Edited by Anna Brożek, Alicja Chybińska,
Jacek Jadacki, Jan Woleński

Tradition of the Lvov-Warsaw School

Ideas and Continuations

CONTENTS

Jacek Jadacki

JAKUB KARPIŃSKI IN THE ORBIT OF
THE LVOV-WARSAW SCHOOL

In the Library of the Faculty of Philosophy and Sociology of Warsaw University there is a copy of an article entitled "The Postulate of the Operativity of Definitions in Social Sciences" with the following inscription by the 22-year-old author: "Let me dedicate this work to Professor Kazimierz Ajdukiewicz – Jakub Karpiński." In the book *Przyczynowość w badaniach socjologicznych* [*Causality in Sociological Research*], Karpiński refers to Ajdukiewicz's description of the difference between an experiment and an observation (1985, p. 73); many mentions on Ajdukiewicz's views can be found also in other works.[1] Karpiński's texts also contain references to other members of the Lvov-Warsaw School, including its founder, Twardowski (1992[1978], pp. 8, 18).

From this point of view, stating that Karpiński's academic output revolved around the tradition of the Lvov-Warsaw School is justified, even in the eyes of those readers of his work who do not know that Karpiński was simply Ajdukiewicz's disciple.

Still, I wish to put forward a bolder proposition: his output did not only revolved around the tradition but also grew from it and developed it. To quote Karpiński's words, Karpiński "reviewed" this tradition, "indicating some of its concealed assumptions, in order to [...] further build on it" (1992[1977], p. 30).[2]

---

[1] See for instance (Karpiński 1964).

[2] An example of such creative development, of Ajdukiewicz's erotetics in this case, is the draft of the theory of questions included in *Wprowadzenie do metodologii nauk społecznych* [*Introduction to the Methodology of Social Sciences*] (1980, pp. 136 *ff.*; also see 1965, pp. 36–37). In this case, the development consists in, *e.g.*, adding the typology of completive questions (questions about descriptions, questions about values of variables, questions about

This is not a new idea. It was clearly formulated ten years ago, and recently repeated, by Professor Sułek (2003; 2013). However, I shall attempt to supplement it with my own, and thus: new, justification, and simultaneously present Karpiński's general methodological views, as he was too preoccupied with his political activity to do such work. It will be merely an inventory, incomplete, simplified, and, let us add, ahistorical, as Karpiński would put it, since we cannot allow ourselves to produce a satisfactory systematization, a neat arrangement, which they surely deserve.

An additional incentive for me to work on this matter is Karpiński's relationship to semiotics, which I deal with. Here is what he wrote about it:

> Analysis of culture uses concepts and statements created in the general theory of a sign: in semiotics. One of the basic functions of a sign discussed in semiotics is its reference to reality, called "the semiotic function" […]. Attempts to determine specific features of science refer to its semiotic character, among other things. (1977c, p. 29)

> The semiotic approach to culture […] assumes that the properties of products of culture (symbolic culture) lead to conclusions about what is not a given product of culture. Works belonging to symbolic culture are considered as signs (often fairly complex), and a sign usually informs about something which is not itself. (1992[1978], p. 9)

1. Ontological Tolerance

A specific feature of the Lvov-Warsaw School was ontological tolerance. Assuming a given ontology is not a matter of philosophical «to be or not to be» for members of the School, but rather, it is a matter of theoretical «necessity».

Karpiński's attitude towards ontological questions was the same. For him, a directing question for ontological analyses is not "What, and perhaps how, EXISTS?" but rather "What, and perhaps how, MUST BE DEEMED AS EXISTING, in view of the requirements of science?".

In the brilliant *Wprowadzenie do metodologii nauk społecznych* [*Introduction to the Methodology of Social Sciences*], written in a communist prison, as we learn from Professor Sułek (2013), Karpiński indicates two basic ontological assumptions which scientists usually assume «irreflexively». The first one is "the claim about the existence of the world," and thus, the claim that "utterances formulated within the scope of science

---

scope and questions about relationships between features) and the notion of the proper answer in the broader sense to Ajdukiewicz's concept.

refer to something besides science" (1980, p. 12). The second one is a claim which specifies "the way in which objects of research of science exist" (1980, p. 12). It is worth noting that the second assumption is «named» by Karpiński, but he does not directly state what the assumption claims: *i.e.* that the objects of the research of science exist in a given way.

This is not accidental. The matter is that scientists differ in the way of determining the manner of the existence of objects they research. There are two main ontological "tendencies": monist and pluralist. Karpiński describes them in the handbook, but does not clearly favor any of them, even though he indirectly rejects monism when he writes:

> One may […] do science and research mythology or examine the reality presented in literary works as a scientist. One does not then have to identify the manner of existence of the researched objects with the manner of existence of elements of the real world. One may state that these researched objects exist only intentionally […]. Someone who examines products of culture deals with objects which exist intentionally and states the features of these objects. (1985, p. 105).

