, scientific understanding of culture or initial knowledge in historical research.

Regardless of the differences, all scholars of the Poznań Methodological School represented an interesting epistemological orientation and practiced an excellent research craftsmanship, while at the same time realizing the intellectual will of their outstanding predecessors. The latter included representatives of the interwar period in the development of philosophy in Poznań (Władysław Mieczysław Kozłowski, Zygmunt Zawirski, Adam Wiegner) as well as scholars related to the Poznań milieu in the postwar era (Kazimierz Ajdukiewicz, Zbigniew Czerwiński, Andrzej Malewski, Jerzy Giedymin). Academic works of the predecessors have inspired intellectual activity of the Poznań Methodological School and had „significant impact on shaping the methodological orientation which dominated philosophy at the Poznań University in the post-war period." [Zeidler 2010, p. 188 — authors' translation].

Generally speaking, there are, at least, five major inspirations which influenced Poznań Methodological School's *Denkstil*:

- The Lvov-Warsaw School — in particular works of the already mentioned Kazimierz Ajdukiewicz (cognitive status of methodology, problem of justification in science, theories of meaning, rationality of fallible methods of inference, typology of styles of reasoning, method of paraphrase) [Sinisi, Woleński 1995];
- Karl Marx — in particular the method of abstraction, the notion of critique and the idea of the historical character of scientific cognition [Kmita 1970, Nowak 1980, Świderski 1985; Pałubicka 2017; Brzechczyn 2017];
- Max Weber — in particular the concept of ideal-types and axiological neutrality in science [Kmita 1976; Nowak 1978];
- Florian Znaniecki and his idea of humanistic coefficient [Kmita 1985, Plummer 2011];
- Karl R. Popper — in particular the methodological naturalism position and the anti-psychologist attitude [Giedymin 1975].

It is important to bear in mind that the above-mentioned inspirations influenced the style of thinking characteristic to the Poznań Methodological School to various degrees depending on the period of its functioning and on the interest of its particular members. When it comes to Kazimierz Ajdukiewicz (1890-1963), given the broadness of his scientific interests and accomplishments, it is not possible to discuss here all of his achievements. Thus bearing in mind the problem formulated in the title of this paper, the

authors will limit themselves to mentioning the distinction he made between metascience, methodology and methodics [Ajdukiewicz 1977 (1948)]. Metascience is meant as a domain of knowledge which is interested in science understood in an ideal way, that is expressed via a certain ahistorical deductive system, which is later verified when it comes to consistency, completeness or fullness. Methodology, on the other hand, is meant as a discipline dealing with products (apragmatic methodology) and scientific actions (pragmatic methodology) which are of a historical character. Finally, methodics of a given science (meta-research) is meant as rules and guidelines formulated by outstanding specialists in a given field, whose competences are universally accepted within a given scientific community. These rules differ from methodological norms in that they are based to a larger extent on empirical premises [ibid., pp. 1-12].

Representatives of the Poznań Methodological School do not agree with reducing methodology either to metascience or to methodics. That is why they reject the assumption that reflection on science should focus solely on sciences which have empirical theories with a strictly defined language. They share Ajdukiewicz's conviction that „methodology, unlike metascience, is a humanistic science" [Ajdukiewicz 1977 (1960), p. 51]. At the same time, they disagree with the notion that methodology can be reduced to methodics because they reject the assumption that reflection on science should be exclusively the domain of specialists from a given special science. It is because the latter do not justify their statements in an exhausting way nor do they reveal all the tacit premises included in their reasoning. They do not have to do it because they find it unnecessary in realizing the planned scientific goal or overly costly because it would require engaging cognitive and practical inputs needed to carry out their scientific research [Kmita, Nowak 1970, p. 45].

The intellectual background mentioned above had an impact on the scope, the formula and the cognitive status of methodology within the Poznań Methodological School and distinguishes the latter from other meta-methodological orientations[2]. The discussion on ways of theorizing and practicing methodology in the Poznań milieu will be limited in this paper mainly to a culturalistic program of methodological research of Jerzy Kmita but this choice in no way undermines the cognitive significance of works of the remaining members of the Poznań School (such as Leszek Nowak's idealizational theory of science or Jerzy Topolski's theory of non-source-based knowledge). What is more, we would like to underline that the culturalistic programme stems from the same epistemological orientation as

[2] In order to compare the philosophical orientation of Poznań Methodological School with rival programmes of methodology see also: [Giedymin 1975; Świderski 1984, 1985].

the two remaining ones. After having sketched in the introductory remarks the intellectual background of the Poznań Methodological School (section 1), in order to contrast the latter with other approaches we take up the diachronic approach to methodology (section 2), as well as the synchronic one (section 3). This enables to arrive in the concluding remarks at the basic meta-methodological characteristics of the Poznań approach to philosophy of humanities and the culturalistic programme of methodology (section 4).

A diachronic approach to methodology

The diachronic approach is applied here to distinguish and discuss the subsequent proposals concerning the development of methodology, that is, changing patterns of the cognitive status of methodology, formula of practicing methodological research, and the scope of the discipline in question. Three basic questions can prove useful when adopting the diachronic approach to the discipline of methodology:

- is methodology normative or descriptive?
- how science is understood?
- what is the unit of analysis of science?

Addressing these questions enables to expose and compare different programmes formulated as alternative to *the theorio-scientific programme of methodology*. The latter, elaborated mainly by Rudolf Carnap, Carl G. Hempel or Ernst Nagel in the form of the sentence view, is commonly considered a foundation of modern methodology. This programme was later taken up and originally developed by Patrick Suppes, Frederick Suppe, Joseph D. Sneed and Wolfgang Stegmüller in the form of the non-sentence view [Zeidler 1984]. Furthermore, it is commonly accepted that during the XX century, the philosophical and methodological reflection on science has taken a few turns. As Thomas Nickels put it:

> There have been many calls even within philosophy for a *naturalized* epistemology or methodology of science and for a naturalized philosophy of man generally. At roughly the same time, philosophy of science also took an *historical* turn, which saw science and method as products of human historical development. More recently, we have heard demands also for „epistemology *socialized*. … Moreover, proponents of each of the three approaches have claimed to supersede philosophy of science or methodology. [Nickels 1989, p. 242]

However, two short remarks to Nickels' comment needs to be made. Firstly, we share Nickles' opinion that the general methodology of science

underwent the three turns he enumerated and that risk emerges that the privileged disciplines behind these turns (history of science, sociology of science, cognitive sciences) will, eventually, be inclined to promote their methodologies as the privileged ones. This certainly would lead to a kind of "hegemony" [Stump 1992, p. 459], "universalism of an 'imperialist' kind" [Gonzales 2012, p. 161], "imperialistic exploits" [Dupre 2001, p. 17], "vicious imperialism" [Woolgar, 1989, p. 217] or "tyranny of ... single approach or ... single method" [Suppes 1993, p. 14]. Secondly, we believe that the list of turns enumerated by Nickles should include the "economic turn" in the contemporary philosophy of science. Our suggestion is supported mainly by the fact that there are some original and systematic research carried out under the auspices of economics of science, research and scientific knowledge. Moreover, as Wade Hands put it, "economics recently has become actively involved in the general study of scientific knowledge; science theory has begun to take an economic turn." [Hands 2001, p. 354]. However, it certainly does not mean that the "economic turn" accompanied by the rise of the economics of science, lay any (explicit or implicit) claim to supersede the philosophy of science. What the supporters of the 'economic turn' would admit is that their approach to science only presents "methodology and epistemology as if economics really mattered" [Wible 1998].

There is a consensus in the contemporary meta-methodological discussion on the foundational nature of the *theorio-scientific programme of methodology*. Two undoubtedly symbolic dates mark the development of the discipline of methodology: in 1929 the Vienna Circle published their philosophical manifesto and in 1962 Thomas Kuhn published „The Structure of Scientific Revolutions." This time framework marks the emergence of two most important research programmes in methodology — *theorio-scientific* and *historical*. The first programmes assumes primordiality of logical statements in methodology resulting from the adopted model of science over findings in the field of history of science. In the second research programmes it is assumed that the subject of methodological research is science as a historically given and entangled discipline [Zeidler 1991, p. 3]. Of course, particular stances within these research programmes were internally diversified but from a general perspective, the *theorio-scientific* stance, using formal-logical methods, called for a logical reconstruction of scientific knowledge regardless of the historical context of its discovery. A dynamic development of mathematical logic and mathematical methods significantly contributed to it. The logical turn undoubtedly changed the shape of philosophical considerations on science compared to the 19th century methodology. The same applies however to the emergence of the historical program:

> It seems remarkable that the "historical turn" changed the philosophic--methodological panorama completely. ... The change in the *turn* includes historicity as an internal constituent of science as well as an external factor. [Gonzalez 2006, p. 3]

Taking into account this fact, it is practically impossible to put the historical dimension of scientific knowledge completely aside in methodological consideration, which, however, does not determine a concrete form of historicizing methodology. Nevertheless, there are some risks attributed to this turn. As Patrick Suppes once noticed, after having witnessed the historical turn: "we face currently a new imperialism of historical methods" [Suppes 1993, p. 3]. Of course, it does not mean that the formal methods are completely relegated from the historicized philosophy of science. They will be still in use in such historicized philosophy of science as long as they are assumed or certificated to be useful in the very discipline of history [ibid., p. 4].