On the other hand, in non-didactic texts Karpiński firmly advocates for ontological pluralism. In his opinion, there are many realities: there is the world of things and people; there is the world of events in which objects take part, as well as actions performed by people, including their experiences; there is also the world of culture (artifacts). The latter is not in any meaningful way reduced to the two former, although it is dependent on them to a certain degree: its source is in the world of human actions and experiences, and the basis of its existence is in the world of things and people (1992[1978]; 1992[1975], p. 44). One could say that the world of culture is created by people only to a certain degree; an important part of it (for example, that which includes at least some regularities) is also discovered. We should add that the world of culture cannot exist without some media, which are parts of the world of people and objects, but is not dependent on any specific media in its existence (1992[1975]).

Karpiński does justice to two Polish philosophers who, in his assessment, formulated similar thoughts in a satisfactory and innovative manner: Chwistek and Ingarden. The former was the creator of the modern form of pluralism, which he named "the theory of the multiplicity of realities" (Chwistek 1921), and he attempted to characterize it in a way which would satisfy the 20th-century standards of precision. The latter, having deemed the world of culture a field of purely intentional objects, subtly analyzed this field, and especially literary work (Ingarden 1931) as well as its relationships with the remaining fields of reality. This is how Karpiński comments on it:

> Ingarden created an in-depth and complex theory […] [of the world of culture], most-
> ly referring to those parts of it which are in the field of interest of aesthetics. […] It
> is not easy to determine the character of the relationships between […] [artifacts and
> psycho-physical activities which are their source]; one would have to analyze more
> closely the basis and course of registering the content of culture with the help of ma-
> terial objects, as well as reading this content from the properties of the objects. These
> and similar problems were noted by Ingarden or even solved. (1992[1975], p. 25)[3]

By favoring ontological pluralism, Karpiński rejects both reistic monism and the idealistic interpretation of the world of culture. At the same time, he speaks against idealism within the scope of the theory of law, which ascribes timeless and spaceless character to norms, in an original way:

> If someone assumes the ideal existence of law but does not identify this ideally ex-
> isting law with the law available to experience, then it is hard to protest against such
> actions while still remaining grounded in science, and on the other hand, assuming
> such an existence of law has little influence over what occurs in science, as it refers
> to experience itself (at least insofar as it is an empirical science) (1992[1975], p. 45).

This could be called "an argument from theoretical neutrality"; besides, a similar kind of argumentation is directed at monism, primarily at reistic and psychologistic monism (1992[1975], pp. 46 *ff.*).

2. Causal Relationships

Two further assumptions accepted in science, according to Karpiński, are the following: the assumption of the recurrence of events and the assumption of their coexistence, which can be combined into the claim of determinism. It is supported by, *i.a.*, the fact that people make correct predictions (1980, p. 75).

A specific kind of coexistence is coexistence based on a causal relationship. Karpiński has a good reason to call such a coexistence "conditioning" (1965, p. 41). He devoted a separate monograph, *Przyczynowość w badaniach socjologicznych* [*Causality in Sociological Research*] (1985) to the analysis of this relationship, within the scope of the field researched in sociology.[4]

According to Karpiński (1985, p. 93), there are three necessary conditions for *A* to be the cause of *B*:

---

[3] In a weaker version of this assessment, Karpiński states that "there are strong arguments" in favor of allowing artifacts (especially literary works) "the status of *sui generis* reality, for instance, intentional creations" (1992[1977], p. 69).

[4] See also (Karpiński 1977b).

(1)   *A* and *B* are events;
(2)   *A* is the condition of *B*;
(3)   *A* is not-subsequent to *B*.

The "event" mentioned in point (1) may be understood as either a change in a certain object in a certain aspect in a certain time (that is, in a moment or a longer period), or a state of affairs which persists for a time (1985, p. 8). In the first understanding, the lack of occurrence of the so-called encumbering conditions cannot be considered as a cause.

Describing the cause in the categories of a condition, and thus, identifying the causal relationship with conditioning is a reference to the trend of thought initiated in the Lvov-Warsaw School with the classic dissertation by Łukasiewicz, "Analiza i konstrukcja pojęcia przyczyny" ["An Analysis and Construction of the Notion of Cause"] (1906).

The condition with which a cause is identified in point (2) may either be an arbitrary, sufficient or necessary condition (which is when causal law is non-exceptional), a conductive condition (then causal law has exceptions) or a necessary (essential) component of a sufficient condition. In the case of a cause understood as a conductive condition, we can only state that *A* is the cause of *B* in the degree *S*, where *A* is the conductive condition of *B*, when *B* occurs with *A* more often that without *A* and this is not a false relationship (1985, p. 15). As for causes identified with the necessary component of a sufficient condition, if all sufficient conditions have one and the same component, it is simply a necessary condition; on the other hand, it is not the case when there are sufficient conditions with different necessary components for all of them.