Representatives of sociology of science justify the importance of their research agenda analogically to historians of science, although, „[t]o be sure, ... 'social turn' is a move that covers a larger area and introduces a more radical scope than the preceding 'historical turn'" [Gonzales 2006, p. 9]. We propose here to shortly discuss the *sociological programme of methodology* by elucidating its negative and positive agendas that were common to this approach. The positive programme focuses on the (sociologically) unquestioned thesis of the social conditioning of science. Of course, this thesis was interpreted in several ways, depending on the concrete perspective taken during the sociological reconstruction of science [Shapin 1982]. The negative programme, on the other hand, aims to problematize the traditional philosophical notion of representation. By the latter we mean cognitive operation in which the subject, intentionally and purposefully, uses a certain object to represent something else. It was this general account on representation that became the target of sociological criticism. In the first stage of this criticism (let us call it an asymmetry-based analysis) the thesis of social conditioning of science was applied, but only to those scientific products which were recognizes as erroneous (false). This stage can also be referred to as sociology of errors and distortions (or blind-alleys and wrong pathways, as one can put it). In the second stage (let us call it a symmetry-based analysis), not only the false scientific knowledge was taken under sociological scrutiny, but also knowledge recognized so far as true (or credible). The rationale behind this was that the social factors can causally determine the production of false (not credible) scientific knowledge, as well as true (credible) one. In the third stage, the sociology of science refuses the traditional notion of representation and claims that scientific cognition

does not represent anything. It is rather "actively constituting objects in the world; in other words, representations create the world rather than reflect it" [Woolgar 1989, p. 206]. At the same time, the ethnographic methods are applied in order to reveal that "representation is not something to be taken for granted when studying science. It is instead an aspect of the culture of science which requires examination" [ibid., p. 203]. The *sociological programme of methodology* has generated many interesting problems, triggered many critiques, as well as many inspirations. Among problems, there is one especially interesting to our further investigations, namely the negative stance of the sociological approach towards a pluralistic attitude to the relation between sociology and other disciplines. According to Woolgar, the very idea of pluralism is based on the presupposition that "objects pre-exist their study" [ibid., p. 217]. The latter in an obvious way contradicts the basic feature of the sociological approach that different research projects constitute their own objects of study. Woolgar states that behind the pluralist programme (e.g. concerning the relation between sociology and psychology) lies the "vicious imperialism of the triangulation myth" [ibid., p. 217], according to which different approaches with their own methods can contribute to the common (fixed) object of study. Such a negative attitude toward pluralism triggered criticism from Thomas Nickles who said that rejection of pluralism in the way Woolgar did, represents imperialistic and "uncharitable display of academic territoriality" [Nickles 1989, p. 226].

When it comes to inspirations triggered by the general sociological approach, one can note that it encouraged the community of economists to announce their own programme. This is how we arrive to "an economic perspective on science [that] has not existed until quite recently. It is time for an economics of science to take its place alongside the other major approaches to studying science" [Wible 1998, p. 14]. Still in 1984 some commentators expressed their discontent with the fact that economic ideas and methods are underrepresented in studies on this field, wondering about a „conspicuous absentee: a dismal science of science" [Bechtel 1993, p. 484]. In the light of existing demand, one did not have to complain long about shortages in the supply of economic ideas in the studies on science. One of the first contributions to the economic analysis of science was the article of Charles S. Peirce regarding the „economy of research". His approach was taken up by Nicholas Rescher and in particular by Gerard Radnitzky [1987a, 1987b] and called "economic epistemology." Rescher focused on the economic theory of research project selection [1978a, 1978b, 1989], while Radnitzky offered an interpretation of Popper's methodology in terms of the cost-and-benefits-analysis (CBA), focusing on the choice of theories and facts [1987a, 1987b]. What is common to this approach is treating

scientific research as an optimization process in which by means of cost and benefits calculus it is possible to frame epistemic as well as non-epistemic (practical) values. Another contribution of economic approach has more in common with sociology of scientific knowledge and can be dubbed here "economics of scientific knowledge." According to this view, science can be referred to as "the marketplace of ideas" [Bartley 1990], a metaphor that had not been previously used in the context of science, while scientific knowledge (or scientific products, more generally) can be viewed in terms of economic goods. The combination of two initial criteria, namely rivalry and excludability, generates four types of scientific knowledge-as-an-economic-good: (i) public good (non-rivalrous and non-excludable), (ii) common good (rivalrous, non-excludable), (iii) club good (non-rivalrous, excludable), and (iv) private good (rivalrous and excludable). The last contribution we want to discuss here is the so-called new economics of science [Dasgupta, David 1994], according to which

> ... what fundamentally distinguishes the two communities of researchers [basic science and applied sciences] is not their methods of inquiry, nor the nature of the knowledge obtained, nor the sources of their financial support ... It is the norms ... in regard to the disclosure of knowledge, and the features of the reward systems that constitute the fundamental structural differences. [ibid., p. 294-295]

The *economic programme of methodology* has generated many interesting problems, triggered many critiques and was a source of many inspirations. It has been noticed among philosophers interested in analyzing the current tendencies of commercialization of science, commodification of research and marketization of scientific ideas and products. At the same time, one should bear in mind an opinion formulated by Phillip Mirowski, according to whom

> The attempt to constitute a thriving "economics of science" ... is not calculated to win friends and converts to the project, nor will it achieve its intended effect of recommending economic analysis to the denizens of science studies, but will rather simply confirm ... pre-existing prejudices about economists being incorrigibly imperialistically aggressive, ... lumpenly lead-footed when trespassing on other disciplines. [Mirowski 1996, pp. 72-73]

Finally, we distinguish the cognitive (naturalistic) programme of methodology, which was founded in the 80. of the XXth century. Its main spokesperson is Ronald Giere, who stated that the cognitive turn results from discontent with hitherto accounts of science, not only with the traditional philosophy of science but also its sociological and historical approaches. Analyses carried out within the traditional philosophy of science turned out

to be disappointing because they failed in finding standards of rationality in science. The sociological perspective, on the other hand, is disappointing in his view, because it does not allow to explain the success of science, especially science-based technology. Finally, Giere's critique of the historical approach stems from the fact that it does not offer an explicit theory of science. However, he underlines: "To be sure, historians do have theories of science, often borrowed from elsewhere, implicit in their practice. But those theories are not the focus of their inquiry" [Giere 1988, p. 18]. Instead, he offers an alternative in the form of a cognitive theory of science. Its starting point is that scientists, just like everybody else, have biologically based cognitive capacities and employ these capacities in interactions with the world. Therefore, the basis for a cognitive-oriented methodology is not rationality or social game of interests in the scientific milieu but rather a causal interaction between scientists and the world. Under such an approach methodology and philosophy of science become cognitive science of science and its deeper theoretical foundation does not lie in epistemology or philosophy of language but in evolutionary theory:

> By looking back at evolutionary history, scientists themselves can better understand their own cognitive situation and investigate the development of their own cognitive capacities. What seem to the traditional epistemologist like vicious circles are, in this alternative picture, "positive feedback loops." Using our evolved cognitive capacities, we extend our knowledge of the world, including our knowledge of our own cognitive abilities. This latter knowledge helps us to extend our knowledge of the world still further. [Ibid., p. 13]

The cognitive programme is internally diversified and encompasses various models of science coming from cognitive psychology, neuroscience and artificial intelligence [Giere 1992]. Despite this diversify, a common assumption of this approach is that individual scientists are the basic units of analysis of science [Giere 1989, p. 8] rather than research programmes, research traditions (historical approach) or professional interests related to particular social groups (sociological approach). *The naturalistic programme of methodology* thus concentrates on individual scientists who remain in causal relation to the world and focuses on individual choices as well as limited and biased judgmental strategies. Regardless of what one's attitude to the naturalistic (cognitive) turn in methodology is, one has to admit it certainly has taken place. The question is whether the supporters of that programme will aim to reduce reflection on science to their privileged discipline. If they do, then we have to start to "worry about hegemony" [Stump 1992, p. 459].

As a consequence, the naturalistic turn has recently put into question the relevance and future of certain non-naturalized accounts of science. However, in our opinion, there is still hope for a sound humanistic account of science and one should be very careful with jumping to premature conclusion that the humanistic account has definitely expired. We agree with David Stump [1992, p. 459] that a sound account of science excludes a return to the view of methodology as an autonomous[3] discipline. Also, we share his belief that „cognitive science will have a role in science studies, *as will the traditional humanistic disciplines*" [ibid., p. 459 — emphasis added]. What is more, this forecast is coherent with approaches treating methodology as a science of culture proposed by Carlos Ulises Moulines and Jerzy Kmita. The first author claims that "[p]hilosophy and history of science both belong to the sciences of culture: they both study that cultural phenomenon that we call 'science'" [Moulines 1983, p. 284]. Jerzy Kmita, on the other hand, straightforwardly stated that "methodology of science ... belongs ... to the sphere of the historical sciences about (symbolic) culture" [Kmita 1974, pp. 47-48]. A comparative advantage of Kmita's perspective consists in having a well-elaborated framework in the form of extended and advanced theory of culture. Kmita intended to create scientific humanities, by which he meant humanities with theoretical and cognitive ambitions. Kmita for many years led a kind of scientific-humanistic investigation aimed at tracking and detecting cases when humanities only imitated or even feigned scientific standards. Kmita called humanities which achieve certain non-cognitive results by imitating or feigning scientific standards, the "humanists' sorcery" [Kmita 2015, p. 116]. In a clear opposition to such a style of running humanistic research, he consistently developed a theory of culture he called socio-regulative theory of culture.