In view of the condition of non-subsequence of the cause in relation to the effect formulated in point (3), Karpiński presents a moderate standpoint. He writes:

> Perhaps [...] the claim of one-way character of causal effects is an analytical claim. (1985, p. 95)

Karpiński notes that sometimes conditions (1)–(3) are supplemented with another condition:

(4)   *A* affects *B*.

In view of this condition, he follows Ingarden (1985, pp. 94–95) in stating the following objection:

> It is sometimes said that the cause affects the effect. However, it is not always easy to explain how an event may affect another event which has not begun yet (affect without necessarily causing it). (1985, p. 14)

This is why he himself does not accept condition (4). In my opinion, as Ingarden would say, it is a premature resignation. Assumption (4), according to which, affecting is 'tied' into a causal relation, which makes it "dynamic," seems most correct. It is enough to reformulate it accordingly so that it does not have any indicated paradoxical consequences. If I could discuss the matter with Karpiński, I would suggest that causal situations should be determined with the help of formulas of the following structure:

(ZP)   The fact that a given x affects a given y is the cause of a certain state (a certain change, in particular) of this y.

The set of events remains the field of relationship of causality, but according to the formula of the type (ZP), it is not the cause which affects the effect but the fact that a given object is affected by something is the cause of the fact that this object changes in a specific manner (or, more generally, is in a certain state).

I would also present to Karpiński, an objection to the characteristics of a process as "the course of events in time" or "a temporal arrangement of events" between which "causal relationships occur" proposed by him (1985, pp. 48, 49). This characteristic contains a categorical mistake: a PROCESS is neither a COURSE nor an ARRANGEMENT of given events, but rather a SERIES (or more generally, a set) of specifically arranged and interconnected events.

Karpiński deems "peculiarities of causal analyses in social sciences" (1985, p. 19) to be the fact that condition (1) is not limited to changes (1985, p. 20), and that condition (4) is very rarely assumed at all; especially in sociology, this causal relationship is very rarely treated as a dynamic relationship, that is, as conveying energy or information to the effect by the cause. Therefore we could say that since "the relationship of causal conditioning is considered […] as a relationship occurring between features or between variables" (1985, p. 20), then the relationship itself may simply be identified with the co-occurrence of these features or variables (respectively, sets of features). Karpiński's general statement on decisions pertaining to terminology may be quoted here, as it well describes his attitude in this respect:

> Perhaps it would be right to use the word "cause" in a more restrictive manner. Yet, it seemed appropriate to review the meanings and situations in which we speak of causes before making the decision to restrict the meaning of the term. Above we used a broader concept of conditioning and distinguished various kinds of it. Realizing this variety may be useful, regardless of how one decides to interpret a causal relationship. Besides, similar distinctions may constitute the basis of relatively rational decision making processes on terminology (1985, p. 20).

Let us also note that the concept of cause reconstructed above is a concept which may be called "an observational concept." Karpiński mentions the so-called operational (respectively, manipulative, or experimental) concept of cause (1985, pp. 72, 98, 100), according to which:

(PE)   *A* is the cause of *B* when *B* occurs after intentional (conscious) induction of *A* by experimenter *E*.

3. The Valuation of the Theory

It would seem that the problem is clear: a given theory is good, always and only, when it is true – or at least when it "aims at truth." History of science shows however that it is a double idealizations: neither "always," nor "only."

Karpiński wrote:

> It has long been thought that science aims at truth. However, it has also long been noted that, firstly, this criterion is not entirely clear and should be fixed; and secondly, that this criterion is not used invariably (in all stages of conduct), and thirdly, even if the criterion is used, it is not the only one (1992[1977], p. 94).

The notion of truth as a regulatory idea is not used "in the usual sense," *e.g.* in deductive sciences; apart from it, the criteria of "economy, informational value, or explanatory power" are used (1992[1977], p. 94). Moreover, the criterion of veracity is not used to evaluate methods; the latter are evaluated according to the cost of their application, "understood as all kinds of losses," *e.g.* "the destruction or damage to the object of research" (1992 [1977], p. 95). It is a kind of a moral criterion. Its presence is especially visible in sociology:

> A characteristic feature of […] [social] sciences is […] evaluating the publication of the results according to the potential influence of the published information directly on the researched persons (1992[1977], p. 99).

Łukasiewicz wrote in a quite similar vein, albeit in a different context, about veracity and the manner of justification of logical truth, in his dissertation *O zasadzie sprzeczności u Arystotelesa* [*On Aristotle's Principle of Contradiction*] (1910).

Karpiński added, modifying (and in this case, radicalizing) the views expressed by Łukasiewicz in his article "O twórczości w nauce" ["On Creativity in Science"] (1912):

> In science we do not aim at the truth about everything. In any case, we do not aim at having knowledge about everything available to everyone (1992[1977], p. 98).