A synchronic approach to methodology

In the previous section we discussed the subsequent proposals concerning the development of methodology using the diachronic approach. In this section we take up the synchronic approach to methodology. As we have

[3] There appear the problem of understanding the expression "traditional methodology was autonomous"? Does it mean that *the theorio-scientific programme of methodology* was autonomous? If the answer is positive, then the next question arises: autonomous versus what? Certainly not versus discipline of logic, because that programme was founded on logic. Possible answer is that the traditional methodology was autonomous versus scientific practice. All of this only confirm that the term "autonomy," as used by Stump, needs further explication.

already signalled, each of the demonstrated programmes faces the risk of being accused of imposing "hegemony" [Stump 1992, p. 459], supporting "universalism of an 'imperialist' kind" or aiming at "predominance" [Gonzales 2012, pp. 161, 158], promoting „imperialistic exploits" [Dupré 2001, p. 17], revealing "vicious imperialism" [Woolgar, 1989, p. 217], being "imperialistic [in] insisting that my alone is correct and that any other approach begs the central questions" [Nickles 1989, p. 226], or finally of exemplifying "tyranny of any single approach or any single method" [Suppes 1993, p. 14]. It appears that each of these turns actually triggered this kind of critique. Their adversaries shared the general presumption that in each of the cases described below, concrete disciplines behind a given turn (logical theory of science, history of science, sociology of science, economics of science, cognitive science of science) were in fact inclined to treat their own methodology as a privileged one:

- regarding the theorio-scientific programme it was pointed out that its „reductionist view of the place of formal methods in the philosophy of science is now faded" [Suppes 1993, p. 3];
- in the case of the historical programme it was noted that after having witnessed the historical turn, "we face currently a new imperialism of historical methods" [ibid.];
- the critique of the sociological programme was twofold: firstly, it was pointed out that the rejection of pluralism in the way Woolgar did, represents "uncharitable display of academic territoriality" [Nickles 1988, p. 226]; secondly, it was noted that the sociological approach "officially opposes the very idea of a universal theory of science and [is] nevertheless inclined to promote a universal method for studying science" [Fuller 2000, p. 343];
- the economic programme faced the charge of „being incorrigibly imperialistically aggressive" [Mirowski 1996];
- in the case of the cognitive (naturalistic) programme, a warning was raised that if it leads to „overextending favoured discipline" and if majority of its proponents opt for reducing science studies to their discipline, one should blow the whistle and start to "worry about hegemony" [Stump 1992, p. 459].

The synchronic approach is applied here to analyze a situation in which the programmes of methodology do not occur one after another in a way that when one disappears it is replaced by the following. What is important to the synchronic approach is the possibility of coexistence of various competitive (rival) programmes of methodology. We believe that it is the synchronic approach Patrick Suppes had in mind when he stated that pluralism is the "normal state of affairs" in the meta-methodological

discussion [Suppes 1993, p. 3]. Other scholars dealing with science studies seem to support Suppes' point: Thomas Nickles says that "[t]here is not only room for everyone but *a need* for everyone, even philosophers of science" [Nickles 1989, p. 225 — emphasis added], while David Stump stated that „cognitive science *will have a* role in science studies, *as will the* traditional humanistic disciplines" [Stump 1992, p. 459 — emphasis added].

If our reconstruction of Suppes', Nickles' and Stump's arguments is correct and pluralism is a normal (Suppes), inclusive (Nickles) and conditionally prospective (Stump) state of affairs in the meta-methodological discussion, then it is reasonable to carry out further synchronic analysis and clarify the concept of pluralism. Undoubtedly, Patrick Suppes in his analyses related pluralism to sciences, as well as to philosophy and methodology of science. These two references generate two autonomous notions of pluralism which should not be confused. As Kellert, Longino and Waters put it: "[i]t is useful to distinguish between plural*ity* in the sciences and plural*ism* about the sciences" [Kellert, Longino, Waters 2006, p. ix]. However, we underline that the first one refers to sciences and the second one to philosophy and methodology of science. While previous debates on pluralism were focused on the former, for instance on the problem of unity or disunity of science, more recently the focus shifted to "discussions about how philosophical, historical, and sociological accounts of science relate to one another" [ibid., p. viii].

It is another case in point that programmes of methodology can be approached not only purely diachronically but also synchronically as we are dealing with coexistence of competitive programmes and not with succession. Competitive methodologies thus function parallelly and the question arises whether it is possible to investigate relations between them. Some tensions between them do occur but according to Suppes, it is a welcome state of affairs. In our opinion, these tensions as well as challenges the pluralism about science faces are visible in at least two tendencies occurring in the contemporary meta-methodological discussion. These tendencies either try to push pluralism about science in the direction of universalism or in the direction of relativism. While discussing them it will be useful to adopt the following question as a criterion:

- whether a certain conceptual scheme allowing a comparison of different languages and conceptual frameworks linked to them is cognitively available (or exists)?