On the other hand, Karpiński's careful phrasing on the influence of the conceptual apparatus on the view of the world echoes Ajdukiewicz's conventionalism:

> Not everyone […] believes that the choice of the language of description is insignificant in science, or that it is a decision whose only justification is convenience, the usefulness of the assumed solutions in the realization of certain objectives within or without science (1992[1978], p. 5).

4. Operationalization of Definition

In the ending of a short text "O jasnym i niejasnym stylu filozoficznym" ["On Clear and Unclear Philosophical Style"], one of the canonical texts of the Lvov-Warsaw School, its founder wrote:

> An author who cannot express his thoughts clearly also cannot think clearly, […] therefore his thoughts do not deserve any attempts to be decoded (Twardowski 1919, p. 348).

Karpiński strongly believed that the lack of clarity of language, criticized by Twardowski here, is one of the sources of "useless banter" (1962, p. 141) in science. One of them, the pseudo-controversy between operationism and anti-operationism (1962, p. 135) was closely analyzed by him in order to isolate real problems concealed in it under layers of verbal misunderstandings, and which the "evolution" of the terms of operationism indicates (1962, p. 136).

The main problem here is the question of the criteria which a procedure of defining in science should fulfill. Karpiński begins with a very liberal definition of a definition. He writes:

> The word "definition" shall be understood very generally here, as any verbal determination of the meaning of a term (1962, p. 140).

At some point definitions in science began to be expected to fulfill the postulate of operationism. The history of the operationism *versus* anti-operationism debate is, according to Karpiński, a history of "the gradual liberalization of the postulate of the operationism of definitions" (1962, p. 41).

Let us assume as our point of departure the following definition of "operational definitions" quoted by Karpiński:

> Operational definitions are such definitions which include a description of verifying operations and certain results of these operations (1962, p. 139).

An operational definition of the term 'T' therefore has the following general outline[5]:

(DO$_1$) If x undergoes operation X, then (x is T when x will be Y).

The property denoted by 'Y' is observable, and its occurrence is the result of operation X.

By reconstructing the postulate of the operativity of definitions, Karpiński refers to Przełęcki (1959) and supplements his proposition of specification (and schematization) of this postulate. He indicates that:

> Operativity is not a sufficient condition for [scientific] correctness of concepts. In order for concepts to be correct, they must fulfill two necessary conditions: (1) [concepts] must have theoretical significance […]; (2) defining operations must be dependable.
>
> Moreover, it is preferable when these concepts fulfill the following conductive conditions: (3) when they are ordering, (4) when their definitions provide important features, (5) when they are accurate; and finally, just as importantly, (6) when […] they are used [and] (7) when their definitions are reporting (1962, p. 147).

In consequence, various methods of verification determine different concepts.

Karpiński proposes to "terminologically differentiate," (1962, p. 150) as he puts it, operational definitions and operative definitions:

> Operative definitions are […] definitions which provide descriptions of observable states of affairs. […] The postulate of defining terms through providing descriptions of observable states of affairs leaves for operationism whatever is rational in it, that is, the readiness to provide an empirical character of scientific terminology; on the other hand, it removes the main drawback of the first stage of this doctrine, namely, denying scientific value to concepts whose definitions are not equipped with descriptions of verifying operations (1962, p. 150).

Therefore, it can be stated, again in a simplified form, that it is ultimately postulated that definitions of the term 'T' have the following scheme:

(DO$_1$) x is T, when x is Y.

Naturally, 'Y' is an observational term in (DO$_1$). This postulate can probably be identified with the postulate of the diagnostic character of definitions (1985, p. 145). Diagnosticity interpreted in this way provides intersubjective communicability and intersubjective controllability of scientific claims to scientific terminology.

---

[5] To be more precise, it is one of possible schemes. This scheme can be weakened by adding an implication rather than equivalence in the consequent, and by preceding the consequent with the auxiliary "most probably," *etc*.

For the purposes of psychological and sociological practice, Karpiński expands the postulate of operational defining scientific terms to include the so-called indicators (in particular, indicators which he calls "definitional indicators," as opposed to dependency indicators), that is, properties of the type α which are indicators of the property β based on the fact that possessing the property β was defined through possessing properties of the type α:

> The postulate of the operativity of the definitional indicator of a given property is synonymous with the postulate of the operativity of a definition of a given PROPERTY. (1962, p. 152)

It is worth noting that Karpiński does not only write about operational definitions but also constructs a certain general theory of definition which, incidentally, is the source of certain difficulties.