In the context we are interested in, it is about different conceptual apparatuses determining different meta-methodological orientations. We would like to distinguish the following positions the pluralism about science is facing:

1. *universalism* which is not a uniform stance and two of its types should be differentiated:
    1a. *universalism in the strong sense* or to put it differently, "universalism of an 'imperialist' kind" [Gonzales 2012, p. 161], where "an assumption of a dominant method (a 'standard') ... might be accompanied *de facto* by a clear disregard for any other alternative method for a concrete domain" [ibid., p. 157]; in this type of universalism, imperialism of a given programme manifests itself both in an active (by imposing privileged methods of studying science), and passive option (by disregarding of alternative methods of studying science); in the context we are interested in, this stance assumes the existence or cognitive availability of a privileged conceptual scheme, and thus acknowledges the possibility of comparing different languages and conceptual frameworks linked to them.
    1b. *universalism in the weaker sense*, which stipulates that „the methodological pluralism [about science — authors' note] can lead to a different version of universalism in methodology of science: the analysis might show something that is shared by the *diversity of methods*" [Gonzales 2012, pp. 171-172]; in the context we are interested in, this stance postulates the existence or cognitive availability of some conceptual scheme; thus it is positive about comparability but what is problematic here is what it means that „something is shared" — if it is taken literally, it makes it a strong statement; if it is understood by means of analogy, it is then a weaker statement; another problematic issue is that while comparability of different conceptual apparatuses is assumed (or postulated) here, it is not demonstrated how to make such a comparison; these problems arise from the fact that proponents of this stance postulate comparability via some form of analysis to escape an imperialistic universalism, but fail to sufficiently specify the non-imperialistic method of analysis of „something that is shared by the *diversity of methods*" [ibid.] for studying science.
2. *relativism* which is also not a uniform stance and two of its types should be differentiated:
    2a. *relativism in the strong sense* (let us call it, after Jerzy Kmita, relativism with a capital 'R') — this stance expresses a „suspicion about calls for a pluralistic attitude" [Woolgar 1989, p. 217]; according to this view, pluralism about science

promotes "a vicious imperialism", because it assumes that there exists a common object for different programmes of methodology; relativism with a capital 'R' rejects this assumption as a „triangulation myth" and claims that what really is at stake is „to decide between approaches" in a meta-methodological discussion [Woolgar 1989, p. 217]; in the context we are interested in, this stance fully rejects the existence or cognitive availability of a privileged conceptual scheme, and thus denies the possibility of comparing different languages and conceptual frameworks connected with them.

2b. *relativism in the weaker sense* (let us call it, after Jerzy Kmita, relativism with a small 'r') — this stance has been devised as the third option in the dispute between strong relativists (relativists with a capital 'R') and anti-relativists. Relativism with a small 'r' is a „neither…nor" option: it neither assumes a cognitive availability of a supercultural conceptual scheme, nor it denies such a possibility. For a cultural relativist with a small 'r', both the positive and the negative theses are of a metaphysical nature [Kmita 1996, pp. 545-547]. In the context we are interested in, no final conclusions as to their comparability are drawn in advance. The decision can be made only on the basis of assessing the attempt of comparison. Thus instead of taking a positive or negative metaphysical judgement, a methodological criterion is offered. This criterion, in a simplified way, can be called a criterion of reconstructive power. In this sense relativism with a small 'r' is a stance according to which:

> … different reconstructions of particular cultures (as well as the conceptual systems connected with them) can be comparatively evaluated from the point of view of the scope of the interpretative and predictive possibilities of those reconstructions. … These reconstructions, obviously, are performed within particular cultures. Minimal or null interpretative and predictive possibilities of all reconstructions of the culture A that come into play within the culture B testify to untranslability of (a conceptual system) of the culture A into the culture B. [Kmita 1996, pp. 541-542]

It can thus turn out that two conceptual systems are incomparable (despite of what the universalism in the strong sense claims), and as there are no "neutral" (supercultural) reconstructions, making a complete comparison is theoretically and practically unachievable. In this sense, taking into account

the criterion of reconstructive power, one can assume that comparability in practice is possible partly at most (despite of what the relativism in a strong sense claims) — some parts of conceptual apparatuses will remain uninterpretable in other conceptual apparatuses.

To sum up, on the one hand, the culturalistic programme of methodology shares the general view declared by supporters of pluralism about science that "[t]rying to force them [different perspectives on science, including the historical, normative-philosophical, and social-scientific — authors' note] into a convergent viewpoint or demanding a choice among them is counterproductive" [Kellert, Longino, Waters 2006, p. xxvii]. It implies breaking with universalism of an imperialistic kind. On the other hand, the culturalistic programme distances itself also from the two remaining stances: while it accepts the possibility of partial comparison of conceptual apparatuses determining different meta-methodological orientations, it also distances itself from relativism in the strong sense. However, contrary to universalism in the weaker sense, while refusing to acknowledge elements shared by different conceptual apparatuses, it indicates the possibility of partial comparability of different meta-methodological orientations based on reconstructability of one culture in the apparatus of another culture.

Concluding remarks. An outline of the culturalistic programme of methodology

In the last section we are going to present some basic meta-methodological features of the Poznań approach to philosophy of humanities with special emphasis put on the culturalistic programme of methodology. As it has been already mentioned (section 1), the culturalistic programme of methodology was founded by Jerzy Kmita and stems from the epistemological orientation formed in the milieu of the Poznań Methodological School. According to Kmita's view, methodology of science is a historical science about symbolic culture. The notion of culture then is understood not in some commonsensical terms, because it was clearly defined on the ground of a well-elaborated and coherent socio-regulative theory of culture[4] (section 2). Making the culture the base for

[4] „Culture is a set of normative and directive beliefs which: (1) are commonly followed by the members of community, (2) intentionally determine functional actions in relation to the global state of community where the state is regarded as the structural context of these actions ... And each of these [types of social praxis — authors' note] is subjectively regulated on a social scale by a certain set of beliefs commonly followed in community, and thereby belonging to culture. Beliefs create socio-subjective regulator of particular types of praxis [e.g. scientific practice — authors' note]" [Kmita 1996, pp. 582-583].

methodology, eliminates the risk of the discipline of methodology yielding to the tendency of both universalism of an imperialistic kind and relativism of a territorialistic kind (section 3).

In order to outline the culturalistic programme of methodology it is useful to start here with some initial questions. The first question is whether methodology is normative or descriptive? The answer provided within the culturalistic programme within the Poznań Methodological School enables going beyond normativism and pure descriptivism and offers a stance called supplementarism. This position is based on the idea that methodology is, in fact, a humanistic discipline. The precursor of such an approach was Kazimierz Ajdukiewicz, one of the leading representatives of the Lvov-Warsaw School. The founder of the culturalistic programme of methodology, Jerzy Kmita agrees with the general statement of Ajdukiewicz that methodology is a humanistic discipline as far as it deals with human research activities [Ajdukiewicz 1977, p. 6]. However, Kmita's account differs when it comes to justifying this proposal. Ajdukiewicz followed, to some extent, the tradition of German anti-naturalistic methodology of humanities by adopting a specific procedure of understanding (*Verstehen*) [ibid., p. 9], while Poznań approach departed from dissatisfaction with the existing methodological dualism. According to Poznań approach there is no such a distinct procedure as understanding and the most general methods of scientific proceedings are common for natural sciences and humanities. This approach is defined as a methodological unism.

How such an orientation manifests itself in daily activities of methodologists? First, evaluation patently accompanies methodologists in their efforts. However, the fact that a methodologist judges a certain scientific activities and products based on their cognitive merits, "is of no relevance for the scientific control of persistence of his statement" [Kmita 1973, p. 184]. This situation refers to a methodologist precisely as a methodologist, not merely a researcher or human being. What is most strongly emphasized by Poznań approach is that the scientific reasoning and scientific practice in general are enthymematic. As Jerzy Kmita stated:

> It is an odd phenomenon that, although in the methodology of sciences the assumption of the enthymematic character of the actually performed research action is generally, more or less, accepted, still, very infrequent is the methodological reflection pertaining to this question; the methodologist exceedingly seldom ponder upon the validity of the means employed for the completion of premises explicitly formulated by the researcher … the formulation of literal, "phenomenalistic" description of the actual research actions, and the definite manners of their completion results from methodology of sciences being a humanistic discipline. [Kmita 1973, pp. 180-181]

There are two ways of methodological investigation of scientific practice: a descriptive one and, what is crucial here, a reconstructive one. The first, which we may call methodological phenomenalism. In this approach, methodological research concerns only what is expressed (given explicitly) in scientific products and based on observation of daily activities of researchers. The second, which we may call the methodological supplementarism, goes further and addresses the question of integral components of scientific practice omitted by the above-mentioned tradition, namely the hidden premises, tacit assumptions, as well as implied presuppositions. These components are "non-observables" inherent to the methodological consciousness of scientists.

This is how we arrived at the second and third question, namely how in the culturalistic programme science is understood? and what is the basic unit of analysis of science? The answer provided within the culturalistic programme enables supplementing the two commonly accepted notions of science, namely, as a set of actions and as a set of products by offering the third general understanding of science in terms of a domain of a symbolic culture. While within the culturalistic approach it is assumed that science is a research practice and that products of this practice are also science, the basic unit of analysis is cultural regulator of research practice, namely methodological consciousness.