First of all, he contrasts semantic definitions with nominal definitions. Semantic definitions are supposed to express assigning: the proper denotation to a given term (1985, p. 100); nominal definitions introduce the defined expressions into the language, "leaving aside semantic relationships" (1985, p. 103). An example of the former is the formulation: The term "social classes" signifies big groups of people which differ from each other in relationship to the means of production. An example of the latter is the formulation: Instead of saying "a situation in which an individual holds at least two convictions such that accepting one of them substantiates the rejection of the other one," we can say "cognitive dissonance." Karpiński is not alone in overlooking the fact that the latter definition (and any other nominal definition) can easily be transformed into an equivalent semantic definition by stating, *e.g.*: The term "a cognitive dissonance" signifies a situation where an individual holds at least two convictions such that accepting one of them substantiates the rejection of the other one – and because of this paraphrase the sentence "Cognitive dissonance is a situation where an individual holds at least two convictions such that accepting one of them substantiates the rejection of the other one" will also be an analytical thesis.

Secondly, Karpiński maintains the traditionally distinguished type of real definitions, that is, definitions which provide "an unambiguous characteristic" of a given object (1985, p. 143).

A closer analysis indicates that the differences between the so-called semantic, nominal and real definitions can ultimately be reduced to differences of modes of expression.[6]

---

[6] This is at least true for identity definitions; equivalent definitions (with equivalence in the function of a definitional connector) obviously do not have an identity paraphrase for purely syntactic reasons.

5. Methodological Schemes

The main methodological postulates of the Lvov-Warsaw School are considered to be the postulate of clarity of language mentioned before and the postulate of sufficient justification of the held, and especially, proclaimed, views. These two postulates may be expressed in short: the greatest possible precision and the best possible argumentation. Still, there is a third postulate: the postulate of correct classification.

Karpiński referred to the latter postulate when he lamented the "low degree" of specification of individual research methods used in sociology, so that "they are far from being algorithms," but also because of "strange rules" (or actually, the lack of explicit rules) for classifications of the researched fields of objects done by sociologists (1976, p. 57). A striking example of this latter methodological fault is what passes as classification of methods used in sociological research: it often occurs that traditionally distinguished kinds of research methods are the effect of a cross between several simpler, «single-rule» classifications.

In place of traditional, methodologically faulty, classifications of sociological methods ("schemes"), Karpiński proposes the following typology[7]:

(a)   structural research – where not only objects of a given group in isolation are taken into consideration, but also the relationships between them;

---

[7] Let us add that Karpiński's propositions do not exhaust the typological variety achievable through using all possible combination possibilities. Such a supplementation of Karpiński's analysis would be a pleasant and useful task to do for those who would like to continue his work. It would be best if it was done in his style, a beautiful example of which is the range of typology of social structures proposed by him (1992[1977], pp. 27 *ff*.), and another example of which, *in extenso*, is provided below (see the introductory characteristic of the comparative method). Such a supplementation would require making a certain «qualitative» correction of Karpiński's proposition. For instance, specific phases of a discussed group, examined diachronically, may be treated as two different groups examined comparatively; with this interpretation, diachronic research is a sub-type of research of acomparative kind. Another example, which is also mentioned by Karpiński on one occasion (1976, pp. 66): instead of examining the relationships between members of a given group as individuals, one could examine the relationships between them as elements of specific sub-classes of that group. Besides, a decision would have to be made as to what actually the method here is; following Karpiński's remarks on schools in science, structural, comparative and diachronic research was not isolated because of the applied research method, but rather because of the object of research (the properties of the elements in a given group, the relationships between the elements of that group, the relationships between two groups, *etc*.). From this point of view, experimental research, certainly distinguished in the manner it is conducted in, would not be "both" comparative and diachronic, as Karpiński postulates. The only issue is that the experimental method is used in both kinds of research.

(b)   comparative research – where not one but at least two different groups with separate matrices of data are examined, but which have at least one «common» column of variables (indicators);

(c)   diachronic research (including panel research) – where for one group, or more precisely, for different temporal phases of this group, there is more than one matrix of data (with the same, or similar, or completely different columns of indicators);

(d)   experimental research – where apart from observation of the examined group an experiment is conducted, and therefore, the parameter of "the researcher's influence on the group" (1976, p. 63) must be taken into consideration.[8]

Let us perhaps add that Karpiński is prone to interpreting the matrices of data mentioned before, constructed by a sociologist, in the categories of responses to completive questions in Ajdukiewicz's understanding.

The kind of research analyzed in the most detail by Karpiński, in a separate work (1977a), is comparative research. The initial fragment of this work is perhaps worth quoting as a telling example of Karpiński's scientific prose:

> In a very broad sense, "comparative research" is what can be […] called research where at least two objects are examined in at least one aspect. In research of this kind, various relationships between objects may be examined: difference and equality in a given aspect, but also being greater or smaller, and differences and relationships (quotients of dimensions) of objects in a given aspect (depending on whether the variable in view of which the objects are examined is a classifying, ordering, additive or quotient variable). In all these cases, objects are compared, and the comparison leads to determining their difference or similarity, being greater or smaller, or the size of the difference and the relationship between the objects. The result of comparative research interpreted thusly can be classification of objects based on their comparison. Conversely, in order to make a classification, we need a comparison (and possibly a measurement) of the objects.
>
> "Comparative research" in the narrower meaning is what can be called research where at least two groups are examined (instead of any two objects), and where both the features of elements of this group and the features of this group are examined. In this interpretation, comparative research is many-leveled in the sense that it concerns both the group and its elements. (1977a, p. 536)

In the course of the discussion of methodological schemes, Karpiński compared the notion of scheme with the notions of paradigm and idealization. It is an interesting issue, although its interpretation by Karpiński

---

[8] Karpiński also discusses survey research as a separate type. Yet, the methodological status of such research is unclear, especially its relationship to empirical research. The same is true for the so-called analysis of content in the sociology of literature (1992[1977], pp. 77 *ff.*), which examines "social references" of the properties of literary works (1992[1977], p. 81).

poses several objections. The methodological scheme is, as it was demonstrated, a kind of a method used to research a specific field of objects. A paradigm is, as it seems, a «substantive» theory (usually reductionist), accepted as current in a given time in this field although, let us admit, a paradigm may also consist of the obligation to use a specific methodological scheme. We could say that idealization is a procedure which replaces the researched reality with abstracted "model situations" (1992[1977], p. 79; see 1985, pp. 89, 91). This is exactly what justifies the statement that "in science, […] non-existing phenomena are also examined" (1992[1978], p. 89). However, this special kind of a methodological scheme is not valued by Karpiński. Still, his main objection does not sound very convincing: it is that in sociology, as well as other related disciplines, and as opposed to physics and related disciplines, "it is still hard to control the correctness of approximations to reality" (1976, p. 69), which may result in the loss of semantic function by the idealizing theory.

6. Identification of Schools

At the beginning of Karpiński's very promising career, the famous work by Ossowski, *O osobliwościach nauk społecznych* [*On the Peculiarities of Social Sciences*] (1962) was published. One of the chapters was entitled "Standpoints and schools." This is what Karpiński referred to when he sought substantive criteria for identification of schools in science.

He drew from an earlier tradition of the Lvov-Warsaw School, that is, to the distinction between actions and the products of these actions, introduced by Twardowski and subsequently commonly accepted (Twardowski 1912). When we speak about science, we must strictly observe this distinction. Science as science-creating activity is different from science as the product of this activity. A sociologist may be interested in either of them.

Let us begin with functional understanding of science. This is what Karpiński writes about science in this aspect:

> Science is a collective activity consisting in acquiring, preserving and processing information (1977c, p. 28). Scientific activity may be treated as a set of actions. The choice of the field of research and the choice of terminology are preliminary actions. They are followed by formulation of problems (possibly in the form of questions), choice of methods and formulation of claims (1977c, p. 32). If we decide to treat doing science as explorative activity, then repeating claims should probably be counted as didactics rather than science understood as creative work (1977c, p. 33). The choice of a specific kind of researched field, terminology, questions, claims, methods or explanations restricts freedom within science. […] Schools in science may be subject to methodological dogmatism (1977c, p. 35).

Let us add a third element to the couple: activities-products, that is, subjects of these activities, and it becomes clear why Karpiński warns against calling the sociology of scientists as subjects of science-creating actions "sociology of science" understood as the sociology of products of actions undertaken by scientists (1992[1978], p. 6).

Schools in science, which became objects of interest for Karpiński, were usually distinguished as schools in science interpreted functionally. Thus, they were simply certain specific social groups. Their identity, just as in the case of the identity of any society, was determined by internal connections (energetic and especially informational), the degree of organization and the feeling of belonging (either consciously declared or being a derivative of ascribing such belonging by people from the outside).

However, Karpiński sought more than just purely sociological criteria, that is, substantive criteria. What distinguishes science understood as creation from other spheres of culture is having, or at least postulating, a semantic reference; art is on the other side of the spectrum in this respect. This is why a substantive criterion (in Karpiński's sense) of distinguishing scientific schools (trends) is, either conscious or unconscious, choice of given factors of reference. In particular, this is the choice of:

(a)   the scope of research (the issue is about "which fragment of reality is chosen for examination" (1977c, p. 30)): *e.g.* observable or non-observable objects, historically or non-historically distinguished, that is, with respect of similarity);
(b)   the language for description of the field of research;
(c)   the manner of examination of that field (*e.g.* introspection, understanding);
(d)   research questions;
(g)   the set of established statements;
(f)   the manner of substantiating statements;
(g)   a kind of desired explanations (*e.g.* one-factor or many-factor);
(h)   the manner of creating a theory (*e.g.* sticking to or departing from experience, applying or not applying mathematics);
(i)   an extra-scientific program which leads the research (*e.g.* meta-scientific or ideological).