Because of the central function it plays within the Poznań approach, the concept of methodological consciousness needs to be discussed here in more details. The notion in question encompasses cognitive norms establishing goals of scientific inquiry and methodological directives determining manners of realization of such goals. Codification, systematization and verbalization of these elements of methodological consciousness is an objective function of methodological investigation. This function is objective in the sense that it is independent from how an individual methodologist sees the nature of her activity [Kmita 1988, p. 18; 1991, p. 22]. We have thus arrived at an important difference, namely individual and collective methodological consciousness. The first one refers to the consciousness of an individual researcher, while the latter — to scientific community limited to a given domain of science and to a historical stage of its development. The exploration of the collective methodological consciousness encompasses among others the reconstruction of its forms in particular currents, schools or epochs [Kmita 1974, p. 44]. What is therefore the relation between individual and collective consciousness? According to the Poznań approach, the latter is not a simple sum or mean of the former. The collective consciousness is prior to the individual one but this does not imply a form of naïve social determinism:

> … nothing forces an individual to respect norms and rules of social consciousness but the need to implement his/her values in a rational and efficient manner. [Kmita 1991, p. 187]

The bridge that connects the collective and individual consciousness is founded on rationality assumption related to the individual consciousness.

Norms and directives contributing to the collective methodological consciousness are commonly followed by members of scientific community. However, there are two ways in which this can happen:

- accepting — believing explicitly, that is, consciously expressing the appropriate elements of methodological consciousness; accepting is necessary to consciously state but not necessarily to act;
- respecting — believing implicitly, that is, acting in accordance with appropriate elements of methodological consciousness (methodological norms and directives) without stating it explicitly [Kmita 1996, p. 579].

It does not mean that methodologists create or invent products of their research. The Poznań approach to this issue is rooted in the work of Kazimierz Ajdukiewicz, who pointed out the following*:*

> These norms are not the product nor the decree of methodology. Methodology does not establish these norms it discovers them in the practice of the historically existing sciences. The norms are known to the specialists in the respective fields, who do not, however, reflect on their function, but just know their practical application like people with a practical knowledge of a language also practically know its grammatical norms. This practical knowledge of the norms consists in the ability to act according to them and in the ability to observe violation of these norms. [Ajdukiewicz 1977, s. 8]

Therefore, the basic task of methodology is the reconstruction of the collective methodological consciousness. This does not mean that the individual variant is unimportant for researchers' activities. As Jerzy Topolski addresses the question to the audience of practicing historians:

> Methodological principles, together with the ideal of science and the view of the world and Man … constitute what is called methodological consciousness … if we add the emotional factor we shall obtain the set of basic elements determining … investigation. [Topolski 1985, p. 149]

Methodological consciousness contains, apart from the cognitive norms and methodological directives, also the researcher's world-view (*Weltanschauung*). All these components are objects of methodological reconstruction. The structure of researchers' methodological consciousness consists of five major components:

- epistemological (cognitive norms);
- methodological (directives);
- meta-scientific (aim of scientific cognition);
- metaphysical (world-view);
- anthropological (vision of Man).

Therefore, the reconstruction of methodological consciousness is based on a diagnosis of its explicit or implicit components. Of course, there appears the problem of *methodological self-consciousness* of researchers as researchers.

The culturalistic programme of methodology underlines the plurality of conceptual schemas, including scientific ones, as well as their cultural and historical variability. In this respect this programme falls between the universalistic and strongly relativistic tendencies:

> ... there are cultures, for which some 'parts' of some other cultures are uninterpretable; on the grounds of this thesis the term 'some' cannot be replaced with the term 'all' in neither of the two instances of using this term. [Kmita 2000, p. 202 — authors' translation]

This specific intermediary location of the culturalistic programme should not be viewed as a result of a simple methodological compromise. It is a far more important issue: comparability or incomparability should not be settled in advance (in this case metaphysically). What is decisive in this respect is the reconstructive potential of the culturalistic programme.

To sum up, the Poznań approach to philosophy of humanities in general and the culturalistic programme of methodology in particular enables going beyond normativism and phenomenalism of methodology and offers a stance called supplementarism. Science is treated here as a domain of symbolic culture so the methodological consciousness of a given scientific community becomes the basic unit of analysis. This type of consciousness is a set of epistemological norms and methodological directives respected or accepted by members of a given scientific community, and becomes an object of methodological reconstruction.

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Methodology through a Cultural Lens. The Poznań Approach to Philosophy of Humanities Against Alternative Meta-Methodological Orientations

Abstract

The article discusses the heritage and prospects of the Poznań approach to philosophy of humanities. It focuses on the meta-methodological questions and demonstrates

the Poznań Methodological School in general, and the culturalistic programme of methodology in particular, against the background of rival meta-methodological orientations. In the introductory remarks the authors sketch the intellectual background of the Poznań Methodological School (section 1) and then take up the diachronic approach to methodology (section 2) as well as the synchronic approach (section 3) in order to contrast the Poznań Methodological School with other programmes. This enables to arrive in the concluding remarks at the basic meta-methodological characteristics of the Poznań approach to philosophy of humanities and the culturalistic programme of methodology (section 4).

Keywords: meta-methodology, methodology of science, philosophy of science, diachronic approach, synchronic approach, method of reconstruction, Poznań Methodological School, the culturalistic programme of methodology, socio-regulative theory of culture.