Karpiński was fully aware that specific positions on the list have complicated correlations. In particular, *e.g.*:

> It is sometimes difficult to determine whether […] [in the case of distinguishing schools of science], the examined problems are connected with the choice of the field of research or the choice of terminology (1977c, p. 30).

Various choices pertaining to points (a)–(i) are the subject of disputes between schools as well as between representatives of various schools. Karpiński thoroughly reviewed the kinds of the disputes. What is more, he constructed an outline of the theory of the disputes in science which has not since become obsolete. According to Karpiński:

> [Disputes are] situations in which someone claims something, for instance, thesis R, and communicates the conviction that if R then not T, and the conviction that someone else claims or could claim the thesis T (1965, pp. 31–32).

Theses R and T described in such manner are called contentious theses. In other words, contentious theses are theses which are mutually exclusive (that is, they cannot both be true at the same time). There is nothing… contentious in such an interpretation of contentious THESES. However, what is original is the interpretation of a contentious SITUATION proposed by Karpiński. It is usually thought that such a situation should include two characters: two «sides» which are in dispute. Let them be opponents A and B. Therefore, we have:

(1)   A claims that p.
(2)   B claims that q.
(3) $p \Rightarrow$ not-q.

Yet, Karpiński believes that in order for a dispute to occur one "active side" is sufficient. The idea is that the case is as follows:

(1′)   A claims that p.
(2′)   A claims that B claims that q.
(3′)   A claims that ($p \Rightarrow$ not-q).

In this interpretation, Karpiński can easily introduce the notion of pointless dispute, when A is mistaken in claiming what is claimed in (2).

One may assume one of the following positions towards contentious theses:

(a)   not accept either;
(b)   accept one and dismiss the other;
(c)   accept both.

If attitudes (a)–(c) are legitimate, then in the case of (a) we are dealing with an irresolvable dispute, and in the case of (b) and (c) we are dealing with a resolution of the dispute. In the last case, we must accept the dispute as only apparent, that is, agree that the so-called contentious theses do not in fact preclude each other. This is the case not only when 'p' and 'q' are sentences which do not fulfill condition (3), but also when at least one of them is not a sentence in the logical sense at all (1965, p. 46). According to

Karpiński, non-utilitarian evaluations are of this character. He wrote firmly about such evaluations:

> A (non-utilitarian) evaluation can usually be treated as an expression of approval (or disapproval) for the occurrence of a phenomenon; the dispute over whether the approval is correct is not resolvable. Sometimes we express our approval directly, by using the predicate "good" or "bad," but in science formulations of the type: "It is good that this and this occurs" are extremely rare. If we evaluate phenomena in a scientific work, if we assume an evaluating attitude towards them, we probably do it without the mentioned predicates and make use of much greater possibilities of extra-cognitive functions of language as well as of more diverse impressive and expressive functions of utterances. (1965, p. 44)

***

Finally, let us present a general impression of a reader who knows the work of the Lvov-Warsaw School fairly well. The simplicity of Karpiński's style is striking. He even formulated… a simple directive in this matter:

> In order to speak of something, it is good to simplify the matter somewhat, at least at the beginning (1992[1977], p. 29).

Since "the propensity of the human mind to simplify phenomena […] facilitates their understanding" (1976, p. 70), the simplicity of Karpiński's works is paired with their clarity. What Karpiński ascribed to Tatarkiewicz's works: "clarity, accuracy and explicitness" (1992, p. 106), was also fulfilled by him to a high degree. There is a… simple test to prove it: it is hard to summarize his statements (just as in the case of Tatarkiewicz), in the sense of an original account of what he wrote, rather than what about. This is the result of following the rule which Karpiński himself, according to Professor Sułek (2013), put in the simple words, "The shorter the better."

The fulfillment of these ideals entails sticking to a specific path, as professor Sułek would put it, namely the path initiated by Twardowski: the path, the direction of which was later determined by his most eminent disciples: Łukasiewicz[9] and Ajdukiewicz[10] (and to a much lesser degree, let

---

[9] Karpiński took over from Łukasiewicz *i.e.* the theory of reasoning.

[10] What Karpiński took over directly from Ajdukiewicz was a large part of the ontological and logical conceptual scheme, *i.e.,* the concepts of the state of affairs and an occurrence; the concept of language and, more generally, semantic concepts; the concept of measurement, experiment and observation; the concept of a theory; the concept of a question and an answer, *etc.*

us add, *e.g.* Kotarbiński, who followed the roadside more often than not, or even strayed from the path altogether).[1]

Jakub Karpiński followed this path, the path of the Lvov-Warsaw School, in science.

We may only regret that it was for such limited time.

University of Warsaw
Institute of Philosophy
e-mail: jjadacki@gmail.com

REFERENCES

Chwistek, Leon
1921 *Wielość rzeczywistości* [*Plurality of Realities*]. In: Chwistek (1961), pp. 30–105.
1961 *Pisma filozoficzne i logiczne*, t. 1 [*Philosophical and Logical Writings*, vol. 1]. Warszawa: PWN.

Ingarden, Roman
1931 *O dziele literackim* [*The Literary Work of Art*]. Warszawa: PWN.

Karpiński, Jakub
1962 Postulat operacyjności definicji w naukach społecznych [The Postulat of Operativity of Definitions in Social Sciences]. *Studia Socjologiczne* 4(7), 135–153.
1964 Metodologia nauk w twórczości K. Ajdukiewicza [The Metodology of Sciences in K. Ajdukiewicz's Works]. In: Karpiński (1989), pp. 97–105.
1965 Typy sporów na terenie nauk społecznych [Types of Controversies in the Domain of Social Sciences]. *Studia Socjologiczno-Polityczne* **18**, 31–48.
1975 Ludzi i zbiorowości. Zagadnienia «poziomu analizy» w socjologii [People and Communities. Problems of the «level of analysis» in sociology]. *Studia Socjologiczne* **3**(38), 131–151. Reprinted in: (1985), pp. 102–120.
1976 Schematy metodologiczne [Methodological Schemas]. *Studia Socjologiczne* **1**(60), 57–70. Reprinted in: (1980), pp. 306–328.
1977a Badania porównawcze [Comparative Research]. *Zagadnienia Naukoznawstwa* **4**(52), 536–548. Reprinted in: (1980), pp. 197–219.
1977b Najprostszy przypadek analizy przyczynowej [The Simple Case of Causal Analysis]. *Prakseologia* **4**(64), 13–31. Reprinted in: (1985), pp. 21–34.
1977c O szkołach w nauce [On Schools in Science]. *Więź* **3**(227), pp. 28–36.
1980 *Wprowadzenie do metodologii nauk społecznych* [*Introducion into the Methodology of Social Sciences*]. Warszawa: WSPiZ im. L. Koźmińskiego.

---

[1] Let us note that Kotarbiński's flagship ontological «invention»: reism (or somatism), was met with Karpiński's critique: harsh in content but gallant in phrasing (1992[1978], pp. 6 *ff.*; 1992[1975], pp. 46 *ff.*).

1985 *Przyczynowość w badaniach socjologicznych* [*Causality in Sociological Research*]. Warszawa: PWN.

1989 *Nie być w myśleniu posłusznym* (*Ossowscy, socjologia, filozofia*) [*Not to Be Obedient in Thinging* (*Osssowskis, Sociology, Philosophy)*]. London: *Polonia*.

1992 *Kultura i wielość rzeczywistości. Szkice o kulturze* [*Culture and the Plurality of Realities. Sketches on Culture*]. Lublin: Wydawnictwo FiS.

Łukasiewicz, Jan

1906 Analiza i konstrukcja pojęcia przyczyny [An Analysis and Contruction of the Notion of Cause]. In: Łukasiewicz (1961), pp. 9–62.

1910 *O zasadzie sprzeczności u Arystotelesa* [*On Aritotle's Principle of Contradition*]. Kraków: AU.

1912 O twórczości w nauce [On Creativity in Science]. In: Łukasiewicz (1998), pp. 9–33.

1961 *Z zagadnień logiki i filozofii. Pisma wybrane* [*From the Problems of Logic and Philosophy*]. Warszawa: PWN.

1998 *Logika i metafizyka* [*Logic and Philosophy*]. *Miscellanea.* Warszawa: Wydawnictwo WFiS UW.

Ossowski, Stanisław

1962 *O osobliwościach nauk społecznych* [*On Pecularities of Social Sciences*]. In: Ossowski (1967), pp. 125–316.

1967 *O nauce* [*On Science*]. Warszawa: PWN.

Przełęcki, Marian

1959 Operacjonizm [Operationism]. In: (1993), pp. 23–38.

1993 *Studia z metodologii formalnej* [*Sketches from the Formal Methodology*]. *Filozofia Nauki* **1**(2–3) [special issue], 0000–0000.

Sułek, Antoni

2003 Jakub Karpiński. 17 czerwca 1940 – 22 marca 2003 [17th of June, 1940 – 22nd of March, 2003]. *Studia Socjologiczne* **3**(170), 5–9.

2013 Światło Jakuba Karbińskiego [Jakub Karpiński's Light]. *Plus – Minus.* Weekly of *Rzeczypospolita* **19**(11–12), 18.

Twardowski, Kazimierz

1912 O czynnościach i wytworach [On Actions and Results]. In: Twardowski (1965): pp. 217–240.

1919 O jasnym i niejasnym stylu filozoficznym [On Clear and Unclear Philosophical Style]. In: Twardowski (1965), pp. 346–348.

1965 *Wybrane pisma filozoficzne* [*Selected Philosophical Writings*]. Warszawa: PWN.